UNITED STATES OF AMERICA

NUCLEAR WASTE TECHNICAL REVIEW BOARD

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FULL BOARD MEETING

***

Task Force Studies, MPC Concept,

System Studies and Performance Assessment

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Doubletree Hotel

Washington Room

South Tower

300 Army-Navy Drive

Arlington, Virginia

Wednesday, January 12, 1994

The above-entitled meeting was convened, pursuant to adjournment, at 8:15 a.m.

PARTICIPANTS:

John E. Cantlon, Chairman of the NWTRB
Clarence R. Allen, Member of the NWTRB
Garry D. Brewer, Member of the NWTRB
Edward J. Cording, Member of the NWTRB
Patrick A. Domenico, Member of the NWTRB
Donald Langmuir, Member of the NWTRB
John J. McKetta, Member of the NWTRB
D. Warner North, Member of the NWTRB
Dennis L. Price, Member of the NWTRB
Ellis D. Verink, Member of the NWTRB
Jeremy Boak, YMPO
Holly Dockery, Sandia National Laboratories
Robert Andrews, M&O INTERA
Abe Van Luik, M&O, INTERA
Jean Younker, M&O, TRW
James Duguid, M&O, TRW
Scott Sinnock, M&O, TRW
Max Blanchard, DOE
Robin McGuire, EPRI
Rip Anderson, SNL
John Garrick, Pickard, Lowe & Garrick

NWTRB STAFF PRESENT:

William D. Barnard, Executive Director
Dennis G. Condie, Deputy Executive Director
Leon Reiter, Senior Professional Staff
Sherwood Chu, Senior Professional Staff
Carl Di Bella, Senior Professional Staff
Daniel Fehringer, Senior Professional Staff
Russell McFarland, Senior Professional Staff
Victor Palciauskas, Senior Professional Staff
Paula Alford, Senior Professional Staff
Frank Randall, Senior Professional Staff
Vicki Reich, Librarian
Karyn Severson, Congressional Liaison
Helen Einersen, Executive Assistant
Linda Hiatt, Management Assistant

C O N T E N T S

P R O C E E D I N G S

[8:15 a.m.]

PERFORMANCE ASSESSMENT

SESSION INTRODUCTION

DR. NORTH: Good morning. Welcome to the second day of this meeting of the Nuclear Waste Technical Review Board. My name is Warner North, and I will be chairing the session today.

Today, we will be focusing our attention on performance assessment. It may be useful to summarize some of the board's recent activities in this area. Since its first report, the board has emphasized the need for DOE to establish a strategy of iterative performance assessment, that would not only help determine compliance to standards and regulations, but would also assist DOE in assessing progress and setting priorities in a very complex program.

At the April 1992 board meeting, we were briefed on Total Systems Performance Assessment (which I will abbreviate as TSPA) studies by Sandia National Laboratories and the Pacific Northwest Laboratory (PNL). I might add that when we talk about the total system in the context of performance assessment we mean the total disposal system, as opposed to the total waste system which was the focus of our attention yesterday. This system is in the context of the Yucca Mountain Project, if we proceed, such that the repository is proposed and then licensed. The board has stressed the need for the DOE to look at the total waste system, that is, transportation, storage and disposal, which was our focus yesterday. Today we are going to focus just on the subset of Yucca Mountain, the disposal system.

In our Sixth Report we commended the DOE for starting the iterative performance assessment process and we are happy that today we will hearing about the second iteration in that process. With respect to the previous iteration, TSPA 1991, the board raised questions regarding the assumed behavior of the waste container and cladding after an assumed failure, the exclusion of colloidal transport, the effects of high percolation rates and the treatment of fracture flow, in particular, the impact of the Ghost Dance Fault on the hydrologic regime. SNL, Sandia, used the so-called WEEPS model, which some have argued bounds the worst case scenario of fracture flow.

Gaseous carbon 14 emerged as the dominant radionuclide release, and in some cases exceeded the 40 CFR 191 standard, depending on what one assumed about the permeability of Yucca Mountain to gases. According to these studies, volcanism did not result in a violation of the standard, even if it was assumed to occur. In addition, the PNL study looked at a tectonic-induced rise in the water table, but gave no insight as to what would happen if the repository were to be flooded.

The board also suggested that increased outside review, more sensitivity tests and greater transparency would serve future efforts well. Greater transparency can turn what might appear to be a complicated exercise in mathematics and statistics into an understandable evaluation of the proposed repository's ability to contain and isolate waste.

Many of the questions and concerns raised are typical of those that might arise in early stages of a developing risk assessment. That does not mean that performance assessment must attain a high level of analytical sophistication before it can be used. On the contrary, the board's main recommendation was that DOE begin immediately to use TSPA and other relevant studies to help assign priorities and to identify critical data needs in the Yucca Mountain project.

In several of its past reports, the board also touched upon the issue of expert judgment. In our Fourth Report, we recommended that the DOE convene a workshop on expert judgment. The workshop was held in November, 1992. We are looking forward to seeing to what extent the DOE makes use of the excellent recommendations coming out of that workshop.

In July, 1993, the DOE briefed the board on its plans for the latest TSPA. Today, we anticipate seeing the results of new data, increased sophistication in modeling, and a wide range of sensitivity studies. Topics to be covered include the impact of different thermal loading scenarios, waste emplacement schemes, and corrosion models.

We will also hear about the effect of shifting to an individual dose criterion, as is now being considered by a committee of the National Academy of Sciences, and of a longer performance period. We have emphasized to the DOE the need to concentrate, in their presentations, on key assumptions, important results, and how the information is being and will be used. We are especially interested in the relationship of the performance assessment activities to the detailed scientific studies and engineering efforts in the Yucca Mountain Project.

We have asked Scott Sinnock to provide us with some insights as to how the conclusions of performance assessment have changed over the years. In addition, Robin McGuire will describe the latest results from the EPRI performance assessment. The board has always been impressed with the ability of the EPRI team to provide clear and understandable results.

We will be hearing from Rip Anderson, with the goal of gaining insights from the performance assessment for the WIPP site that should prove helpful in the Yucca

Mountain effort. Finally, we have asked John Garrick, a eminent risk analyst, to provide some comments from his perspective and wide experience. The biographies of the speakers should be available from the TRB staff.

We have a time problem. Today, Dr. Garrick and several members of the board have to leave the hotel sharply at 4:30. We would like to encourage questions and discussions, and we have alloted time that you will see on the schedule for public comments, at the end of the day. I think to be safe, I am going to have to play strict timekeeper. That means for the presenters from DOE and the other speakers, I would really ask that you stay on schedule and allow about ten minutes at the end of the alloted time for discussion and questions.

To make sure that happens, I am going to be holding up my hand with 15 minutes to go. At ten minutes to go, I will ask you to finish up. At five minutes to go, I am going to insist on it, so that we can distribute the time allocated in the schedule and give ourselves an hour for lunch, which yesterday's experience would suggest is about the minimum to get everybody fed and back here.

In this fashion, we hope to have at least 15 minutes at the end of the day for public discussion. The order, as usual, will be that the board gets the first chance to ask questions. The staff, the second. If time permits, we will take questions from the audience. I would also ask in the public discussion period that we keep the questions or comments one apiece and try to keep them relatively short, so that everybody who wants to speak will get the opportunity.

Apologies, but with the tight schedule I think this is the only way we can proceed and be fair to everybody.

That ends my introductory comments. I am seven minutes ahead of schedule. At this point I am going to introduce Jeremy Boak, who will introduce the speakers to discuss the DOE performance assessment.

INTRODUCTION TO DOE PERFORMANCE ASSESSMENT

[Slides.]

DR. BOAK: Thank you, Dr. North. Part of the Christmas bounty for me at least, was a couple of documents which I am very pleased to have. There was a bit of assembly required, and I am still tinkering with it. I wanted to share the pleasures of the holidays with a larger crowd. I am very happy to have the two TSPA draft documents in hand. I haven't read everything in them, but there's a great deal there to go through.

Hopefully as we proceed we will work on the transparency part of it, and have something in the end of the spring or early summer that will be a summary document from the DOE that tries to address those questions of transparency while referring you to the meat of the matter contained in the two large documents that the management and operations contractor and the Sandia National Laboratory have given to us.

I want to talk about a couple of things. First of all, remind you of some of the objectives we had in TSPA-93, go through some of the differences and talk about some of the details that we have added to total system performance assessment in this iteration. I will show you quick the participants involved, and talk about the future of what we plan to do with TSPA-93.

[Slides.]

DR. BOAK: This slide is identical to one that I showed you in July. The major objectives we had were related to addressing in somewhat more detail the engineered barrier systems, bringing better detail to engineered barrier systems into our TSPA by looking at different thermal loads, by looking at two different emplacement modes, in drift emplacement and borehole emplacement, to look at several different waste package designs, so that we would get a little better idea of the actual effect of repository heating as well as the engineered barrier system performance on the total system performance.

We also wanted to bring in some of the wealth of new site characterization data that we have been getting, and start incorporating that into our models. It was our objective as mentioned, to look at dose as a measure of performance, not only because we thought there might be some changes in the standard that we have but also because it was an important way of comparing some of our data to other performance assessments in other areas.

Finally, we wanted to incorporate more readily and more completely in the actual initial issue of the total system performance assessment, some of the sensitivity uncertainty analyses that sort of came out after the TSPA 1991 and wanted to have them fully incorporated.

[Slides.]

DR. BOAK: There are some differences. Again, as I said, we did in fact incorporate coupled thermal and hydrological processes for aqueous flow. It was an important change in our previous iteration. We essentially had ignored the thermal effects. We wanted to enhance, again, the radionuclide inventory and improve our decay and solubility modeling for transport. Those were more or less incremental gains over previous years.

We wanted to look at the question of statistical and geostatistical correlations in a way in which we had not in the past. It's quite a daunting task to take on this geostatistical issue, and we didn't get as much of it in as we would like to have. Again, we wanted to look at the question of fraction-matrix coupling. We wanted to go beyond the way in which we had implemented it before in which we felt we had bounded the degrees of fracture-matrix interaction but had not, as far as we could tell, we had some doubts about whether we had actually bounded the performance effects of differing degrees of fracture-matrix interaction. As I mentioned, we wanted to look at different engineered barrier systems.

[Slides.]

DR. BOAK: I want to emphasize graphically, the two different modes of disposal we had in mind, the SCP design with spent fuel, and high level waste glass disposed of in boreholes in the floors of drifts. These are relatively thin walled waste packages in general, for operational reasons.

Alternatively, thicker walled, generally larger waste packages emplaced in drifts, you have had presentations on some of these design concepts. These generally require higher thermal loads than the SCP design load, although we did actually have one case that we ran in which the large waste packages were spaced out far enough to have SCP thermal load.

We looked at three different thermal loads. Sandia evaluated the 57 and 114 kilowatt per acre loads, with the vertical emplacement, SCP type design, alloy 825 at about just under a centimeter thick, and in-drift emplacement MPC like waste package with a ten centimeter overpack. The M&O evaluated all three loads also, both designs with variable overpacks and variable alloy 825 corrosion resistant internal waste package.

[Slides.]

DR. BOAK: I know who the participants are by heart, so I don't need the shading that you have in your handouts. The M&O, actually, we pulled data from virtually every part of the M&O so that they essentially performed entirely across the pyramid, from the design features that we got from some of the M&O design folks all the way up to the modeling done in RIP in total system performance assessment. Sandia provided a wide range of support for very broad characterization of total system performance, but substantial detail on the subsystem performance and some mechanistic process modeling.

The major feeds from Livermore, Los Alamos, LBL and the U.S. Geological Survey, we got a great deal of valuable data from all of the participants. At the lowest level of the pyramid where site design and data gathering and some of the mechanistic process modeling, Lawrence Livermore provided subsystem modeling in terms of runs at the YMIM model and actually passing the YMIM model that they have developed over to Sandia so that it could be incorporated into their total system modeling. They also provided extensive interaction in the development of RIP and in further development and use of RIP during this total system performance assessment.

[Slides.]

DR. BOAK: We have done some evaluations for periods greater than 10,000 years. The major reasons for doing that are, of course, contained in here. One, is to build a robust safety case involving more than one performance measure. The second, to use one that in fact the National Academy of Sciences is looking. They are certainly reconsidering the question of longer time periods. When considering dose it's fairly common to look towards the peak doses and start asking questions about when those happen.

We wanted to go beyond that, to see whether in fact performance over a periods longer than 10,000 years could be reasonably understood by looking at the 10,000 year period as well. That's a critical assertion that was made by the EPA in its 40 CFR 191 standards. They said if a site performs within these standards for 10,000 years its performance is adequate for the time period after that.

I am going to skip over the next performance assessment model integration and go on to the schedule, in order to give Dr. North a little bit of extra time.

[Slides.]

DR. BOAK: We have a number of near term program decisions that we are hoping to support, both with TSPA 1993 and with follow on iterations. We are currently gearing up a site suitability evaluation, a successor to the earlier site suitability evaluation. We have a great number of design decisions that need to be moving ahead as we move towards the completion of advanced conceptual design for the repository. We certainly hope to provide some useful insights to the testing program. I think we have done that and I think we can do that with this and subsequent iterations.

Of course in the longer term, the whole suite of activities that must be completed if Yucca Mountain continues to be a viable candidate site, including advanced conceptual and license application designs, preparation of a site recommendation report looking at the suitability issue, and the environmental impact statement and license application itself.

[Slides.]

DR. BOAK: The schedule for finishing off TSPA-93 is fairly ambitious. We keep finding new things that need to go into the products that we are getting out of TSPA-1993. This was done shortly before the end of the year. We are fairly close to on schedule with these products here. It looks to me like my staff, including myself, are a little bit behind schedule on this particular item. I think we are moving ahead all right.

Publication has been a tricky thing because these documents are so large, and there are a lot of small corrections to be made. I hope we can get in ahead of this schedule. During the same time we will be preparing a summary document that merges the two major documents that we have now, and tries to address the question of transparency. We hope to be getting that out not only for external review in the summer, but we also hope to convene a review of it by the performance assessment advisory group of the OECD NEA, and finish that up by the time the fiscal year is over so that we can use the suggestions made by our reviewers in our next iteration.

[Slides.]

DR. BOAK: Looking into the grander scheme of things, I have shown here some of the major project milestones for design up here for EIS, the major model stages that the USGS has described in presentations to you, and continuing evaluation of the suitability of the site. I have only shown the current one that is just beginning and one further. There will probably be other suitability evaluations, leading up to the final one. These are derived from the 2001 exercise. We know to some extent that has slipped.

We have not, however, produced a baseline version of this so that we can look at how we want to connect our series of total system performance assessments to these various major milestones. At present, I think given this schedule, they reasonably feed the various products that are needed. That will be something that will have to evolve as we go along and change and slide some of these dates around.

I think right now they are reasonably well phased so that we can be providing useful products to the major project milestones.

Those are the main things I wanted to say. I will, at this point, turn it over to Holly Dockery and then to Bob Andrews, to talk about the work that our participants have done in completing this total system performance assessment.

DR. NORTH: Before Dr. Dockery proceeds, do any members of the board have any questions?

[No response.]

DR. NORTH: Any of the staff?

[No response.]

DR. NORTH: If not, let's keep going then. We are five minutes ahead of schedule. Dr. Dockery. I will remind everybody asking questions, please identify yourself for the transcript at the beginning.

TOTAL SYSTEMS PERFORMANCE ASSESSMENT

FOR YUCCA MOUNTAIN

SANDIA NATIONAL LABORATORY SECOND ITERATION

[Slides.]

DR. DOCKERY: Good morning. I am going to be talking about the Sandia contribution to the TSPA exercise, which we have called Total System Performance Assessment for Yucca Mountain - Sandia National Laboratory Second Iteration, 1993. The person who put together our whole document, after looking at the size of it, said she wanted to call it the combination plate, or at least the whole enchilada. It's a massive document.

I also wanted to make the point that as Jeremy said, the production of the Sandia second iteration is essentially complete. It's into policy review right now. It spans a greater range of scenarios and processes than did TSPA-1991. We had a whole suite of people that helped us put this TSPA together. As you can see, it was a multi-participant effort.

While we have a large number of Sandians involved in producing this document of which Mike Wilson is the primary contributor to this document along with Jack Gauthier and Rolly Barnard, the other people that helped us out quite a bit were the Lawrence Livermore folks with the

YMIM model for source term as well as the geochemistry information that came from the Los Alamos group. We really brought together a lot of different people in different expertise, to try to span a wider range of information.

[Slides.]

DR. DOCKERY: This one isn't in your package. I did want to sort of briefly let you know where we were going. What I am trying to do with this presentation is talk about how we set up the problem. What was the process we went through to try to tag on from one iteration to the next iteration, how were we building from one to the next step. And then, go over a very brief review of the new data and analyses, the critical assumptions if you will, to help give you an idea of where we were doing the most work and where we thought the important assumptions lie and give you a rundown on some of the results.

Finally, and most important, get into the guidance. We feel like this is certainly where total system performance assessment has its role, and that is in helping the project understand where they need to go in terms of site characterization, design, regulation assessment and where we, ourselves, need to go for the next iterations of total system performance assessment.

[Slides.]

DR. DOCKERY: Some of these things are similar to the sorts of things that Jerry said. What I want to talk about is, how do we start thinking about the problem. Where do we start, and where do we go.

Since it's an iterative process, TSPA-91 really was the springboard for the next iteration, TSPA-93. We first went back and looked at the important processes and parameters that we had identified in TSPA-91 and the sensitivity studies that were done subsequent to that iteration. Then, the next step was to look at the new project information, to look at the project needs where there were important issues that needed to be resolved.

In particular, some of the areas we found were of utmost interest to the project, were to deal with some of the design features and issues that were of interest at the time. In particular, the thermal loading studies and the multiple waste package concepts. As Jeremy said, as a result, Sandia did four analyses cases which included two aerial power densities, the 57 and 114 kilowatt per acre and also looked at the SCP vertical borehole design, as well as the MPC in-drift type of design.

Then, we determined that it was going to be necessary to address some of the dose effects, to explore most importantly the effect of how the regulation assessment that is ongoing by the National Academy might affect performance assessment, might affect site characterization.

Where were the important issues that come up and how would they be different from the sorts of issues that we were looking at from a different regulatory standard.

[Slides.]

DR. DOCKERY: After we have our shopping list if you will of everything in the world that TSPA would want to do and everything that TSPA wanted to have done for them, we prioritized our list based on these guiding elements. Where would we be able to get the most bang for our buck in terms of site characterization priorities, where could we help with some guidance on design requirements, and where specifically could we deal with some of the regulation assessment issues.

These actually defined what our goals should be, what were the objectives going to be for our total system performance assessment.

[Slides.]

DR. DOCKERY: Based on TSPA-1991 and the subsequent sensitivity studies, we identified some very specific areas, places that we felt we needed to do more work and needed to increase our information base or process models. These were identified as the board knows very well -- they have seen this several times now -- the percolation flux for the composite porosity model and the source term, were identified as important. For the WEEPS, fracture apertures and episodicity of the flow, were identified as very important. For gaseous flow the bulk permeability, the retardation and source term, and for direct releases what is the likelihood of occurrence and some of the source term issues.

All of these particular areas are where we put a fair amount of work, in trying to increase our ability to model these processes.

[Slides.]

DR. DOCKERY: What we were trying to do is, in one place show all the elements that were in the Sandia total system performance assessment, and show how those pieces fitted together; where the information flowed. This was the raw data, if you will. This was the interpreted information. Here, were the detailed calculations. Here, were the probabilistic models that all of this information fed into, and the final results.

What I have shown on this viewgraph and tried to show it in a little different way is in red, I tried to highlight the areas where there is substantially new or completely new information. The purple is where we simply expanded information sets that we already had before. Over on this side I wanted to show that although there were a lot of connections that have been made with other participants and Sandia for TSPA-1991 as a result of the road shows or technical interchanges that DOE sponsored back in the early part of the year, we really had a larger group of participants dealing directly with this total system performance assessment.

We had a number of different interactions with all of these different participants over the time period from February up until the very last part of the document production, in effect. Dwight Hoxie of USGS was one of our internal reviewer's on the document. We did have some survey input into the final stage of the document as well as along the way. As I said, both Los Alamos and Lawrence Livermore were contributors to the document.

You can see that we had, as I said, for the stratigraphy and hydrogeologic parameters, we did incorporate some of the information that LBL had been working on. We talked to the USGS a number of times, going back and forth on the data sets, how good were our data sets, where might there be problems and in some cases where were there interpretation problems.

Climate change, is another real good example of where we had several interchanges with different groups in the USGS, trying to extract the maximum amount of information for a very important process. Geochemistry, as I said, Los Alamos. The thermal effects, the Livermore folks helped us out with that as did some of the M&O folks.

The saturated zone, we had Dick Luckey helping define the model and also review the model. The gas flow was produced entirely by Ben Ross and Ning Lu at DSI.

In the source term and EBS processes we tried to go farther afield and talk to some of the people at Oak Ridge and Iowa State, as well as the M&O and Lawrence Livermore, to try to handle some very important processes in a better way.

[Slides.]

DR. DOCKERY: Now that I have sort of set up what the problem setup was like and what the information that was incorporated was, I wanted to go through briefly some of the critical models and some of the critical assumptions that we incorporated into TSPA-91, with just enough detail to try to show why we thought they were important or what kind of impact that they ultimately had on our results, and work up to the slides that we really wanted to get to which is what kind of information can PA give back to the rest of the project, what kind of guidance can we give to the rest of the project and what are the bases for these recommendations.

Back in July when we talked about TSPA-1993, we talked in some detail about the geostatistical stratigraphy. This is an area where we have made some very significant progress, we think, toward representing the site more realistically in a three dimensional sense. It's one of the steps along the way. We are working on ultimately determining whether or not we need the degree of detail and information on the variability that the project can provide.

Also, to find out how can we handle uncertainties since we can't drill a borehole over every square inch of the site, how can we extrapolate based on the information that we have and how can we help guide where the next information should be taken.

As a result, we constructed a three dimensional stratigraphy based on information from the 22 boreholes shown in this viewgraph. After the drill hole stratigraphies of which there were ten different realizations, non-welded and welded units were sort of the delineator as you may recall from the July meeting -- I didn't show another variagram but we did have ten -- we chose to pick eight columns based on sort of a general representation of the repository area.

The eight columns, each were sort of basically taken or represented equal area within the repository. They also tried to represent several different geographical subtypes such as, there were areas along the Ghost Dance Fault, there were areas representative of the Solitario Canyon slope face, and there were several other points that were picked based on which tuff units would intercept the water table directly.

The next step was to take these 1-D stratigraphic columns and use the information in these columns of which we only have to date run one set of the columns and intend for a next step to do the sensitivities on the geostatistical stratigraphy, and find out how much the variability within the units is going to make a difference to our total system performance assessment efforts.

[Slides.]

DR. DOCKERY: So, as you can see, since we had these eight different columns and have the different units defined within the columns, for each one of these we had to define hydrologic data sets to define the individual units. Those distributions were developed last TSPA, but this year we tried to not rely so much on analog information as the site specific information. We got information from the SEPDB and we got information from some outside sources, some of the file reports from the USGS and other information from the USGS, and tried to expand our data set so we could be more representative of the larger geographical area in the repository.

In addition to having a greater data set for TSPA-1993, we also -- you my recall from the fracture data set last time -- we used sand as an analog. This time, we used an empirical relationship to try to derive some of the parameters that we didn't have last time, to see how much information we were going to need for the fractures.

The basic results in expanding the data set and using the information specifically from the site were in general, we got increases in the values. There was just a general increase in the matrix parameters, and there was quite an increase in the fracture parameters, about three orders of magnitude higher. There was definitely information to be gained by expanding our data set and trying to use just the site specific information.

[Slides.]

DR. DOCKERY: Another area in which we did a lot of work and had a lot of interactions was in the area of percolation flux distribution. Last time you may recall, TSPA-1991 looked at climate change in one distribution. We used a distribution of percolation flux from low to high, and you could randomly sample across any of these distributions. You could get a high value and next year you could have a low value. We felt that obviously, the correlation might be important.

We also wanted to incorporate some of the site information and some of the intuition that people have developed in this last several years on climate change. We represented two different climates, interglacial climate and glacial climate, with the glacial being a wetter time period and the interglacial being a dryer time period, which is what we think is happening right now.

We pulled in Rick Forester and Alan Flint and we also pulled in some of the WIPP people, to help understand how they treated climate change in their performance assessments. This distribution reflects all of the uncertainty and all of the disagreement, and all of the strong opinions that people have and different opinions that people have on how climate can change and how that can have an effect on percolation flux at the repository horizon.

Also, in the climate change scenarios, we allowed a water table rise on the order of 50 to 120 meters associated with the wet periods. We were trying to couple all of the different effects in 100,000 year timesteps, 100,000 year for wet and 100,000 year for dry. Here are the distributions, that you can see that there's a much higher distribution for the wet time period as opposed to TSPA-1991, a much lower distribution for the dry period.

[Slides.]

DR. DOCKERY: The saturated zone is another area where we felt there were critical assumptions that needed to be explored in a great deal of detail. As a result, we did some three-dimensional modeling of the saturated zone, and tried to capture some of the structure that existed within the saturated zone.

What I have over here is kind of the slice that was modeled. This is based on the USGS model for the diversionary and non-diversionary models for the high gradient region. In one case water drains into the tuff aquifers and stays there. In one case the water in the high gradient region pours down into the carbonated aquifer. We were trying to match the information that had been gathered in the few boreholes with the three dimensional model, and determine were there methods to determine whether the models we were using were right or not.

Indeed, we were able to match both the diversionary and the non-diversionary, depending on how we treated specific fracture parameters for major faults within the repository block. As I will show later, George Barr came up with a suite of specific recommendations basically drill here, get this information to help us understand whether this model can be delineated even more than it is now.

You may recall seeing this in the past. This is the modeled block. It goes from the water table to 200 meters beneath the water table. What the colors represent are the different units that intersect the water table in this region. You can see a little bit of the fault structure in here, where you are offsetting units and the units are tilted. There's a great deal of vertical exaggeration, obviously. Here, this is 200 meters. This is about eight kilometers.

The repository block sits basically here, so you can see the Prow Pass and Bullfrog were the major units that were intercepted by the water table beneath the repository and from which we got information. Over here what we have is, taking this block and putting it in black and white, you can see where the repository sits. You can also see the five kilometer accessible boundary limit.

What this is, is a break through curve that shows concentration versus time versus the node points along this particular circle, this fence is what George calls it. You can see that there's a lot of structure in here. You have very different concentrations at very different times hitting this boundary. We think that for the dose assessments this shows how much more detail we may have to be incorporating in order to find out how the saturated zone can have an effect on our dose calculations.

[Slides.]

DR. DOCKERY: As I said before, the geochemistry came almost entirely from some expert elicitation from Los Alamos and from some Sandia participants. Last time around we used basically one expert, Arand Meyer, to try to get us information on sorption. This time, we expanded and had between four and five experts, helping us understand how we should distribute this information. We also expanded to include solubilities. This was a major increase in our information base.

I should add, in the case of the solubilities, that is representative of Yucca Mountain as it is now, with some temperature and some PH changes based on what we think would happen in the near field. We don't really have specific near field information. This is just the best we have right now, and we put that in to try to find out if that made a difference.

[Slides.]

DR. DOCKERY: In terms of source term which you may recall from TSPA-1991, there were a lot of different sensitivities that were brought up in relation to the source term. As Warner mentioned, there were certainly areas where it was well recognized that we needed to have better information, more robust mechanistic models, in order to really explore what effect the EBS would have on the total system.

Sandia and Livermore began working jointly in very early 1993, to define the areas in which the source term should and could be developed. Then, we iterated back and forth with Livermore, working very hard to implement a lot of our very pressing needs. They hated to get phone calls from us. We have the INTERNET, and we could have never done this -- there were a lot of files flying back and forth on the INTERNET during the time period from about May to December. This is one area where Livermore was extremely responsive in helping us get the information that we thought was important to implement in our total system performance assessment.

[Slides.]

DR. DOCKERY: This is a flow diagram of their model which we did not implement every aspect of, because in some cases there was not information available or the models were not as robust as we would like. In some cases we just didn't have time to implement everything that was in every box in this model. As you can see, we have dramatically increased our ability to model the source term region.

[Slides.]

DR. DOCKERY: This is an icon. Trying to find one single picture that could show you everything that was done in the thermal area was impossible. I have a few backup viewgraphs if we want to see some of the specifics of the hydrothermal modeling that was done and the specifics of the repository scale thermal modeling. We took information from the repository scale, from the panel scale and from the drift scale, and did some very detailed thermal design calculations.

Based on those, then we abstracted out the thermal histories that were consequently used to come up with a conceptual hydrothermal model. What this particular icon shows is sort of the results of everything folded in together. It shows a container wall temperature, and it's showing it for a center container. In the case of Sandia we sort of split the containers into center containers and edge containers, to show the expansion and contraction of the thermal dryout region, if you will. This is in the center of the repository, so you are seeing the highest temperatures.

Based on Eric Ryder's and Tom Buscheck's very detailed thermal calculations, we came up with these sort of two extremes in thermal profiles. What you have here is the vertical borehole emplacement at 57 kilowatts per acre. You can see the temperature from zero to 600 degree C, up to 10,000 years. You can see what the profile looks like for that particular one.

Then you can see for the in-drift emplacement at 114 which was our highest thermal load, you can see how much higher and you can also see the effects of operational issues that we incorporated in. This is when the backfill goes in. You get a big insulation effect. We are starting to see some of the details, how we have determined which of the details we may have to pull out and get more information on, to find out if our assumptions are right. You will see that I said we want to look at some of the assumptions in backfill. Obviously, this is making a big impact on how the container behaves.

[Slides.]

DR. DOCKERY: I don't know if I would want to call them magmatic effects or the critical assumption, but it was something that was and still is, very interesting to elements of the NRC. In this particular case we took advantage of the magmatic project which had been operating at Sandia a number of years ago, to look at how different metals would behave in contact with different magma types and volatiles from magmas. We incorporated change in source term based on aggressive volatiles and heat impacting the waste package.

As you will see, it turns out that incorporating that detail and the change in source term, again, does not have a big impact on the results.

[Slides.]

DR. DOCKERY: Here is where we were trying to get to for months and months and months is, these little diagrams and little CCDFs. What do we see? We see that for a composite porosity model, our matrix dominated aqueous flow model at 10,000 years, the SCP type arrangement in the aerial power density, the lower aerial power density that we looked at, again, just as was true with TSPA-1991, the gaseous releases dominate the flow pattern.

You may not be able to see on yours but on mine you can see that the total which is this blue, is almost the same as the gaseous because of the orders of magnitude that are involved. The aqueous is a little bit lower than it was, and human intrusion is very similar, and volcanism is also very similar. I did just for comparison, in case you didn't remember exactly -- if you haven't had this indelibly imprinted in your mind -- here's TSPA-91 releases. You can see what the changes are between the two.

[Slides.]

DR. DOCKERY: What I haven't shown you is another sensitivity diagram. We found, again, that the highest sensitivity for the composite porosity is, again, to the percolation flux. The difference in the aqueous flow -- the slight decrease in the aqueous flow releases -- are directly a result from the percolation flux. Again, as I said, human intrusion and volcanism aren't major contributors to the release in the case of the composite porosity.

[Slides.]

DR. DOCKERY: In the case of the WEEPS model for TSPA-93, you can see that there's a little bit of difference. Whenever we start looking at fracture dominated flow, where the unsaturated zone is not playing much of a role in retarding the flow or increasing the flow or decreasing the flow in packages, we have a very different suite of curves. Human intrusion starts to become more important in the overall CCDF. The total CCDF is over here. Here is the gaseous and aqueous, and human intrusion and volcanism, again, is down in the graph. We are not really worried about the volcanism. We can see that we can't disregard the human intrusion in this case.

[Slides.]

DR. DOCKERY: I thought for your memories, this was TSPA-91. I wish I could overlay them exactly but they are not quite at the same scale. You can see, maybe, the differences between the two.

Maybe more importantly what you see when you look at the WEEPS model and look at the composite porosity model is, maybe our greatest sensitivity is in which flow models we use; how we model the flow through the mountain. We have two alternate conceptual models. They may or may not be end members of how water flows through the mountain. They may or may not capture the span, but it certainly shows the area in which we really need to refine our understanding in how the water flows.

I know that this is a big surprise to everybody in this room. Pat, you have never thought of this before, right? This had never occurred at any point in your life. It shows that under any of the standards we really have to understand what kind of flow model we use, and how we partition these flow models.

[Slides.]

DR. DOCKERY: In terms of the cumulative releases for the four different analyses cases, the two aerial power densities and the two waste packages for the composite porosity, you see kind of a clustering up here. The first thing everybody says is, obviously you can't tell. There's no difference between the different thermal loads or the different waste packages. What it really tells us is that gaseous release dominates the flow paths or release paths, and we don't have a real good understanding yet of how waste packages degrade.

What this tells us is where we need to look for information in terms of waste package degradation, and we need to understand that better before we may be able to really discern which ones of these may be better or worse for our long term use.

[Slides.]

DR. DOCKERY: I thought you might be interested in simply seeing how the WEEPS model behaves. You can see that there's a little bit more spread in the WEEPS model, and it is definitely lower than the composite porosity. It still is not something that you could say I would want to pin my program on the combination of these two conceptual designs because there's not a lot of spread, and there's certainly a lot more uncertainty between those two models than there was in within one model.

[Slides.]

DR. DOCKERY: I should add that we did do calculations out to one million year for the releases. However, the regulatory standards don't say anything about going to a million year release standard. It seemed to be un-physical in a lot of sense, some of the results that we got from those million year calculations. We did run the million year calculation, or showing you that for the dose. What we wanted to show is that most of the peak doses do occur within about the first million years.

So, running your calculations out for a million years will capture the majority of the peak doses, that they can occur from the repository. We also have uncertainty building on top of uncertainty on top of uncertainty to get to these curves. We feel like we are spacing ourselves farther and farther from what we can defend once we start adding up these uncertainties.

Also, obviously, the dilution in the saturated zone is an area that we need to have a little bit more information. We would have to have a lot more site specific information that we are not gathering now, to start getting a better handle on what the dose implications would actually be. Our program is not dealing with -- right now with cumulative release model, you are sort of moving toward trying to prove how well the site will contain after the release.

What you are going to have to do with the dose is move toward how well will the site behave before the release, because once a release occurs, if you go out to a long time period, it will get out into the accessible environments.

[Slides.]

DR. DOCKERY: Some of the other conclusions. I showed some of the conclusions based on the WEEPS model and composite porosity model, and the details that we have time to show here in terms of how we came up with all of these different conclusions, we can't really go into it in this timeframe. I did want to let you know that the other important conclusions were that in terms of the waste package model, it's obvious that the failure of the waste package is going to be very strongly dependent on the thermal, mechanical and hydrologic processes and how those are coupled.

That's an area that we need a lot more work in drawing that information together and find out how they interact with each other.

In the Sandia model we didn't see much

differentiation in the corrosion resistance of the various designs. That's probably a vagary of the way the corrosion occurs in a very small temperature window as you go below about 100 degrees C. That may or may not be real, but that's an area where there's obviously a big sensitivity and we need to get more information.

In general, the larger containers, the MPC, showed poor performance for both the WEEPS and the human intrusion model. That's because it's a larger geometric outline. There's a larger footprint in the case of the WEEPS model, the fractures. The number of waste packages that fail is dependent on how large the area is. The same thing with the human intrusion.

There is little difference in the releases for the two different thermal loads, but that certainly can be due to some of the simplifications in the processes that we incorporated. The improved saturated zone representation --the one where I showed you with the Prow Pass breakthrough curves at the five kilometer fence -- there's a lot more structure in those plumes than we had envisioned or seen in any of our previous two dimensional or one dimensional simulations. We understand that that's going to be important to refine our knowledge in that area.

[Slides.]

DR. DOCKERY: Some of the limitations of our model is that we didn't have any barrier effects from cladding, and this may be very conservative. We didn't have the information on how cladding might behave, and we understand from Bill Clarke that there may be more information just now coming available to us, that may indicate that the cladding may be gone if you store the waste at very high temperatures.

If indeed we cannot take credit for any cladding maybe it isn't conservative. On the other hand, we would like to understand how cladding behaves a little bit better before we give up that potential barrier.

As I mentioned, we don't have the near field geochemistry explicitly modeled. We don't have a knowledge base on what will happen in the immediate vicinity of the container, given the PH, given the temperature conditions. Those, we can extrapolate to, but all the effects we really don't understand yet.

We don't have diffusive releases from the waste package in the Sandia model. The abstraction of hydrothermal properties may be much too simplistic. The more detail that Eric put into his thermal models as you may recall from some of the thermal load studies that you have seen also again and again, as you put more detail and as you put the drifts in, as you put the panels in, you start to see less coalescence of the extended dry area. You may also get more focusing into the areas where you don't have strong coalescence when you get a better understanding of how we handle it and how real some of those interactions are.

In terms of the magmatic effects, we didn't really have a good alteration of the waste form based on the different constituents in the magma, and we also are waiting on some of the information that Greg Valentine and people form Los Alamos will be coming up with in the near future, which may change some of our information on how likely an event is to occur. If they come up with information that says volcanism is much more likely than we thought because we didn't have this information on subsurface intrusions, then this may move this up into an area of concern.

That's one of the reasons that we are continuing to try to improve our model incrementally as we can, so if that does happen we will have the information already incorporated.

[Slides.]

DR. DOCKERY: Now, we will turn to the need of performance assessment, why do we do this, what is the justification for our existence, guidance for site characterization, what did we see in terms of Sandia's PA, how would we go to site characterization and say here's the information we would like to have from you based on the performance assessment needs. This is not to say that there are not other constraints and other needs out there, but what did we see in terms of our total system model. This is almost in terms of priority for us.

We need to obtain information to help us understand which flow models occur. I show the difference between the WEEPS model and the composite porosity model. I show that there's a big difference, based on the way we handle aqueous flow. We really would like to know, is there evidence of WEEPS at the mountain. Can we see places where, through time, we have had flow through fractures and have either plugged up or changed from fracture to fracture, how big were the fractures, how connected are they, how long does flow occur in those fractures.

Those are the sorts of things that we would like to suggest that site characterization put their emphasis on trying to find the information.

We also need a refined understanding of gas flow and retardation. As we saw, gas flow dominated the model. But when it comes right down to it, there's only like seven numbers in the unsaturated zone from bulk permeability in terms of gas flow. So, trying to expand your knowledge base right now or trying to make any really meaningful assumptions and conclusions is a little bit restrictive, until we get more information on bulk permeability and retardation in terms of gas flow.

Percolation flux. If we could get a better handle on how infiltration at the surface translates to percolation flux at the repository horizon, what kind of ranges are we looking at. We see such a large range of opinions, that we need to have a little bit more information to help us understand. We need a lot more information. We don't need a little more information, we need a lot more information on percolation flux.

We need to characterize the saturated zone flow and the dilution, and how it is coupled with the unsaturated zone. These are things that we are kind of force fitting at this point in time and we don't have much information on. If we go to a dose base standard, this is going to become a critical element.

There are some simplifications, where we might have masked the important processes or results in terms of colloids. We did participate in a colloid workshop and did work with the people who have information on colloids. It's just that the information is not to the point where it can be usefully incorporated into total system performance assessment. How well are the matrix and fracture coupled, and how long do flow paths flow. Do they flow forever or do they switch back and forth, how do they change through time.

[Slides.]

DR. DOCKERY: Where do we feel like there's incomplete data. Where do we feel like there are holes that might be having important processes slip through our fingers. We feel like the Southern and Western portions of the repository area are not very well representative of the data set. If you looked at the way the holes were distributed, you can see that the correlation lengths are getting longer and longer and our guesstimations are getting more and more tenuous as we get into those areas.

In terms of using the information that we have, once we have it -- and realizing that we are not going to get as I said before a borehole every foot to help us completely characterize the mountain -- we have to understand how to use the information we do get most effectively. For that reason, we need to determine scaling properties. We need to get more information on the spatial correlations and the correlations. The SD project is going ahead in the next year, so we hope that we will get this information.

We also hope that as that information becomes available the geostatistical stratigraphy that we developed for this exercise can also be used directly to help understand where the next holes should go, where is our understanding the weakest, and where are we having the most variability that we need to decrease.

In a very specific sense there is one hole where the hydraulic conductivities were very different than every other hole that we had, they were orders of magnitude different. For a specific recommendation we would like those people to go in and tell us, are these real values, is this a problem, is this something because it's in the fault zone they are just extremely high, or do we have a bad value. How should we change our distributions.

We also wanted to obtain information on the hydraulic characterization of the unsaturated zone fractures and the rock matrix.

[Slides.]

DR. DOCKERY: For the near field we would suggest performing integrated testing on waste packages for water contact under saturated and unsaturated conditions, coupling as much as possible the thermal, mechanical and chemical effects. We need to look at the coupling of the processes. In terms of colloids in particular, we would need to characterize the interaction of the man made and the natural constituents in the repository. Jean Younker talked a little bit about some of the waste isolation studies and how we are trying to start looking at that type of information and how it may eventually be drawn into something like a total system performance assessment, and how they will be using TSPA-91 and TSPA-93 as the basis for some of their calculations.

We need a lot more information on container corrosion and the waste form alteration processes. There are great sensitivities, particularly in the WEEPS model.

[Slides.]

DR. DOCKERY: Guidance for design. The spike in the thermal load, we showed you that we needed to characterize the thermal and hydrologic properties of any potential backfill, look at the real benefits of horizontal versus vertical emplacement. We have already had an indication where perhaps the large horizontally emplaced cask may not optimize performance.

If we could minimize water contact -- Bill Clarke has never said anything like that -- that would be a good idea. We need to look at how much cladding can have an impact on how the containers perform. Neptunium was the element that contributed the most to the dose. We would like to look at the feasibility of having long term reducing environments, if that would help reduce neptunium solubilities.

[Slides.]

DR. DOCKERY: In terms of regulation assessment, the dose calculations in general, as I said, require a little different or somewhat different data set. There would have to be changes in the site characterization program and priorities if we went to a different kind of regulation. The sorts of things that we are going to need are the saturated zone information, probably look at a much larger area. We will have to look at the biosphere in a lot more detail than we have at this point in time.

For the very long time periods, as I said, the retardation of the unsaturated zone may not buy us a whole lot if we go to extremely long time periods. As we get more and more uncertainties we go to longer and longer time periods, and have all this additional information.

[Slides.]

DR. DOCKERY: Last, a shopping list for ourselves, what do we want to do in TSPA. Although we have made a big leap in the number of scenarios, we want to make a larger leap yet again. We need to have increasingly larger suite of scenarios. We need to work more on validation of TSPA abstractions. We have to update our parameter distributions with the new information as it becomes available. We need to look at the effects of heterogeneities.

We need to look at some additional detailed modeling for the hydrothermal effects, as well as abstractions. We need to look at models for coupled effects in the near field on the waste package and on the waste form, and definitely improve the aqueous and gaseous modeling capability by incorporating information on fracture matrix coupling, parameter scaling, climate change, hydrothermal effects, et cetera.

In other words, if we can get more information we will happily incorporate it. Are there any questions from the board?

DR. NORTH: Thank you for staying on time. Are there questions from board members?

DR. DOMENICO: Neptunium gave you the most trouble. Are you using Tom Pickford's model or something close to it for transport because you are dealing with chains. Neptunium was not part of the original inventory, so you have chains involved.

DR. DOCKERY: Bill, do you want to answer that? Bill or Mike, can you answer that question?

DR. DOMENICO: Is it a one-dimensional transport model?

MR. WILSON: I am Mike Wilson, Sandia Labs. Yes. The probabilistic calculations were done with one-dimensional transport though the saturated zone part was based on the three-dimensional model, as we said. We did not model the whole chain that neptunium was a member of. We increased its inventory to account for all of its in growth, and just applied the entire inventory initially.

That means that it is conservatively high for the first 1,000 years or so, and then it's about right.

DR. DOMENICO: You did model transport in the unsaturated zone as well as the saturated?

MR. WILSON: Yes.

DR. DOMENICO: The role of temperature is only brought in as its effects on solubilities.

MR. WILSON: In fact, the temperature was involved in the container corrosion and in the waste form alteration. We did not use a temperature-dependent solubility, though our solubility distribution included the solubilities from different temperatures.

DR. DOMENICO: The last question, how did you get the gas releases?

MR. WILSON: How did we get them?

DR. DOMENICO: What model did you use there?

MR. WILSON: Just a calculation of the amount of release of carbon 14 from the matrix as it alters and an estimate of the amount that is on the outside of the cladding that is released as soon as the containers fail.

DR. DOMENICO: It seems that in a sense you have a one-dimensional transport model involved here, that if Holly was given all of this information she said she would like to have, you wouldn't be able to use it anyway. Not true?

MR. WILSON: No, not true.

DR. BOAK: The model that actually calculates the releases might well be a one-dimensional model, but it does in fact benefit from everything we have learned over the past years. It is part of the process of abstraction to take three-dimensional understanding and bring it into a simplified model which we can in fact run multiple times.

We wouldn't have the detail of the modeling we did if we hadn't looked at it in three-dimensions and then simplified it to produce the final calculation. We wouldn't get the same results, I don't think.

DR. DOMENICO: I think you would have the difficulty incorporating all of those complex couplings into a simple, one-dimensional model. There's just no provision for it.

DR. DOCKERY: As we have said before, our abstracted models aren't necessarily always simple. In some cases the abstractions -- some abstractions -- can go very far to very simple models and some of the abstractions still have to remain very detailed. One of the areas that I wasn't able to get into but it's included in the TSPA document was a study that Roger Eaton did, on how appropriate are 1-D simulations for looking at flow and transport in the unsaturated zone.

DR. DOMENICO: I read that.

DR. DOCKERY: Whereas in all cases, it does not work well. There are some cases that it does work surprisingly well, by incorporating in one case the unit boundary gradient method to handle tilted units. Also, the way some of the heterogeneities occur within the grid actually are not as bad as we thought. You probably already read that paper.

DR. DOMENICO: I did.

DR. DOCKERY: We were fairly pleased to see that some of the areas that we were most worried about in one-dimensional, there are programming methods to help deal with some of those problems. As you said, there are some areas that we are probably going to maintain the complexity in order to do a good job of the performance assessment.

When you get the document, you will see that there are some areas where we did maintain some pretty detailed process modeling.

DR. DOMENICO: Thank you, Holly.

DR. NORTH: Dr. Langmuir.

DR. LANGMUIR: Holly, I notice that you just model the 57 kilowatt per acre and 114. Isn't that limiting at the front end, your analysis to a high or very high loading approach. You are not looking at a below boiling as a possible alterative for the thermal loading choice.

DR. DOCKERY: When we started the calculations at the time, the extended dry concept was certainly one of the things that was of most interest. People were very interested in finding out what effects that might have. Given the suite of calculations that we had to do we looked at the SCP loading, and then we looked at the higher regime.

We would like to look at the lower in the future. INTERA has done some more of the simpler RIP model calculations to see what the sensitivities are to that. I don't think we are conceptually limiting ourselves, but time-wise we did have to limit ourselves.

DR. LANGMUIR: One of your surprising conclusions that relates to that is that you found apparently that given what you know -- which obviously, there are big holes -- the overall implications of the SCP 57 kilowatt versus 114 were that they were very similar, which I find quite surprising if in fact we are looking at an above boiling and then going below boiling for a significant period of time, with all corrosion implications of getting below on the SCP design at some later date.

Yet, with that still happening we are saying that the loading approaches give you about the same result overall.

DR. DOCKERY: I know Mike would very much like to answer part of that question. Like I said before, what it pointed out to us is that there are simplifications in our models that we really need to get a better handle on. Hopefully, we would see these differentiations, because they didn't show up the way we expected.

The gaseous releases, the travel times are so short, that once it gets out it moves very rapidly. Maybe we don't have what we need to discriminate.

DR. LANGMUIR: My guess is that they are similar, because you know so little about the near field interactions at this point in time.

DR. DOCKERY: Yes.

DR. LANGMUIR: One of the big flags you raised which I had to say amen to was, near field geochemistry is not explicitly modeled. The work isn't being done yet.

DR. DOCKERY: That's right.

MR. WILSON: I wanted to point out something that perhaps you may have misunderstood. Even at the higher loading we did have above boiling and below boiling. It did not stay above boiling for 10,000 years. That's part of the reason they are as similar as they are.

We conservatively chose to not assume an extended dry concept. In the higher loading we had a nearly complete dryout for maybe 4,000 too 5,000 years. We did not assume that it continued to stay dry after that. That's something that obviously is possible, but we didn't want to put all of our money on that. It's kind of the difference between a dryout of 1,000 or 1,500 years and 3,000 to 5,000 years.

DR. ALLEN: Holly, does your current conclusion that volcanism contributes to releases only in a very minor way, is that based solely or entirely on Bruce Crowe's statistics of volcanic occurrences?

DR. DOCKERY: It's based --

DR. ALLEN: How does it incorporate the UNLV estimates?

DR. DOCKERY: If you may recall from TSPA-1991, we used the same distributions that we used in 1991, those used Bruce Crowe's as well as UNLV in the entire distribution. Those were the two end members incorporated in the overall pattern. The occurrences typed Bruce's as low and the UNLV holds higher ones, and they were sampled along that distribution.

We didn't do just one or the other, we tried to incorporate the information from both experts into that pattern. As you know, Greg Valentine is doing some work where, before we assumed that a basaltic intrusion got into the vicinity of the repository it would get to the surface. That was sort of a given.

DR. DOCKERY: Now, Greg is finding some information that may say that that's not quite true, and maybe the probability of occurrences aren't as low as we thought. We don't know that, but they are looking into that.

Once we get that information then we would put that into the distribution or change the distribution based on that. In answer to your question, yes, we are trying to incorporate both end members. It's still pretty low.

DR. LANGMUIR: Holly, it was interesting to me when you suggested that significant changes in percolation flux were important. Something I remember hearing a year or two ago from the USGS was, with reasonable variations in infiltration that might be expected with a fairly significant possible change in climates, that the percentage of water in the site which would be involved relative to what's already there was a few percent only. And, that if you heat a repository over 1,000 years or so, what you are really looking at is recycling existing water more than introducing any additional water that would change the system.

Is that correct or have you heard that as well, or do you disagree with that.

DR. DOCKERY: We have heard a range of opinions on this, and within the USGS, I might add. The USGS folks like Alan Flint, will definitely state that you will only get a few percent change and that this is a dry site, and it's always been dry and will be dry for the foreseeable future and there's a small variation percolation flux.

You have other people who, through other means of doing scientific investigations, have come up with very different ranges for infiltration at the surface and for transforming that to percolation flux at depth like up to like 40 percent of the values to move into the depths of the mountain.

We felt like it was incumbent on us, given that we know so little about the flow paths and the percolation flux that we see what the sensitivities are, and as we define two different climate regions and as we refine our understanding, do we still see that sensitivity increasing. There's no doubt that for at least the composite porosity model that it is our dominant sensitivity. We need to have a better understanding.

DR. NORTH: Let me ask a follow up on that. I wonder to what extent the models you have in place now have investigated the following scenario. As a result of high thermal loading you increase the precipitation in fractures in the Calico Hills, where I understand fractures are scarce, such that you effectively seal it locally and permit perched water to accumulate that would impact on the repository zone in the containers.

DR. DOCKERY: That's not something we have certainly gotten to an explicit modeling. We don't have information on sealing of fractures. That was one of the things we were interested in is, how temporally persistent is a flowing fracture and how connected are they, how much water can get in there. In the WEEPS model the water simply moves through the mountain rapidly and just goes to the saturated zone.

These variations are certainly things we need to have in our expanded scenario base.

DR. NORTH: I think as a sensitivity case that might be worth exploring with the WEEPS model, where you assume essentially the fractures seal. In three dimensional flow the water has to come out some other way. Could you get essentially a local bathtub effect, or does that turn out to be impossible, and what do the models say about the extent of precipitates that might form. Is the ceiling a realistic scenario, or is there some means of dismissing that.

DR. DOCKERY: I think that's interesting as well. Hopefully, there will be field work to tell us, do we see that sort of thing happening. I think that part of what you are saying is that definitely a very detailed process model as opposed to the higher level, that's one of the areas where we might want to work with the USGS on their information.

DR. NORTH: That's a situation where the top of the pyramid and the bottom of the pyramid have to communicate, and that's one of our main themes. We would like to assure that you are doing that.

DR. DOMENICO: I think it's important that the board realize that temperature plays no role in the fluid movement in these models except for its effect maybe on viscosity. Temperature is fed in to feed the canister breakdown and things of that sort. You are not dealing with circulating flows. Someone can correct me if I am wrong, but I do believe that is true.

This is really not a coupled model in the sense that we talk about the effects of one process on another. I think it's important that the board realizes that, that these are the kinds of models you are dealing with. I am not saying there's anything wrong with them. I am just saying that I believe this is true.

DR. DOCKERY: Although there are hydrothermal effects that were abstracted in terms of moving liquid from the sides from the boiling fronts, concentrating them in certain areas, finding out how -- as I said, there were edge containers and center containers -- how much more water do you concentrate on the edge versus the center, as the expanded dry expands and contracts.

DR. DOMENICO: This phenomena was incorporated in this model?

DR. DOCKERY: Yes.

DR. DOMENICO: I know this happens in real life. This phenomena was incorporated in your source term; is that correct?

DR. DOCKERY: Yes.

DR. LANGMUIR: Holly, have you talked to Bill Glassley about what he's doing with Tom Buscheck on trying not to couple with Tom's models because that's impossible, rather with his own models. Bill is looking at the kinetics of precipitation of silica as a function of the thermal fronts moving away from the waste package. You can get a fairly straight handle on that -- I shouldn't say straight--you can get some sense of what might happen with coupling and with silica precipitation using very simplified kinetics for the precipitation approach that Bill suggests.

It may be the only way you can get anything that you can defend and explain without doing it in some thermal testings.

DR. DOCKERY: I think Bill Halsey from Livermore, as the source term representative, would like to address that.

MR. HALSEY: Actually, it folds in several of the comments that you and Pat have both been making. The answer is, we are starting to do that, and we are hoping to incorporate some of those features into the next round. As Holly said, we could not incorporate the near field geochemistry this time around. We had a large suite of things to try to include, and we couldn't do them all.

That also gets to some of the issues you were raising previously. We did try and examine hydrothermal flows to allow for increases in water flow driven by the thermal field in the source term. A certain amount of spatial variability in some of the models -- it was a two zone, sort of the center of the repository and the edges and some of the hydrothermal results that we provided with RIP -- I think because it's a different architecture, it was able to incorporate a little more detail. I think you used seven zones of hydrothermal flow in the source term.

The next step as you pointed out is then incorporating some of those details into the transport. We haven't been able to do that yet. I think that's something to be left for the next round. Getting it into the source term is the first place, and then see how complex the results of that are into a multi-dimensional transport problem. That's where you would begin to address the issues that you brought up, of alteration of the unsaturated zone flow paths.

We didn't have the bits and pieces or the time, to try and address that this iteration. What we are trying to do is put in some of the thermal effects from the percolation flux altered by the thermal field, to give a more realistic source term. The next step is trying to couple in the geochemistry there, and then try to make that consistent with the unsaturated zone transport.

DR. DOMENICO: You can make the source term as complicated as you want, because what comes out of there is a number that varies over time, basically. You do put a lot of details in there because basically that's what comes out of the source term.

MR. HALSEY: It also varies over space.

DR. DOMENICO: Over space, too.

MR. HALSEY: That gives you complexity in the unsaturated zone flow, time and space, both.

DR. DOMENICO: But it's a number.

MR. HALSEY: Right. But you have different water flows as a function of time and space, coming out of the source term.

DR. DOMENICO: Thank you.

DR. NORTH: I think at this point we had better wrap up the discussion to stay on schedule and take our break, and resume at five minutes of ten, 9:55.

[Brief recess.]

DR. NORTH: Let us resume our session. I believe the next speaker is Robert Andrews, of the M&O INTERA, who is going to tell us about the other performance assessment.

TSPA EVALUATION OF ALTERNATE THERMAL LOADS,
WASTE PACKAGE DESIGNS AND PERFORMANCE CRITERIA

[Slides.]

DR. ANDREWS: I would like to talk about the same performance assessment, actually, but it's just a different code and there are different assumptions that I will elucidate as we go through. We are going to focus on the alternate thermal loads issues, the waste package design issues and alternate performance criteria, which may be put forward for a high level waste Yucca Mountain site.

I should point out that there are two other individuals whose names do not appear on the cover page, Jerry McNeish and Tim Dale, who did the analyses that I will be talking about today.

[Slides.]

DR. ANDREWS: I want to walk through these topics and start with the general objectives, general approach, and walk through the results from the thermohydrologic stuff all the way through to dose, with the interim performance measures if you will as we go along, and then give a summary and conclusion at the end.

[Slides.]

DR. ANDREWS: Objectives, very straightforward, to enhance the realism or representativeness of the TSPA that was conducted in 1991 by Sandia, to update the analyses with new information that has been acquired in those two years, analyze the effect of various design options, both repository and package design options, and to evaluate different measures of performance.

[Slides.]

DR. ANDREWS: Under the first objective, to enhance the realism, primary here it's to focus on the thermohydrologic regime and to directly incorporate its dependency on package lifetime and on release from the package, and ultimate release to the accessible environment. It's incorporated in these five ways.

Thermohydrology, first in terms of the initiation of the corrosion processes on the can, the corrosion rates are thermally dependent, the alteration and dissolution rates are thermally dependent, the solubilities are thermally dependent, and the advective release parameters from the package are also thermohydrologically dependent. We will come to that in more detail later.

Also, to enhance the realism we include the defense high level waste inventory and a more complete radionuclide inventory in this case is 39 radionuclides, and incorporate climate change.

[Slides.]

DR. ANDREWS: Incorporate new information, available since the completion of TSPA-1991. A lot more solubility information reflected by Los Alamos in some of their work. It's a function of temperature and geochemistry, predominantly PH, retardation coefficients, very small functions of temperature although they investigated that. The range of Kd as a function of temperature was very small, so not incorporated. But it did vary with geochemistry and that was incorporated.

Waste form alteration rates are a function of temperature and geochemistry; i.e., PH carbonate content, work from Livermore and PNL on that. Gaseous phase velocities coming from Ben Ross at Sandia that Holly talked about earlier, are functions of temperature. The saturated zone velocities are not functions of temperature, but that's new information since TSPA-1991.

[Slides.]

DR. ANDREWS: Alternate designs, we looked at three thermal loads, three outer barrier thicknesses. These are MPC designs. Ten centimeter mild steel, 20 and 45 centimeter mild steel, and the inner barrier of the corrosion resisted material, alloy 825 in this particular case, two designs looked at 0.95 centimeter thickness and 3.5 centimeter thickness.

[Slides.]

DR. ANDREWS: Alternate performance measures. We looked at the cumulative release over 10,000 years to 40 CFR 191 which applies to all non-Yucca Mountain high level waste repositories including WIPP, promulgated in December of last year. Then we also looked at two other ones, individual doses over time periods of a million years and cumulative releases over 100,000 years.

[Slides.]

DR. ANDREWS: General approach, we talked about this back in June when we presented the approach that we were going to follow, and it was the approach that we did follow. We abstract the primary functional relationships, and in this particular case temperatures, saturations, aqueous fluxes and gaseous fluxes, from more detailed process models. The detailed process models here are TOUGH 2 that we are talking about, on a slightly smaller scale than the repository scale.

To define the dependency of the various exposure release and the transport properties on those temperature saturations and fluxes, incorporate both of these functional relationships and those dependencies directly into a program called RIP. I will talk about that in the next slide. Finally, to evaluate the system performance, those three performance measures that we talked about, and the sensitivity of that performance to the uncertain properties that are input.

[Slides.]

DR. ANDREWS: What is RIP. Rip was developed by Golder Associates in 1991, 1992 timeframe. It used the Monte Carlo method to propagate uncertainties in parameters, to predict total system performance. However you want to define total system performance is up to the user, and its sensitivity. It has to use abstractions from the more detailed process models. It is essentially a glorified spread sheet which is as complex as the model builder or user wants to make it. It can incorporate as many dependencies or non-dependencies as that user thinks are relevant to performance, and then cranks it through to calculate releases and eventually doses.

Allows inclusion of all relevant domains and processes. We did not in this particular iteration look at any disruptive events because we knew that Sandia was devoting a lot of effort to revising their volcanism study and human intrusion work, so we did not look at any disruptive scenarios.

I am going to go through each of the domains, waste package/EBS first. What we have done is conduct panel scale thermohydrologic analyses to get temperatures, saturations and fluxes, use those temperatures or saturations to determine an initiation delay for aqueous processes, and then based on corrosion rate information generated from both B&W for the M&O and also by Lawrence Livermore Labs, determine penetration as a function of time through the outer barrier and subsequently inner barrier. Determine failure, failure is the first pit penetrates through the package, both the inner and outer barriers.

Then, initiate waste form alteration which is temperature dependent, and then release from the package. That release can be either solubility control if the solubility limits are low enough or alteration rate controlled if the solubility limits are very high as they are in technetium and iodine.

Geosphere transport, we talked about carbon 14 transport coming from Ben Ross, under subcontract through Sandia. Saturated zone transport, very simple, one-dimensional equivalent continuum through the unsaturated zone. The assumption being made, that matrix inhibition and/or matrix diffusion exceeds the fracture transport. Exponential percolation flux, equivalent to Sandia's dry case, with exponential mean at .05 millimeters per year and a climate change now being represented by a flux multiplier on that background flux.

Saturated zone transport using the velocities from George Barr of Sandia that Holly talked about, and retardation values based on values coming from Los Alamos.

Biosphere, also very simple. The mass release -- remember, when you are doing cumulative releases you are only concerned with mass. When you are doing dose, now you are concerned with concentrations. Now, you have a diluted in something. When you dilute it in the saturated zone flux through the cross sectional of the repository with an assumed mixing depth of 50 meters, as Holly pointed out earlier, the assumed mixing depth that one might want to consider can range from 2,400 meters as EPA assumed to something -- we have just assumed a 50 meter value. Clearly, doses are linearly related to mixing depth.

The dose is determined from dose conversion factors. We have a concentration and then convert that directly to a dose.

[Slides.]

DR. ANDREWS: Let's walk through the results. First, thermohydrology and then failure time distributions, releases, accessible environment releases and then dose. In general, I will show you results from the 57 KW case. When I show sensitivities on CCDF, I will show 57, 28 and one-half and 114 KW case. Just to show you the process of what's done, first, we have these panel scale thermohydrologic calculations with TOUGH 2. These particular cases are temperature versus time at different locations within the repository.

We are at an outer location and an inner location. Clearly, hotter on the inside and cooler on the outside. We did not take any advantage that the thermal load is evenly spread across the repository. We didn't try to do any optimization of a higher thermal load at the edges and a lower thermal load at the interior.

Similarly, water saturations in the rock as a function of time for the 57 KW case. Outer portions of the repository essentially staying at their ambient saturation which in this case I think is 68 percent or something like that. Inner portions of the repository reducing significantly but not to the residual saturation which is 8 percent for the rock. Of course, you see something very different for 114.

[Slides.]

DR. ANDREWS: Using those thermal profiles and saturation profiles, we come up with a cumulative distribution. Of course, we had to go through the corrosion and the pitting depth penetration rate, et cetera. The ultimate outcome on waste package lifetime, if you will, is a CCDF of cumulative number of packages failed as a function of time. In this particular case the number of packages is like 10,500. I think this is a 21 PWR case. There's an extra 3,000 which is the defense packages. You have 7,000 and some spent fuel and 3,000 defense packages.

You might note at the 57 KW case has all of them failing based on our definition of failure by about 4,000 years. The 114, all of them failed about 8,000 years. The 28 and one-half, all of them are failed by about 14,000 years. You might wonder why. There's competing factors going on here. You have a dryout period which is longer for the 114 case than it is for the 28 and one-half case, and the 28 and one-half case the dryout period is zero. But, the corrosion rates are highly the function of temperature as well as some geochemistry parameters, but much more a function of temperature, and the rates are higher at higher temperatures.

So, once my time period of delay of aqueous corrosion has lapsed, which you can see here starts at 700 years for the outer packages -- these are 114 case. These are all the defense packages which are sitting on the outer portions of the repository. Once that period has lapsed the corrosion rate can be accelerated; therefore, the time failure decreased.

[Slides.]

DR. ANDREWS: In tabular form, the same information. Looking at all of the options, not to get bogged down with numbers at all. As you might expect as you increase the outer barrier thickness, you dramatically extend the life of the package, no matter which thermal load you might be happening to look at. We did look at the difference between using a saturation versus a temperature criterion for initiation of aqueous corrosion. There's a lot of uncertainty. I think Dan McCright from Livermore talked to the board last summer about the uncertainty on the near field hydrological regime that drives initiation of aqueous corrosion, very uncertain.

We used both the saturation, i.e., if it got to residual saturation then there could be no water present at all so aqueous corrosion could not occur, or we used temperature. We used temperature cut off at 100 degrees C. There are slight differences there.

[Slides.]

DR. ANDREWS: I think we have more or less talked about these summary results from the package lifetime.

[Slides.]

DR. ANDREWS: Given that we have a package that has degraded and we have the one pit that has penetrated the package, we now have releases from the package. The releases from the package are dominated by carbon 14 but there's also technetium and iodine, the high solubility ions, coming out. I presented this, just simply as a normalized cumulative release.

I am normalizing to table 1 of 40 CFR 191. This is for the ten centimeter outer, .95 centimeter inner barrier, and the saturation criterion being used for corrosion initiation. We see exactly the same thing, of course, as we saw in the package lifetime results. That is, the releases from 28 and one-half are slightly less than 114 or slightly less than 57. Factors of two, given all the other uncertainties in this system, are insignificant.

I have in your handouts two tables which summarize for the expected cases, i.e., everything being sampled from expected values, the normalized cumulative 10,000 years and normalized cumulative 100,000 year releases, primarily to show sensitivity to thermal load and sensitivity to outer package thickness and also sensitivity to this criterion that we used, whether it's saturation or temperature.

The difference between that assumption, between saturation and temperature initiation of corrosion, becomes much more dominant at the higher thermal loads, as one would expect. It is inconsequential at the lower thermal loads. Clearly, that's for 10,000 years and the same for 100,000 years. The 100,000 years that you saw earlier, the 45 centimeter package at the lower thermal loads, lasted longer than 100,000 years. There's absolutely zero release from the package over that time period for that thickness of outer barrier.

CHAIRMAN CANTLON: The units in the table 2.9, what are --

DR. ANDREWS: None. They are normalized to table 1 of 40 CFR 191. You have taken curies per metric ton and divided by curies per metric ton, and you have non-dimensional units.

[Slides.]

DR. ANDREWS: When we went to the 100,000 year case point of information, we did not normalize it to 10 times table 1. It would still be normalized exactly to table 1.

[Slides.]

DR. ANDREWS: Summary results. For releases only from the package, the 10,000 years releases are controlled by the failure times and temperatures. That's also true to 100,000 years, but in 100,000 years it's generally insensitive to the thermal load and the corrosion initiation criterion. It is still very sensitive to that thickness of outer barrier thickness, if you look at very large thicknesses of the outer barrier.

I have only put here that principal nuclides contributing at least 1 percent to that normalized release for information purposes.

Looking now at accessible environment releases. We have gotten releases from the package and now we are going to releases to the accessible environment. Your standard CCDF way of presenting that for integration over 10,000 years, normalized again to table 1 values, these are all carbon 14. This is all gaseous. I don't even think I included a plot of the aqueous release component, although I do plot a sensitivity of the aqueous release to flux.

Not surprisingly, you see the exact same trend as release from the package. That which is released from the package which is dominated by carbon 14 is relatively quickly transported in the gaseous phase and the carbon 14 solely to the accessible environment. Travel time to the accessible environment of carbon 14 are in the hundreds of years range. What came out from the package comes out to the accessible environment.

[Slides.]

DR. ANDREWS: Looking at 100,000 years normalized cumulative release, now I want to show something that Holly pointed out a little bit earlier. That is, the sensitivity to thermal loads and sensitivity to outer barrier thickness is relatively small for larger time periods, not surprising. What happens due to thermal perturbations over tens of thousands of years when you are considering hundreds of thousands of years or a hundred thousand years as in this case is relatively small. That's what is indicated here.

In my summary slide this is about 60 percent carbon 14 -- this is aqueous, sorry. I am only showing aqueous here. This is predominantly technetium, almost solely technetium 99.

[Slides.]

DR. ANDREWS: Aqueous release integrated over 100,000 years, again, showing sensitivity to package thickness. Again, the difference between ten and 20 centimeters is minimal. The difference between .95 and 3.5 centimeters for the inner is minimal. The difference between 20 and 45 for the outer package thickness becomes significant.

[Slides.]

DR. ANDREWS: Showing some sensitivity plots. Now, back to the 10,000 year normalized release which is dominated. If there's any aqueous release at all, it's technetium 99. The expected value of percolation flux was 5 times 10 to the minus 4 meters per year, .5 millimeters per year. You see the normalized total release to the AE aqueous now is 10 to the minus 14 of the table 1 values in 40 CFR 191.

This incredibly steep portion here, you are essentially looking at the arrival curve. You are looking at the disperse of a arrival curve of the front coming to the five kilometer accessible environment boundary.

Looking at 100,000 years now, we saw essentially plotting out percolation flux at the repository level, as a function of normalized release. You again see the very steep portion here which is the arrival portion of the dispersed front. Once that front has arrived it plateaus out, as expected.

[Slides.]

DR. ANDREWS: Summarizing those plots. The normalized releases over 10,000 years are virtually all carbon 14. The minimal amount of technetium that comes through in the 10,000 year time period has a probability less than 10 percent of exceeding 10 to the minus 6 of that table 1 value. I haven't shown that plot exactly.

The normalized release over 100,000 years if I consider everything, is 60 percent carbon 14 and technetium about 25 percent. Iodine is a large portion of the rest. There's a few minor constituents that are coming out, but it's predominantly carbon 14 and technetium and iodine. Normalized releases over 100,000 years are insensitive to thermal load and waste package thicknesses, but at thicknesses greater than 20 centimeters -- at the 45 centimeter range it's significant again. Normalized aqueous are controlled by the percolation flux, not surprisingly.

[Slides.]

DR. ANDREWS: Now, to look at dose. I thought it would be useful to illustrate the expected value time history plot of dose. Sometimes you only see CCDFs or peaks. You don't know when exactly those things are occurring or what in fact is controlling them. I wanted to point out that in the first 100,000 years and generally well after my 10,000 year period -- which on this plot would be sitting there -- in the first 100,000 years I am dominated by technetium and iodine. I didn't even bother putting what these minor things were.

At larger times, generally on the 600,000, 700,000, 800,000 year time period, the neptunium is starting to come out. The neptunium peaks and is always for the solubility values that are being sampled off now which are under oxidizing conditions, higher solubility values than previously used. Neptunium is always the dominant dose contributor at very large times. Some of its daughters, also.

[Slides.]

DR. ANDREWS: Plotting some CCDFs now of the peak individual dose which are dominated by the neptunium as a function of three alternate thermal loads, we see essentially no sensitivity. Once you start looking at the 500,000, 600,000, 700,000 year time period the thermal load makes no difference. The same is also true of the thickness of the outer barrier. When we are looking at those very large time periods for the kind of releases that we have from the package -- I should maybe have talked about the package a little more.

It's diffusive release from the package, that diffusion being the function of temperature and saturation. We are using the Conca curves that I think you have probably seen several times from Mick Apted and others, directly in the calculation. Then, we have one-half meter of diffusive release from the package through the bottom of the in drift emplacement. That's also diffusive release, with that diffusion again being a function of the very near field saturation.

[Slides.]

DR. ANDREWS: Now, I have my mea culpa. Unfortunately, when you have a presentation right after the Christmas holidays sometimes things unfortunately happen, especially when you are shut down over the Christmas holiday. I will try to describe this, and please try to bear with me.

What essentially has happened in the next two curves is, the bottom axis have been switched. This really is sensitivity of the million year peak dose to percolation flux not saturated zone velocity, and the axis should be as they are on your next curve. As I increase my percolation flux -- and we will give you a corrected version of this after the meeting. It's in the report but it didn't get into the viewgraph. As I increase my percolation flux I increase my peak dose.

[Slides.]

DR. ANDREWS: The next slide, this is saturated zone flux, but the axis should have been from the previous slide. As I increase my saturated zone flux I decrease my peak dose. I get more dilution. I apologize about that.

[Slides.]

DR. ANDREWS: Summarizing the dose results. The long term individual doses are dominated by technetium over the first couple of hundred thousand years, and dominated by neptunium at time periods greater than a couple hundred thousand years. The peak doses are insensitive to thermal load and waste package design when I am looking at those very long time periods, but they are very sensitive to percolation flux and the saturated zone flux, the latter being a dilution factor.

[Slides.]

DR. ANDREWS: Summarizing the results, first, on the thermal load. This is more of a reiteration. First, in terms of integrated release. Over integrated releases over 10,000 years there's a slight sensitivity to thermal load. Factors of two and three I call slight, in the overall scheme of things in performance assessment, with all of the other uncertainties that are buried in the analyses.

They appear to be slightly lower for the much lower thermal load, predominantly because of a much lower corrosion rate at the lower temperatures associated with that lower thermal load. Peak dose for a million years is insensitive to thermal load.

I should state there, that we have not directly incorporated Tom Buscheck's very long term extended dry period. The thermohydrology results are predominantly affecting the releases from the package, they are not dramatically affecting fluxes in the unsaturated zone for the hundreds of thousands of year time period that one might get for the 114 KW case.

For the waste package outer barrier thickness, we see relative sensitivity over the 10,000 year time period because the 20 centimeter package at the lower thermal loads lasts longer than 10,000 years or is predicted to last longer than 10,000 years. That's true for the 45 centimeter case in particular, but also true at 20 centimeters at the lower thermal loads. For the million year time period it's insensitive again to waste package design, no effect.

[Slides.]

DR. ANDREWS: Significant conclusions. In this TSPA-1993 we did incorporate much larger detail in that near field thermohydrology in terms of its impact on delaying corrosion, its impact on corrosion rates. All of the processes going on in the package are thermally dependent. We didn't compare a CCDF to a 1991 CCDF like Holly did, but what we have seen is that incorporating all that detail didn't dramatically change the results from 1991. The bottom line there is, the guesses that were made in 1991 must have been pretty good and pretty robust guesses.

We included 39 radionuclides. All of the chains are being directly modeled. Defense waste is included this time, and that has very little difference in comparison to

TSPA-1991. Again, the conclusion being that the assessments made in 1991 that these are the principal nuclides, these nine that were looked at at that time for the aqueous flow and transport and carbon 14, are still the most dominant.

Repository percolation flux and the representation of that matrix fraction coupling which in this particular case we have considered that matrix diffusion dominate the transport as I said, they still remain the most significant uncertainties affecting post-closure performance.

Over the ranges that we investigated matrix flow and properties themselves, i.e., porosity, bulk, density, KD's, et cetera, generally much less sensitivity. I haven't shown you those plots but they show much less sensitivity than the percolation flux itself. I did want to put a proviso in there, that understanding those matrix properties and especially the fracture properties are important for understanding the overall system of flow through Yucca Mountain.

[Slides.]

DR. ANDREWS: What are the remaining uncertainties or significant uncertainties remaining following this TSPA-1993. One, is the definition of the very near field, sort of the drift scale, package scale, thermohydrologic environment, with and without the presence of some sort of a backfill. What do the saturations really look like as a function of time and space. Clearly, there will be some spatial dependency here. We have smeared out the heat source on a panel scale, and tried to extrapolate that into a much finer scale.

Until very recently work being done by Tom Buscheck at Livermore and some people within the M&O, there hasn't been much emphasis on the very near field when I say that the drift scale thermologic assessment.

Secondly, it's very crucial, the understanding of aqueous corrosion processes in our thermohydrologic system. Our temperature regimes and our hydrologic regimes is very uncertain and remains uncertain. We have not incorporated any of the cathodic protection that Dan McCright alluded to in his presentation to the board, between the mild steel and the alloy 825. It has not been considered. If it penetrates the mild steel then we can initiate penetration of the alloy 825. There's no time delay due to cathodic protection.

Livermore has a number of studies going on to try to get a better handle on the actual corrosion rate and the processes of corrosion, pit corrosion and stress corrosion cracking, et cetera. I think this has been mentioned enough times so I don't need to mention that again. Finally, the conceptual representation of that matrix fracture flow and transport through the unsaturated zone.

With that, I will open it up to any questions from the board.

DR. NORTH: Thank you very much, for staying precisely on time. We have about ten minutes for questions. First, from the board, Dr. Langmuir.

DR. LANGMUIR: Bob, you mentioned that one of the big uncertainties is that you haven't been able to deal with backfill, have not put that in the models yet, or perhaps potential fillers that might go in the waste packages. Have you at least looked at what that might do if you were to assign for example effective diffusion coefficients from Conca's work to the transport of nuclides, and maybe even lower diffusion rates for any colloids that might remotely get there? What would that do to your modeling?

DR. ANDREWS: What we have done through the package diffusion from the package and then diffusion through one-half meter of what essentially -- gravel, you might say, if things were emplaced in drift sort of mode -- we have allowed there to be diffusion through both of those pathways, if you will. That diffusion is saturation dependent. We have relatively quickly over the 20,000 or 30,000 year time period get diffusion coefficients that are such that it doesn't make too much difference.

You really have to be at very low saturation portions of the curve, not too much above residual, before you are in the 10 to the minus 6, 10 to the minus 5 meter squared per year range of the Conca curves diffusion coefficients, where you have a really big effect. That occurs at such low saturations in comparisons to the ambient, if you will, of the rock, that you only get that beneficial effect over the first couple of thousand years. Once it starts re-saturating that beneficial effect is mitigated.

DR. LANGMUIR: You are focusing on the long times, but obviously the short times are highly relevant to licensing. Under those conditions those things do work, right?

DR. ANDREWS: It's much more sensitive there, yes. It's much more important there.

DR. LANGMUIR: I had one other thing. You talk about percolation fluxes as a critical input to your models. Have you looked at the reflection that is likely to occur with repeated flow of the same water over and over again in the system without any additional infiltration, what's that going to do to your failures and your transport calculations?

DR. ANDREWS: On failures, I think that will have a minimal effect. We have kind of covered the range, whether we use temperature or saturation as a criterion for failure. That's essentially moisture presence, if you will, not moisture flux.

On transport, once failure has occurred, that's not that flux as a function of time and space. Aqueous flux is a function of time and space. Outside of the engineered barrier has not been directly incorporated. Like I said, if you considered the extended dry sort of Buscheck kind of flux distributions then that might have some significant effect even for very long time periods.

For the gaseous phase velocity fields affecting carbon 14 those are from Ben Ross' work being directly modeled, you might say. Those velocities for the 57 KW case are such that the carbon 14 travel times are in the few hundred to a few thousand year range. It's a relatively narrow distribution here.

DR. NORTH: Dr. Domenico.

DR. DOMENICO: Bob, you assume matrix flow in the unsaturated zone essentially.

DR. ANDREWS: It's transport that is dominated by the matrix, yes.

DR. DOMENICO: How much does that contribute to the arrival time at the accessible boundary of some unretarded substance like technetium or iodine. How much time does it spend in the unsaturated?

DR. ANDREWS: Virtually all of it.

DR. DOMENICO: Virtually all of it. If you had fracture flow --

DR. ANDREWS: And, there was no matrix or matrix diffusion?

DR. DOMENICO: Then, your arrival times would be much -- your peaks would occur much earlier; is that correct?

DR. ANDREWS: The peaks of what?

DR. DOMENICO: The peak concentrations.

DR. ANDREWS: If everything were fracture dominated?

DR. DOMENICO: Yes.

DR. ANDREWS: Not just some distribution between matrix and fracture?

DR. DOMENICO: The more fracture flow you have in the unsaturated zone the arrival times are higher, and you reach your peak concentrations higher.

DR. ANDREWS: Right.

DR. DOMENICO: Which is steady state, I assume.

DR. ANDREWS: Right.

DR. DOMENICO: You are spending virtually all of the time in the unsaturated zone.

DR. ANDREWS: Yes, all of this time in the unsaturated zone.

DR. DOMENICO: You are saying the saturated zone contributes very little to --

DR. ANDREWS: The time, that's true.

DR. DOMENICO: To the arrival time. The other thing is, would you conclude that if you are going to go with an extended dry thermal loading concept you better supplement that with a very robust thick barrier. Is that a fair conclusion from your work?

DR. ANDREWS: I don't know if you would conclude that from our work, but I would make that as an observation, yes. I think that's fair.

DR. DOMENICO: Doesn't your work demonstrate that the thickness does have some effect, at least over the shorter timeframes. If you consider 10,000 years a short timeframe, the thickness does have a --

DR. ANDREWS: Thickness does have a very big effect.

DR. DOMENICO: That has to be coupled.

DR. ANDREWS: Yes.

DR. DOMENICO: The last point is, I don't understand percolation flux as cubic meters per square meter,is it a velocity? You said the higher it is the more dilution you get.

DR. ANDREWS: It's a Darcy.

DR. DOMENICO: How does that contribute to dilution?

DR. ANDREWS: It's not -- the percolation flux doesn't contribute to dilution, it's the horizontal flux through the saturated zone that contributes.

DR. DOMENICO: I thought I heard you say the higher the percolation flux the more dilution you got. I didn't buy that.

DR. ANDREWS: I must have said that wrong. No, it has no effect on dilution.

DR. DOMENICO: I think the important thing is that most of your retention before you get to the accessible environment is in the unsaturated zone, and that's based exclusively on the assumption that the fractures are not taking any flow. I think that's an important uncertainty in the model, let's put it that way.

DR. ANDREWS: It's an uncertain part of the model, and although the fractures might be taking water the nuclides that are in that water are allowed to diffuse into the matrix.

DR. DOMENICO: Okay. You have a matrix diffusion.

DR. ANDREWS: I allowed matrix diffusion, yes.

DR. NORTH: Don.

DR. LANGMUIR: Can we look at your overhead 15 one more time. You covered an awful lot of material that was tough to follow and understand it fully. This one struck me as kind of important. This is the waste package failures plot, number of failed versus time in years. Of course you are focusing on long times. Until we heard you today we have been focusing on short times mostly in this program.

The first parts of those curves are of interest to me. You said that you felt uncertainties in these plots, you verbalized later on in your talk maybe 2 times or 5 times uncertainties.

DR. ANDREWS: For release.

DR. LANGMUIR: For release okay, not for failures.

DR. ANDREWS: Not for failure.

DR. LANGMUIR: Can you again explain why we have this flip over. The sense that I had was that the failures reflect the onset of corrosion because you come back to saturated conditions as being the basic argument. This then presumably --

DR. ANDREWS: And rate.

DR. LANGMUIR: And rates, okay.

DR. ANDREWS: The corrosion rates. There is two factors in this curve. There is the onset of corrosion, and that is delayed for the 114 case with respect to the 57 or 28 and one-half KW case. There is a delay for most of the inner packages. For those packages that are sitting in the outer portions of the repository which we assume to be the defense packages, those packages have a lower thermal load and they are sitting out at the edge, so they in fact get wetter earlier and start failing here at 700 years or something like that.

DR. LANGMUIR: The consequences in terms of release of radionuclides is perhaps less than it would be for the fuel, are we saying that, because they are not as radioactive?

DR. ANDREWS: We have not segregated the release portion of the curve to those portions that are defense packages and those portions that are spent fuel packages. I tend to agree with your comment, but I think we probably should go back and reallocate the inventory from the releases to determine which ones are dominating releases, is it defense packages or is it the spent fuel packages.

The other point that I was going to make about these curves is, there is delay of initiation of corrosion and then there's the actual corrosion rate. The corrosion rate for the Livermore model is temperature dependent only for the B&W fuels model, temperature plus there's a time sort of relationship in there.

That rate is higher at the 95 degree C range than it is at the 60 degree C sort of range. Once corrosion is initiated at the higher temperatures, then the rates can be relatively rapid. In fact, the failures can occur -- everything predicated based on the modeling that we have done and the assumptions that are in there -- the failures can occur earlier.

DR. LANGMUIR: Does this tell you that you want below boiling repository? It sure looks like it.

DR. ANDREWS: For these cases it behaved a little bit better. That little bit, I want to emphasize, is that factor of two or three on release.

DR. LANGMUIR: I am wondering too, is the below boiling situation one where you are at 90 or 95, or are you down at 40 or 50.

DR. ANDREWS: I would have to look at the temperatures that we actually got for that 28 and one-half case. They seemed to me that they were in the 40, 50 degree C range, something like that.

DR. LANGMUIR: Throughout the repository?

DR. ANDREWS: No. It varied with space in the repository.

DR. LANGMUIR: On average.

DR. ANDREWS: Yes, on average it's in that 40 degree C range. I would have to look at the report, to tell you the truth. They are significantly lower temperatures here than here.

DR. NORTH: Dr. Price, you had a question?

DR. PRICE: Just a short one. Why did you go to a million years? You stretch me to 10,000 years and you go to one million years, and I snap.

DR. ANDREWS: The NAS committee -- and maybe Chris

Whipple who is here wants to talk about their charter -- has a whole open new ballfield to play with in terms of what kind of a standard and what kind of a time period they want to recommend to EPA. We happened to have chose -- because the time period is an open sort of issue and all of the significant releases occur after the 10,000 year time period that we had been looking at, we said why don't we go to one million years.

That way, we are pretty sure of getting the peak which is dominated by the neptunium and then we can look at those and see what those values are. Other countries do go to extended time periods.

DR. PRICE: It isn't too surprising to me that waste package design doesn't make much difference over a million years.

DR. DOMENICO: Nothing makes much difference in a million years.

DR. PRICE: How comfortable are you with the validity of your models for a million years?

DR. ANDREWS: In the model itself, there's a lot of uncertainties. But, when you look at longer time periods the number of parameters that really control this system is relatively limited. It's the solubility of -- it's dominated by neptunium. It's controlled by neptunium solubility in water which is very uncertain. I think everybody acknowledges that, very little study on neptunium processes in aqueous systems and how that might be complexed by colloids or temperature or other rock kind of properties.

It's affected by dilution in the saturated zone, and it's affected by an assumed dose conversion factor which is also based on -- EPA uses it and NRC uses the same value, but it's also an assumption, a big assumption on what does the biosphere and how does neptunium affect one's dose. I think all of those things are very uncertain. I think there's a lot of uncertainty on three relatively simple aspects of this system.

DR. NORTH: Dr. Verink.

DR. VERINK: I predict the influence of the cathodic protection question, and a related one. The volume of the corrosion products formed between the two layers which will tend to choke off ingress of moisture could be very profoundly important in this.

DR. ANDREWS: We agree wholeheartedly. I think Livermore folks are spending a lot of effort trying to address that.

DR. NORTH: I think at this point we are going to cut the discussion off and go on to our next speaker, Abe Van Luik, who is going to tell us about the integration of these two efforts.

INTEGRATED REPORT ON SANDIA NATIONAL LAB AND M&O TSPA'S

[Slides.]

DR. VAN LUIK: This will go very fast because this is an integration by the integrator of the two talks that you previously heard. It's kind of like a chapter review, and there will be a quiz at the end.

I want to talk about basically the outline given me by Leon, using two approaches by Sandia and the M&O, why we did that. Then, talk very quickly about the implications of what we learn in terms of loading, mode of emplacement and design alternatives, compliance, what are the challenges of dose, and performance period.

[Slides.]

DR. VAN LUIK: First, let's go to the benefits of a dual effort. Total system performance assessments are complex undertakings. There's a lot of opportunity for the analyst to influence the outcome, never mind the data or the code. Analysts must make simplifying or abstracting assumptions, and hopefully -- this was mentioned previously by someone on the board -- the abstractions should reflect a correct understanding of the physical system and reasonable data interpretation.

[Slides.]

DR. VAN LUIK: If you are familiar with the INTRACOIN, HYDRACOIN, INTRAVAL series of intercomparisons, the very first one tacked transport. That turned out to be such a difficult problem they then dropped HYDRACOIN to just hydrologic modeling and then INTRAVAL brought it back to the more complex. Why did they drop back, because they found out that even codes embodying the same conceptual model but using different numerical techniques may yield comparable results for the same person. There's the key.

They generally do not yield comparable results because of analysts' needs to interpret the physical system. Both its initial and boundary conditions were found to be extremely important. Data sets generally do not allow unambiguous specification of these judgment based model inputs. You have got to tell the analyst how to interpret the data or you are going to get different interpretations.

We found in this INTRACOIN exercise, that the experience and understanding of the analyst is vital to the credibility of the analytical result. I think you will find that that's true in any complex modeling exercise.

[Slides.]

DR. VAN LUIK: TSPA-91 has been mentioned. It was also a dual effort. I think Dr. North mentioned this, that PNL and Sandia, both, did this exercise using two different calculational capabilities. Basically, we think that confidence was built into the analysis and in the results.

Another example that was mentioned this morning by Dr. North was the basaltic volcanism modeling. In 91, Sandia used a simplified model to evaluate releases. PNL used a slightly more mechanistic model which they developed themselves. Sandia used the work that was done for the Yucca Mountain project. PNL was based on the more general regional volcanism literature.

The results that both of them gave insignificant releases, I think really boosted our confidence that that was a robust analysis.

[Slides.]

DR. VAN LUIK: For the same reason, we had a new team this time, the M&O. It first had to establish its own credibility and its own confidence. The first thing we did in the M&O was to benchmark our capability by basically doing a comparative calculation using the RIP code with TSPA-91. This was quite a compliment to Sandia. Their data set as it was published, was found to be sufficient to recreate the TSPA-91 results. Having suffered through the INTRACOIN and some of these other exercises from a distance, that's not a mean feat.

We also showed that the RIP code in the hands of capable analysts -- we have to pat ourselves on the back a little bit -- can be used to perform TSPAs. It is very flexible, and it can be used very effectively for sensitivity studies. Work began early in 1993. The results of this particular comparison were published in mid-1993.

This is not to say that we are very fast, it's to say that a lot of work goes into creating a data set, which Sandia did for us in that case.

[Slides.]

DR. VAN LUIK: The second step was to ensure that needless differences in the two analyses -- now we are talking to TSPA-93 -- would be avoided. To the extent practical, the M&O would use the results of the extensive Sandia gathering effort, which Holly talked about. The structure of the RIP code, however, as compared with the TSA model which was used by Sandia dictated some difference in use and of coding of some data. Also, some differences in the analytical approach.

[Slides.]

DR. VAN LUIK: However, we did not consider this a re-benchmark, because there were purposeful differences in the approach retained to the analyses to give additional insight, and we did run some different cases just to multiply the usefulness of the total exercise.

[Slides.]

DR. VAN LUIK: In the appendix which is appended at the back because I realized that I could never get through all this material, I talk about some of the specific implementation detail differences between the two. Basically, on a larger scale, they are comparable in approach. When you look at the actual implementation, almost everything you look at had to be somewhat differently because of the constraints of the code and the system.

We don't have time to go into that detail but it's appended to the back of your sheet, if you want to see what those differences were.

[Slides.]

DR. VAN LUIK: If we want to look at what was the meaning of the differences between the way that the M&O and Sandia modeled, if we look at the next two viewgraphs -- this is the Sandia. This is one of the secrets of doing performance assessment. If you are not sure about something what you do is do it three ways, then statistically you are bound to get it right one of those times.

If we look at the Sandia aqueous and gaseous releases and then look at the results from the M&O, you can see that the theme is pretty much the same. If you are looking at it from a compliance calculation, the gas line intersects the violation line. The aqueous line is at least five orders of magnitude away in the area where it really counts. Generally, we see that the results are comparable.

Now that we have demonstrated in our view that we know how to do TSPAs and we know how to do it two different ways and come out with about the same results, what we would like to do now is redirect our PA resources to connect the top to the bottom of the pyramid in a more rigorous way. We would like to evaluate the appropriateness of the conceptual models of unsaturated flow in view of the alternatives, and we would like to especially link our modeling more directly to the results that are now coming from the site program, especially the 3-D site modeling effort, LBL, USGS.

The comment that was made -- I forget who made it, either Dr. North or perhaps Pat -- that we would like to see you link a little bit better to the bottom of the pyramid, that is exactly the way we feel about it.

[Slides.]

DR. VAN LUIK: Three viewgraphs in one. We will get through this just fine. Let's talk about thermal loading a little bit. Here, we have a little bit of consciousness raising going on. I want to introduce you to a different set of units that in the future we are going to start using. For now, we will stay with the kilowatts per acre, because all of our other viewgraphs are in kilowatts per acre. If we are going to go to the correct units according to DOE orders, we should be talking about kilowatts per hectare.

We looked at three cases and they looked at two cases, for the thermal loading. When you look at the other cases they ran we actually ran an equivalent number of total cases. In fact, the next viewgraph illustrates that.

[Slides.]

DR. VAN LUIK: Sandia looked at these four cases. We looked at these three, and then threw in this little ringer to also give us four cases. We did an equivalent amount of work, it just stacks across these charts differently. These, you have already seen from the other presentations. Let's go to results.

[Slides.]

DR. VAN LUIK: If we look at the Sandia cases -- and this has already been explained -- there is very little difference really, between the 57 and 114 kilowatt per acre cases for the 10,000 year case. This is largely because of the contribution of carbon 14. If we go to the M&O analyses -- Bob just went over this a few minutes ago -- we see that the lower thermal loading case gave somewhat better results, which is directly related to assumptions we made about container failure rates.

The temperature range of 80 to 100 degrees is where corrosion rates are the highest in the model that we are using. Now, whether this is correct or not, I think leaves a little bit of experimental work to be done.

Corrosion models used were based on a very limited experimental record. In fact, some of the references go back to 1946, and expert judgment applied to those very short records. Here is a plea, let's get some more realism into those curves.

[Slides.]

DR. VAN LUIK: Moving right along, to mode of emplacements. Jeremy showed these viewgraphs a while ago. When we talk about in drift we are talking about this kind of a concept, where, if you are looking in the drifts these things are spaced apart to create a certain thermal loading. Then, we are talking about vertical borehole too, with a thin, relatively thin waste package with a shield cap over the top so that this person can stand here without worrying about his health.

[Slides.]

DR. VAN LUIK: This viewgraph may seem familiar to you by now. Notice that nested within the 57 and 114 are also vertical and in drift emplacement. For the Sandia results we saw very little difference in the results for those. There was one exception to that, and that was in the Sandia human intrusion analysis.

The site people always accuse us when we do these long term calculations of engaging in science fiction, and here we have an example of science fiction. Either there is going to be some evolution of pack rats at Yucca Mountain or else this is going to be the site where the warlocks create their underground habitations.

I don't know quite how this happened. It should read nominal cumulative release and human intrusion.

[Laughter.]

DR. VAN LUIK: You can see right here that there is a difference between the two, and as Holly already explained it, it's based strictly on the likelihood of a vertical penetration hitting a large area versus a smaller area.

[Slides.]

DR. VAN LUIK: We look at waste package design variations. The Sandia analyses evaluated spent fuel waste packages in two sizes with two outer wall thicknesses. We, in the M&O, looked at three outer corrosion material thicknesses and two inter corrosion resistant material thicknesses. I can skip the next viewgraph because you have seen it already. It gives us specifications of what those thicknesses were.

[Slides.]

DR. VAN LUIK: Let's talk about what the results were. Bob showed you a while ago what the difference in failure distributions was for the M&O cases. If we look at the Sandia cases we get a very similar picture. If we look at 57 kilowatts per acre vertical emplacement, this line right here, look at 114 kilowatts per acre vertical emplacements and then 114 in drift and the 57 in drift, you can see that the in drift seems to make some difference and the 57 and 114 seem to make some difference, with the 114 acting the better of the two.

We have already had explanations of why this is so. I must say that nested with the in drift, for the Sandia vertical case there was no ten centimeter overpack, for the horizontal case there was a ten centimeter overpack. What we have is a nested effect here, and it's very hard to explain. In terms of 10,000 year cumulative releases as was explained before, the results were not significantly different.

[Slide.]

DR. VAN LUIK: Again, we see this right here. You have seen that before.

[Slide.]

DR. VAN LUIK: The M&O analysis, you have seen this one before. For the 10 centimeter, 20 centimeter and the 10 centimeter with a thicker interior lining, the results are all kind of a wash. However, for the 10,000 year case, the 45 centimeter packages had not yet begun to fail. So whether 45 centimeter overpack is a realistic design or not given some of our other constraints, we really can't say.

[Slide.]

DR. VAN LUIK: This is an illustration of where the 45 line falls and you can see that when I said that there were no failures I was wrong. There are some failures but the failures lag so far behind the others that they are a good order of magnitude lower at 100,000 years than the others. That was 100,000 years.

[Slide.]

DR. VAN LUIK: I was asked to talk about general compliance.

DR. NORTH: 10 minutes.

MR. VAN LUIK: All right, very fast. It is difficult when you are doing these kinds of calculations. Aqueous releases generally were five orders of magnitude below the requirements. Gaseous releases generally violated requirements. We did not address the engineered barrier subsystem requirements.

[Slide.]

DR. VAN LUIK: Are the insights different if you go to dose?

[Slide.]

DR. VAN LUIK: The key site issue is conceptual model for flow and transport through fractured-porous media and the magnitude of unsaturated zone percolation flux. The validity of the composite porosity flow model assumption needs to be evaluated.

[Slide.]

DR. VAN LUIK: The representation of the possible increase in flux that may be attributable to future climate changes is uncertain and important to either result. It is important to note that increased saturated zone flux and mixing depth are important to dose. Doses from gaseous release of Carbon-14 to the accessible environment was not evaluated in terms of dose in TSPA-93 because we only did the dose calculations for the long term when carbon-14 was already way down.

Bob showed a graph that showed the early carbon-14 calculations. What I meant was that the dose results for a million years, carbon-14 just doesn't play much of a role at that point in time.

[Slide.]

DR. VAN LUIK: Technical challenges of dose calculations, I think instead of showing you the viewgraphs here I can just tell you that we need to have a much better handle on the saturated zone and we need to have a much better handle on biosphere modeling.

[Slide.]

DR. VAN LUIK: I have been to a couple of conferences on biosphere modeling and I believe that what they say there is correct. There may be greater uncertainty in long-term biosphere modeling than in geosphere modeling. That is just a warning that we are going to invest a lot of time and money.

[Slide.]

DR. VAN LUIK: The next viewgraph on the greater than 10,000 years has been shown already by Jerry and I think basically the question was asked and answered in the last presentation.

[Slide.]

DR. VAN LUIK: This is a very useful viewgraph from the Sandia work. It looks just at aqueous release, cutting out the carbon-14. We look at 10,000 years and the EPA standard. We look at 100,000 years and a million years. There is not that much degradation in the performance of the site over that long time span.

The reason that the doses keep coming up though is because we are in a site where everything is concentrated rather than diluted.

[Slide.]

DR. VAN LUIK: When we look at 10,000 years carbon-14 dominates. Bob just showed this viewgraph. The aqueous releases are much below the EPA limit. That is not a problem. They are generally insignificant from a regulatory perspective but the percolation flux in the conceptual model for fracture matrix interactions is important to understanding the results.

[Slide.]

DR. VAN LUIK: For 100,000 years gaseous releases dropped to about half the total release. Technetium-99 is very important. Thermal load becomes much less important and unless you have a very thick outer package, it becomes less important. If you go to 45 centimeters, it will make a definite impact on your 100,000 year performance.

[Slide.]

DR. VAN LUIK: This is my last viewgraph. Peak doses are generally attributed in the very long timeframes to Neptunium. Where this is not the case and there were some instances were this was not the case, either the Monte Carlo simulation picked a low flux, a high Neptunium retardation or a low Neptunium solubility, all things for future research.

Insensitive to thermal load at a million years in waste package design but as was already pointed out, that is not a very interesting point. It is very sensitive to saturated-zone mixing depth and also sensitive to the dose conversion factors we selected.

Thank you very much.

DR. NORTH: Before we start the questions, I notice further in the package you have two plots on sensitivity and peak dose percolation flux and saturated zone flux. Are these the correct versions of the ones that were incorrect in your presentation, Dr. Andrews?

DR. VAN LUIK: Yes. They are essentially the same

-- they are exactly the same ones. Bob responded to a comment on the dry run to get rid of those little four letter acronyms on the bottom. In making that change is where things got balled up.

DR. ANDREWS: Yes, these are correct.

DR. NORTH: So, we don't need to have other versions distributed as long as we understand what these four letter acronyms mean.

DR. VAN LUIK: Right.

DR. NORTH: With that, why don't we go to questions. Dr. Domenico.

DR. DOMENICO: Just one question here. The M&O model and the Sandia model, with the M&O model were you able to reproduce basically the same results obtained by Sandia for more or less the same assumptions and parameter values? Are there differences in the outlets, significant differences?

DR. VAN LUIK: There are no significant differences. There are differences that are explainable because of the differences in the geometries and the smaller assumptions involved, but there are no significant differences in the outcomes. That's why we did the TSPA-91 comparison.

DR. DOMENICO: For all cases, for the corrosion, for the different rates of corrosion, for different fluxes, they reproduced more or less the same results.

DR. VAN LUIK: More or less the same results, yes.

DR. DOMENICO: You can use either one in the performance assessment and be confident.

DR. VAN LUIK: Yes. I think the collaboration between Sandia and the M&O helped to limit some of the stuff that I was talking about at the beginning, that the analyst can really influence by selecting the boundary conditions and really influence the outcome. We did put a cap on that by discussing things back and forth.

The closest comparable cases gave very comparable results, no significant differences.

DR. NORTH: Are there other questions from the board?

DR. LANGMUIR: This maybe isn't a question for you. We had discussion that neptunium, and you are summarizing everyone else's work this morning.

DR. VAN LUIK: Yes.

DR. LANGMUIR: If we are going to worry about 200,000 years on when neptunium becomes an issue, I would argue that the possibility of creating reducing conditions in the unsaturated zone and maintaining them for those periods of time is about impossible. The unsaturated zone is going to be aerobic. You can't prevent that from ultimately taking over the conditions.

I am afraid I couldn't encourage that as being a possibility even, that you can minimize neptunium transport by maintaining reducing conditions in an oxidized zone.

DR. VAN LUIK: I would agree with that. However, we have the right to dream.

DR. NORTH: Are there further questions?

[No response.]

DR. NORTH: Any questions from our staff?

DR. REITER: This is a question that sort of grew out of the Sandia stuff, and maybe you could help me with that. I think I am right. I was talking with Mike before. It looks what is dominating the gaseous releases in the 10,000 years, it's not the percolation or flux regime in general, it's just those amount of packages that get affected by water. What you are assuming is, if you have fractures in the WEEPS model water is concentrated and has less packages.

On the other hand, if you assume even very slow flux in the composite porosity models, a lot of packages get damaged. Therefore, since it's very little travel time, this stuff just gets to the surface. It sort of tells you that if you believe that, that if there's really concern about gaseous flow I would welcome the presence of fractures.

Those are going to guarantee me, according to that model, that a lot less packages are going to be affected.

DR. VAN LUIK: That's true, according to that model. Reality at the site may be that you have a combination of matrix flow and fracture flow, in which case you have the worst of both worlds. I think that eventually your model is going to be able to encompass that scenario.

I think if gaseous releases are important, that the engineered barrier system is the key to controlling those for 10,000 years for sure and maybe for 100,000 years. I can't believe that they really are a problem since you are allowed to reprocess the fuel and put all that stuff in the air in a matter of a couple of years. That's beside the point.

DR. NORTH: Holly, you had a comment?

DR. DOCKERY: One of the other aspects of the WEEPS model is that if we get any more information on how WEEPS may change in time you may distribute over a larger range of packages than we are right now. That's one of the reasons we want that type of information, how long will a single fracture flow, if indeed they do flow.

DR. REITER: I guess I am trying to get at -- if you are saying that reality may be a lot different than those models and reality may be combined models, I am trying to see how much using those two extreme models, how much insight that gives you into the process. Perhaps maybe you shouldn't draw conclusions based on those two models.

It seems to me that everything is dominated by the number of -- by the way you set up the model and the number of packages that you hit. If I understand what Abe was saying, reality may be a lot more complex. You have both, and the two end members are really not end members, but the worst is the combination.

DR. BOAK: That's an insight we only came to Leon, by doing the exercise. Hopefully, with time -- Mike can say more about the particular models. I think that that's an insight that we have come to as a consequence of trying to make the WEEPS and the composite porosity models match that.

DR. REITER: At some time in the past some people from Dewey stated that WEEPS represented some sort of worst case model.

DR. BOAK: No. What we have said is that it represents an extreme in terms of degree of fracture matrix interaction.

MR. WILSON: Could I make a quick comment?

DR. NORTH: Yes, please keep it quick. We want to stay on schedule.

MR. WILSON: This issue of how many containers is not only important for the gaseous releases but it's the critical factor in dose calculations as well.

DR. NORTH: At this point we will go to Jean

Younker from the M&O, talking about waste isolation impact evaluation.

WASTE ISOLATION IMPACT EVALUATION

[Slides.]

DR. YOUNKER: What we are going to talk about is, one of the areas where performance assessment is being applied in just about as close to real time as you can possibly talk about it. The people who work in this area are in their office right now using performance assessment to think about and make judgments about whether any of the activities, the construction activities, facility development or preparation for testing at the site, could potentially have any long term impact that would be adverse to the fundamental performance of that site over the long term.

What we are going to shift to now is kind of the question of, could any specific or cumulative effect during site characterization and facility development have an adverse impact on the way the site will fundamentally perform as you have seen in these previous presentations.

I am going to give you a very quick regulatory background, and then talk a little bit about the way we have thought through the waste isolation evaluations as we call them. It's kind of a little bit of a misnomer, because it sounds like we are talking about how the site would perform to isolate waste, and that's not what we are talking about. We are talking about how we could potentially impact the site's performance. Try to bear with me on that.

[Slides.]

DR. YOUNKER: The reason that we are doing this in part at least is, because back when we submitted the site characterization plan to the Nuclear Regulatory Commission they raised some concerns that were issued as objections. Part of their concerns were related to this whole topic of possible adverse impacts on future performance of the site.

When the DOE revised the site characterization plan and issued the final one in 1988, some of the concerns were addressed by commitments that the Department made to do these types of analyses, to look at the potential for both interference among and between tests that could cause the data not to be as good as it should be, as well as the potential for any kind of cumulative effect on the site that could adversely affect future performance.

The final SCP in the site characterization analysis that the NRC issued on that there was still an objection which was later lifted by additional discussions between the NRC and the DOE, where the NRC continued to be concerned that we still hadn't convinced them that the analysis that we were going to do and that we were committed to would be sufficient to assure that damage to the ability of the site to isolate waste would be avoided during site characterization.

Of course, the kinds of things that I think they had in mind at that time were major excavations in the Calico Hills for example, taking out a large volume of rock of that unit that underlies the repository horizon that would be potentially your major natural barrier.

[Slides.]

DR. YOUNKER: The approach that we have taken then to think through and make sure that we are being conscientious in our decisions to go forward with testing and construction activities at the site are to evaluate potential impact of site and construction activities on the ability of the repository to isolate waste. Clearly, this is a performance assessment, performance based question that you ask yourself, because how can you ask whether you are going to impact it without thinking about what part of performance you could potentially impact.

The types of potential impacts that we are concerned about clearly, many of the things we talked about today is, is there anything that you could do that would somehow enhance radionuclide transport, somehow increase the amount of water that you would be flowing into the repository, somehow contribute to the actual flow times in the saturated zone, some kind of adverse thermo mechanical effects. As I mentioned before, clearly, the thing that is the hardest to get a handle on but probably the most important potentially, is the cumulative effect of site characterization.

[Slides.]

DR. YOUNKER: The kinds of activities and the kinds of material applications that we obviously are thinking about as we go through and characterize the site are use of surface and subsurface water for dust control and other activities, disturbances to the actual pathways that that water would travel under natural conditions versus induced conditions, other applied materials other than water such as organic materials, and what kind of seal materials and what would their potential long term reactions be.

I will go through each of those very briefly. Applied surface water is used for dust control, fuel compaction, wash down. Cooling water for the concrete batch plant is a fairly significant potential source. We have infiltration studies where we are going to actually apply water, so we have to think about what the total volumetric effect could be. As I said before, I am sure you are thinking the individual effect of any one of these has to be fairly insignificant, and that's certainly our conclusion as we go along.

On the other hand, I still think the major thing that we have to get the handle on is the cumulative effect of all of this surface and underground activity. The subsurface water, the same sort of reasons for applying subsurface water.

In terms of disturbances to existing geohydrologic pathways, increased infiltration due to some kind of ponding where we haven't been careful enough to make sure that we have taken whatever engineering precautions we can to avoid additional infiltration, potential for flood waters entering into exposed boreholes, the way in which we deal with perched water when it's encountered in the underground excavations, and then different changes that you can make to the surface materials such that you change in some way the manner in which infiltration will occur.

[Slides.]

DR. YOUNKER: Some of the ones that I think are going to be the most difficult to deal with -- and my example that I have in the end is from this list -- that is, how do you deal with other materials that you are going to add to the rock volume such as soil stabilization materials, grouts, gasoline and diesel fuels from spills or from leaks, hydraulic fluids and lubricants. A certain amount of that material is likely to be at least released to the rock surface. The question is, can you control that in such a way that you can recover most of it.

Materials in the subsurface such as tracers, exhaust emissions which is the example that I will talk about, hydraulic fluids and other construction materials, all of these things are individually probably not a problem. But when you look at the cumulative effects, I think that's where I believe the NRC's original concern was actually directed rather than any one of these individual activities.

[Slides.]

DR. YOUNKER: The example that I wanted to run through very quickly with you is one that I know some of your staff as well as probably some of the board members are aware, it's not an evaluation that's complete. It's an evaluation that is in progress, and it has to do with the question of potential impacts of using diesel locomotives in the exploratory studies facility.

The alternatives that are under consideration are electric and diesel, and the electric has the advantage of avoiding the exhaust and the diesel fuel, potential for fuel spillage. The diesel has simpler design and construction. It's a little bit more -- the experience base that we have at the site is a little bit better for diesel, and it offers you some flexibility that you don't have with the electric.

The use of the underground equipment, clearly, as I am sure you recognize, is just transportation of personnel and materials, earth moving equipment and then other construction equipment.

[Slides.]

DR. YOUNKER: As I am sure you also recognize the concerns are, what is the possibility of enhancing waste package corrosion by some of the breakdown products of the exhaust materials, potential for changing and increasing the acidity in the environment due to some of the inorganic gases that are released. What is probably going to be the most difficult to get a handle on but also may be the biggest concern is, the organic materials that are released acting as a nutrient for microbes and causing microbially enhanced corrosion. This is a real question that is going to have to be looked at.

The question also, of course, of whether anything you are adding could enhance radionuclide migration is a concern. How are we looking at this. As I said, this is one that is ongoing so I can't tell you the answer but I can tell you the approaches that we are taking.

Determine how much of the exhaust materials will be retained, compare it to the background materials, background natural concentrations, and then determine if significant adverse impacts exist. This, of course, will come down to a risk assessment for DOE management, to make a decision whether the apparent risk of introduction of these materials is significant enough for them to go with a choice of electric.

The electric probably has some kind of a cost impact, and I don't have good data for you. I can tell you the best data that I have over the life cycle usage for the ESF right now, is probably about a $10 million cost increment for the choice of electric. That's not a hard number that I would want you to quote me on, other than to say that's the best number I have.

[Slides.]

DR. YOUNKER: Here are some real quick preliminary results. We have done some modeling. Clearly, these results are based on the assumptions that you make in terms of the hours that the diesel would be in operation. There are some other assumptions that are fairly significant in determining this.

Just to give you an idea of what we are working toward, we basically will look at the inorganic constituents, how much comes out per year, what the incremental of that is at the area where there could be waste emplacement. This involves a transport calculation with an assumption of how far the material will have to move in order to get into the waste package emplacement area.

What kind of emission control technology could be used to reduce the emissions that we are calculating, and what the natural background is, just for you to compare so that you can get a feeling for what kinds of numbers we are dealing with. If you look at what you can do with the emission control technology, it looks like the significant ones that you really can't reduce very much are the nitrogen and sulfur species. Of course, sulfur particularly, is one that the waste package people are worried about potentially feeding the microbes that would possibly have an impact on corrosion rates.

[Slides.]

DR. YOUNKER: Our preliminary conclusions are that some components appear to be permanently retained, could alter the natural water composition, and that could eventually or adversely impact waste package corrosion or transport. But, where we are right now is, with some careful planning and working with the Livermore people, some field where we are going to actually look at some tunnels that have had some extensive diesel usage in them and do some surface samples and some block samples, to try to get a feeling for how far the depth of penetration of the exhaust materials are and where they go, as well as some laboratory studies and some EQ 36 modeling.

We hope to get a handle on how big of an effect this could be over the time period of operation. Then from that, we will have to take the step over to consequences, and say what difference would that make to corrosion and potentially to the actual releases.

This is just to give you one snapshot of the kinds of analyses we are doing. I did prepare a list which I think one of your staff is going to have to hand out. That's a list, just to show you the comprehensiveness of the kinds of questions that we deal with. There are about 100 individual evaluations that have been done. Many of them are qualitative, many of them are simply just a thought experiment to say, is there any potential for this to have an adverse impact significant enough that we should be concerned. Therefore, recommend to DOE that they take a different approach. Sometimes it's as simple as using a liner in a pond.

Some of these have limited cost impact. That's just to give you an idea of the thought process and a real time application of a performance assessment base process. Thank you.

DR. NORTH: Thank you. Are there questions from the board. We have about five minutes.

DR. LANGMUIR: An angle on the pressure that -- maybe it's not relevant -- it occurs to me that when you create all these gases from combustion of the fuel, there may be a pressure effect. You are not just replacing oxygen gas at one bar with another gas at one bar. Monoxide, you get two moles of CO for one mole of 02, as an example.

Are you increasing pressure significantly when you have a bunch of diesel engines in this.

DR. YOUNKER: Remember, the thing is going to be extensively ventilated. You have to take into account the ventilation effects during operation. I assume that the ventilation would overwhelm that effect.

DR. LANGMUIR: What about the contamination of any pneumatic tests that are being run, the drifts by these gases, and their effect on instrumentation and on the measurements.

DR. YOUNKER: A big part of this, besides the potential for long term adverse effects on the site, is the potential for interfering with any of the tests that are going to be done. That's exactly right.

DR. CORDING: Jean, are there some areas where diesel would be used more in the facility? For example, in the first portion of the ramps where, before you set up a conveyor operation you have to have diesel to actually haul muck? Whereas the conveyor later will haul it which is not diesel. Some of those sorts of considerations, in terms of location and use of diesel.

DR. YOUNKER: That's exactly the evaluation that we are doing right now. I have some preliminary data, where we have had the TBM operators put together the hours of operation at various points along the drift, so that you can look at the exposure times for different places. It will be very different, depending on where you are as you go down. That's exactly what we are going to have to look at.

DR. CORDING: One other area. On the fire water situation, are you looking at the potential for accidental release valves going off or something like that, and unloading a whole pipeline?

DR. YOUNKER: Yes.

DR. CORDING: Something between valves.

DR. YOUNKER: Yes.

DR. CORDING: I do understand that a lot of the system underground will be electrical, so electrical fires will be a concern. That whole use of water to handle fires, I think, needs to be looked at.

DR. YOUNKER: That's right.

DR. NORTH: Any further questions?

[No response.]

DR. NORTH: I think we will declare lunch three minutes early, giving you 63 minutes instead of 60. We are going to resume promptly at 12:30. Please, let's have everybody back at that time. Thank you.

[Whereupon, at 11:27 a.m., the meeting was recessed, to reconvene at 12:30 p.m., this same day.]

AFTERNOON SESSION

[12:30 p.m.]

DR. NORTH: Let's have everybody take their seats and we will begin the Afternoon Session with Dr. Duguid, "Performance Assessment Efforts in Support of New Environmental Standards."

DR. DUGUID: Thank you.

PA EFFORTS IN SUPPORT OF

NEW ENVIRONMENTAL STANDARDS

[Slide.]

DR. DUGUID: Today I would like to talk to you about two topics. First, I would like to present -- I better turn it on -- the objective of our analysis.

[Slide.]

DR. DUGUID: We wanted to examine uranium ore bodies and parameter sensitivity of simple repository performance assessment as input to DOE positions on new Yucca Mountain standards and a secondary objective was to present this material to the NAS Committee.

[Slide.]

DR. DUGUID: An outline of what I am going to present today.

I will first discuss the effects of uranium ore bodies; second, I will present the results that we obtained running the model UCBNE-41.

I showed you the preliminary results from this model in July. Then I want to show you a comparison from the baseline case using UCBNE-41 with the model RIP and the model NEFTRAN-S.

[Slide.]

DR. DUGUID: The reason for using different models in these sensitivity analyses are each of the models have different bells and whistles that allow you to run different sensitivity cases.

For the uranium ore body we based this on the premise that the repository should produce no more risk than the unmined uranium ore from which the fuel was derived.

We derived two uranium ore bodies, one in an oxidizing environment and one in the reducing environment based on a review of the literature. The concentration of Uranium-238 in the groundwater in our reducing ore body was 20 parts per billion and for our oxidizing ore body was 500 parts per billion.

These represent kind of the middle of the range of ore bodies that you find written up in the literature.

The retardation factors that we used for uranium and the daughter products were taken from the WISP report and for oxidizing conditions we reduced the retardation factor of uranium slightly, about a factor of 8.

[Slide.]

DR. DUGUID: We assumed that the dissolved 238 and its daughter products were in equilibrium within the ore body. We then used the model UCBNE-41 to calculate the concentration of uranium and daughter products 5,000 meters down-gradient from the ore body so that we would have a comparison to the accessible environment in the repository.

The reason we used this model is because we were using it for other things and it was handy.

The hydrogeologic and geometric parameters were taken from the EPA study by Williams and at the back of the handout you will see the geometric parameters that we used. It was about a 10,000 metric ton U_3O_8 ore body and it takes about 620,000 tons of U_3O_8 to produce 100,000 metric tons of fuel. We did consider a 100,000 metric ton repository.

[Slide.]

DR. DUGUID: Dose to an individual from drinking water -- here we assumed that the individual drank two liters per day or 700 liters per year and these are the concentrations of uranium and daughter products in the groundwater for reducing and oxidizing conditions, the dose conversion factors that we used, and here these dose conversion factors are the most conservative from those used by DOE, NRC and EPA.

We found that the dose from drinking water for reducing conditions, 39 millirem, for oxidizing conditions, 320 millirem per year.

[Slide.]

DR. DUGUID: We then tried to take this to an integrated health effect over 10,000 years. The trick here is to figure out the population. Now we didn't want to say that all of the groundwater flowing through the ore body was consumed as drinking water. That would be unrealistic. We calculated the amount of groundwater flowing through the ore body, assumed that it was all used in household use at 150 gallons per day, and that the dose only occurred from drinking water.

We took the number from EPA of 500 health effects per 10 to the 6th personrem and used this and the population we derived to calculate the number of health effects, and found it to range from 2,000 to 17,000 over the 10,000 years.

The basis for the EPA standard is 1,000 health effects over 10,000 years.

[Slide.]

DR. DUGUID: We then went further to look at the integrated release over 10,000 years and we found that for total uranium we had 74 curies, Thorium-230, .04 curie; Radium-226, .4 curie.

The EPA release limit for those nuclides for total uranium is 100; for thorium it's 10; and for radium, it's

100.

[Slide.]

DR. DUGUID: Thus, we found that the dose from our uranium ore bodies ranged from 39 to 320 millirem per year. The number of health effects, 2000 to 17,000, and the integrated release for the oxidizing conditions, which was our highest release, was lower than the EPA standard.

Thus, the average uranium ore body would meet the EPA release standard but not the basis for the standard.

[Slide.]

DR. DUGUID: Now moving on to sensitivity analyses, these are some of the assumptions and parameters that we used. We used a groundwater travel time of 25,000 years, infiltration rate of 1 millimeter per year or percolation flux, porosity of 10 percent. We used an aquifer thickness of 2400 meters. This means that we were mixing 2400 meters deep in the aquifer. If you look at my values, if you want a mix only 100 meters deep, multiply by 24 or thereabouts -- it's actually 25 because you get some dilution from the infiltration.

Dispersion coefficient, we used a relatively high one. We assumed that Iodine-129, C-14, Technetium-99, Selenium-79, and Cesium-135 were alteration controlled.

The remaining radionuclides were solubility limited.

[Slide.]

DR. DUGUID: We worked with 39 radionuclides, the same number that was used for TSPA, and this figure I showed you back in July.

The only difference in it is we have now figured out how to plot one more curve on it and we showed Selenium-79, which isn't one of the highest contributors.

Note here that the doses start to occur at about just before 10,000 years. The first contributors to dose are C-14 and Iodine-129, and that you should note here that I bring the Carbon-14 out in the aqueous release when actually most of it would have gone out at gaseous release, so don't pay too much attention to Carbon-14.

At about 100,000 years, Technetium-99 is peaking and out here beyond the 100,000 years and out to 10 million years you get a neptunium peak. Remember that these are diluted 2400 meters into the saturated zone so the actual doses could be higher by a factor of 24 or more. If you assume 50 meter as Bob Andrews did in his calculations, it would be a factor of 50.

[Slide.]

DR. DUGUID: Sensitivity to percolation flux -- here I show Iodine-129, Technetium-99, and Neptunium for three percolation fluxes, .211 and 4 millimeter per year, which translate directly into groundwater travel times of

100,000 years, 25,000 years, and 10,000 years, respectively.

For Pat's question, in this number of 100,000 year travel time, 95,000 years of that is in the unsaturated zone.

[Slide.]

DR. DUGUID: Let me go back and find my baseline case and put it over here.

[Slide.]

DR. DUGUID: If we then look at waste package life, and here is the sensitivity to waste package life with a life of failure immediately, after 10,000 years and 100,000 years here it says 30 -- we were using 30 year old fuel -- then the time frames we are looking at, 30 and zero, are the same number.

Note what happens with this waste package life and these primary dose nuclides. We are simply shifting the peak over by the life of the waste package. In other words, 100,000 years is not long enough to effect neptunium, which has over a 2 million year half-life. You won't change the dose.

Another thing I should point out here, let's assume that Buscheck is right, that the hot repository buys you 10,000 years heating up, 100,000 years for re-wetting. We show it right here. We move the peak over 100,000 years. We do not change the height of it.

Also, had Homo Erectus build a repository here, we would be somewhere in the neptunium peak.

[Laughter.]

DR. DUGUID: To put these things in perspective.

Sensitivity to neptunium solubility -- for our baseline case we used the solubility that the WISP panel used, which was 10 to the minus 3 gram per cubic meters, which is a little low. We did go all the way up to the solubilities of TSPA up to 100 gram per cubic meter.

Notice as you increase in solubility as you get up here to the higher values, there is not much change and that is because neptunium is beginning to be alteration limited.

[Slide.]

DR. DUGUID: Summary of Sensitivity Analyses, if the new Yucca Mountain standard were for all time, waste package life has little effect on long term dose. You didn't see that neptunium peak changing very much.

Long-term doses are sensitive to flux through the package and neptunium solubility. In other words, the source term and long-term dose could be reduced by controlling the release from the waste package after failure -- in other words, the diffusion barriers.

[Slide.]

DR. DUGUID: Real quickly, the baseline case with UCBNE-41, the baseline case with the RIP.

[Slide.]

DR. DUGUID: The baseline case with UCBNE-41 and the baseline case with NEFTRAN-S.

Thank you.

DR. NORTH: Okay. We're running a little late, so we have got time for a few questions from the Board. Okay?

DR. DOMENICO: Domenico. Your dilution factor, 1.5 times 10 to the minus 4, I notice you probably did some sensitivity analysis. What sort of dilution factor do you need to be in compliance with the dose requirement. Do you know offhand?

DR. DUGUID: No, we didn't do the true sensitivity on that but it is linear. You can calculate very easily --

DR. DOMENICO: Do you know?

DR. DUGUID: -- because it is s linear. No, we didn't, but it is diluting deeper in the saturated zone than you realistically could based on the dispersion coefficient.

DR. DOMENICO: I think that dilution factor can be increased considerably if it's fracture flow in the saturated zone because you have vertical gradients. Some of the mass would be moved.

DR. DUGUID: Right. If you had a vertical gradient.

DR. DOMENICO: It will move up and be replaced by water.

DR. DUGUID: We did have some mixing.

DR. DOMENICO: And you can have sizeable mixing as that slug moves along, losing mass, gaining water.

DR. DUGUID: That's right.

DR. DOMENICO: But it could be a more significant number than that.

DR. DUGUID: That's right, but the numbers that I show are for mixing uniformly across the 2400 meters.

DR. DOMENICO: That's one big fat slug moving.

DR. DUGUID: Probably the best you'll ever do, if you can do that good. We used that number because EPA did.

DR. NORTH: Don?

DR. LANGMUIR: Langmuir, Board. It's occurred to me a lot of the nuclides we conservatively identify a maximum value based on the solubility of the element within the waste perhaps and some of these are very insoluble so one can comfortably feel that there will be some left, but when you take neptunium and go to 10 to the minus 3rd grams or one gram per cubic meter, isn't there some point at which you dissolve all the neptunium in any waste you might have on the site --

DR. DUGUID: Yes.

DR. LANGMUIR: -- and if can, use that as a limiting upper bound.

DR. DUGUID: Yes, it is. That's what was

happening also with my neptunium is I went up to higher solubility. You think of it as kind of running out of neptunium. It can't get any worse than that.

Anybody else?

DR. NORTH: Further questions?

[No response.]

DR. NORTH: Okay, let's go on to the next speaker, Scott Sinnock on "Evolution of Performance Assessment in the Yucca Mountain Project, the Historical Perspective."

EVOLUTION OF PA IN THE YUCCA MOUNTAIN PROJECT

[Slide.]

DR. SINNOCK: Thank you. As Dr. North said, I am Scott Sinnock and I want to thank the Board for their gracious invitation to speak before you today and provide my perspective on perhaps some conclusions we might draw from the performance assessments and its related activities that have been conducted at the Yucca Mountain site for some considerable amount of time now.

[Slide.]

DR. SINNOCK: I am going to look a little bit at a couple of topics. I will put an outline here. First, a little perspective on the context in which we have been conducting performance assessments at the Yucca Mountain site and then secondly, a look at the history of some of these calculations that result and then finally, to close with a few thoughts about some criteria for closure on some of the performance issues perhaps. So that I can remember, too, to follow through on this side with sort of outline sheets as to where we here.

[Slide.]

DR. SINNOCK: Let's start with the context of performance assessments. There are several that I would like to draw attention to and this shouldn't be any new information. We have a concept of a multiple-barrier system.

Through the SCP, we set some ideas for managing information flow through the principles of system engineering culminating in performance assessments of the site and we used performance allocation to identify particular data needs to support this managed information flow and we have the concept of interactive performance assessments.

So at the bottom then, these iterative performance assessments can also support an idea that has come up since the SCP and that is periodic suitability evaluations and we are talking about interim suitability evaluation as has already been talked about earlier, the ESSE, Early Site Suitability Evaluation. This is sort of the context in which we have been conducting our performance assessments a little bit.

[Slide.]

DR. SINNOCK: Now let's look very briefly at the multiple barrier concept for our particular mountain. Our mountain has many different barriers that we have to develop some sort of modeling or understanding capability for.

We have heard a considerable amount about our dry desert environment. This is one of our barriers. It influences everything. It limits the amount of water available. This is a two-sided coin as we will see. The limited amount of water is good for reducing the quantity of releases and perhaps transport time but it is very poor for diluting any waste that might eventually be released.

Whatever water gets into the system has to migrate through some unsaturated environment above the repository, moving through the repository host rock and eventually into a set of engineered barriers.

I have conceptually here have shown a set of engineering capillary barriers, perhaps an in-tunnel emplacement, eventually contacting some sort of waste container, that is a barrier, and eventually the waste form itself that can provide some limitations, like cladding of the waste, et cetera.

Any waste that then escapes must migrate back through any engineered barriers through the host rock, down through the saturated zone, and eventually out into some saturated environment, with the exception of the gasses, which can migrate, perhaps vertically, to the surface.

So for all of these various barriers we would have to develop some sort of understanding and modeling capabilities in order to predict the performance of this system, accounting, for all these various barriers.

[Slide.]

DR. SINNOCK: That is just a very brief review, but let's move down then to a concept we are using and have used for managing our information flow to support our performance assessments. I think we have plans to follow through. We called this in the SCP following through on a process that we started the first time within the SCP.

This process, which we will go into in a little more detail in the next few slides, has the possibility, I think, of carry through to provide the traceability of the information we are gathering to the effect on our performance and suitability evaluations through various applications of system engineering principles.

Also, I think if you properly can address some of the problems we have heard about -- transparency -- can help provide transparency of how information is used to help support our conclusions.

[Slide.]

DR. SINNOCK: Let's look at that process in just a little more detail of what we have done within the SCP. We have what we call the upper level where we define the requirements of the system, ground water travel time, compliance with the EPA standard, their design requirements, operability of the system.

Dwight Shelor yesterday provided quite an overview of the requirements documents we have developed to capture these requirements. We started with a very early version in the SCP.

Within the SCP we then defined a description of a system, the physical elements, to which we assign particular functions to achieve our requirements. For example, the function of the unsaturated zone was to delay ground water travel times sufficiently to meet the requirement for a thousand year travel time. That is an example I will keep coming back to.

We also then identified the physical processes that were necessary to allow us to assess the operation of these functions. Ground water travel time, then -- we said the process is Darcy flow. Once you do that, as applied to the saturated zone, that, in effect, defines a model for you that you have to model. That model has particular parameters you need in order to assess compliance, or assess that function, vis-a-vis that model.

So once you have a model, you have identified your data needs, if you will. They come out of this model that you are going to use to show compliance with your requirements. Going on down, of course, once you have your parameters you then define the test to go get that data, collect your data. Then we are just getting to the point down here, which I will get to.

Once you get your data, then you can perform these sensitivity analyses. One thing that I have added that is not in the SCP is this box that I will keep returning to. It is value of information. Sensitivity in and of itself is only a component of the value of the information you gain from further testing. We will come back to that.

But to basically answer your question, you have enough information. If you do, you draw a conclusion. That conclusion could be your site is suitable or your site is unsuitable. But eventually you reach a decision and have sufficient data to make the decision.

[Slide.]

DR. SINNOCK: I am going to have to skip through some here. I see a five-minute --

DR. NORTH: No, not yet.

DR. SINNOCK: Oh, you weren't holding that up.

DR. NORTH: I was just switching slides.

[Laughter.]

DR. SINNOCK: I thought I would have to do a real quick one.

This is just my opinion, then, of where we are in this process. I think we certainly need to update these. I think we have done, as Dwight pointed out, a good job of identifying the requirements.

Our system description, I think, has been done very well on the engineering side, the elements within the engineering side. I think we need to give a little attention particularly to the relationship between the description and the elements and their functions on the site side, with the function identifying and being more explicit of the geochemistry in the site. I think we need to be more explicit on that.

I think we have certainly identified the process. We are now getting pretty good in our performance models at accommodating, at least in some abstracted level, a representation of that process. Perhaps some of the geochemistry needs to be a little more thought about in representing of those models. We certainly know the process.

Our performance measures are pretty good and I think we have done a fair job in identifying performance parameters, defining tests. I think I have heard many people say, "I am sure we are sufficient. Are we very efficient in our tests"? Perhaps these could be backed off a little bit. I think we are probably sufficient in tests.

We started collecting the data. We are just starting to begin to look at performance analyses, sensitivity analyzes and value of information.

[Slide.]

DR. SINNOCK: Okay. Let's change gears a little bit. Taking that context of how we have sort of structured or thinking about what the use of performance assessments are, and look a little bit here now -- we have talked about this information or modeling pyramid -- and start looking at what assessments have been done at various levels of this conceptional informational pyramid.

Oh, when I thought I only had five minutes, I jumped over two of my viewgraphs. Excuse me. So let me back up to say: How do we advance those bars on that previous chart?

[Slide.]

DR. SINNOCK: I think there are methods that are being developed -- and I apologize, Warner, for another influence diagram. We have seen quite a few of these. You have been briefed on various activities that have used these, but I think they are very instructive. They are ways to help us organize how we communicate the information. I propose we can even use them more explicitly if we think of these diagrams as identifying at the top our requirements and explicitly what data are required to satisfy those requirements.

Not only what data are needed, if you look at these diagrams, each set of arrows leading to a particular item in this diagram is in and of itself a model. So not only does it identify the data, it identifies the model.

There is a model that takes these three parameters and translates them into groundwater travel time. I can write this as an equation. The travel time equals the distance, times the porosity divided by the flux gives me the model. I can write an equation for that.

Some of these, like the distribution of fracture matrix flow, perhaps I can't write as a particular algorithm or model, but I can explicitly define what I need to define this model to identify inputs into my higher levels to define the parameters.

Each one of these, I think if constructed properly, is a parameter that we need information on. Most parameters we are dealing with are outputs of models. They are not measurements. Down here we get to the measurements. So we have seen this pyramid before.

[Slide.]

DR. SINNOCK: If we overlay that diagram on this pyramid, I think we have a way of better understanding what it is we are moving upward through this pyramid to come to the conclusions about the performance requirements.

The measurements sit at the bottom of the pyramid. We can explicitly identify those. Then we have various data reduction models that have to translate these measurements into data that is more amenable for direct import into the abstracted performance models.

So I think we can more explicitly use this kind of organization to help us understand exactly what information we are collecting and explicitly how we are moving that information through to particular performance measures, which themselves are parameters.

This differs a little bit from the other pyramids you have seen. I have added something on top of this. Performance modeling puts out a parameter, ground water travel time. We need to then evaluate that output in the context of the regulations.

[Slide.]

DR. SINNOCK: So now let's move and look at those four levels of the pyramid historically of what we have done. Down here is representing our data collection. You can see there was a hiatus of data collection. This isn't all of it. These are just representations.

We have developed over models over time by adding, I think, more and more of the processes into the models as computing capabilities. As the software algorithms have improved, we are able to add more and more processes and still not overwhelm the computing systems.

But fundamentally I think the equations are the same that we have used throughout this process. But we have increased the efficiency of our ability to model more processes in greater geometric complexity over time.

What I really want to focus on is we heard considerable briefings on the TSPA work. Later you are going to hear Robin talk about the EPRI work. But we have now developed the abstracted models for performance assessments. We had some earlier versions also, and we will talk about that.

But sitting on top, and Max later will talk in more detail about this, the results of these have then be used in other assessments, including value of information assessments, of what does the information from the performance assessments tell us that we can draw conclusions either about suitability or prioritizing further work in the tests.

At the back of the package that was handed out, the explicit references for each of these are provided, which I won't be going over.

[Slide.]

DR. SINNOCK: Now, let's back down a little bit and look at a different summary that has been said several times today of the same things that we have seen from these performance evaluations.

I think if we look back over history we can see a robust pattern of examples starting to become apparent. I wanted to say "emerged" but these ideas, these concepts, have emerged quite a while ago.

If we look at this chart, down here, under basically aqueous releases for most radionuclides under normal conditions, we see very low releases sitting way below the EPA standards, resulting in quite low doses. We started seeing these way back in the WISP days, the early supporting analyses for the environmental assessment.

Basically these wander around a little bit in the order of 10 to the minus 4, to 10 to the minus 6 of the EPA standard consistently over quite a period of time.

At the same time, we are starting to consistently see a pattern that gives the possibility perhaps of being very near the EPA standard, just above/just below about 1, which has to do with our Carbon 14 releases for cumulative curie release, or back here in the EA for very unlikely aqueous releases, very high fluxes, no retardation, no matrix diffusion, a large quantity of water interacting with the waste. So either very unlikely aqueous or gaseous releases might start to jeopardize compliance with the EPA standard.

Then what we saw today several times is if we carry out the calculations for very long time periods, we get very high doses, hundreds of rems perhaps, a sievert or two, very high doses occurring at very long times to an individual to get back because we can't dilute the waste within this environment.

DR. NORTH: This is your signal for the five minutes.

DR. SINNOCK: Fine. We are right on schedule.

I want to point out that this is also a very robust kind of calculation. In effect, what we are looking at are the effects of radioactive decay. Neptunian stays around in the system. The WISP report found Neptunian doses of up to 10 sieverts back in the early 1980s.

The GEIS found high Neptunian as well as high Radium-226 doses for their "generic non-salt repository." But I think as we look historically over what performance assessment has told us, we start to see a consistent pattern of results. Maybe there is some reason to start getting confidence in these results if they stay consistent with different analysts, different models.

As we add more detail, the fundamental output of these models hasn't been changing. These seem to be fairly consistent patterns. Unsaturated sites are not good for capturing gaseous radionuclide releases. If they get out of the package, they are going to get out to the accessible environment. I don't think we should expect an unsaturated site to capture gaseous releases.

Very high doses for very long time periods -- this is the two-sided coin. Unsaturated sites are very good for limiting the quantity of water, but any releases that do occur can be very highly concentrated. We can't have both. We can't depress the total population dose, and at the same time continue to contain waste and depress individual doses. I think historically what I have seen is a set of consistent releases.

[Slide.]

DR. SINNOCK: To sort of summarize those in a word form, I think all those multiple barriers we have had, we can look at it. They interact into one thing in earth site, and that is delay releases.

If they delay releases sufficiently, the radionuclides decayed away, 8, 10 half-lives, then there is no problem in terms of health effects. However, if in combination those barriers can't delay the releases for 10 half-lives, then you have a dose problem. I think in our particular site -- and I am not sure that this wouldn't apply to any site -- for those very long time frames, I am not sure that we can find a combination of barriers that is going to delay for 10 half-lives of a 2 million half-life type of radionuclide.

Yucca Mountain wasn't there 10 million years, and 10 million years from now I won't think it will be, but we will still have doses, if not Neptunian-237, then perhaps Radium-226 as a daughter of U-238.

[Slide.]

DR. SINNOCK: So as an overall summary, I think we do have calculational tools in place that we can start placing some reliance on. The data are becoming rapidly available. Over history, we have seen a consistent set of performance results that I think are indicating robust behavior and a good understanding of that robust behavior at this site.

That is not to say I think we may need to cross some "t" and dots some "i"s and use that influence diagram-type of construct to better make transparent these results to other audiences.

I think we also have to take a look at the confidence issues if we have this robust results, and the technical analyses capture technical confidence in a way or quantify the confidence. But yet we still say, "I'm not sure about the model. I am just not sure yet."

[Slide.]

DR. SINNOCK: I want to put up this last slide to sort of leave you with. I think we have to be very careful to draw the distinction of what is, if you will, a technical confidence statement out of, say, system engineering, and a confidence statement of an individual that says, "I am just not sure yet of whether we sufficiently understand the system."

Yes, I understand your performance analyses, and I understand that it is a good analyses as far as you know, but I am not sure there isn't something that you've haven't incorporated or there isn't something we need to deal with further.

I think we have to very carefully separate that and bring to the socio-political dialogue that must occur over here, including the scientific community in its assessment, a firm statement out of here of what this set of performance analyses is telling us.

I will entertain any questions at this point. Thank you for your attention.

DR. NORTH: Thank you very much, Scott. That was exactly on time. Forgive me for flashing the five-minute sign by mistake early, but you recovered nicely.

DR. SINNOCK: A little panic.

DR. NORTH: Let me entertain questions from other members of the Board.

DR. DOMENICO: I do, as usual.

Scott, I am looking at this relative progress, your opinion, where you have given yourself very good marks on all of those items.

[Laughter.]

DR. DOMENICO: I would suggest one more item where I don't think you can give yourself a very good mark and that would be the conceptual model of the unsaturated zone, of which relatively no progress has been made. The model results are ultimately based on the conceptual model where you have put all the movement into the matrix and onto the fractures.

So I would say that relative progress in obtaining a good conceptual model of saturated/unsaturated flow, you can't give yourself too high a rating. Like you say, that's an opinion. Would you agree with that?

DR. SINNOCK: Yes, I have certainly heard at length the idea that we need to better understand the relationship between matrix and fracture flow in the unsaturated zone.

DR. DOMENICO: Your whole model depends on that. The output of your whole model depends on that.

DR. SINNOCK: In terms of the time of flow through the saturated zone, yes, the discounting. I believe there was allowed water flow within the fractures within some of the models.

But, yes, we have some to go. I want to come back to value of information. We have to very carefully consider what tests specifically can we design that will help us better formulate that model. We can treat it parametrically. We can put it in all in the fractures, all in the matrix to see what the bounds are.

I think we need to do that. If we still comply, if we still have robust performance, it may not be that necessary to resolve that uncertainty.

DR. DOMENICO: You have Tritium pretty far down the mountain. You have Chlorine-36 pretty far down the mountain. When we open up that tunnel down there, when it rains on the mountain, if it rains in the tunnel, we will know that it is not all exactly coming in the matrix, I think.

DR. SINNOCK: Yes, that could be true, but if that water has to interact with waste dissolvant and carry it out to some accessible environment, we need to do a sensitivity study to show indeed the concentration in fractures is something that is absolutely to be avoided.

DR. NORTH: I will break in here as Chairman and resurrect my scenario from this morning. It seems to me that your picture of going up and down the pyramid is a very good conceptual framework for addressing the kind of question that Dr. Domenico has posed and resolving it.

What does it take to make the repository fail against whatever performance measure we have used to define a failure? Then, what are the conditions under which this failure might occur? How likely are they?

If our failure scenario is that we have fast pathways from the surface down to the repository level, and then we have got a failure mechanism involving sealing up the Calico Hills creating saturated pockets of perched water, including canisters, and then a corrosion process by which those can fail quickly, maybe then we have found a situation that is credible for leading to repository failure.

Then we go back and ask of the models: Have they captured what we need to be able to describe this situation? Do we have our understanding represented well enough to explore that scenario and find out whether it is indeed credible or whether there are good reasons for dismissing it, at least to the extent that it becomes a lot less likely to cause failure against those performance measures?

I am impressed that we are making a lot of progress in this process, but I think we should be very careful about concluding that we are nearly there. I think we have some major areas with weaknesses that need to be carefully explored before we conclude that we really do have robust results in terms of the probability that the repository is going to fail.

So, I will urge that we think about a systematic look at areas where the modeling seems to be a little weak in terms of the connections between the top of the pyramid and the bottom of the pyramid where those issues might lead to a repository failure against the various performance measures that could be involved.

I doubt if we disagree on this, Scott, but I thought I would make my little speech and the question part.

DR. SINNOCK: Yes, I want to thank you.

DR. NORTH: Are there any other comments or questions?

DR. SINNOCK: Yes, I would like to respond and turn that point over a little bit that I think that before we are too enthusiastic to draw the conclusion that we have very robust behavior, I think we have to be just as careful not to draw the conclusion that our uncertainties are critical to reduce, that we have to systematically identify the influence of that uncertainty on the performance measures that we are particularly interested in. Sometimes those connections aren't obvious.

DR. NORTH: I am in hearty agreement. I think we can't judge just by large uncertainties that something is important. The issue is: Can this uncertainty cause the repository to go from success to failure against those performance measures?

So, we have to be careful what those performance measures are. In some cases now they are undefined, such as the individual dose issue and the length of time we are going to consider. We have to wait for the Academy to come back with their report and then see what EPA is going to do based on the Academy's recommendations.

So all this puts considerable additional uncertainty on performance assessment as to what it is that you need to do. But I think we are getting a great deal of additional insight there about how performance assessment will do the job against various potential measures of repository acceptability.

Anyone else for questions or comments?

Leon?

DR. REITER: Scott, could you just put up Slide 11 once again?

DR. SINNOCK: Mine don't have numbers.

DR. REITER: The one showing the persistence of conclusions, selected analyses for tuff, you know, what is okay and not okay.

DR. SINNOCK: Okay. The 3-D?

DR. REITER: Yes, that is a really interesting plot.

DR. SINNOCK: Yes.

[Slide.]

DR. REITER: Before I asked you what you conclude about this, vis-a-vis Yucca Mountain, there is another performance parameter that was not shown today which I have seen sometime in the past, and that is the NRC release criteria, mainly that you shall not release more than one part in 100,000 per year after 100,000 years.

At least in 1991 in some of the other conclusions, I saw that criteria routinely not okay, namely routinely failing that criteria. Is that a consistent picture?

DR. SINNOCK: This picture does not address that.

DR. REITER: No, but if you plotted --

DR. SINNOCK: There is not an exact correlation between compliance with NRC's one part to 10 to the 5th in compliance with EPA's rule.

DR. REITER: I understand that.

DR. SINNOCK: I would rather not comment here. I am not prepared to go into the details of the nuclides one way or the other.

DR. REITER: But is that correct to say that many of these assessments show routine non-compliance with that specific NRC criteria?

DR. SINNOCK: I think I am going to defer that to some of the Performance Assessment people.

[Slide.]

DR. DUGUID: Jim Duguid. Here is the release --

whoops, I have it upside down. It is better than that.

[Laughter.]

DR. DUGUID: If you just meet the NRC release criteria of 10 to the minus 5th of the 100,000 year inventory -- and notice here that it even gets the Neptunian peak higher than I showed it could possibly go. The reason for that is all the parents are behaving like they're alteration-limited, rather than being solubility-limited. But Yucca Mountain doesn't look very good if you just meet that criteria.

DR. REITER: The question is: If the criteria, as itself stands, do the performance assessments that have been done routinely show that you cannot meet that criteria?

DR. DUGUID: I think the C-14 does. There are probably other parts of it that do also. I think there is a lot of it that is violated. But meeting that doesn't assure you have met the standards.

DR. REITER: Right, I understand that. The question is that somebody --

DR. SINNOCK: Let me take a cut. My recollection, as I understand the question, is outside the scope of this a little bit: Are there several nuclides or many that do not comply with the one part in 10 to the 5th and release criterion for the NRC?

My recollection is not many and it takes a high solubility and some sort of segregation in the waste in order to get considerably high releases. Maybe Mike Wilson could address that.

MR. WILSON: My memory is not really good enough to go back much beyond 1991, but in TSPA '91 and '93, both, we violated that criterion by a fair amount.

As Jim Duguid pointed out, if you have a dose standard that requires some pretty low dose, you are going to have to have a much lower limit than that to be able to meet it.

DR. NORTH: I think at this point we had better conclude the discussion and go on to our next speaker, Max Blanchard at DOE, "How Will the Information be Used"?

HOW WILL BE THE INFORMATION BE USED?

[Slide.]

MR. BLANCHARD: What I would like to do now is take a few minutes and share with you from a Project Management viewpoint how performance assessment is being used.

One obviously is interested in asking the question: Is performance assessment mostly incorporated in the program where the PA people go off in a corner and do their calculations with an occasional output going to assist design, or is part of the culture of managing the program?

Well, at least in my view, I think it is part of the culture. What I would like to do is to describe some of the process where the Project Management activities count on and use performance assessment to make decisions.

You can also ask whether or not it is enough of the culture even today. I will be first to admit that we can do better. It could be more comprehensive. It is where it is.

I am going to try to give you a very birds-eye view in a simplistic fashion. Be that as it may, it is coming from those people who are using PA and who are trying to make Project Management decisions on an annual year basis.

[Slide.]

MR. BLANCHARD: The four topics I sub-divided into is first starting with the review of what were we trying to accomplish with a performance-based strategy when we started the goal of characterizing the site to determine whether or not the site might be suitable.

I will describe my current view, anyway, of the implementation of that strategy and whether or not there is an indication for revisiting that and changing what was the performance-based strategy that evolved in the mid-1980s and was released in 1988.

Then the use of performance assessment as a tool in a number of special studies that have been conducted, caused partly by questions that the management raises, caused partly by questions that a number of outside oversight bodies have raised, so that we can better define how to manage our program and how to put the components together more appropriately.

I, too, will also touch on things that previous speakers have said with respect to what the most simplistic view is of changing to a new standard might mean from a Project Management standpoint.

Then I would point out that there are other uses of performance assessment that spread across a number of activities that occur almost on a daily basis within the project. I will share with you some of those.

[Slide.]

MR. BLANCHARD: Starting first with the performance-based standard strategy, we evolved as we were developing the site characterization plan before it went to the Nuclear Regulatory Commission.

It was established on the basis of what we felt was expected site performance, as naive as we were about the characteristics of the site, but also the potential for disruptive events, the need to better understand the magnitude and recurrence interval of things that could be disruptive of waste containment and isolation.

It was also used to identify areas of emphasis in site characterization activities to help better define the 106 studies that were in the SCP. It is continuing to be used to better define how we want to spend our money on an annual year basis.

[Slide.]

MR. BLANCHARD: At the management level, we need to simplify a lot of these complicated things. So in an attempt to distill the performance assessment strategy that is encompassed in the 1988 SCP, we have recreated this table which is derived directly from the SCP.

We have it divided into postclosure and preclosure, and engineered barriers versus the natural barriers. Of course, the natural barriers are important in the postclosure calculations and not so important in the preclosure.

Now, we have also taken the barriers and divided them up into the simplest things, the unsaturated rock and the air gap, and of course, the objective. The performance objective we have there from a management standpoint is to limit the water available to corrode and dissolve the waste, from a container standpoint to serve as a principal containment barrier, from a waste form standpoint, to limit dissolution and leaching.

With respect to the natural barriers and the unsaturated rock below the repository, the Topopah Springs and the Calico Hills, to act as a barrier to radionuclide transport, especially the zeolites in the Calico Hills. Then for the saturated rock to extend the total travel time and to aid in retardation.

From a preclosure standpoint, we are looking at the surface and underground facility construction. We wanted the underground facility and its operation to provide a beneficial benefit to a postclosure system and to have no adverse impact, or to mitigate that adverse impact. We want a safe operation to meet worker and public standards.

So, in a simplistic fashion, while the modeling effort to look at radionuclide releases and how that reacts with design and how you understand those processes, in order to manage what we are doing, we have to distill it down to what we think are the simplest forms.

In my view, from a day-to-day standpoint, and from an annual year budget standpoint, things haven't changed. This is still what we are relying on. This was derived from the fundamental parts of the performance assessment that was done very early on when we were trying to use performance assessment to help us guide the development of the test program.

[Slide.]

MR. BLANCHARD: From a testing standpoint, we have used, and it is encompassed in that first document for postclosure, we have used PA to help us to determine the processes and characteristics of water flow in the unsaturated zone. I think you have heard from Scott and you have heard previous speakers talk about that.

That is fundamentally our number one question. We are still focusing on that and a number of you, including Don and Pat have said, "Well, you still don't know enough." We will surely admit that.

To investigate the conditions and characteristics that could affect how long that waste package will last. Then once it starts releasing what the radionuclide transport is like, a second most important aspect of the performance-based set of conditions for testing the site.

Then to identify and characterize the potentially significant disruptive processes and events. Again, we need test information about the processes that is apt to change the site from its current conditions. The kind of information that we need to understand the magnitude of the recurrence intervals of those events and how those events will reduce waste package lifetime and cause radionuclides to transport.

From a preclosure standpoint, the essence of what we need from the site program is better understanding the seismic hazards. That, we think, is the largest site information that is contributing to design from preclosure and looking at operational conditions.

[Slide.]

MR. BLANCHARD: Should that strategy be revised? Well, in that simple table form I think today where I stand as a Project Management person, probably not yet. However, things that are happening will impact that strategy. Changes to the standard. People are discussing that now. Some of you involved in it.

We have new evolving waste package concepts, a shift from thin wall stainless steel to MPC. There are thermal loading alternatives in front of us. The PA people are examining that and trying to provide information to design to build a better decision basis. Then, results of trade studies that are going on within the engineering departments.

So I would say it is probably premature to say that the 1988 performance-based strategy in a simplistic form is obsolete. However, these things are going to cause us to change that strategy. It won't be very long before we relook at that in a more thorough fashion.

[Slide.]

MR. BLANCHARD: Now we will move into another area, the status of the implementation.

[Slide.]

MR. BLANCHARD: I would just like to share with you what I would see as the highlights of our natural barrier site characterization program.

Again, I know that you have had briefings by Dennis Williams and by Russ Dyer who have talked much more comprehensively about all of the study plans and the various stages of the implementation, but what I am trying to share with you is a tops-down view of where you are trying to drive those department heads, where you are trying to drive the contractors into spending money, and what you are trying to achieve with that.

Well, the highlights, I would say, of our characterization program right now is here -- unsaturated zone. We are looking at focusing -- and we are focusing -- on deep bore holes in the unsaturated zone and understanding shallow infiltration. Our objective is to understand the flux and the flow mechanisms in the unsaturated zone and then to estimate the variability and the quantity of net infiltration.

With respect to the saturated zone, we have on-going a C-well complex test program where we are building a better understanding by testing of the properties of the saturated zone. Our goal is to better understand the flow and transport in that zone.

This is a cursory indication -- very, very brief as it is -- of the status of our progress. We are spending money there. We are doing tests there. We are analyzing models, analyzing data, incorporating into models.

[Slide.]

MR. BLANCHARD: With respect to the engineered barrier highlights in the waste form area, we are most interested in learning more about solubility and speciation because we need to expand the data base so that we can incorporate that in models.

From a container standpoint, we are looking at alternatives to the reference case. As you know, the MPC is a major alternative, but there are other alternative thicknesses in design materials. We are considering more robust alternatives and we are contributing a lot of work in the performance assessment area to aid the designers to better understand how to refine design concepts for the MPC.

[Slide.]

MR. BLANCHARD: With respect to the highlights of the disruptive conditions, we are looking at climate. We are doing paleoclimate field studies and modeling. Our goal is to better understand how to predict the effect on the hydrologic regime of future climate changes.

In volcanism, we are evaluating primary and secondary effects of volcanic eruptions to improve our understanding for the basis of calculations of the probabilities of disruption.

In hydrotectonism, we have had an on-going study for quite some time. We are assisted by a special panel in the National Academy of Sciences. We looked over the Trench 14 observations in the Calcite-Silica studies about hydrotectonism and hot rising hydro-thermal solutions. I think, although we haven't finished all the studies in this particular area of hydrotectonism, I think we have a major nail in that particular process.

From a seismic hazards standpoint, we are trenching and monitoring seismic activities. Our goal is to better understand what the basis for the design for the surface and underground facilities needs to be.

[Slide.]

MR. BLANCHARD: Moving on to the performance assessment studies that provide input to special studies, well, from a management viewpoint we have had five special studies. The first one was a test prioritization. Here we were ranking all of the tests in the site characterization plan in an attempt to determine how best to allocate our resources for improving confidence in 10,000 year release predictions.

Two of the major things that came out of that study were that we needed to focus our test program in understanding the mechanism of gaseous releases.

The second one was that we needed to continue to have a very high priority on funding for unsaturated zone flow and transport. So, that became a formula very early on for priority of spending money within the WBS structure that encompasses the site test program.

We conducted a risk benefit analysis for excavating in the Calico Hills. We were looking at the value of data that would be obtained, as well as what the potential was for adverse impacts because we were removing materials from the Calico Hills.

The knowledge we gained from that was that the excavation in the Calico Hills could increase confidence significantly in hydrologic models, and our understanding of those processes and the properties that contribute to those.

Also, that the excavation appeared not to be a significant impact on what we would expect to count on in the long-term performance of Calico Hills.

In the exploratory shaft alternative study where we were comparing 30 different alternatives, the first of which was the reference case where we had two vertical bore holes, large as they were, for the ESF all the way down to Option 30, which was north of the south ramp coming in with 14 miles of drifting.

We were ranking these alternatives in terms of mini-parameters, but two of the most important ones from the management viewpoint were increase of thickness between the repository disposal horizon and the bar table and another was to avoid direct connections between the waste emplacement area and the Calico Hills. Performance assessment was used to predict releases for all of those options.

[Slide.]

MR. BLANCHARD: In early site suitability evaluation, which was released a year and a half ago, our ranking criteria were all of the things that contribute to disqualifying and qualifying conditions in 10 CFR 60, the Department's siting criteria. I just wanted to make sure that it didn't look too myopic because that regulation encompasses all of 10 CFR Part 20, Worker Health and Safety, as well as 10 CFR 60, which is the Repository NRC Regulation.

What we gained from that was that at least in our view, and the view of those people who are outside the program who are experts that were used to peer review that document, was that disqualification of the mountain based on the available information appears very unlikely.

The highest priorities for completing suitability evaluations in the future, we needed to gain more information if we are going to close it off with this as a ranking criteria. A better understanding of gases releases, the worker health and safety part -- more design work so that it is more clear what probablistic risk assessments will mean for designing to worker safety.

Here is this one we started with a long time ago and kept there -- understanding unsaturated flow and the saturated flow and predicting climates in a more knowledgeable way than we have now.

[Slide.]

MR. BLANCHARD: The last special study we have just finished was integrated test prioritization. At a recent meeting with Russ Dyer, you all heard Russ describe how he uses the ITE results. We have been comparing results from the ESSE. We were comparing the things we were doing to try to improve compliance with Part 60. We were comparing how much different types of test data and modeling would help improve the general oversight confidence from a scientific standpoint that we know what we are doing. We were looking at costs.

The knowledge we gained included -- probably the most important thing is that we need to improve models and acquire data to help convince others that the scientific confidence should be there. So, we have spent more time and effort trying to associate funding levels in the test programs with improving scientific confidence.

A little bit lower in high priority was making sure that we continued to conduct tests so that we could acquire enough information to complete those parts of the license application identified in Part 60. But they weren't as important as gaining more knowledge about scientific confidence, how the process is really going to work at the site.

Then because we are at a state of maturation where it is not really evident that there is a disqualifying condition at the site -- in fact, most of the information looks just the other way -- we are beginning to place lower priority on funding test areas that would provide information on the suitability, or the disqualification of the site.

[Slide.]

MR. BLANCHARD: The next topic, status of compliance, I think that I can't really add anything except a very brief summary. The previous speakers, and there have been a number of them, who have talked about where the National Academy of Sciences is going.

For quite some time, it has been clear that Carbon-14 releases, as small as they are in terms of public health and safety, are still likely to exceed the existing EPA's remanded standard, and that the 10,000 year calculations, like what Jim Duguid has shown you, show that significant peak doses for any site with low dilution factors occur with or without long-lived waste packages, like the Neptunian problem.

[Slide.]

MR. BLANCHARD: In terms of 10,000 year cumulative release standards, except for Carbon-14, the site appears, given the state of information we have and the development of the models and the lack of maturation of the development of the models, the mountain appears right now as robust, in my view.

Continuing site characterization to improve scientific confidence that the probability of disruptive events and these other things that are called "unknown-unknowns" is low and it seems to be acceptable at this stage, but that may be naive because it is predicated on our understanding of the models and all of the available information that we have.

It is clear that if new standards are developed from this National Academy of Science effort that is going to advise EPA, and that they require 100,000 year protection rather than 10,000 protection, that this program as it is conceived now, will have to change its reference case to increased reliance on a more robust engineered system, and continue to have as an additional barrier, the natural barriers.

No matter what, we will have to continue to have focus priority in our test program to better understand flow and transport in both the saturated and the unsaturated zone. In this case, we will have to expand our test program to improve our knowledge in the saturated zone.

[Slide.]

MR. BLANCHARD: Now, sharing with you a little bit about how we use performance assessment in other areas of the program, I think you heard a discussion just before lunch by Jean Younker talking about waste isolation impact evaluations. I don't have any more details on this right now, but actually that is driven by the Q-List.

We have our Design Team develop what they think that are those things that are important to safety and isolation. Those items -- engineered items, designed items, or natural items go on the Q-List if we need to protect them because we are relying on them for releases.

We have an independent multi-disciplined team that evaluates all those analyses and then advises the manager as to whether or not we have an adequate control program. They ask from the performance assessment departments for independent PA analyses to help them review what the other scientists and the engineers are doing when they Q-List items to decide what type of quality assurance program to apply to their work.

So we do an independent check on that. Then we also ask this group up here that is doing waste isolation impact evaluations, to give us additional information to help us determine the warm fuzzies, if you will, as to whether or not we've got adequate controls on the test program.

For instance, it was controls that lead to the conclusion that while the TBM isn't a Q-Listed item, the release of oils from the hydraulic system could cause some things that we are not sure we would like to see happen in the rock as we excavate our way through the Topopah Springs.

So what we want to do is to place better control on eliminating or avoiding or mitigating releases of oil from the hydraulic system on the TBM.

That meant the engineers had to redesign the hydraulic system so that it wasn't characteristic of the way the hydraulic fluid is handled on a TBM under ordinary things, like if they open up the hydraulic system and drain it out, they just let it go. The oil just goes down in the ground and it stays there.

Well, in our scenario we said that is not good enough. We placed a management control. We forced the engineers to redesign the system so that we don't have those kind of leaks. So that is just an indication of how we used these two things in combination to assure ourselves that we have management controls in areas for things that aren't on the Q-List, but could have some sort of an adverse effect on future tests or on the long-term waste isolation.

When we go into license, we will have to show the NRC that we have responsibly conducted and managed the program in the area of those things that affect releases and adverse impacts on the site. We want these in our records so that we have a good cadre of people working on it and good conclusions that are supported by analyses.

Also, as you have heard, TSPA '93 provided input to engineered barrier designs. There was a lot of discussion on thermal loading and container thickness. I think Bob Andrews shared a lot of insight with respect to what our Design Team has learned as a result of those analyses.

[Slide.]

MR. BLANCHARD: Finally, every single year we take this spread sheet and reprioritize the test program and put the dollars where we think we will get the most bang for the buck relative to those uncertainties about the test program that we need to know more information about.

Finally, in the area of annotated outline and interactions with the Nuclear Regulatory Commission on issue resolution, performance assessment is providing information into topical reports. You know that we have released an erosion topical report. We've got low probabilities of extreme erosion.

We are continuing to provide performance-based arguments. They will be the seismic hazard methodology and these other subsequent reports that are in process, in the pipeline. The next updated version of the annotated outline will incorporate the results of TSPA '93.

[Slide.]

MR. BLANCHARD: So, in summary, then, we do have a performance-based understanding for site characterization. We have been and are continuing to refine that and improve it. We use it at all levels within the management. We have input from performance assessment to reestablish priorities for every year for funding. We make those decisions.

We allocate resources and the personnels, as well as place priorities on study plans. We rely on those arguments in managing site characterization and feeding the evolving design of the engineered barrier system as some of the speakers here this morning indicated.

That concludes a Project Management's view of how valuable performance assessment is to us on a day-to-day and on an annual year basis. To be sure, it will play a larger role as we go along. There will be changes as the program components are put together differently, especially as we begin incorporating some new constructs that Dr. Dryfus shared with you yesterday.

If there are any questions, I will be glad to try to answer them.

DR. NORTH: Thank you. Let me ask just as a point of information about the ESSE. Was final documentation on that ever issued?

MR. BLANCHARD: Well, final documentation, being the contractor's report, the Department, since it has had a change in directorship, has not revisited the method by which it is going to officially accept that document.

But we have incorporated at the management level into our decisions, into our priority of funding resources -- there is no official DOE report yet that says, "Here is what we are going to do, but the facts are we are managing with it on a day-to-day basis.

DR. NORTH: Is there a plan taking shape as to when that exercise is going to be iterated?

MR. BLANCHARD: Only so far as I know in the area of ensuring that we are putting the money and our personnel resources to produce another ESSE, if you will, 1995, 1997, and 1999, the scope of which will be changing. But each one will rely on the earlier one and have some higher degree of importance at the management level, and with respect to the suitability of Yucca Mountain.

DR. NORTH: I think I heard in Dr. Dryfus' remarks yesterday an urgency to get on with the process of determining whether there could be any disqualifying features at the site.

MR. BLANCHARD: Yes, I believe I heard that, too.

DR. NORTH: I would hope that the Project Management structure is going to take that guidance to heart and formulate essentially how this issue can be pursued aggressively, that is, making sure that all the lessons got learned from the last iteration of ESSE, and that the new iteration takes shape with all deliberate speed.

MR. BLANCHARD: Well, we are sharing our views and recommendations with me about how we could accelerate that so long as we don't get in front of the data that is needed to interpret the models to make the case and defend the suitability of the site, whether it is good or bad.

DR. NORTH: other questions from the members of the Board?

[No response.]

MR. BLANCHARD: Thanks for bearing with me. My voice is a couple of octaves lower than normal. I know it must be hard for your all to hear back there.

DR. NORTH: Well, I think we have the problem that with the different thermal loads yesterday and today, various flus and colds may have been aggravated a bit.

Ellis, at least I notice you are not wearing your coat anymore.

[Laughter.]

DR. NORTH: Any other questions from the staff? Dan?

MR. FEHRINGER: Fehringer, staff. Max, we heard a lot this morning about the potential for high individual dose rates at this kind of a site. Yet I didn't hear anything about what you are going to do about that. Are you actively considering ways to reduce those dose rates, or just hoping that they will not be an issue at the time of licensing? What is the Department strategy?

MR. BLANCHARD: Well, I think the latter would be very naive. I think it goes without saying that we would like to have the flexibility to make the waste packages and the engineered barrier system more robust than it is, considerably more robust. That is the simplest answer I can give you. I hope that is enough.

MR. FEHRINGER: I am not sure I understand what you mean by "have the flexibility." I presume you do have that flexibility, or do you feel that you do not?

MR. BLANCHARD: No, what I am trying to do is to say I am a little uncertain with respect to what the outcome will be when the new EPA rule is released, and then what the NRC does with respect to incorporating it into 10 CFR 60. I would hope that in that process there would be no procedural glitches which would take away our perceived flexibility to do that.

DR. NORTH: Don?

DR. LANGMUIR: Max, can you go back and show us overhead 8 again? That is the SCP strategy? This is the original strategy. I was taken by --

[Slide.]

MR. BLANCHARD: This one here?

DR. LANGMUIR: Yes, those two. There was one that discusses the natural barrier characterization and then the other engineered barrier. I worry that things are falling through the cracks if this doesn't get changed at your level.

I sense that at Los Alamos they are starting to think about the interfacing between the EBS and the near field. I see a disconnect. I see the unsaturated zone studies as the natural barrier characterization with a look at flow in the unsat zone, perhaps chiefly under ambient conditions, is the sense I get.

Then I see a limited view of the EBS looking at it strictly as a waste form in a container without considering what would happen if you added backfills and if you looked at the interfacing of radionuclide movement from this EBS part of the system to the near field. Who is responsible for making that connect?

MR. BLANCHARD: Well, the managers are. The managers are responsible for the connection.

DR. LANGMUIR: That is a big gap. But you need that for a full performance assessment.

MR. BLANCHARD: The reason it is the way it is right now is that we have a controlled process for the way we change the design concepts. The current Change Control Board package of things that drive the program doesn't have, for instance, a lot of the new things that are studies going on now, like the MPC is not the new baseline yet. It is about to become.

The orientation switch of the first five miles of drifting is not yet in the baseline. In fact, the Change Control Board package just came into our office this week. The M&O Design Team has just finished it.

So those things will be reflected in these new strategies as we change the baseline. So, you are right. You are very alert to recognize that some of the things we are going to are different than our reference case. We will be back again should you invite us to explain how we have updated that reference case and what the basis for the update would be.

Those are the kind of things that the MPC and the new exploratory studies, orientation and whatever comes out of the new examination of: Where should we go in the Calico Hills? How should we do it? Those are all things that are currently being examined by multidisciplined teams putting together a package in a controlled way like what you would say, "design control."

DR. LANGMUIR: Well, what I was thinking about here in particular was the source term which includes not only the corrosion of the canister in the waste form, but also the movement of radionuclides or how they might get moved through a backfill, or some sort of a filling material into the near field under thermal conditions. I have not seen anything on that at this point in the program.

MR. BLANCHARD: You are right. Those would be conditions of adopting, at least from a manager's viewpoint. Those kind of things would have to be filled in as a condition of adopting a new baseline which went to a MPC. So they should be there. They are there in terms of what is needed to be done to implement the strategy. They come to us at the Change Control Board level as saying, "If you are doing this, then you also have to do all of this."

So they should be there. They are not there yet, but this is a reflection of the way it currently is in the reference case.

DR. NORTH: Dr. Price?

DR. PRICE: Max, I am forever bringing things on your watch that don't necessarily completely pertain to you. This is no exception, but I wanted at some point to be able to make some kind of comment about this since. I understand we have a little time, so I will try to do it.

In the performance assessments that we have been briefed on, I haven't really seen much of anything about operation of this system and the impact of the different alternatives and operations. We have to operate this thing.

The different heat loads have something to do with how you operate it. The different emplacement strategies have something to do with how you operate it and so forth.

MR. BLANCHARD: Especially retrieval.

DR. PRICE: Retrieval, exactly, yes. So there are a number of things that appear not to be being considered at this point with respect to operations. Maybe it would have been more important to bring this up on my watch in Systems Engineering aspects of it because we have been asking for Human Factors since the beginning of the Board.

We are beginning to see some hints that maybe there will be Human Factors. But it belongs at the very conceptual stage when you are doing these trade-offs. You have to run this thing. There have to be people in it. Systems Safety as well has to be in there.

It seems to me that we have a big hole. We have mentioned it before. I am sure that there must be some performance assessment interest in the aspects of operation of these facilities in the human role in the operation of these things. But it isn't yet part of the performance assessment.

But I know there should be a big system engineering interest in these aspects of those things. Now, that is as close as I can get to performance assessment on the comment that I wanted to make which had to do with Human Factors.

I do have some other questions about Systems Engineering things because, as indicated last time, I am somewhat pleased with the Systems Engineering, but I think that we have some questions.

One of them has to do with project systems engineering and how it interacts with headquarter and M&O Systems Engineering. Are you both talking to each other? We heard from headquarters and M&O yesterday, but I am not too sure that we got the same kind of picture when the project talks about Systems Engineering.

So, I am concerned about whether or not what goes on at Headquarters and M&O has something to do with the project with respect to Systems Engineering.

So those are two things that I got off my chest, Max.

MR. BLANCHARD: Good. Well, I think both of those are astute observations. From the Systems Engineering linkage standpoint, things are working well in terms of requirement documents and the flow of that information and what you have to do to assure that you understand your requirements.

Moving into the operational sense, we have not done very much at the project. I don't think that an awful lot, other than conceptualizing the MPC, has been done at the headquarters since.

The reason for that at the project -- although it may be a feeble reason to you -- is that we chose to put our money in the test program and the underground excavation program for the ESF in order to get enough money to operate things as they are, occasionally a second shift for a drilling rig or for a window of two or three months, a second shift for underground excavation.

We just haven't had the money in the evolving area of maturing the advanced conceptual design for the repository and the engineered barrier system. So, for the last three years we have literally starved that team. They have been down to very few people. We have spent very little money there.

It is an area like you perceived that needs, let's say, a transfusion of money in order to help better build an understanding so that the operational sense is as well understood from a worker safety hazards standpoint as the rest of the program. The PA hasn't been doing that; neither has the Design Team. We haven't even assembled a Design

Team to do that. Our goal in the next year or two is to bring that level of maturation up.

So I would suggest you keep on the same focus you have been and keep trying to measure us with respect to where you think the baseline ought to be.

DR. PRICE: Ye, I think it isn't just safety. It also has a lot to do with total life cycle costs, particularly with the thermal loading aspect of things. If these analyses are all done devoid of the human element, when the human beings get in there and start operating that system, they will wonder what in the world these people were doing with all of that money?

MR. BLANCHARD: It may be impossible. Yes, I understand your point. All I can say is we make some decisions on where to put our finances. History may question those decisions, but right now we short-shifted the Design Team and the money. We are continuing to do that for this year. But we recognize it is a problem. We are going to try to reallocate resources soon.

DR. NORTH: Thank you. At this point we are a few minutes over. So we will conclude this discussion and go to Robin McGuire of EPRI.

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[Slide.]

MR. McGUIRE: Thank you. Well, it is always a pleasure to come here and share some thoughts with you on the EPRI performance assessment effort.

I should point out I am merely reporting it. I have conducted the performance assessment, but have used inputs from a large number of consultants to EPRI as well as technical people at EPRI itself.

[Slide.]

MR. McGUIRE: I would like to review some of the general objectives and then of the specific objectives just to emphasize that we are operating completely independently of the other performance assessment methodologies and applications here.

I will just go through briefly some of these bullets. The idea is to identify alternative descriptions, both of current conditions and of future scenarios, identify randomness and uncertainties in those and their associated probabilities.

[Slide.]

MR. McGUIRE: Another general objective was to explore calculational methodology for how the repository works, how to estimate site performance, and how to calculate site performance for a range of these descriptions of current conditions, future scenarios, and their probabilities.

[Slide.]

MR. McGUIRE: Interaction among the different disciplines was an important characteristic of the kind of model we are trying to build. We saw great benefit in providing this interaction and cross-fertilization among the diverse disciplines.

[Slide.]

MR. McGUIRE: Finally, in terms of general objectives, of course, we wanted to investigate site suitability, sensitivity to the input assumptions, and also explore how we could take these results from performance assessment and use them to derive recommendations on priorities for refining interpretations, in other words provide, priorities for site explorations -- site experiments.

[Slide.]

MR. McGUIRE: The specific objectives, of course, were related to Yucca Mountain, develop an understanding of the relationships at Yucca Mountain, and develop a mathematical model that quantities site performance for Yucca Mountain.

[Slide.]

MR. McGUIRE: We exercised that model with reasonable probability estimates to make a current calculation of the likelihood of site suitability, to put priorities on site investigations, to reduce the current uncertainties, and to evaluate to what extent future data collection would reduce uncertainty.

I wold say that with our current application, we are saying that the models that have been developed with our consultants demonstrate reasonable probability estimates through results for release. I will show results for dose later. Those are more illustrative. I would not say that we can defend those as the best estimates, or in any case, even perhaps reasonable estimates that are more illustrative of what we get in terms of dose.

[Slide.]

MR. McGUIRE: Just briefly, we had experts and teams in each discipline. We held workshops to talk about the problem, provide first sets of interpretations, refine those interpretations, and then finally documented the input, the methodology, and the demonstration results.

[Slide.]

MR. McGUIRE: The disciplines that we had involved are shown here. This tries to organize them in terms of external influences, climatology, tectonics, volcanism, intrusion. The effects on the waste package -- geochemistry and the source terms, and those interacting, each of those sets interacting with hydrology and rock mechanics to allow us to calculate risk analysis for release, going then to dose and then producing those results for documentation purposes.

[Slide.]

MR. McGUIRE: There are some restrictions or limitations on the model that we have used. It is good to review those briefly.

We use a one-dimensional flow and transport model. However, we have multiple pathways considered. I will illustrate what those are a little bit later.

This is my mea culpa here. This should be time invariant calculations with respect to several things. Unfortunately our spell-checker isn't smart enough to catch the error there. But we have time invariant calculations with respect to the elevation, the water table, and the saturated/unsaturated state of the repository. Also, with respect to fractions of the repository that are in various states of saturation.

We don't consider daughter products at this point, although we are considering adding calculations of daughter products this year, and we don't considered dispersion in nuclide transport.

[Slide.]

MR. McGUIRE: We use a logic tree methodology as opposed to Monte Carlo wherein any uncertainty that we have is represented by a discrete distribution with alternatives. The probabilities are estimated for those alternatives here for external impacts, resource terms for hydrologic properties. This gives us a set of assumptions here with a set of parameters for which we can assign probability, which is just the product of the probabilities of those branches.

[Slide.]

MR. McGUIRE: We can then use that set to make release calculations. For each of those sets of in-branches are sets of assumptions. Each of those curves or releases has an associated probability for which we can derive a CCDF.

That is in a nutshell how we do the calculations and also allows us some flexibility and advantages in terms of doing sensitivity studies.

[Slide.]

MR. McGUIRE: This is an illustrative logic tree. It replaces the one that is in your hand-outs because it illustrates that we have in the decision analysis sense included decision nodes as well as uncertainty nodes. The boxes represent decisions.

We treat these as design decision, that is the heat loading in which we consider 57 kilowatts per acre, 114 and 36 kilowatts per acre, a decision node representing the choice of the container, and then uncertainty nodes representing, for instance, net flux, the heat transfer mechanism, et cetera. I don't show the entire logic tree here. It consists of all those inputs from all those disciplines that I illustrated earlier in the box chart.

[Slide.]

MR. McGUIRE: Each of those nodes has quantified values associated with it. In this illustration I show the one here for net flux where we have three values of flux associated with three probabilities.

It is easy to say, "Gee, that is pretty simple." I should point out that getting these six numbers probably involved about six man months of effort -- a climatologist and a surface water hydrologist -- to develop models, look at global climate models, look at models of surface hydrology, derive those, derive distributions, and represent those with a discrete distribution of three values with three probabilities.

So although it looks simple in the end a great deal of effort goes into making those representations that go into our logic tree.

DR. NORTH: I would like to encourage a parenthetical comment. You have all of that documented in EPRI reports; is that not correct, and they are published?

MR. McGUIRE: Yes, absolutely.

DR. NORTH: And available for anybody in the audience who wants to request them of you?

MR. McGUIRE: Absolutely.

DR. NORTH: Thank you.

[Slide.]

MR. McGUIRE: The latest calculations with our program which has the label "IMARC" extends our calculations to 100,000 years. We have a new source term that is developed from one of the consultants to EPRI, INTERA, for moist-continuous conditions. Again, it is a very elaborate source term that we synthesize into a very simple representation, actually using surface techniques.

We have episodic fracture flow included. We have alternative heat loadings, as I mentioned. We have three thermal mechanisms that we evaluate, and I will get into that a little bit later.

We have 16 flow paths representing combinations of four water contact modes -- that is, dry, moist-continuous conditions, wet drip conditions, and episodic conditions -- and four temperature profiles.

[Slide.]

MR. McGUIRE: Those are shown here. For historical reasons they are labeled the way they are -- alpha, beta, gamma, and delta. These represent a range of interpretations on what might be temperature conditions in various areas of the repository.

[Slide.]

MR. McGUIRE: In terms of these 16 classifications, or fractions of the repository -- the four moisture conditions and the four temperature conditions -- this illustrates a set of those fractions that derived from our consultants for an areal power density of 57 kilowatts per acre, and assuming that the thermal transfer mechanism is conduction dominated. That happened to have a probability of 0.5 assessed.

Here the assessment is that the 75 percent of the repository falls under that alpha curve, or that moderately high curve, 10 percent under the beta, and 15 percent falls into gamma.

[Slide.]

MR. McGUIRE: Those tables are kind of hard to look at, so we have formulated some cartoons here that represent the same information. I will go through a few of them because it is a lot easier to visualize what is going on in the repository.

For that same case of 57 kilowatts per acre, and conduction dominated heat flow, we have most of the repository in the alpha curve here -- that is the yellow curve -- and lesser fractions in the beta and gamma curves, and some fraction of the repository under wet conditions. Most of the repository will be under dry conditions.

[Slide.]

MR. McGUIRE: For convection-dominated heat flow, or high permeability in the repository, we see less of that area in the alpha condition. This is the beta curve here. That consumes a larger part of the repository.

[Slide.]

MR. McGUIRE: Finally, if heat pipe conditions are applicable, or what we call water being mobil in fractures, we associate it with heat pipe conditions. Then an even larger part of the repository would be under wet conditions, and that's the estimate of our consultants.

[Slide.]

MR. McGUIRE: Some other extremes. Here is the case where the areal power density is 36 kilowatts per acre, then we see a large fraction of the repository is in very cool conditions. Again, not much of it is very wet.

[Slide.]

MR. McGUIRE: The other extreme is for 114 kilowatts per acre. Here much of the repository follows that very high curve, the delta curve, and smaller parts of the repository, smaller fractions, follow the other curve.

So we have ways of then representing all parts of the repository, how they might act in time and under what moisture conditions.

[Slide.]

MR. McGUIRE: Let's go to some results. Here, first for release, this shows the CCDF as a function of choices of the waste container. As has been pointed out, I think the others, if you have a multi-barrier or a stronger container, add 10,000 years. These results are for 10,000 years. You get a lower CCDF than if you have a single barrier container. The ones we considered were steel and alloy containers.

[Slide.]

MR. McGUIRE: That is the case for 10,000 years. If you look at longer time periods, of course those differences among containers don't matter so much. The previous curves are shown here for 10,000 years. These two, the solid curves, are for 100,000 years, so that you see the releases come together. As you go out in time, of course, it doesn't matter that your containers may last for 10,000 or 20,000 years. At 100,000 years, you tend to get the same performance. That should not be surprising.

[Slide.]

MR. McGUIRE: Here is a result that shows the sensitivity of the steel container at 10,000 years and an areal power density of 57 kilowatts per acre to changes in the water table.

The integrated cases, the slight blue curve here, the lower cases are for moderate changes in the water table, and the high curve here is for a 230 meter change in the water table, which I think addresses at least in concept Dr. North's concern that if part of the repository is flooded, you can get very poor performance compared to when it is not. That is, if it is a saturated repository, it is not going to perform very well. In this case, we estimate half of the repository, as an illustration, would be flooded.

[Slide.]

MR. McGUIRE: Another sensitivity here is for the same case to the velocity in the saturated zone. Here we have the integrated case. The highest cases is for 10 meters per year and the lower curve is for 1 meter per year of horizontal velocity in the saturated zone.

I put this up to compare it to later results for doses in which these curves are exactly reversed. That makes sense. Here you get less release at 10,000 years if you have a slow velocity, but if you are looking at dose, that concentrates the radionuclides higher so you get a higher dose for this lower velocity.

[Slide.]

MR. McGUIRE: Here is a sensitivity to fracture/matrix coupling. The weak coupling represents our equivalent to the WEEPS model in which water is shooting out fractures. A strong coupling represents more of the matrix-dominated flow.

[Slide.]

MR. McGUIRE: Let me go on to illustrate how we calculate doses. I want to emphasis that word "illustrate" again. We use two factors -- one probability of exposure to the critical population, and the second, what fraction of that critical population is that receives the dose.

We looked at several scenarios. The small population, which is the farming scenario, and a large population, and we calculate those populations and fractions according to some numbers that I won't go into in detail, except to point out that they are calculated differently in a logical way.

For instance, this factor, P5 that I will talk about and will illustrate on the next slide, is the probability that if you drill a well into the contaminated plume, that that contamination is identified and corrected. That is logically represented as a fraction for the small population because you would make individual decisions if you drill farm wells, for example.

It is more of a probability for a large population because those wells would contribute to a community water system, for example, so you would either test all of the wells or test none of the wells.

[Slide.]

MR. McGUIRE: I won't go through these probabilities in detail, but just to illustrate that we have made quantitative estimates of them. Bob Wilems has contributed to that quite a bit and has provided most of these numbers.

[Slide.]

MR. McGUIRE: The critical distinction between the probability in the fraction that should be brought out is that the probability modifies the probability of exposure, which is a vertical axis on one of these CCDFs. The "F" modifies the dose to the average individual, which is a horizontal axis.

[Slide.]

MR. McGUIRE: So what we have, then, is a calculation of dose to the maximally-exposed individual, which is the top curve. That is obtained using our release numbers and also using dose conversion factors provided by INTERA.

We then modify those by those fractions and P sub Es for current technology, small population; advanced technology, small population; et cetera, to get these curves. So I think to compare our results with the previous ones that have been talked about, this is the worst exposed individual. That is the person who has the drinking straw right down in the plume and drinks two liters a day, and waters his garden, et cetera. Then these are CCDFs for other populations.

The analogy I would make is that we should look at -- we are not trying to control the risk to that worst exposed individual, just as for instance, in controlling risk in the airline industry, we recognize that there will be a plane that takes off, flies this summer, gets into a micro-burst and crashes as it is trying to land in Des Moines or some place. We are not trying to protect the person who gets on that plane. But what we are trying to predict with airline safety is the probability to the exposed population, which is all of us in this room that fly, that the probability of that scenario is very low.

So that is the distinction. We recognize that somebody potentially will get a large dose here -- that worse exposed individual with a drinking straw, but the exposed population, that is, the people in the area who drill wells and get their water from the region, will have lower doses here according to other CCDFs. The question is: Are those low enough?

The vertical line here is at 4 millirems per year which has been mentioned as a possible standard.

[Slide.]

MR. McGUIRE: Okay. Wrapping up here, I will go to some illustrations of the dose curves that we get. We get very similar results for the different container designs. Again, this is because we are looking at maximum doses over 100,000 years, so the container design doesn't make much difference.

[Slide.]

MR. McGUIRE: This is again the case for the change in the water table. Again here we see if part of the repository is flooded, we do indeed get high doses to all individuals.

[Slide.]

MR. McGUIRE: These results are for that small population, current technology, that worse case except for the worst exposed individual. Here is the illustration that in this case, contrary to the other one, when we have a low velocity in the saturated zone, that provides a higher dose than if we have a high velocity in the saturated zone, in which case we get more dilution.

So I think I will wrap up there and entertain any questions.

DR. NORTH: Questions?

[No response.]

DR. NORTH: Don't tell me everybody is exhausted. Any questions from the staff?

[No response.]

DR. NORTH: What is the status of the publication of this, Robin? Is this most recent version in a draft report form?

MR. McGUIRE: It is not. That is in the pipeline and due to be submitted to EPRI in a couple of months, including the dose calculations.

DR. NORTH: So what would be a reasonable estimate for when it would be available to interested parties and the public?

MR. McGUIRE: Probably this spring.

DR. NORTH: Do you want to comment just for everybody's information on the level of effort this represents? You mentioned one piece of it and the effort there.

MR. McGUIRE: Well, it has been going on since 1989. Roughly -- I don't know if I should speak for EPRI -- but on the order of maybe --

DR. NORTH: Person-months or person-years might be of help?

MR. McGUIRE: Maybe 10 to 20 person-years, something like that.

DR. NORTH: So by comparison your effort is very small relative to the person-years in the DOE team?

MR. McGUIRE: I would think so, yes.

DR. NORTH: Leon? Leon gets hidden behind Pat's head, so I have difficulty seeing him.

DR. REITER: Robin, I am not quite sure I understood whether these were conditional CCDFs or not. The one where you showed sensitivity to the water table change, you said that the integrated case. If I understand, that is sort of either comes close or just about nicks the exceedance criteria. Are those conditional CCDFs? Are those CCDFs that --

MR. McGUIRE: Let me put that up. Here we go. Sorry.

[Slide.]

DR. REITER: Okay. So, the integrated case -- is that light blue light that just sort of nicks the bottom of the criteria there?

MR. McGUIRE: That's right.

DR. REITER: What does that mean? Is that integrated case a weighed probability?

MR. McGUIRE: That integrated case is a weighted probability of each of these scenarios, as well, of course, as many others. I am glad you brought it up.

The point is in this curve that the high end of our curve is driven by this probability that half of the repository may be flooded. We estimate that probability as about 10 to the minus 3, which is the difference between this value and this value.

So, if you say, "Well, I think the repository is flooded. What is going to be the CCDF?" you would put probability unity on this blue curve and this would be your calculated CCDF. We don't think that is the case. We think that the probability is about 10 to the minus 3. So our best estimate of the correct CCDF today, given our uncertainties, is this light blue line. That is what drives the high end of our curve.

DR. REITER: Yes, but that assumes it takes care of all of the other elements you have in your model, right?

MR. McGUIRE: Yes, certainly.

DR. REITER: So am I incorrect in saying that that is the highest aqueous release for 57 kilowatt acres that we have seen anywhere yet? I can't remember whether it was the Sandia, or the PNL, or the RIP showing any aqueous releases for 10,000 years that came anywhere near that.

MR. McGUIRE: I think that is right. Their curves were over here.

DR. REITER: Right.

MR. McGUIRE: I think the reasons are two. One, this probability of flooding half of the repository, which gives you high release, and second, we have a shorter travel time in the saturated zone.

So, both of those things, I think, lead us to aqueous releases that are up here versus one or two orders of magnitude lower.

DR. REITER: So if the travel times in the saturated zone are like you indicate, and the probabilities, I guess those are your expert indications of what the probabilities are?

MR. McGUIRE: Yes.

DR. REITER: What would cause the experts to say there is a one in a thousand probability that the repository will be flooded?

MR. McGUIRE: They didn't say that. That came out of a combination of things. One, a high infiltration which raises the water table about 100 meters, and second, effects of earthquakes -- tectonics -- which raises it another 100 meters or so. So it a combined effect. That itself has a very low probability. It is a product of probabilities.

DR. REITER: You mean effective earthquakes vis-a-vis Szmansky?

MR. McGUIRE: Yes.

DR. REITER: Long-term effects?

MR. McGUIRE: Yes.

DR. REITER: So you are assigning some level of credibility, albeit it very low, to the fact that Szmansky is right?

MR. McGUIRE: Yes, that is in the model.

DR. REITER: Interesting. Thank you.

DR. NORTH: Of course, this scenario I brought up of localized flooding could be fit into this framework as well?

MR. McGUIRE: Yes.

DR. NORTH: We could argue about what that probability is.

MR. McGUIRE: Yes.

DR. NORTH: Do I have any more burning questions?

[No response.]

DR. NORTH: Let me then take a minute, as we are suddenly on schedule again, before the break, to introduce a new member of the Waste Board staff. Dr. Victor Palciauskas is in the process of joining us. We are delighted to welcome him to the Board Technical Staff.

He has a degree from Dr. Cording's institution, the University of Illinois at Urbana. His Ph.D. is in physics in the area of solid state, but he then turned his career in the direction of the Geology Department where he was an assistant and then an associate professor, receiving tenure in 1976.

Subsequent to that, he went on to Chevron field research, where he worked on the interpretation of geophysical data in rock and fluid properties. He has extensive publications, including a book of which he is the co-author, entitled, "Introduction to the Physics of Rocks," and a number of technical publications, some of which were co-authored with Dr. Domenico.

I would also like to note that for any of you interested in chess, Dr. Palciauskas is a world-class chess player, having won the 10th World Correspondence Chess

Championship, and being inducted this last year into the Chess Hall of Fame, for which we congratulate him mightily.

DR. LANGMUIR: A book has been published to translate it into Polish.

DR. NORTH: The book has been translated into Polish.

[Laughter.]

DR. NORTH: On that note, let us take a 15-minute break. Please be back at 2:45 sharp.

[Recess.]

DR. NORTH: Please take your seats.

It was not the Chair's intention to get people to sit down by causing the shiver and freeze.

Okay. We are now to hear about the lessons from WIPP. We are pleased to have with us Rip Anderson from Sandia.

Dr. Anderson, where are you? Dr. Anderson will now give us the view from the Waste Isolation Pilot Project where he has been involved in the performance assessment activity for, I believe, the lifetime of that program, or at least the last several iterations.

Dr. Anderson, please go ahead.

THE VIEW FROM THE WASTE ISOLATION PILOT PROJECT (WIPP)

[Slide.]

MR. ANDERSON: Thank you very much.

In some ways I feel like I have been here for a long while because I know many of the Board, and in some ways I feel like I am a total foreigner because we are talking about the Nevada program rather than the WIPP, although I know quite a little bit about the Nevada program.

[Slide.]

MR. ANDERSON: What I would like to do today is walk you through about four components of the WIPP, talk a bit about the regulations -- I know you know a lot about them, so I won't spend much time -- talk about the PA methodology. Again, I think you know quite a little bit about that so I won't spend much time -- the present status of the WIPP PA and finally some lessons learned, at least from the standpoint of WIPP.

Before I get started in this discussion, I would like to point out that both Holly and Robin identified -- although they didn't come out and say it in this way -- what we consider the remaining large technical hurdle that we have to cross, and that is that we have all this multitude of conceptual models sitting out there, some of them, two, three, and four in one sub-unit within the program. It isn't clear how you blend those individual conceptual models in order to give you the final CCDF.

I will spend a lot more time talking about that in a little while, but I did want to bring that up right off the bat as one of the lessons that we are learning right now rather than that we have learned.

[Slide.]

MR. ANDERSON: I put this up here to remind me to point out to you that until the new standard was promulgated, we were using the 1985 standard as our target for 191, agreed through an agreement with the State of New Mexico, and with the DOE. So, the information that I pass to you today will be based on the target of a 1985 standard, not on the 1993 standard.

I have indicated here with this slide with a line-by-line that we have indeed done that. If anybody is so interested, contact me later and I will send you back a copy of the line-by-line, which is a easy way to determine where the changes in the regulations have occurred.

[Slide.]

MR. ANDERSON: I have included a half a dozen of the line-by-lines, but for reasons of time, I will not cover those line-by-lines.

[Slide.]

MR. ANDERSON: I will go directly to the summation here and spend a few minutes talking about that.

The new regulation for WIPP, in effect, changes the undisturbed scenario for groundwater protection. By the way, it changes the number as well. Some of my viewgraphs are old so you will have to pardon me if I have a different subsection for the groundwater protection.

For the undisturbed scenario for groundwater protection, it changes it from 1,000 years to 10,000 years in the calculations, and for the individual protection, it changes it, again, in the undisturbed case, from 1,000 years to 10,000 years.

It does not change the release limits. They were at 10,000 years for all scenarios. They remain the same. It did not change anything in the institutional controls areas. This is an area which you will hear me talk about a little bit later as one of the benefits and one of the conceptual model changes.

Also, the National Academy of Science has asked us, in addition to these types of calculations, to complete a set of safety calculations. Assuming that we get direction from DOE to do so, we will again do safety calculations as well as the compliance calculations.

Again, pointing out that the calculations and the results that I show you to date are aimed at the 1985 standard, not the 1993 standard.

[Slide.]

MR. ANDERSON: The standard, then, for 1985 -- and it hasn't changed in this aspect -- indicates that you should do an analysis that identifies the events and processes, combines those events and processes and in some way measures the performance, estimates the cumulative releases associated with all the uncertainties for the significant processes and events, and then presents them in a probability distribution function to the extent practicable.

I bring this up because lately we have been questioned to some extent on why we are putting it in a cumulative distribution function. The point, again, is that the regulation continues to suggest that that is the way they would like to see it.

[Slide.]

MR. ANDERSON: Now, in addition to 40 CFR 191 for WIPP, we have another regulation that is equally important, and that is 40 CFR 268.6, or RCRA. This regulatory performance is a measure of the concentration of specific hazard materials at the boundary. That includes volatile organics and heavy metals. The regulatory boundary for that set of calculations is different than for the 191.

[Slide.]

MR. ANDERSON: Just to point out where those boundaries are so that we can get a picture of where the calculations have to terminate, for 191, it is the land withdrawal area or five kilometers, whichever is smaller, and that cone of that land withdrawal area, to the center of the earth.

For 268.6, it has been negotiated, and possibly will change, but I think probably not, it is the top of the salt, the bottom of the salt, and the land withdrawal area. So, in effect, we have this boundary is common, but the boundary below and above are not common for the two calculations.

[Slide.]

MR. ANDERSON: We, however, plan on covering the same time period, use the same data and conceptual models, and use the same comparable computational models. The main difference between the two regulations at this point in time is for 268 -- we are talking only of the undisturbed. For 191 we are talking of the undisturbed, or, if you will, the groundwater and the individual protection. In addition, we have the other section, which is the human intrusion section, or the adverse conditions.

[Slide.]

MR. ANDERSON: How do we do those calculations? Well, this is old methodology for almost everybody, but I need to spend a minute talking about it anyway.

You gets a systems description which includes the waste characteristics, the facilities, and the site. Those are used to develop a set of scenarios -- really, events and processes which are built into the scenarios. Those are screened.

Then when you get the tools, the calculations tools over here to do your consequence modeling, you do the sensitivity uncertainty analyses. You present them against either the 191 standard or the 268 standard. In this case, groundwater containment or individual protection.

Early on when I was working with the people developing the methodology, we thought that this was about the size of the box that we would be looking at as far as the computational tools that would be needed in order to do those kinds of calculations that would show how close you were to some standard.

[Slide.]

MR. ANDERSON: Let me suggest to you that now the world has grown, at least in the case of WIPP, to be very, very complex. Rather than have a four-box model, we got a multiple unit box model where we have broken it down into system, sub-system, and component.

Other than to point out that the complexity of WIPP has gotten large, I do want to suggest that we do sensitivity analysis on the subcomponent. We do sensitivity analysis on the subsystem -- and you will see some of those examples in a little while -- and on the system itself to point out the important and then non-important components.

[Slide.]

MR. ANDERSON: Let me then suggest that the interfaces between the R&D groups and performance assessment looks something like this where annually the models and the parameters changes flow from the individual research and development groups to the Performance Assessment Department -- my department. After the analyses are complete, the sensitivity guidance flows back to the individual R&D departments.

The problems -- I shouldn't say problems -- the methodologies for data flow along this arrow are pretty smooth. the flow of conceptual models from these groups to here are still confused at this point in time in that we don't know how to handle them once there is more than conceptual model for any one subset of the overall process, in other words, if there are three or four conceptual models for how the source term might perform.

[Slide.]

MR. ANDERSON: Well, how is this all wrapped up, all of these components, into a set of models that we can use to show compliance? Here, in effect, is a cross-section where we have the main ones identified where we have what we call CCDFPERM which it constructs a CCDF. Then you have the CUTTINGS model which addresses the cuttings that occurs for any human intrusion hole.

We have a transmissivity field model which, in effect, takes the multiple transmissivity fields from the somewhat limited data we have and gives us those.

Then there is the SECO2D and SECOTP which is the flow and transport model in the Culebra. We have BRAGFLO which is a two-phased flow in the panel. We have two phases, remember, because we have gases generated from the waste, and we have brine coming into the panel.

Then there are the panel which handles the radionuclide concentration and the gas generation. Of course, when this is all coupled together, we get flow up and into the Culebra Dolomite out through the Dolomite which is the only operable aquifer and out to the boundary, where we count it.

We also have the GENII-S code which handles the dose as it comes up through CUTTINGS and is treated by Mother Nature in wind blowing type activities and, of course, released to the stock pond which is then cow-to-man type dosage.

We have a couple -- I missed one on purpose until I looked at my notes, I missed it -- SANCHO. That is a room closure code. It turns out to be a very huge code. So what we have done is to run SANCHO on the site to give, in effect, how the room closes because the salt is plastic, and then use that as a look-up table.

We have talked earlier about other models which, in effect, are look-up table type models which in the end analyses, you could make all of these into look-up tables. Then you would have something that would be very quick and would be very high level.

But remember, all of those look-up tables have to be backed up by some kind of analyses or data like the SANCHO. Okay. Enough of that.

[Slide.]

MR. ANDERSON: The total codes are controlled by a unit called CANCON, which goes back and forth here until you finally come up with a CCDF. If anybody is interested, I can get you the documents on that at some later date.

[Slide.]

MR. ANDERSON: For 191, we have iterative preliminary performance assessment calculations starting in 1989. We just finished one in 1992. If anyone needs those publications, just give me a holler. I will send them to you. The next one is planned for 1994. Those have been used to provide interim guidance and to allow for early peer review of the documents.

I might add that we just got back last week -- I think last Thursday or Friday -- EPA's preliminary review of the 1992 calculations. I find them very, very useful and very, very good. If there is anybody from EPA here, I really thank you for that because it does give us an early look into the things that you really think we are doing right and the things that we also doing wrong.

The methodology, of course, is the Monte Carlo technique. We use, in effect, multiple deterministic simulations to end up with the final analyses.

This 1994 calculation hopefully will feed into the first draft compliance that will be sent to EPA. Remember, the 1992, there is really not a compliance application, but an annual snapshot of what we have learned between the two years. Okay.

[Slide.]

MR. ANDERSON: The present status of the WIPP performance assessment. Again, remember that we are focused on the 1985 standard rather than on the 1993 because it came out long after our documents were published.

[Slide.]

MR. ANDERSON: I am going to spend just a second, again pointing out the sources of uncertainty in performance assessment. This is no surprise, I think, to anyone. Model parameters and data are one area. The models themselves are another and the future states or the conceptual models are the third.

What we have found over the last while is we have a reasonably good handle on how to handle that. We have a very difficult time at this point in time of figuring out how to handle conceptual model uncertainty.

[Slide.]

MR. ANDERSON: The summary of the CCDF for the WIPP program looks like this. This CCDF, the conceptual model, system we think is the right one. But you will notice that we have different conceptual models. This one is dual porosity with the benefit the markers and barriers. These progressively have less benefit for the geologic formation until this dotted line out here, is that which was released from the human intrusion hole.

I need to point out to you that these curves are all human intrusion inducted because the undisturbed cases down here have a very, very low probability of any release at all. So we really addressing human intrusion.

[Slide.]

MR. ANDERSON: We have taken from this set of data sensitivity analysis for the parameters. Here is a list of those parameters where we list them critically important, very important, and so on. Two important things you need to see from this. Number one, two of the top listed parameters are ones that you cannot measure. The more of the ones that boil to the top that you cannot measure, the more complete your analysis is -- drilling intensity, borehole fill.

You will notice also the top nine are very similar, if not almost exact, to the top nine in the sensitivity ranking for 1991. This is the ranking for 1992. You can also see that in some cases it is not applicable, human intrusion for 268, whereas it is very important for 191.

[Slide.]

MR. ANDERSON: I am going to jump a couple of viewgraphs for timesake, and to point out that we do have a bunch of worry about conceptual models. Here is a list of those that we are going to try to include in the next set of calculations. But we still don't have a methodology to combine conceptual model uncertainty.

[Slide.]

MR. ANDERSON: For RCRA, we, in effect, have yet to do a calculation where we are measuring the concentration at a boundary. We have been using as a conservative measure to this point in time the gas front, and in our preliminary 1994, the gas front, 46 of the 50 realizations, the gas front did not reach the boundary in the others, before they did.

[Slide.]

MR. ANDERSON: Finally, I am going to jump over the next two because it shows how the performance assessment has matured over time. You can read those and talk with me later if you wish.

Go directly to Rip's lessons learned. First of all, iterative PA analysis is needed to guide the R&D. No surprise there.

In the case of WIPP, human intrusion is the dominant release pathway of the undisturbed, if you will, calculations. The probability of a release is very, very low.

We have a problem here in that we have sufficient calculational complexity at this point in time that Scott indicated earlier that there were lots of models available and the computational capability was getting such that we could calculate more and more and more detail into the analyses.

What we are finding within the WIPP is that we are now so damn complexed -- excuse the French -- that we need to go back and do something a little bit more simplistic so that we can present it to the audience in a clear enough fashion so that you can understand it. So, it is a very interesting trend that I am afraid soon you will also run against.

We have data spacial variability problems that are still with us that I am not sure we will ever handle, but within the uncertainty distributions for any data, we may be able to treat those appropriately.

Conceptual model uncertainty, as I indicated, is the most worrisome of the problems we are facing. I noticed today that in talking with Les and others, conceptual model uncertainty is really yet way in Yucca Mountain's future on how to meld those together. Since we are looking at a 1995 first draft compliance, it is not so far in our future. So, we are going to have to face it.

We are needing a definition, of course, of future human intrusion -- in other words -- what does the future human look like 10,000 years in the future? I am hoping that in 40 CFR 194 of the EPA guidance we'll get some guidance on what we should do here.

We also need a figure of merit for RCRA. This probably doesn't apply very much to Yucca Mountain. From my interactions with the international community, we are at appropriately the same level of developing in our PA as far as some of the important things like conceptual model uncertainty as the international program is.

Warner, thank you very much. I am open for questions.

DR. NORTH: Thank you very much. Thank you for speeding up and giving us five minutes for questions.

Pat?

DR. DOMENICO: Domenico, Board.

Rip, I know with EPA you have to sort of file that no migration petition, which is designed for injection wells and bores media. They have a regulation that says the fluid pressure can't exceed 80 percent of the overburden. Does that same regulation apply to any gas pressure that might be developed in WIPP? How does that restriction affect you?

MR. ANDERSON: No, at this point in time we are still negotiating, but I don't think that regulation does affect us. What the problem is at this point is that EPA has three components of the RCRA regulation -- air contamination, soil contamination, and water contamination. We don't know which one of those should be used at the boundary of the site.

DR. DOMENICO: But it is true that you have to treat that almost like an injection well with what they call the no migration petition for EPA?

MR. ANDERSON: We have to look at the migration of radionuclides out to the boundary, yes, VOCs and heavy metals out to the boundary. Of course, the heavy metals are going to be transported by the brine only. We know from our calculations with the radionuclides, that the brine front does never reach the boundary in 10,000 years.

So, in effect, anything that is transported by brine, assuming the conceptual models that we have to date, of course, then it is a "no, never mind." But the VOCs could be transported by the gas front. They, in some of the cases, reached the boundary.

DR. DOMENICO: Is that right? They have reached the boundary?

MR. ANDERSON: Well, 4 out of 50 of the realizations, as I indicated earlier, showed that the gas front reaching the boundary. Now, what we haven't been told, what I haven't been told, is which of the components of the regulation should I look at? Is it the gas regulation? Is it the soil regulation? Is it the water regulation at that level?

None of them are really applicable gas regulations for breathing. There is nobody down there at 2,250 feet. Soil regulation, we could do that volumetrically, but does that make any sense? Of course, if we look at water, there is some brine down there, but no one is going to drink it. There is not much of that.

So, we need some guidance there. I suspect we will get some in the next few months.

DR. DOMENICO: My last question is: Is gas generation, in terms of pressure -- never mind transport of nuclides -- is any gas generation detrimental to WIPP in any way such that you might approach overburdened pressures?

MR. ANDERSON: I need to answer that in two components. Component number one is for radionuclide transport, it is the brine in which the radionuclides and heavy metals are transported. If you have gas in the repository, it, in effect, keeps the brine out. So, from a 191 point of view, and from a heavy metal -- a 268 point of view, it is a benefit. From the transport of VOCs, it may not be of benefit.

DR. DOMENICO: It is detrimental, yes.

MR. ANDERSON: Yet, some of the realizations that we have looked at using the preliminary very crude gas model that we have been given from our scientists indicate that you reach near lithostatic pressures within the room.

DR. DOMENICO: Well, am I dealing with that same regulation in disposal in salts, that people in EPA tell me that that is a violation. We have to keep the gas pressures below 80 percent of the lithostatic. But you may have had different luck with them.

Thank you.

MR. ANDERSON: Are the gases dangerous?

DR. DOMENICO: No, it is a question of stability at the top of the dome. It is a blow-out problem. It is not a transport problem.

MR. ANDERSON: I understand. Well, the migration direction, of course, in WIPP is not up but laterally in the marker beds.

DR. DOMENICO: Okay.

MR. ANDERSON: So you are going along out past that vertical boundary out into the surrounding geology. It seems like the marker beds are pretty good seals for any vertical movement, assuming that the shaft works. Joe Tillerson thinks he can make shafts that will hold if he is given enough time for them to solidify or consolidate before the gas pressure builds up.

DR. DOMENICO: Thank you.

DR. NORTH: I think we are going to push on to Dr. Garrick. I would like to come back and make some comments on the conceptual model issue as part of the general discussion.

Dr. Garrick is going to give us his perspective from a long career in risk analysis. That includes being in on the opening efforts in and probabilistic risk analysis as applied to nuclear reactors.

He has been in on a number of National Academy committees involved in the performance assessment area and has been the president of a consulting firm, very active in this area for a number of years.

So, John, we are glad to have you here. Welcome. Give us your story.

THE PERSPECTIVE OF A RISK ANALYST

[Slide.]

MR. GARRICK: Thanks, Warner.

I want to first point out an advantage I have over all my other colleagues here that have been making presentations today and that is the full knowledge that at least one copy of my presentation has been used. That is, our esteemed chairman used my copy to make his warning signs to keep us all on the ball.

[Laughter.]

MR. GARRICK: That will probably be what I will remember this meeting most about.

DR. NORTH: John, that was for your benefit.

[Laughter.]

MR. GARRICK: Some years ago when I first got involved with WIPP and attended the first few meetings of the National Academy of Sciences, I was struck extensively by the magnitude of the performance assessment problem, or I should say the magnitude of the repository design problem.

Being new, I could ask all the dumb questions and being also somewhat uninhibited, I did that. The questions I started asking quite deliberately were the questions of: How do you measure where you are? How do you know what you are doing in the experimental program or in the analysis activity has anything to do with achieving closure of this project? Whereby here I mean realizing a repository.

I was quickly pointed to the performance assessment which elevated my confidence a good deal until I sort of looked around and discovered that, well, not too many people paid too much attention to the performance assessment work.

So, I am pleased to hear today that there has been much progress in that regard. I can certainly say from the point of view of WIPP there has been progress.

[Slide.]

MR. GARRICK: Now, the only way I knew how to address this since I am trying to represent the risk perspective here was to sort of deal with the question was: If a risk analyst were going to do performance assessment, how would they approach it?

So that is kind of the tack that I would like to take. I will apologize for moving from series of presentations that had a great deal of substance and a great deal of results and calculations, moving from that level to a very conceptual level. But please bear with me because I think there are a few points that might even be worth it.

So, a risk assessor sort of first likes to understand what question it is that we are trying to answer. Of course, there is a global question here much beyond this having to do with the nuclear industry, nuclear power, and the management of the waste.

But without starting at that level and coming more to the point, the kind of questions that performance should answer, in my judgement, are these kinds. I will also say that the words that are used here not necessarily have the same meaning as the words that we have been hearing today because when I say: "What will the performance be if the repository is undisturbed?" I believe that what I mean there is the nominal case in the DOE language.

But we need to know, first of all, if things pretty much are as they are, how does the repository perform? In a moment we will talk about performance indicators.

Then what will the performance be in reality considering the likelihood of events that can disturb it?

[Slide.]

MR. GARRICK: So, we sort of try to anchor ourselves to some fundamental questions and then move to the issue of what a performance assessment should be.

Now, from a risk perspective, what we really want to do is define our performance assessment, recognizing both the undisturbed and the various possible disturbed scenarios.

One of the things that you have heard a lot about today is the notion of scenarios. This is pretty much the accepted way of doing risk assessment, that is to say, the scenario-based approach to risk assessment.

Develop a systematic set of output forms that together express repository performance quantitatively in terms of the uncertainties present.

Now that is an extremely important statement. It captures a lot of what risk assessment is all about and what we are really talking about here is not so much a regulatory requirement or compliance, or what have you.

What we are really talking about, we like to think is something much more basic and that is answering the fundamental question of what is the risk, on the theory that if you do that, in most instances the regulatory requirements are in there somewhere as well. So, that is the level that, of course, some of us would like to see performance assessment shoot for.

Now, rather than suggesting that we start over and do risk assessment on a broader scope basis, the thought here is that we have done a lot. As you know from today's activities, a lot of the kinds of calculations that you do in this more general way of thinking about risk, in fact, have been done.

So the question is: Why don't we organize those in a way that answers as many questions as we possibly can answer, but in the spirit and context of quantitative risk assessment, such as:

What radionuclides dominate the repository risk over the time periods of interest? The preference here is to look at the whole problem. It is interesting because I am on committees that have been pushing DOE to look at beyond 10,000 years. Today, of course, there was the comment that, "I was just getting used to 10,000 years. Why did you go beyond"?

What are the uncertainties in the individual radionuclide calculations? To me, this is the underpin of contemporary and quantitative risk assessment, is this business of quantifying the uncertainties.

When we talk about quantification of uncertainties, we don't mean doing something that can't be done. What we do mean is somehow displaying what is known about these scenarios so that we clearly know what is not known. That is one way of looking at quantification.

Now, what alternatives exist for reducing the dose burden from these radionuclides? Certainly we have heard a lot about that today. We know, for example, how sensitive Carbon-14 is, at least in the short time periods, where short in this case means 10,000 years or less.

How important the waste package design is? We know how critical transport time is with respect to Iodine-129. We know how important release rates from the waste package are with respect to actinides.

So, these are the kinds of questions that we want to get a handle on so we know how to deal with them.

What is the effectiveness ranking of the alternatives? In other words, which ones do we get the most value from where, for the moment, we will describe value as risk reduction.

Then, of course, we have to answer the question of: What are the cost of the most attractive alternatives? So what I am suggesting here is that we create this perhaps more full-scope concept of risk-based performance assessment with all of its holes. We fill in as many of those holes as we possibly can immediately. Then, of course, in the end we proceed with the more expanded version of the performance assessment to capture the answers that we are looking for.

[Slide.]

MR. GARRICK: So how do we do this? I am not going to dwell on this too long, except to conceptually indicate the approach that has worked very well in all manner of applications that we have come up against. Even the business of looking at terrorism and sabotage, this same thought process has worked.

So what we are suggesting is that we adopt the following "set of triplets" definition of repository performance, by which we mean that we are talking about here repository performance being simply the answer to three questions: What can go wrong? How likely is it? What is the consequence?

The way we answer the first question is the way you have been answering it -- in the form of a scenario.

The way we answer the third question is a matter of choice. It is a matter of, first off, the genuine experts -- the health experts, the engineering experts, the social science experts -- agreeing and deciding on what kind of damage index is appropriate to the problem.

What you find out, of course, is that for most problems it is not a single damage index. It is many. I will show you an example of that.

Then the likelihood issue, of course, is a matter of looking at the evidence and trying to figure out what that evidence says about your confidence in the various scenarios.

[Slide.]

MR. GARRICK: So here is just an attempt within the language of this kind of performance assessment how we might define some of the critical terms.

The S-0 scenario, of course, is the reference scenario, the undisturbed case, or the "as planned" scenario, or whatever we wish to call it, or the "nominal" scenario.

The other "Ss" are all of the scenarios that we can think of, and what we are really thinking about here is scenario categories, not explicit micro-level scenarios, but rather scenario categories that represent when we have done all of this, a complete set.

The way in which we do that, of course, is to embody the notion of uncertainty to recognize and convey, communicate, that we don't know for sure everything we need to know about each of these scenarios. But we convey that with the way in which we present it.

Now as far as the indices are concerned, and for a repository, this is just to illustrate what some of them might be -- dose rate, cumulative dose, total dose, total health effects, et cetera.

[Slide.]

MR. GARRICK: So what is a real world result going to look like? Well, we have seen some of them today. Here is an example, just cartoon-wise.

The individual dose rate, but the aspect here, or the feature here, that we want to put the emphasis on, is to treat probability as a parameter here and to indicate in our presentations how much confidence we have in those curves as being the correct curve.

While we didn't do the artwork very well, what I tried to convey here is that, of course, the uncertainty diverges with time. to be able to illustrate that emphatically, explicitly, is very important.

The same kind of curve with respect to cumulative individual dose. Now, the reason you want to do this by addressing the question of uncertainty, is that there are certain discrete points in time that you want to look at -- maybe 100 years, 1,000 years, 10,000 years, 100,000 years, or whatever.

In other words, we could look at cut curves along any of these and then look at those this way where the breadth of these curves indicate our level of confidence, or our level of uncertainty. The broader they are, the more uncertain we are.

But the value of this, of course, is to be able to show in very explicit fashion how much confidence we have in these performance indicators at discrete points in time. If you, for example, can show that for 1,000 years we have very high confidence, and maybe 10,000, pretty good confidence, then you may be doing something that has never been done before with respect to the risk of facilities such as this.

Then, of course, the one that we have been hearing a lot about today that has become affectionately called the risk curve, namely the complimentary cumulative distribution function, can characterize and embody our confidence about various other performance indicators like injuries, fatalities, property damage, different types of cancers, or however we wish decompose it.

So what I am suggesting here is that regardless of the standard we have from a standpoint of what should we know about this repository, these are the kinds of things that we can go forward with immediately. Not only that, if we do this, it will probably more than anything else shape the nature of the standard as we have seen in some of the nuclear power work.

So when I hear people suggest that we can't do performance assessment because there is no standard, I don't have a great deal of patience with that because you won't like the standard anyhow when you get it. You know better than anybody else what constitutes realistic and informative performance indicators of the repository. So, get on with it. We know how to do it.

[Slide.]

MR. GARRICK: In the nuclear business, just to give you some real examples -- and that is the only reason I included these -- here is a case where way before the safety goals, way before the requirement for PRAs, way before all the existing family of regulations exist that are based on PRAs, these analyses were done and were done from the point of view I just conveyed, namely we know what kind of questions need to be answered.

Fortunately we were lucky because all of the subsequent questions that came out as the standards were developed were already embodied in these analyses. That is what I say is the opportunity in the repository area.

[Slide.]

MR. GARRICK: So here we have core damage frequency. Uncertainty here is in the 10 to the 100 range. We have various plant damage states. We have various release states. Then we have various consequences. So these are nothing more than those Xi's that I was talking about earlier.

What are the consequence states? They are acute fatalities, early injuries, thyroid cancers, other cancers, whole body dose, property damage, et cetera. So here is a case where some nine performance indicators were used to convey the risk of a facility.

The reason there are so many curves, more than the usual two or three, was simply a matter of preference on the part of the owner/operator of this facility.

[Slide.]

MR. GARRICK: Now we have some of those kinds of curves and information available, as I stole one from Bob Andrews' package of an earlier presentation. This certainly could be considered a dose rate case as a function of time. The only thing that is missing here, of course, is the uncertainty analysis.

But nevertheless it is information that we can begin to use as a basis for digging in and asking questions about how can we impact Carbon-14 release. Well, we know how to impact Carbon-14 release. We have heard about it all day. The same thing is with respect to Iodine, Titanium, Selenium, Cesium, Neptunian, and so on. Some of them are much more difficult than others.

[Slide.]

MR. GARRICK: So what I wanted to do was just sort of conceptually on this one exhibit -- if I had to give one exhibit, this would be it -- to illustrate how this whole process works.

The fundamental building block of the process is to identify those category of events that otherwise cause an upset, or a change from what one might consider a nominal state, or a normal condition, such as these.

Episodic events directly affecting water access to the waste package, events indirectly affecting water access, such as repository heat-driven condensate, thermal mechanical events, human intrusion events. Of course, you always have to have a catch-all category.

This is very conceptual. This is just to kind of illustrate the thought process of what you go through. But this becomes the absolute key part of the analysis because everything flows from here.

So, we start with some sort of an event. We then play the "what happens if" game. We structure the problem into stages that are manageable. The same kind of analysis was done on the shuttle after the Challenger accident, where the initial stage may be the launch pad, and the first-stage launch, the orbiting maneuvering, the orbit, and preparation for reentery, and then a couple of stages in the reentry.

The same thing works if you are walking through a refinery, but you want to design these stages in such a fashion and the interfaces in such a fashion that the problem within the boundaries is a manageable one.

So out of the first stage, which is the emplacement to degradation of the work package, you get a set of outcomes here. Those outcomes become the initial events, or the initiating events, if you wish, of the second stage, which conceptually here is just characterized as the engineered barrier, and so on.

But now, of course, this process is very much automated in most cases. But the thought process behind the basic driver is not something that you can automate very well.

So, I wanted to end with that except while we were talking today, and through the hand of one of the staff members here, I wrote down what might be considered the key points. These are not in your hand out.

[Slide.]

MR. GARRICK: They are not thought out as carefully as they might be, but just an hour or so ago.

What are the key points? Well, the key points are the connotative performance assessment is a framework for measuring the importance of specific activities associated with the design -- now here is the scope problem -- the design, the construction, the operation, and the closure period of the repository in relation to its risk.

Quantitative performance assessment properly implemented forces employment of the systems approach, forces the systems engineering. Why? Because the scenarios don't care about anything but how they can occur.

So, you have to bring in all features of the facility and all features of the plant, or all features of the system that could in any way impact those scenarios. That really is a driver for making sure that you are dealing with the dependencies, making sure that the subtleties are really being manifested. That has been the biggest and most major contribution that has come out of the use of this stuff.

It is essential to make the interaction between performance assessment and repository program activities visible, logical, and understandable to all, including the public. We really do need to have a means of knowing how everything we are doing fits into the grand scheme of things. There may be another and a better way to do it, but the way that has done this the best that I have had experience with, is what we are talking about here.

It should be used to quantify the risk and benefits of alternative solutions to specific repository-related problems and issues, including the experimental program.

It was the performance assessment thought process that weighed heavily in the National Academy questioning and challenging DOE as to the relevance of the experimental program connected with WIPP. It was the same way of thinking, including the experimental program, the waste package design, and the engineered barriers.

[Slide.]

MR. GARRICK: My final comment here is the thing that first caused the questions to be asked was performance assessment, if done properly and diligently should provide a measure of project status, a measure of project completeness.

So I appreciate that this is less than what some of us engineer types like to present in terms of hard results. We have got lots of those to present, but this is not the time to do that.

DR. NORTH: Thank you very much, John.

Questions from the Board?

[No response.]

DR. NORTH: Any questions from staff?

Leon?

DR. REITER: John, you have had long experience looking at the nuclear power plant, doing the nuclear power plant's risk. Are there any two or three key lessons that you would have us give to DOE or you might want to give to DOE from what you have heard today with respect to the performance assessment?

MR. GARRICK: Yes, one of the most important lessons that I think we have learned from this is that risk and safety is very facility specific. As you know, the NRC was pretty hell-bent on regulating nuclear power in somewhat of a generic fashion, whereas the inspection activities were very plant-specific, the analysis and evaluation activities and the safety issues were quite generic.

That has all changed. A major component in that change has been the implementation of the individual plant examination program. So I think that the fact that risk and safety is very facility specific, very crew specific, and very procedure specific, is an extremely important lesson.

The other lesson that is awfully important is that the risk in most things that are highly reliable with diversity and redundancy is generally not coming from the first-line systems. It is generally coming from the support systems, the systems that the first-line systems are dependent upon.

This was a very major breakthrough, in my opinion, as to what we have learned from the risk assessment, not that we didn't realize that dependencies were important, but we didn't give enough emphasis and attention to them.

I am talking about designing a highly reliable HVAC system for our room that houses safeguards systems, that those safeguards become dependent upon a chiller, an HVAC system, or what have you, or you lose the mainline system. I am talking about the kind of failures you get when you have the failure of a diesel generator in one train in combination with the service water that is the heat sink of the diesel generator in another safety train.

Those kinds of combinations were the ones that were not getting the level of visibility that they needed before these exercises were done.

DR. NORTH: Don?

DR. LANGMUIR: John, my sense is that what you have been looking at historically in your experience is more fully engineerable systems. I am wondering what your thoughts are on the fact that we have a geologic environment that cannot be well-known or controlled as a major piece of this whole program.

MR. GARRICK: Yes, that is an excellent question. It deserves an answer. You are right. Most of the experience and most of the development of this methodology involves systems for which you can collect actuarial data.

On the other hand, the breakthrough in the whole PRA thought process was to be able to decompose that system in such a manner that you get insights on extremely rare events. It was the rare events we were worried about.

So, the discipline that was developed to take a rare event such as a core melt or a major release, and be able to back that down, and decompose that problem down to levels about which we had information, was in many respects the major achievement of the whole idea.

I think that the same principle is here. I think that we have to take the problems in increments and decompose them in such a way that we indeed can see the information that does exist with respect to the unsaturated zone and how transport might occur through that zone.

I am finding that in most cases it is not a heck of a lot different. It is a different kind of problem. It is a passive problem. You didn't hear me once talk about a fault there in this because fault trees are normally so strongly identified with active systems rather than slow changing systems like geological systems.

The other thing that gives me confidence that you can do this is that we looked at a number of problems having to do with the occurrences of diseases and the impact on animal disease of importing, for example, other animals.

That certainly is not an engineered system, but it is the same kind of way of thinking that you have to go through to get it.

It is not easy. But I think it is important. I think the discipline is extremely valuable of getting the analysts to tell us what those curves are. That is also where the excitement of the whole business resides.

I am trying not to be a zealot with respect to the use of the method, but I have not seen any problem where this way of thinking can't be intelligently and effectively employed. It can also be poorly employed and unintelligently used.

DR. LANGMUIR: My sense is perhaps also that there is far more creative research that has been inherent in this program, and necessary in this program to create the information base. I would guess that that is not common in your experiences, either. Have you any insights first-hand?

MR. GARRICK: Well, I wouldn't say that because where the emphasis and the research now and the problems where there is a large experience base, they are in areas quite different from what they were 10 years ago. They are in the human response modeling side. They are in the dynamic modeling side. How do you know, for example, the status of a facility under different configurations?

You know, the whole business of risk assessment grew up by addressing the at-power state. Now we have discovered that there is a substantial risk, in some cases, at the zero power state, and states in-between.

So, I think that the input that has come from this as to what research we should be doing has been quite dramatic. But it isn't without its problems.

GENERAL DISCUSSION

DR. NORTH: I think at this point we will open it up to the general discussion. I will do that by first letting the Board express itself, staff, and any previous speakers who would like to come back in with a comment or a question for other speakers.

I will start off with a couple of comments myself that others might respond to. My point of departure was a comment of John Garrick's that the computation as illustrated in the last slide of his hand-out is automated. The thought process is not and should not be.

I would like to apply that to the problem of multiple conceptual models mentioned by Rip Anderson in his remarks. This is, I would say, a pervasive problem wherever risk analysis methods are being used, what do you do about conceptual models and the fact that we don't know which model is right and there may be a large multiplicity of potential models that could be used.

In other words, it is not a clear choice between model 1/model 2. The problem is we don't understand the process well enough to be able to model it with confidence.

Now, part of our problem is to define our objectives. What is it that we are trying to do in the modeling? Are we trying to get a reasonable bound, or are we trying to get a exact prediction?

What we are doing in regulatory compliance, it strikes me what we are usually trying to do is to understand adverse events such that we can come up with a reasonable bound or a level that is judged to be acceptable.

Now, what do you do in this situation? I think it is well illustrated, actually, in some of the last slides that Robin McGuire used where we had a case for a flood repository and a judgement that the probability of that case was one in a thousand.

This is one that most of us know pretty well, having followed the Szmansky debates and the findings of a distinguished group of scientists within the National Academy structure that Szmansky's Hypothesis did not look credible.

Now, someone might judge -- and apparently they did -- that that might be summarized by a probability of one in a thousand. Now, one can put such a probability and such a conceptual model with its predictions into an analysis and folded into Monte Carlo or decision-tree logic.

I would argue that that is exactly the wrong thing to do. We are then automating the process and all the insights that went into that aspect of it get hidden in the numerical computations.

The right way to do is to haul it out for detailed scrutiny. Think about it. Do not automate it. That suggests that what we might do as a procedure is to avoid folding model uncertainties into analysis directed at regulatory compliance.

A big caution flag. If you are going to put model uncertainties into the analysis, make sure that everybody in the receiving community -- from the regulators to the affected public -- understands that you are doing that.

On the other hand, when you are working on questions of research priorities, you do want to assign probabilities, even very small ones, to those cases as a way of guiding your research to determine what is important.

So, yes, put it in the analysis, especially for value of information calculations, but don't make it automatic within the calculation of numbers and the performance measures for regulatory compliance.

Now that is a set of personal views on this subject. For a detailed discussion from the National Academy of exactly this problem in the context of diseases

-- in this case cancer as it relates to air pollutants -- I recommend for everybody a National Academy study called "Science, Judgement, and Risk Assessment" that is going to be unveiled a week from today.

I have just given you a summary of material that is discussed in two appendices and several long chapters on how to deal with uncertainty. I would welcome comments from Dr. Garrick, from Rip Anderson, or from any of the panel who would like to discuss this issue further.

[No response.]

DR. NORTH: Okay. Consider that a commercial for next week's National Academy study.

[Laughter.]

DR. NORTH: Do we have any other general comments or questions from the Board, from presenters?

Let's see. The first hand I see is Robin McGuire.

MR. McGUIRE: Robin Mcguire. Let me respond to some of those comments, Dr. North, because they involve some of the slides that I presented.

I think I agree with you that those analyses should not be folded in and only represented with a mean curve. To clarify the record, our analysis included that that flooding condition was a combination of two things. One, the possibility that climate would change such that the general groundwater table would increase by, I think, 130 meters, plus there would be an additional change of 100 meters caused by tectonic conditions. We left that in to include all other possible conditions.

So, it was not just the Szmansky Hypothesis shoving the water table up 230 meters. I am aware of that National Academy panel that judged the Szmansky Hypothesis because I was on that panel.

But I think that I agree with you that the right way to conduct these kinds of performance assessments is not to just fold those hypotheses in and present the final result, but use them as a tool to understand what is driving the result and go after those critical hypotheses that do drive at the upper curve. I think that is what you are saying.

So the performance assessment is not just that final CCDF compared to a criterion, like 40 CFR 191. It is the understanding with all the sensitivities thrown in, and the final judgement, perhaps, that we can't reduce those uncertainties any further. Therefore, a repository is or is not safe, judged against that standard. But it is not just a single curve.

DR. NORTH: I don't disagree. I am sorry if I simplified the scenario and made it seem narrower than what you had intended. But I wanted to pick that up as an example and put a well-known label on it, hopefully to encourage other people to understand what I am talking about.

I would commend your practice because as you presented it, you did so in such a way that I think it highlighted what you had done and the importance of what you had done rather than simply showing us a cumulative distribution and asserting that that effect was included, too.

Of course, the issue cuts the other way. It might be that the scenario that we want to take out and look at in more detail is one where we have the potential of relaxing a conservative assumption.

For example, we have not included the cladding as a barrier. Supposing we do an analysis that looks at the cladding as a barrier. How might that change the results? How should that analysis guide our thinking as to whether to invest in doing the cladding as barrier investigation?

It might turn out that we judged there is a 50 percent probability that we will be able to reduce the predicted releases in the 10,000 year period by some factor which is big enough to be attractive, or we find it is a very small effect and we decide maybe that research should not have a priority.

The point is this is a system for guiding the evolution of the risk assessments. I think Sandia's experience has been that it works quite well. You have learned a great deal from it. I think I can react as a member of this Board that seeing TSPA '93 for the first time today. It is clear that the iterative process is useful to DOE and that the participants and the management are learning a great deal from the iteration as well.

That makes me feel good because in the first Board report we said you should do this. We think it is going to be very valuable for the DOE program. I would say now there is a lot of evidence out there that, in fact, we were right.

MR. GARRICK: There is one comment, Warner, I would like to make. I have been threatening to make it all day.

DR. NORTH: Good.

MR. GARRICK: Part of it was covered in one of the DOE presentations. It does have to do with this business of model complexity and how you deal with it. I have always been a great believer in this old concept of the method of successive approximation. You start out with very simple models, very simple conditions, and you see clearly the growth of the complexity of that model. You leave a trail. Sometimes I don't think we do enough of that.

The other thing that I think is very important on scoping this is to look very hard and turn up the microscope extensively on the end points. Now, I was pleased to hear today the discussions about the waste package and the effect of changing the waste package design on long-term releases and short-term releases.

So, clearly there has been considerable progress in that arena. The arena I have not heard much about that I think also would be extremely illuminating would be on the entire other end of the spectrum, and that is on the health effects side.

What level of health effects constitutes a problem? Now, I am reminded of a little that I just got here, out of a newsletter, that is a brief history and critique of the low-dose effects paradigm by Sagan, the medical scientist at Florida Electric Power Research Institute.

He says in here in this that, "Tens of billions of dollars have been spent in the clean-up of chemical waste sites without any persuasive evidence that human health has benefitted."

I would really like to know if we build this repository with what we now know about what the risks are, whether or not there would be any public health benefit. I don't think we have -- at least I have not seen that issue driven home. I don't think we are going to get there until we do because I think it is to the core of the whole no-threshold hypothesis, the linear dose hypothesis, the whole thing, that is keeping us from solving the problem.

I don't know whether the Board is involved in that end of it or what have you. But I do think that turning up the microscope on those two ends of the problem would provide considerable illumination on how to maybe better focus or more narrowly focus the modeling process.

DR. NORTH: John, you have raised a wonderful issue that is like a very slow pitch right through the center of the strike zone, and I am going to swing at it.

[Laughter.]

DR. NORTH: The problem is for the Board as a whole, we really aren't encouraged by our statutory charter to go after the issue of the regulatory criteria. That really is a job that Congress has in the short-term given to the National Academy. A little earlier they gave it variously to EPA, the NRC, and maybe to DOE in terms of the siting guidelines.

So I think our job as a Board is to try to understand these issues and guide DOE in its activities rather than to express ourselves to the Congress as to how to set those regulatory standards.

Nonetheless, for me personally, I think those issues are very interesting. The National Academy report deals directly with model uncertainty at the level of low-dose linearity versus threshold for carcinogens. Radiation is a very important element, although it is not regulated under the statute that the National Academy was asked to address.

Nonetheless, I hope this report will provoke more debate on this subject. Leonard Sagan is a close friend. At one point he used to be my personal physician. I am discussing with him and with Donald Kennedy who was, until recently, the President of Stanford and has now gone back to running the Stanford Center for Risk Analysis, a course at Stanford to be given in the winter quarter of 1995, a year from now, focusing on both the technical and policy aspects of low risks.

So, I look forward to investigating those issues, and I think others may join in the campaign to publicize their importance for the setting of regulatory standards. Nonetheless, I think as a member of this Board, I ought to say that it is at least at the margin of our scope.

MR. GARRICK: Yes, my comment, though, was not from the point of view of standards, but from the point of view of developing insights on modeling of the specific scenarios. But I understand what you are saying.

DR. NORTH: Would anyone else like to weigh in on this discussion or to raise another topic?

MR. POLONSKY: Alex Polonsky again, without a suit.

Someone earlier this morning, I guess, brought up the point of the study between the outer thickness of the various MPC that is proposed to be used. It seems that people are saying the longer we go on -- 100,000 years -- that this scenario may be brought into actually licensing, if NAS brings that about in a year, that we may go from 10,000 to 100,000 or a million years, that people are saying, "Well, we might go to the cheaper option which might be 10 centimeters for the external shell, because after 100,000 years, it just won't matter."

But I think we are overlooking something there that if you make something 45 centimeters, (a) I was told that it is self-shielding in itself, and (b) that even though it might cost more now -- and we will laugh at this 20,000 years from now how little it cost -- well, we won't.

[Laughter.]

MR. POLONSKY: But in 20,000 years that canister will still have some integrity. 20,000 years from now, if we used the 10 centimeter, it won't. But if we decide when DOE, for example -- and that is the reason why I am mentioning this -- starts to hand out an RFP for design proposal, I don't think they should make an option for a 10-centimeter or a 20-centimeter canister. It should just be, "Let's make it the thickest canister you can without creating an engineering nuisance from moving it underground."

If we can move something 150 tons underground, then let's make the canister 150 tons so that as long as we can move it, the thickest possible, the longest it will last. That is what we should be looking at, not whether or not a million years from now there is going to be Neptunian, or whatever it is, releasing into the atmosphere, which seems inevitable.

Thank you.

[Laughter.]

DR. NORTH: Thank you for your comments. I am encouraged that the analysis of the cost of these strategies versus what they imply for performance assessment is under way. Hopefully, we will all learn from it, including DOE as it decides how to design that RFP.

Any other questions or comments, now open to anyone from the audience as well?

Rip?

MR. ANDERSON: Only one comment on your first discussion. If we indeed -- and I agree with the position of not folding in other conceptual models, but having them stand out and be very clear -- what this does is put you in another arena of debate that has occurred around the system for many, many years, and that is how much benefit do you use in expert panels?

The end analysis on multiple conceptual models coming down to one conceptual model is going to come through an expert panel, I am afraid. We still have that terrible debate of whether we can use them, whether we can't use them; whether they are good, whether they are bad -- how much weight to give them.

DR. NORTH: It seems to me the issue is expert judgement compared to what? If we are making decisions as opposed to doing science, where we have to decide, are we going to spend the extra bucks to make it the order of 40 centimeters thick or is 10 centimeters enough?

If expert judgement is all we have to go on, let's do it but let's do it as well as we can and make it clear how good we are in that area. Let's not pretend those numbers are precise. Let's make it clear if experts disagree, or if they feel they are speculating, that that is how they feel.

But time and again when we consider individual decisions which might have to do with our health status as individuals, or litigation in this litigious highly regulated society, then the questions are, "Doctor, what are the chances this is going to cure me?" versus "I am going to have severe side effects," or "What are the chances if we go to Court I am going to win"?

So, these are the kinds of things that we often need judgement from experts in order to make our own decision. I would assert that is something that is sort of existentially given. We have to do the best we can. Why should it be different in a situation of a social decision involving consequences far in the future?

Let's do the best job we can to do that decision, but do it in a way that is open so that the interested members of the public can understand: How is science being used in this process?

DR. VAN LUIK: Abe Van Luik, INTERA.

The thing that I wanted to say is that we should not confuse the results of these analyses as being strong indicators of any particular thing. The reason that we went to the three overpacks is to see if overpacks made a difference. We may even go to a higher overpack because the system studies that are going to look at the operational phase are not yet done.

Another question that came up earlier was the linking of the near-field and the far-field. I think Dr. Blanchard was a little too modest because he did not mention that we have two very comprehensive and capable studies fully planned out for the ESF when that gets done.

Personally, since I am not yet convinced that water can get to the waste form as easily as we model it, I think that there is going to be a different outcome of the model after we have those results in hand.

DR. NORTH: In other words, the process continues. We are going to continue to learn more and more which is how it should work. We are not doing a final risk assessment for Yucca Mountain yet, even at the level of some of the, shall we say, more detailed program decisions. We are going to continue. There is work in progress. As it comes to the point where we all see it, hopefully our knowledge is going to improve and the decisions will improve accordingly.

Does anybody else have a question or a comment?

[No response.]

DR. NORTH: At this point, then, Jerry, I will invite you to make your closing comments, and then I will make a few of my own.

CLOSING REMARKS FROM THE DOE

DR. BOAK: There are a lot of interesting things that I would like to respond to that have gone on during the course of today.

I think I want to start by mentioning a comment made to me by Ed Kwicklis at the end of an ACNW meeting. He was astonished. He said that how pessimistic some people were about our ability to resolve some of the uncertainties we have about the site.

He felt that we were just poised on the verge of really making some major improvements in our understanding, that we have learned a great deal, and I guess before Pat Domenico gets to say again that we have made no advances, no changes, in the unsaturated zone modeling, that he needs to be locked in a room for about an hour and a half with not only Ed, who is relatively meek and mild, but Alan Flint and Bo Bodvarson.

If he can emerge unscathed from that, then maybe he can make that claim again. If you restrict yourself only to the codes, and particularly to those codes which give us a performance measurement, the statement is, in fact, valid.

I have been racking my brains for a better metaphor, but the metaphor I have in mind of the embarrassment of riches we in the NPA have and the things that we would like to implement, the things we would like to get incorporated into our modeling, the only one that comes to mind is of a python having just strangled a mule, and is busy trying to spread its jaws wide enough to get it down there.

The slow migration of that thing down through there produces a bulge, which, of course, makes that python really uneasy for quite a period of time and also not willing to take on more challenges.

[Laughter.]

DR. BOAK: That is really an unsavory image, but I sometimes feel that that is the sort of way people see PA.

So I wanted to say that I feel that there have been performance assessments in the past. They are reflected, to a large extent, of a relatively primitive knowledge of the site that we are looking at now, and that the scientists that we have talked to have a whole lot of things they want to get into our models.

So I think it is a really exciting time for us. We are trying to figure out ways that we can pull in some of the very specific knowledge that Alan Flint has about how infiltration works its way into percolation, trying to incorporate ideas coming out of Bo Bodvarson and his three-dimension model in a way that we can simply calculate it many times to produce the CCDF.

I would say that underneath the CCDFs, which in many ways look very similar between TSPA in 1991 and TSPA in 1993. There is a great deal going on, hidden underneath, some really frantic paddling, to intrude with another animal metaphor, maybe -- that if you strip away the one thing over which we really haven't seen much that changes our view of things, the gaseous release of Carbon-14, if you strip that away and start looking at some of the details -- and they are not contained in an hour and a half briefing to you -- they are in a 500 and a 1,000 page document which we are hoping to get cranked out in time for you to read it.

There is a great deal of that underlying discussion, but as with all large documents, there is the question of transparency. There are only so many of those interesting details like the 230 meter water table rise that you can bring forward in a way that is easily presentable to a wide range of people because they won't sit down and listen to you for long enough to get all of those interesting side-lights presented. So the transparency issue remains one of the biggest and toughest ones we have.

I do want to say it is clear from our work that this question of fracture matrix coupling and the related issue of what is the actual flux through the repository horizon, remains the most important conceptual issue that we have to resolve.

That point has been made by virtually everyone in the hydrologic realm -- the State of Nevada, the Board, all of our participants. We fully agree. It is the major issue we have to address, I think, in order to demonstrate that the performance is adequate for this site.

Some of the ways that that point has been made, we have had strong disagreement with, but the fundamental point that what the flux through the repository really is, and how that affects performance, are really important.

I did want to come back to a point that Max had made and show the viewgraphs that are in your package. I have modified them slightly, giving you longer titles for the activities, but not categorizing them by priority from the 123 site budget.

[Slide.]

DR. BOAK: What I have done here in this column over here, is listing of the activities under the work break-down structure, their SCP number, the study and how much funding that is going into them for Fiscal Year 1994. I have some copies of this hand-out showing with the shaded area here which activities we expect to see data coming out that we hope to use in our next iteration of total system performance assessment.

It is to some extent subjective. It was done hastily to try to have this here to present. I have it for all of the activities that are funded. I have four pages of those, but I won't show them.

[Slide.]

DR. BOAK: I will just say that of the total $59 million budget that is going into site, approximately $26 million of it is shaded like this. There is another fairly hefty chunk of it that I have to say is activities like drilling which are funded separately and which won't provide direct feeds to us. But without that drilling going ahead, we wouldn't get the information out of these SCP studies that we will expect to use.

In addition, there are quite a number of studies in that remaining $38 million or so that clearly are feeding design, especially for the ESF, and hence, constitute longer term activities that ultimately PA must be involved with, which aren't expected to produce something that we will use in our next iteration.

There are quite a number of studies like that, that are funded, some of them at relatively low levels, things such as the Natural Resource Assessment Study shown here, which is simply is at too early a stage to provide us some benefit for our next iteration.

[Slide.]

DR. BOAK: But I think it is a reasonable indication that whether it is because we have been telling them or not, or whether they just thought about it themselves and realized that these were the important issues, I would like to think it is because PA has been telling them they are the important issues, that the site program is, in fact, addressing issues that we think are going to be important that we hope to be able to incorporate in our next iteration.

We have hawks on either side of many issues. We have Bo Bodvarson and Alan Flint arguing on one side of the great thermal divide, and Tom Buscheck, Bill Halsey, and many others arguing on the other side of it. They tend to drive performance assessment to certain kinds of conservatisms. Those conservatisms are often what drive the higher releases that you see.

Which gets me to my last point about conservatisms heaped on top of one another. I think as Abe says, there is a reasonable likelihood that performance of the site will be better than anything we have actually put into our models that we have constantly tried to incorporate extreme values, constantly used performance assessment, as a way to search for failure mechanisms.

In some respects, I think that it is possible that our view of performance might be substantially more optimistic if we simply said, "Okay. What do we really think is going to happen out at the site"?

I think that is major thing we have to do with respect to these long-term doses, is look back and see in our dose models, in the release models, in every aspect of it, have we heaped conservatism on conservatism on conservatism on conservatism, and lead to extremely high doses, or is that genuinely likely the performance of this site?

Thank you.

DR. NORTH: That is a good question on which to end because I think the issue of how the numerology gets communicated to regulators and to the public becomes quite critical.

If we, in fact, are building lots of conservatism into the models and the probabilities, they need to be labeled accordingly. I am not sure the program has done all it might do to be careful about that communication.

I think that has been a very serious problem for EPA in its cancer-risk assessment, that what they calculate, if you read the fine print, is a plausible upper bound. They do it by a standard process. So once in a while you can identify cases where it is a clear underestimate, and there are reasons for that.

Yet when you see cancer risks in the newspaper, they look like numbers that are as precise as estimates of how many people are going to get killed in the next holiday weekend. In fact, those numbers really can't be compared on that basis.

You are comparing a very conservative estimate based on great uncertainty in some case with another number which is predictable statistically to a rather high level of accuracy, unfortunately, for us who are on the highways.

SUMMARY AND CLOSING REMARKS

DR. NORTH: Let me make a couple of comments in the time that we have before a number of people have to leave for planes.

Jerry, if you have the second of those charts that you showed handy, I would like to get that back up because I am not sure I was quick enough to get exactly what some of those categories are.

[Slide.]

DR. NORTH: In the unshaded region, that amounts to about half of the money, but one that struck me was run off in stream flow with -- let's see, that is thousands, so that is 400,000 -- approaching a half a million.

Then another one that caught my eye was saturated zone, hydrologic system synthesis and modeling. Now, it strikes me that when you are looking at infiltration, understanding and having a data base for run-off in stream flow, could be quite important, especially with the year-to-year fluctuations.

So I would see both of those areas as candidates where you at least need the qualitative understanding of how that issue affects performance assessment, even if you are not getting data on it.

So, it strikes me that the scientists in that program with their activities at the bottom of the pyramid definitely ought to be into the system. Maybe you don't have an influence diagram yet as to how their work communicates up into the top level performance assessment, but at least you ought to have a good conceptual overview and some communication.

Another point I would like to make is reiterate the issue of peer review. If Alan Flint and others of his reputation within the program go out and say these models are terrific and I really believe that performance assessment is accurate in its projections of the probability that the site will be acceptable against the performance measures, I am not sure that is going to cut a whole lot of ice with many people in the public who are disposed to doubt the credibility of anybody who has ever taken money from the Department of Energy.

If you have all those people that would be judged by the community to be Alan Flint's peers, that have looked at his work and looked at the way his work is being used in the performance assessment, and they are willing to say, "This is very good. It certainly meets the highest professional standards," that is far more persuasive than anything that can be done by people within the program unaided.

So, the value of getting the larger community involved and having them look carefully at what you were doing will be very valuable. I think John Garrick can probably tell some stories from the nuclear reactor analysis on the value of getting people from outside to look over the science.

I will tell a story from my own experience. I was involved in the analysis of the Superfund site, with very complex groundwater transport models and probabilistic analysis that I had done under funding from the PRP -- "Potentially Responsible Party" -- who would pay the clean-up bill.

I figured my credibility before a public group of the people who live in the area who were disposed to go in and protest that the clean-up ought to be done very, very carefully at great expense -- my credibility would be rather questionable.

So, we encouraged the local government agency to sign up a number of very well known scientists who happened to live in the community to advise them on the scientific issues so that they, rather than county supervisors and state health officials, would review this complex analysis.

When they gave it a clean bill of health that it was good work to the county government and the state officials, then it was adopted. The result was to take this Superfund site and to turn it into a park that the county administers. We believe that is the only example of its kind.

As you probably know, less than 10 percent of the Superfund sites in the country have been successfully addressed and cleaned up over a 10-year period.

So it strikes me that this whole issue of peer review and credibility ought to be a leading edge of the performance assessment program. I hope you can take the steps to go much further in that area and achieve progress, as well as progress on the technical dimension.

The last point I would like to make is with respect to potential show-stoppers, items which might cause the repository to be unacceptable. The flooding example is one that I used earlier and Robin McGuire has in his presentation.

I would like to commend to you that one of the most valuable things you can do is to try to identify any other such show-stoppers such that they can be subject to a great deal of investigation and analysis, that you have a very good story for why they are impossible or highly improbable. You can support that with a lot of data, and if not data, expert judgement.

It seems to me this is the place you will get into the most trouble as this program proceeds if someone thinks of a good one, and you don't have that base fully covered to the best that you can do with resources that might be available to you.

So I would urge as you look through the many, many details in this program, try to find those important issues and really focus attention on them so that you become convinced that you have done all you can reasonably do on those issues.

Well, I think it has been a very useful two days. I think today has done an excellent job of summarizing the program's progress and the views of a number of outsiders on the program's progress within relatively tight time limits.

John, I am sorry that I sullied the backs of your slides in order to do my time-keeping, but on the other hand, I think the time-keeping has been effective. We are now three minutes before 4:30. Everybody has stayed on schedule. I thank them for that.

I thank the speakers for their thoughtful presentations and the extensive preparation that clearly went into them.

I think we have had a very useful exchange of views. I thank the commentators from the public, even those that did not come dressed in uncomfortable suits for the comments that they have made.

[Laughter.]

DR. NORTH: At this point, I am going to declare that the meeting is closed. We look forward to seeing you in future meetings.

[Whereupon, at 4:27 p.m., the meeting was adjourned.]