UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

***

MEETING OF THE PANEL ON RISK AND PERFORMANCE ASSESSMENT

***

Nuclear Waste Technical Review Board
1100 Wilson Boulevard
Suite 910
Arlington, Virginia 22209

Monday, May 20, 1991

The Panel met, pursuant to notice, at 8:30 o'clock a.m., Dr. D. Warner North, Panel Chairman, presiding.

NUCLEAR WASTE TECHNICAL REVIEW BOARD MEMBERS PRESENT:

Don U. Deere, Board Chairman
D. Warner North, Panel Chairman
Clarence Allen, Member
Patrick A. Domenico, Member

Dennis L. Price, Member

Ellis D. Verink, Member

Leon Reiter, Senior Professional Staff

William D. Barnard, Executive Director

PARTICIPANTS:

Russ Dyer, DOE

Felton Bingham, SNL

Dave Dobson, DOE

Albin Brandstetter, SAIC

Les Jardine, LLNL

Jerry Boak, DOE

Holly Dockery, SNL

William O'Connell, LLNL

Maureen McGraw, PNL

Rally Barnard, SNL

Michael Wilson, SNL

Paul Kaplan, SNL

P R O C E E D I N G S

[8:30 a.m.]

DR. DEERE: Good morning, ladies and gentlemen. Welcome to our new office building, and to our conference room. We're very happy that the meeting of the Panel on Risk and Performance Analysis is going to take place today. I am also very pleased that you all could come from various parts of the country to be here to participate. I am particularly pleased that seven of our Board members also have been able to make this particular meeting.

I would now like to turn the meeting over to Dr. Warner North, who is Chairman of the Panel on Risk and Performance Analysis.

DR. NORTH: Thank you, Don. I would like to start off with a couple of announcements.

First of all, no smoking on the premises, here, in the room, in the halls or anywhere else on the premises. If you need to smoke, please go down to the mall.

Second of all. Telephone calls. The offices up here are in use, so please don't take advantage of an open door and go in there to make your phone call. Go out to the left, down the hall, past the reception, and I'm told there is a phone down there that you can use. Since there are a lot of people here, please try to make the phone calls short. I believe there is a public telephone down on the mall level for people who need to make a call of greater duration or find the line too long up here.

Let me start off by introducing the Board members that are present. Dr. Ellis Verink, who is the Chair of our Engineered Barrier Systems Panel; Dr. Dennis Price, who is the Chair of the Transportation and Systems Panel; Dr. Clarence Allen, who is the Chair of the Structural Geology and Geoengineering Panel; our Chairman, Dr. Donald Deere. And let's see, we have -- and Pat Domenico, who is Co-chair of our Hydrogeology and Geochemistry Panel. Now, let's see, did I miss anybody else?

Well, we're here for a meeting of the Risk and Performance Analysis Panel. This the first meeting of this Panel we have had in two years, since May of '89. However, the Panel has not been at a loss for things to do because of the very strong emphasis on risk and performance issues within the activities of the Board, as a whole, and of many of the other panels that are represented here. In particular, over the last year, we have heard a great deal about the Task Force exercises that DOE has been carrying out; all of which have a very strong emphasis on performance assessment.

Now, I might add, in passing, the name of our panel is risk and performance analysis. I, very deliberately picked a broader term, because what I want our panel to focus on is not just the calculational aspects of performance assessment, as that has been viewed, but also the broader questions of communication and representation to the interested and affected groups in the public, of what is the risk, from the repository operation, and how can we deal with their questions and concerns?

So, the mission of this Panel, is to take a very broad look at risk and performance issues.

Now, in our first report to Congress, we made a number of recommendations, and DOE responded to those recommendations in an appendix, which is in our second report to Congress. Our first recommendation was to develop methodology to demonstrate performance assessments.

DOE's response was that continuing development of the principles, practices and procedures for performance assessment is a primary goal of the performance assessment program.

DOE has described its general approach in the Performance Assessment Strategy Plan and in the SCP. The specific activities for implementing the Performance Assessment Strategy, are described in the Performance Assessment Implementation Plan. DOE has also initiated Preliminary Performance Assessment Calculational Exercises or PACE. These assessments have helped to demonstrate what DOE needs to accomplish, in order to further develop its approach to performance assessment.

So, in today's meeting, we're going to hear a series of presentations from DOE, and we will be told about the Performance Assessment Strategy Plan, you'll be told about the specific activities for implementation, and we'll be told about PACE.

Our second recommendation from the first report was that DOE should carry out preliminary performance assessment calculations to demonstrate that such computations are possible, and to determine if any site characteristic has been detected that would disqualify the site. DOE's response to that recommendation was as follows: DOE has been involved in Performance Assessment Calculational Exercise, PACE, since 1989. The goal of PACE exercises was precisely to evaluate the current state of models, computational capabilities and available of site data. These exercises are expected to be a continuing activity.

Performance assessment teams have also provided significant input to the activities evaluating the ESF alternatives, performing risk-benefit analysis of Calico Hills shaft penetrations and establishing priorities for the surface-based testing program.

So, as I mentioned, we have heard a great deal about three task exercises, ESF, Calico Hills and the testing program over the course of the past year. What we want to focus on today is what has been learned in those various task exercises, and in other exercises carried on, under PACE, and how does all this fit together? What is the overall plan? How can we ensure consistency in performance assessment? And what has been done to update, based on learning from these exercises, the performance assessment strategy plan and the provisions of the site characterization plan?

Now, in the second report to Congress, we also added some new recommendations. One of these concerned natural analogs, and since we focused on that at our April meeting, I won't say anything further on that subject.

I will review the other two. The first was, DOE should continue to use decision-aiding methodology to provide more explicit and formal means for relating program decisions to risk and performance issues. Such methods should be used in an interactive and ongoing fashion, to explain the reasoning behind major programmatic decisions, before these decisions are committed. The four existing DOE task force studies applying these methods should be closely coordinated.

The second recommendation was that DOE should continue to develop methods for assessing expert judgment in areas of significant uncertainty. Furthermore, the DOE should incorporate, into the current task force studies, the views of technical experts outside the DOE and its contractors. The needs for each expert judgment needs to be carefully documented.

So, these are themes that are also of great importance to us, and we will listen, with interest, as DOE describes its plans as to how they are responding to these recommendations, and, again, how the plans are being adapted, through the learning from the activities that have been carried out so that, in the future, we will have an even clearer vision of what the risk and performance issues are and an appropriate set of tools and capabilities and plans for dealing with the risk and performance issues.

Let me conclude my introduction here, and introduce Russ Dyer, who will introduce the DOE presentations.

[Slide.]

DR. DYER: Good morning. Thank you, Dr. North.

I'm Russ Dyer from the Yucca Mountain Site Characterization Project Office in Las Vegas. And what we intend to do you for you today is try to give you a feeling of the broad spectrum of things termed "performance assessment" within the Yucca Mountain project.

[Slide.]

DR. DYER: I tried to come out with an outline, and I think what we're going to try to do is to run you through the basic questions of what the scope of the program is; what the current activities; what the results of some of the activities have been; and how these activities have tied into some of the decision-making functions within the Department.

[Slide.]

DR. DYER: Let me run through the batting order here and give you just a brief introduction to the people you'll be hearing for the rest of the day.

I will follow myself here and give you a brief overview of performance assessment, what performance assessment is and isn't, within our program.

Felton Bingham of Sandia Labs will give a brief history of performance assessment, how performance assessment has evolved over time, in response to evolution of regulations, programmatic evolution.

I'll be back again to talk about the programmatic aspects of performance assessment within the mined geologic disposal system.

Then I think we're shooting for a break about 10:15.

[Slide.]

DR. DYER: David Dobson will talk about examples of performance assessment support to the decision-making process, and some examples from recent history of the four task groups that Dr. North alluded to.

Albin Brandstetter of SAIC will talk about performance assessment integration with site characterization and design efforts, and some of the programmatic controls to assure that this integration occurs.

Les Jardine of Lawrence Livermore will talk about preclosure performance assessment, a topic that doesn't get too much press these days, but is nevertheless a part of the performance assessment program.

Felton Bingham will be back pinchhitting for Scott Sinnock, who unfortunately was impanelled on a trial out in Nevada, and being a good citizen of Nevada, he is sitting on his jury. But Felton will present Scott's talk about model hierarchy, model uncertainties, some of the fundamental principles of model development.

[Slide.]

DR. DYER: In the afternoon, we'll be hearing primarily technical presentations dealing with results from the PACE-90 exercise. Jerry Boak will give an introduction; then Holly Dockery will give overview and definition of the problem that we looked at in PACE; Bill O'Connell will look at the source term development that fell out of PACE; Maureen McGray, Pacific Northwest Labs, will talk about PACE-90 flow and transport results.

Following that, Rally Barnard of Sandia National Labs will look at ongoing and future performance activities; Mike Wilson of Sandia will look at total-systems analysis; and Paul Kaplan will look at using groundwater travel time model to identify information needs from site characterization. It was an adjunct of PACE, but ties directly back into the site characterization program.

And finally, I'll wrap things up, hopefully not too late this afternoon.

Any questions from the Board before I go into the next presentation?

[No response.]

DR. DYER: Before I go on to the next presentation, before it slips my mind, out in the foyer with the presentations, and I think we gave each of the Board members a copy, is a bibliography of performance assessment-related publications over the last six to eight years that you may find of interest in following some aspects of the program.

[Slide.]

DR. DYER: To switch topics -- well, not switch topics, but start up at the top of the performance assessment hierarchy here, and I'm going to start with an overview of performance assessment.

[Slide.]

DR. DYER: And I know we're going to have a roundtable tomorrow afternoon, so I'm going to start out here by bringing up some points that might be topics of discussion that could be discussed in tomorrow's roundtable.

First off, what isn't performance assessment?

There is a feeling, on some people's part, anyway, that performance assessment is an elaborate computer simulation system that, untouched by human hands, makes decisions based on intricate computational models applied to some suite of data.

And I will make the point that that's not performance assessment, not in our program, and the reasons why are detailed below.

Performance assessments don't make decisions. They support decisions.

There are different decisions to be made, and they require different tools.

Computers, although useful, are not mandatory. And that kind of gets us to the next topic:

"Expert opinion" will always form the basis for performance assessments, at some level.

And simple models are often the most appropriate tools to support decisions. They need not be incredibly complex, end-dimensional computer models.

[Slide.]

DR. DYER: With this in mind, the objective of our performance assessment program is to develop and apply a suite of analytical tools to support performance-related decisions throughout the life of the project.

Key words in there, "support decisions."

[Slide.]

DR. DYER: These decisions are going to change over time; and different decisions will require different tools.

[Slide.]

DR. DYER: As an example of one application of this, let me show one of John Bartlett's favorite slides, which is the schematic of the site suitability evaluation process, where we start with an approach here, working toward a decision regarding the suitability of the Yucca Mountain site.

And within this internal box here, we have an iterative interplay between testing and performance assessment. Testing the testing program acquires information. Performance assessment interprets, evaluates, analyzes that information, and passes it off to a body, an organization, or a person that performs a decision regarding early site suitability.

The decision may be to abandon the site; the decision may be -- essentially there are three choices: yes, no, and maybe.

If the decision is no, that is, Yucca Mountain is not a suitable site, then abandonment is the logical choice. If the decision is maybe, as in there's not enough information to either recommend nor deny the site, then we go back and iterate through the testing performance assessment loop.

And finally, at some point, there is, if abandon has not been chosen previously, there is some point where one comes out with a suitability decision leading to a site recommendation and a license application.

[Slide.]

DR. DYER: We have both long-term and near-term decisions facing the program.

What are some of the near-term decisions, near-term applications of performance assessment?

Well, the one I just talked about, to support site suitability evaluations.

To support testing prioritization evaluations, with a limited budget and a zero-sum game, what are the tests that need to be brought on-line first to most effectively test site suitability?

We need to provide support to regulatory interactions. I'm sure you're aware that 40 CFR 191, the third draft is out now. We are expected to make comments on that. We would hope that some progress is made toward finalization of 191 in the not-too-distant future, and we need to provide quantitative support for DOE's input to our comments to the Environmental Protection Agency.

Performance assessment supports strategic decisions.

I'm sure you are well aware of the "hot versus cool fuel" issue. We'll be talking to you in, I believe, the October meeting about that. And performance assessment is actively involved in looking at this issue, looking at the pros and cons of these two end-member states, essentially.

Partitioning transmutation. This is an initiative that came out of DOE Headquarters, looking at what the impacts of bringing on-line a new generation of actinide-burning reactor might be. If we change the characteristics of the fuel going into the repository, how would this change the performance of the repository?

Support design tradeoff studies. Certainly these are things very much on our mind, as we're looking at the next iteration of exploratory studies facility design.

And to support the test and evaluation process. Specify/evaluate test controls that need to be prescribed for individual tests so that a test neither interferes with another test nor jeopardizes the ability of the site to isolate waste, just by the mere conduct of the test.

And of course, to evaluate and interpret, analyze the results of the testing process.

[Slide.]

DR. DYER: I put a keyword here on the front of this: Possible longer-term applications of performance assessments. Of course, if there is a finding of unsuitability of the Yucca Mountain site, all of these things are moot.

But assuming that site characterization goes forward and there reaches a point where we are thinking seriously about a license application, then these things are longer-term issues where performance assessment will definitely be contributing to advanced waste package in repository design, the environmental impact statement, the formal site recommendation, the safety analysis report and license application, and finally performance confirmation.

DR. NORTH: Excuse me.

DR. DYER: Sir?

DR. NORTH: The word "possible" that you had there, you're using it only in that the site might be abandoned before you get to that stage.

DR. DYER: That's correct. That's correct, sir.

DR. NORTH: Not that the first one wouldn't be done necessarily.

DR. DYER: That's right.

[Slide.]

DR. DYER: The objectives -- that is, the things that specify criteria for what performance assessment must do are mainly laid out in regulations. The things we'll touch on during today's presentations cover, I believe, virtually all of these categories: preclosure radiological safety; doses to the general public and workers from normal operations and accidents; retrievability of waste, which is also a performance objective; preserve the option of retrieval of waste for up to 50 years after initiation of waste emplacement operations; the performance of natural barriers; the prewaste emplacement groundwater travel time criteria.

[Slide.]

DR. DYER: Postclosure performance of engineered barrier system. These are the subsystem performance requirements, the waste containment, the waste containment time for waste packages, and the rate of radionuclide release from the engineered barrier system. And finally the one that most people seem to think of when they think of performance assessment, the postclosure performance of the total system, the total system performance criteria, the cumulative release of radionuclides to the accessible environment out of 40 CFR 191, and also we have radiation doses to maximally exposed individual, and the concentration of radionuclides in special sources of groundwater.

[Slide.]

DR. DYER: As you will hear the following speakers amplify on, we have a six-step --

actually I guess I would say it's almost a seven-step process that is used for conducting performance assessments. The zeroeth iteration here is a system description. Once a system description is obtained, then we go through this logical sequence of develop scenarios; estimate probabilities; develop models including conceptual models, mathematical models, and computer codes; identify parameter values and distributions appropriate for the analysis; conduct analyses; interpret and evaluate results, and, of course, there is an iterative loop here.

And it's not mandatory that one goes through this in a linear sequential manner. As you'll see, it's possible to work on several steps of this at the same time. In fact, it's mandatory.

[Slide.]

DR. DYER: Scott and Felton are going to talk at some length about models, but I wanted to just broach the subject here and talk about the hierarchy of models from the most detailed, big process-oriented computer codes down at the lower level, which look at some aspect of geochemistry or thermal response or hydrology, maybe fracture matrix interactions, which are very good for determining or modeling how a particular process can affect some small part of the system up through a relatively simple model which captures the essence of the performance of the system.

There must be an intermediate level of modeling also that forms the connection, the link between these lower level codes and the upper level codes.

We have, oh, approximately 1500 site parameters that we would carry down at this bottom level. The simplest models, total system models that are capable of doing probablistic performance analyses will carry, oh, a couple of dozen parameters perhaps.

There needs to be some mechanism by which we ensure that the most important parameters or aggregates of parameters are carried forward to the higher level models, and that's one of the most challenging parts of this program, as I'm sure you'll hear from some of our following speakers.

[Slide.]

DR. DYER: Because so much of this program has to do with selection of models, justification, validation, verification of models, I thought I would lay out for you just an overall scheme of how the various levels of models fit together within our program. And there are some -- there are feedback loops on here. This is not a one-way flow diagram.

But starting from the top, we have our understanding of the system from the site, waste properties, a preliminary design, and we form models of the repository and waste package, the waste form, the waste package environment, groundwater flow model.

We also develop scenarios which are developed from lists of features, events, and processes. These scenarios drive the later performance assessments.

Much of the sensitivity analysis that is done can be done on this level, providing immediate feedback into the site characterization program. This is where the testing program resides.

These things aggregate up to a source term model, coming down to transport, total systems simulator. Uncertainty analysis is listed down here. Here are the regulatory requirements listed at the bottom. There is another feedback loop that would go from the uncertainty analysis back up into site characterization. I've tried to capture that here just with this two-way arrow.

Now that concludes my introductory comments here before I turn the floor over to Felton Bingham, who will give you a tour de force of the history of Yucca Mountain performance assessment.

Are there any questions from the Board?

[No response.]

DR. DYER: No? Felton?

DR. DOMENICO: Russ, one point.

DR. DYER: Sure.

DR. DOMENICO: Where are the dose models required in the regs?

DR. DYER: That would be out of the groundwater protection.

I think this is your question?

DR. DOMENICO: There are stipulated concentrations for the underground water protection sections of 40 CFR?

DR. DYER: Yes, I believe so.

[Slide.]

DR. BINGHAM: I've never been asked to be a historian before, and I found that I had to learn some new disciplines in order to do it. It's hard to keep from doing what our Communist friends call revisionist history when you do this.

[Laughter.]

DR. BINGHAM: It turns out to be so easy to make yourself look good and everybody else look bad.

So I swore that I would take an oath of complete objectivity, and that's what I'll present to you today.

The Yucca Mountain project started a long time ago. It started at a time when the world was really very different. The word or the phrase "performance assessment" really didn't even exist.

I've chosen to start the history of performance assessment at something like the time that phrase came into use.

The history of the project has been a history of change, and we historians like to label change, so that it's easy to understand. I've chosen to break the history up into three ages, and I've tried to give them names that will mean something in terms of what happened during those ages.

[Slide.]

DR. BINGHAM: The first one I've chosen to call the Age of Innocence, and this graph is one -- this chart is like one I'll use for all three of the ages. The other times at the top, you can see I started around 1980, and I ended around 1986, and I hope by the time I'm finished talking about this age, you'll understand why I called it the Age of Innocence.

At the top row of things on all of these slides, I've put what was happening in the regulatory business, and those of you who were around at the time remember that there was a confidence rulemaking that the NRC conducted. The DOE contributed heavily to it, and that appears to have been the time when the term "performance assessment" got chiseled into stone.

It became something that people were doing. Before that, we didn't know what to call it. Was it risk analysis, or was it something else?

About that time, the word began to show up. The watershed in all this, of course, was the passage of the Waste Policy Act. In fairly short order after that, the most important rules came out.

In the next band that I've got on these charts, I've tried to show some of the big things that the project itself was doing, not necessarily performance assessment. At the time this age began, something was going on called area screening, and I think it's important to point out that at that time Yucca Mountain hadn't been picked. The Department of Energy was just looking at the test site for some place. Tuff hadn't been picked. They looked at all other kinds of rocks. In fact, the difference between saturated and unsaturated rocks wasn't even being thought about much. A different world in many ways.

Down here I'm putting things that performance assessment did, and I hope that you will be able to see a relationship between what happened here, what happened here, and what happened up here [indicating].

This is the date that I've chosen to be for performance assessment beginning. There are documents around where the word shows up at the Yucca Mountain project around that time. So that's the time I've picked to be here.

Well, let's see what was happening here. That area screening took awhile. In fact, it began somewhere off to the left, back in the '70s somewhere, and that's why the arrow is on there.

That was followed. Once Yucca Mountain was picked at this point, there was a phase of picking a unit. Should be the Bullfrog, the Tram, the Topopah Spring, what? Should it below the watertable or above it?

Somewhere around this time, a decision was made to go to the Topopah Spring unit, as you know.

One thing hardly anybody seems to remember anymore, except that the participants in it will never be able to forget, is thing that I've called the Orange SCP. The project decided that it needed to write a site characterization plan, and then it needed to do something that proved prophetic, which was something like deriving the needs for data from some higher set of something or another, regulations or the need to get a license.

That document was produced, and I would like all of you to notice how short this line is. In fact, it was really even shorter than that line, but you've got to put something up here on the chart that you can see.

The thing that really characterizes this age is the environmental assessment. Making that required a lot of effort from the project and from performance assessment as well, and I've chosen the end of it to be the end of the Age of Innocence.

Now really I've chosen some time like this to be the end of the age. We historians don't have iridium layers. We don't go by extinctions the way geologists do. So it's going to be kind of fuzzy, but that's an historian's prerogative.

The site characterization plan was starting up, the little dots in here, at this time, but the bulk of our attention was directed to the environmental assessment, and that was a very important thing, and I'll try to point out why in a minute.

Well, what was performance assessment doing? The main thing I'd like you to get out of these little words that are done here is, that even at that time, even early, the major features of what was going to be required had been identified. There were scenarios, events, processes that might happen. They were early; they're now obsolete. But there was an effort to look at them, even in advance of this site characterization plan.

There was a set of bounding calculations on the performance of the site that turned out to be very useful in writing the EA and then later in writing the site characterization plan.

People began to understand the importance of formulating a description of fluid flow in the mountain. Those features that still nag at us today, I think were mostly recognized at this time.

Now the trouble was that at this point, nobody knew what performance assessment was, except maybe one or two people who called themselves performance assessors, and they were probably wrong.

Throughout this time, there was an increasing understanding of what it meant and what it was supposed to be for. I think an example of that is from these -- people didn't understand. What was the point of doing those bounding calculations? Isn't that kind of a waste?

Of course it wasn't a waste. With the hindsight we have today, we know why it was important.

There was technical work going on, and as a surrogate for that, I've just chosen some computer code names to put up here, and these are names that you've probably seen before and you will here again, most of them, today.

The point is that these codes, most of the ones that are in use today, were being mentioned as early as 1983 or 1981. And these codes contain the models that describe Yucca Mountain.

I thought I ought to put one thing, at least, on here that's not just the name of a code, so I put in this one. The first benchmarking exercise with those codes took place in here, and there was a great deal learned from it.

Well, this chart -- these are the facts. Now an historian is supposed to interpret the facts for you, and that's what the next slide is about, to give you a little feeling: What was it like to live back in that Golden Age?

[Laughter.]

e behavNGHAM: Well, one of the most important things about it was, there weren't very many people working in performance assessment. It was a small group. And that led to this second thing here, what I've called the research-like atmosphere. I don't know exactly the right words to use for this, but the surrogate that I've chosen for it is the way reviews were expected to be done.

I believe the people in the program thought it was going to work the way the scientific community usually work. You publish. There's debate over what you've published. You retract it, or somebody publishes a paper proving you're wrong. But eventually you arrive at conclusions about the way the site behaves, just through that old-fashioned scientific process that really had worked pretty well. There was, of course, then an emphasis on publication for this kind of peer review.

Another characteristic was that there wasn't a great deal of formal control. The people who were working it thought they would be in control, but as you'll see when we get to the next Ages, they hadn't seen nothing yet.

[Laughter.]

DR. BINGHAM: This one, I think, is very important. They believed that the millennium was about to come.

[Laughter.]

DR. BINGHAM: I have in my desk a schedule dated 1983 that shows the license application in 1987. That led people to do things somewhat differently from the way they do them now.

There was throughout, as I mentioned already, a growing understanding of performance assessment, but even by the end of the Age, nobody believed that performance assessment should drive the project, that it should really be the place you go to find out what's need for a license application. That still just wasn't felt.

But things -- an understanding had occurred. The way I've kind of described this in summary is this bottom thing. It seems to me it worked kind of like the way I understand the European and international programs are working now. There's a good deal more of a university research like atmosphere and a kind of collegiality, not being driven by formal controls or regulations so much as just by the need to collect information.

That led the group at that time, I think, to be somewhat more interested -- and this is for example -- in natural analogs than they are now. When you don't have a site to characterize, you probably feel a little better about allocating your resources to characterizing things that aren't your site and trying to see what you can learn from them. This kind of emphasis changes, as the Ages change.

Now, I want to tell you one more thing as to why I called this the Age of Innocence, you'll remember that the environmental assessment ended with the end of this age and the Site Characterization Plan had just really begun. A high ranking official in the Department of Energy, who today occupies an even higher position, was asked once around that time what are we going to have to do about the SCP? And he said, "I don't think we're going to have to do very much about the SCP." And the reason he said that is going to show up on a later slide.

[Slide.]

DR. BINGHAM: Well what are the things that we accomplished during this time? The main things think were that we recognized the most important technical issues, the ones that are here today. The one thing I think of that is probably missing from this is the lack of understanding of the importance that gas phase motion might play in the repository.

People didn't think about it as much at that time, but many of these things were there. The need for conceptual models, I'm sure, was recognized at the time. I think everyone felt a little surprised when they were accused a few years later of not having thought of alternative conceptual models. They hadn't used that phase probably, but the notion that we needed to understand how the mountain worked and we needed to understand the possibilities, the hypothesis that are consistent with the data we have, I think was recognized even then. And the need to test for them was implicit in that thinking.

As I point out the bounding calculations have been done and computer codes were really largely in place.

[Slide.]

DR. BINGHAM: Well, at the end of the age there were expectations and I've chosen, these aren't real quotes, but I've just put them down here. The first thing I think was an optimistic one, that by making that EA we kind of brought performance assessment into the rest of the project, although not everybody understood it yet, there was a growing unification of performance assessment with the rest of the program with site characterization.

There was a feeling that performance assessment has finally figured out what it needs to do. I think that was mostly through the technical work that had gone on during that time.

The codes were being used to help improve understanding of things that you can't just get with a pencil and a paper. And then there was this statement, the SCP is going to be easy to do because we've already done the orange SCP. It contains most of the information. And the Environmental Assessment brought in a great deal of information and put it all together and made people work together on it. So the SCP isn't going to be very hard to do. We probably won't have to devote many resources to it.

I don't have to show you the next slide. I'm sure for all of you in this room to know what a naive thing that was to say.

[Laughter.]

DR. BINGHAM: I hope after that there can be no doubt why this was called the age of innocence.

[Laughter.]

DR. BINGHAM: Well, innocence, when the historian uses that word he means something is going to happen at the end of it.

[Laughter.]

DR. BINGHAM: There is another world to move into and we had awaken to that. There were a lot of things I could have called this age.

[Slide.]

[Laughter.]

DR. BINGHAM: But I thought maybe the least inflammatory, but also --

[Laughter.]

DR. BINGHAM: -- Also an informative one is this age of awakening, because the theme I want to pull through this is performance assessment, although it had thought, remember the preceding side, we thought, now performance assessment knows what to do. We learned here that we really didn't. The big thing in this and this is the part that shows what the project was doing in great simplicity was the Site Characterization Plan.

And I've chosen, essentially, the end of it and the beginning of replies to comments to be the end of this age of awakening. Not much happened in the regulatory field at the time. There was proposed change to Part 60 that there was a flurry of activity about.

It's kind of hard to believe now that the EPA standard has been remanded longer than it was in effect.

[Laughter.]

DR. BINGHAM: The watershed here was the revision to the policy act that selected Yucca Mountain as the site to be characterized. That had, of course, very important implications, but most of them began to show up a little later, into the next age.

Well, what did Performance Assessment do during this? Well, the heavy emphasis was on the SCP was of course the main thing that characterized what they had to do. There were two big things to do. We had to derive the strategies in the SCP from Performance Assessment Requirements or at least a lot of them. I suppose all of you have been told many times how the Site Characterization Plan is made up on the basis of a set of issues and then resolutions for those issues.

Those issues called Performance Assessment we required the Performance Assessment people to work with the rest of the project to decide how to resolve those issues. That was done, partly through just native cunning and partly through a process called performance allocation. The detailed guidance for site characterization: what parameters do we need; and how well; and what kinds; and all of you are aware of the many many pages of reduced type tables in the Site Characterization Plan that express that performance allocation.

Well, I wanted to show down in the performance assessment's own part of this that there was something going on besides just going to meetings about the SCP. It's hard to remember much else now, but people were doing some things. Some of them were really related to the SCP. There were some -- a number of calculations about what used to be the exploratory shaft facility and is now the exploratory studies facility. To show how it would fit into a repository. There were a number of hydrologic calculations done for it. Many of them related to the calculations for the site, but both of them did result in reports, so I'll put a tic mark at the end to show where they finished.

That required a good deal of the performance assessment effort. And one thing that people don't remember much now, and Les Jardine is here to remind you of later today, is that during all of this time, preclosure kinds of assessments were going on. I didn't think it fair at all to leave them off of this. Now, this of course, is just a selection of what was happening, but I think it was the more important things, and I want to get across the point that that even though the SCP was soaking up most of the resources, there was still some development going on down here.

I've chosen two more code names as surrogates for that kind of involvement. These two codes began showing up in the literature, at least the ones I could find, around 1989. And you're -- you have heard of these codes, the Waste Package Code and this is an ordinary differential equation solver, that's useful for hydrologic calculations.

Well, those are the facts. What was the Age of Awakening like? What was it like to be in that time when the sun had come up at last?

[Slide.]

DR. BINGHAM: Well, I mentioned the heavy emphasis on the SCP. And of course that meant that there were decreasing resources for technical analysis, but I've tried to point out that there were some done.

One of the biggest things that happened that changed us was the growing number of participants. That that in part along with this increasing formality of the control lead to a kind of different atmosphere in the community. It was no longer the expectation of doing things in the traditional scientific ways. I've chosen reviews again as the surrogate, to just get across the point of how things were different.

The reviews which used to be done by little groups, how did we review the environmental assessment? Oh, half a dozen of us got in the room and spent a few weeks reviewing. For the SCP the reviews became increasingly from outside the project and they came from large ad hoc groups. And I hope you are catching on a little to why I call this the Age of Awakening. The performance assessment people haven't learned that this isn't an aberration, this is the way business is to be done, in a highly controversial, highly regulated project like this one.

There was a new understanding of the relationship with the NRC and this may be the bullet that might surprise you a little. I think most of the technical people in performance assessment were a little surprised on how non-technical the comments from the NRC were. Also, they were surprised at how formal and stiff they were. You remember one of the things that was -- that was said before was that we now know how to do our job and we can get on with the SCP, throughout all this time there was the expectation that we would have a pretty close relationship with the NRC. Not in the regulatory sense, but at least in an information passing sense that we could explain to them the things that seemed in hindsight to be so arcane in the Site Characterization Plan.

That really wasn't exactly what happened. There was a bright note about it, at least from the performances essence point of view. The rest of the project began to cooperate more and more as time went by through the Age of Awakening. As they began to understanding more what performance assessment was for. That performance allocation process brought a lot of people together.

It split a few people too. I had never been called "Great Satan" before that.

[Laughter.]

[Slide.]

DR. BINGHAM: Well, these are the things that happened during the Age of -- the difficult SCP tasks were of course the most important things and I've mentioned them all, all ready. One I probably haven't mentioned is that there were some agreements with the NRC. The NRC had objected to some of the first ESF analyses, at the end they seemed to think that the ones that appeared in the SCP with the supporting references were adequate. That was a good sign.

Of course, there was the other technical support to the SCP and the important thing about that is, we wouldn't have been able to do them if we hadn't had that Age of Innocence when we developed some tools. Maybe that's self-evident, but I thought it was worth pulling out.

And there was some development of capabilities as I evidenced by those two code names that I put up and some analyses got completed in spite of all those heavy demands from the SCP.

[Slide.]

DR. BINGHAM: At the end of the age people had gone through something and were expecting something again. Well, the first is the pessimism, there's a lot more control going on now than there was a few years ago and there's a lot heavier review. That probably is going to slow us down. It doesn't seem to be as easy to get things through. We thought things took a long time in the Age of Innocence, but boy were we innocent. This was the optimism, we finally got to start talking with the NRC at last. The SCP is out and now we can communicate with them, we can find out what they want, they can find out what we can do, and we can proceed more quickly.

This may be the most important of the expectations. We're just about ready to start site characterization. It's not exactly the millennium, it's not the license application that's staring at us now, but site characterization, and where the license application and how it had kind a fierce and unfriendly look to it, site characterization looks pretty good. We're going to learn a lot of things that we need to know during site characterization. We're looking forward to that.

And then the last, our strategies have been accepted. Those strategies in the SCP, I mean. Now, we can get to work, we can follow those strategies and we'll come to where we need to be.

There were still some innocents around.

[Laughter.]

[Slide.]

DR. BINGHAM: Well, the last age I've called the Age of Responsibility because the chickens have come home to roost for Performance Assessment. It wanted to be recognized and understood back even in the Age of Innocence, now it's not so sure that was a good idea because it has learned that it has grown up now. It's an adult. It's got to support small children and maybe a passel of chickens in the hen house.

This age hasn't ended yet, I don't know when it will. That isn't going to stop me from making predictions later on, but we historians do that. Nothing much has happened about the -- on the regulatory scene that I can think of that's important. We've seen drafts of the EPA standard flooting around, and I guess some day they'll become final. That hasn't slowed people down much.

The project has done some things that I just listed here, the first meeting with the NWTRB was a landmark of course and that happened back about this time, about two years ago. There was a strategy plan written, but perhaps the most important of the things as far as performance assessment in the project is the test and evaluation plan that Russ has already mentioned that sets out the notions of how testing and performance assessment will work together. This plan has all sorts of diagrams in it, flow charts to show how that interaction will be accomplished.

The band that shows what performance assessment is doing is a good deal wider. It goes along with that responsibility word up at the top. And this, of course, is by no means conclusive. Russ has talked about things that are going on and going to be done and you will hear more about them. But I thought I'd sort of run through this just to give you a little more of the flavor of the Age of Responsibility and why it's different from the past.

You've heard, of course, the presentations, or you will be hearing even more, about the support for the ESF design. That's developed into a formal process that now seems well understood. I'm sure you all will be following that carefully.

There's another kind of support and it appeared in several places on Russ' slides a few minutes ago. Performance Assessment has been called on to give support to a lot of things. He had a whole slide of support, I picked one to put down here which wasn't even on his slide because it's been done. Support to the consideration of the ESF alternatives. But his slide had many others: support to test priority setting; support to the Calico Hills study; lots of kinds of just support; that aren't solving the strategies in the SCP, just support.

Some things happened that have been completed that I think ought to be pointed out, at least they ended in the times since the last -- since you last heard about this. There's some detail studies of two dimensional flow at the site. This is kind of parameter variation and sensitivity studies, a kind of thick document with a lot work in it. It's given us some insights. The second benchmarking of the code.

And then starting out very long ago, development of a capability within the project to produce the CCDF's that the EPA standard requires. People have at last recognized gas-flow modeling and that has been going on for some time.

The probabilistic compliance strategy is something that Paul Kaplan goes around talking about. His talk later today won't really lay it all out, but I think he will touch on it. It's a notion of ways of being rigorous in supporting decisions of compliance with regulations.

Two things that you will hear about in some detail are these exercises, Pace-90 and then the "early" probably the word "early" should be on here, site-suitability evaluations. We expect that to continue into the later site suitability evaluations and the later, later site suitability evaluations and so on, because that, will of course, be an indurative process. And then detailed scenario studies that you will hear a little bit about today.

Now, the thing I would like you to notice about this is -- comes from remembering one of the expectations at the end of the other age which was, our strategies are in place and we can get to work. Very few of these activities are directly connected to those strategies. The scenario studies are. The CCDF's are, except that in the SCP they come at the end rather than here at the beginning. The gas flow modeling is implicitly there as part of the strategy because it states that we need models of things. But most of the others aren't in those strategies.

Now, that's not to say that they haven't been expected. The SCP continually harps on the notion of iterative interactions between performance assessment and testing, things like these. But I believe the difference between this age and the preceding one is that at the end of the preceding age people thought, yes, we're going to do those things, but we're going to do them when we get site characterization data. That's what will lead us to go through all these iterations. We'll refine our understanding of the site.

What we've learned now, in this age, is that performance assessment has a responsibility to do those iterative interactions even when there aren't any new data. Now, it turns out that there are some, and I'll mention that in a later slide. But that, I think, is one of the principle new things that performance assessments had to learn, something that distinguishes this age from the others.

[Slide.]

DR. BINGHAM: Well, what's it like to be working here? Well, the number of participants and critics has increased substantially, and of course after the Yucca Mountain was picked as the only site to be characterized, a lot more talent became available from the other projects, and some of them have lent their expertise to performance assessment now. There are increased expectations, the calculations that we had really thought would be required right away have shown up here.

There is a great deal more reliance on experts now, and I think there are a couple of things that drive that. I've picked -- there are several, but I've picked only two. One is the need for more formality in the decision making for establishing the trails and basing it on a way that can be tracked and the other is the need that without site characterization data, when you need some results to make decisions experts are about the only game in town for finding out how to do them. Now, the experts have to use, of course, the data that are available, the calculations that have been done and whatever calculations they can do when they are being called on, but in any case, in this age there is much more use of experts than there was in the preceding ones.

There is less emphasis than folks would have expected on carrying out those SCP strategies and getting them done. But the bright side is that there is increased availability of some site data and that is coming about partly because of this trend through all the ages of an increasing understanding of performance assessment and the need to cooperate with it. I think because some of the later speakers today will mention some data that have become available in the last couple of years. I don't believe they are published data yet, but they are available for people to work with to sharpen their understanding of the site.

This increased cooperation within the project is an important thing to notice. That's what has lead to new data even when we really haven't started site characterization.

[Slide.]

DR. BINGHAM: Well, what are we going to look back to a year or two from now? It's hard to say. I don't know when this age is going to end. I think if we've learned anything from the past ages, it ought to be that there's going to be another age after this one. Something will happen to change. Perhaps the start of site characterization. But a year or two from now I think people may say we do have a better understanding of the site. We understand those important facture-matrix interactions better, there are tools in place, and there is some data available that are helping us to understand that.

We'll probably have models we can believe as well as we can without site characterization or gas-flow. And we'll know a lot more about the features of instant processes that are likely to be significant there.

I think we'll say PA has made a lot of contributions. I think we'll be able to brag about the things that performance assessment has contributed to the design of the ESF, through the test and evaluation plan that's going to play an important role in this, the testing part, and I think the efforts to come up with compliance, strategies for showing compliance with the regulations are going to pay off as well; that people will look at that as an accomplishment in a year or two. And I think they will think there has been some progress made in spite of all the other demands for carrying out the strategies.

[Slide.]

DR. BINGHAM: Now, it may be the height of arrogance, but we historians are used to that charge. What are people going to be expecting a year or two from now? It's really arrogant enough, I suppose, to say what the accomplishments are going to be. But this is this historian's guess on what's going to happen.

Well, most all the quotations I've had before have had some pessimism and some optimism. The optimism one is this one, I think that a year or two from now people will say performance assessment and design and site characterization are really working together. The support to the ESF will establish a strong link with design. In fact, I think it's there already. The work with the test and evaluation plan and especially if site characterization begins, are going to help pull all those things together.

The pessimistic one is, boy we're being asked to do a lot of stuff now a days, I sure wish we had a few more people. If this sounds like an impassioned plea to the DOE, it really isn't because even back in the Age of Innocence, of course, people were always asking for more resources and more funds. But I do think people will be a little worried that the capabilities in spite of this increased number of participants are stretching it a little thinner than the people who are working in it would be comfortable with.

These may be entirely different. This age may end with some enormous bang. Most of them have ended with whimpers, and I expect that this one will too.

I enjoyed putting these things together. If any of you are aware that your institutions have an endowed chair in the history of performance assessment --

[Laughter.]

DR. BINGHAM: -- I'll be glad to submit my resume.

[Applause.]

DR. NORTH: Thank you very much for that historical overview. Do we have any questions or comments from the board?

DR. PRICE: Just a comment. Given the length of your ages of the past, it looks like we have 2,000 to 20,000 ages ahead of us.

[Laughter.]

DR. BINGHAM: That may be true and they all come in the next fiscal year.

[Laughter.]

[Slide.]

DR. DYER: I put myself in this slot with some trepidation because following Felton is somewhere between following William Jennings Bryan and Bob Hope.

[Laughter.]

DR. DYER: Since the last presentation to the board, as Dr. North noted, in May of '89, in fact a year ago last summer, there has been a fairly major reorganization in the performance assessment program within the Office of Civilian Radioactive Waste Management. Prior to that time there were essentially two parallel programs in place. One, an oversight program that was administered through headquarters and then there was the program that was administered through the Yucca Mountain project.

Coincident with the reorganization of OCRWM, there was also a shift of all performance assessment responsibilities to the project office. In the past year we have gone through a not always painless effort to integrate the performance assessment organizations that previously were working for both the project and for the headquarters. And what I'm going to present now is an overview of our organizational structure and the -- the scope of work, if you will, laid out by organizations and different -- well, how we figure out who is going to do what, I guess is the theme of this part of the talk.

[Slide.]

DR. DYER: This is a very simplified organizational chart. As you see here, performance assessment activities come through the Yucca Mountain project office, I'm the WBS manager, Jerry Boak is my able assistant. We are assisted the by the T&MSS organization, also commonly known as SAIC who provides much of the day-to-day assistance on managing the program.

We have four major participants in performance assessment. Far and away the organization with the most assets assigned to it, Sandia National Labs under Tom Blejwas and here's a list of the things that Sandia is involved in, total system performance assessment; post closure; repository analysis; preclosure radiation safety; waste retrieval analysis; seal performance assessment; groundwater travel time; favorable adverse conditions; higher level findings; development and analysis of -- development and validation of flow and transport models; supporting calculations; performance confirmation; development and verification of computer codes. These are all categories tied to specific WBS elements which I'm going to go through in considerable detail in a little while.

The Pacific Northwest Labs is one of our new members of our family. Cheryl Hastings is the technical project officer with a little "t" for PNL. They are involved in these five categories of the WBS. Lawrence Livermore National Labs, Bill Halsey, heads up the performance assessment at Livermore. Livermore is primarily concerned with waste package near field environment issues and geochemical modeling.

Los Alamos National Lab involved primarily with transport modeling. And we have what I'll call minor participants, Lawrence Berkeley Labs/UCB, we've combined these as one organization, Chin Tsang heads up one group, Tom Pigford heads up another group through this funding path. U.S. Geological Survey, this is our interaction with the testing community, primarily the saturated and unsaturated zone testers. Through Chicago we have linkages to a couple of fairly small efforts, some university funding passes through here. It may make a little more sense if we look at it as a pie slice.

[Slide.]

DR. DYER: And you can see in the grand scheme of things, where different organizations fall. As I said, far and away the bulk of the performance effort -- performance assessment effort is performed by Sandia, Pacific Northwest Labs is number two, Livermore is a close third, Los Alamos sitting in here, T&MSS, this includes some subcontracts, here's LBL and then everybody else is about six percent.

[Slide.]

DR. DYER: In 1990, this was a fiscal year 1990 was a time of change and transition. This is when the organizational change was taking place, but we still accomplished quite a bit. And what I would like to do is just give you a brief walk through of some of the things that were accomplished last year. Albin Brandstetter will talk about what's on the plate for '91 and '92, fiscal '91 and '92.

But fiscal year '90 we completed the PACE analyses, we developed some scenario event trees that have been used as the basis for some of the early site suitability work. We initiated ground-water flow calculations for support of the Calico Hills Risk Benefit Analysis. Those were actually finished up in early fiscal year '91. Continued support of the exploratory studies facilities I think it was ESFA, the exploratory shaft at that time, but repository design alternative studies, those studies have also likewise finished up; analyzed performance of borsilicate glass waste form.

[Slide.]

DR. DYER: In a more R&D vein, things that were done, I guess I harken back to Felton's Age of Innocence here, we still have some R&D going on. Preliminary total system performance assessment computer codes were completed. We continued development of various computer codes and models. We initiated laboratory experiments on unsaturated fracture flow and transport, not everything being done under the guise of performance assessment is necessarily just cranking big computer codes.

There is also a fairly large effort involved in model validation which I will talk about shortly. Participated in the INTRAVAL, the International Validation Experiment and PSACOIN, another international -- I believe it's a code comparison. Formulated a general model validation approach which we are processing through the system now to try to establish a project standard for model validation. Initiated software and experimental QA program and procedures. This is a non-trivial exercise, it was establishing a workable software QA program.

[Slide.]

DR. DYER: I want to revisit a slide here. Well, this is what I've accused Rally Barnard of, being his six-fold way to the path to performance assessment.

This is the train going down from a system description through development of scenarios, estimation of probabilities, development of models: conceptual, mathematical; computer; identification of parameter values, conduct analyses, interpret and evaluate results.

This is the logical progression of a performance assessment.

Now how do we translate into something that can assist the program at the proper point in time?

[Slide.]

DR. DYER: There are several ways to look at our program.

One you've been exposed to I'm sure is that part of the Site Characterization Plan that describes performance assessment. This is Section 8.3.5 of the Site Characterization Plan; 8.3.5 can be broken down into basically three categories, three subsections, if you will.

The first subsection, which is 8.3.5.1 through 7 talks about pre-closure performance assessment and the various parts of the program that have to deal with this general category of performance assessment needs.

[Slide.]

DR. DYER: The second part of the SCP or subsection if you will deals with the post-closure performance assessment program, 8.3.5.8 through 8.3.5.16. One thing I'll point out is 8.3.5.13, total system performance where the logic of constructing a total system performance assessment model application is laid out.

[Slide.]

DR. DYER: The third subsection is other applications and kind of cats and dogs.

This is applications of performance assessment here, how the applications will be brought about, a listing of completed analytical techniques and techniques that we knew that we needed to develop at the time we wrote the SCP.

The SCP is not terribly flexible for managing a program on a day to day basis, so we have another way or organizing the program and I would like to take you through that structure.

[Slide.]

DR. DYER: This is the DOE's work breakdown structure classification and in this classification we have nine subcategories for performance assessment.

In the general scheme of things, performance assessment falls under the systems category, which is a third level WBS element. At the fourth level, 1.2.1.4, we have performance assessment and then there are nine subcategories of this performance assessment subcategory which deal with various aspects of performance assessment. I am going to walk you through here just to give you some understanding of what kinds of things fall under the purview of performance assessment and who in the organization is involved in different efforts.

[Slide.]

DR. DYER: 1.2.1.4.1 is a part of the program that I'm sure you're familiar with. This is the total system performance assessment. Sandia National Labs, Pacific Northwest Labs, Golder are the major participants in total system and their main objective, to integrate the physical process submodels and data in the computational models for prediction of post-closure performance including uncertainties and to assess compliance with the overall system performance objective of 10.CFR.112.

[Slide.]

DR. DYER: 1.2.1.4.2 is waste package performance assessment.

Lawrence Livermore National Labs, Pacific Northwest Labs, Lawrence, Berkeley actually primarily UC-B (Tom Pigford's group) and some efforts through the Chicago Operations Office of DOE, namely Roger Staehle of the University of Minnesota, Claudio Pescatore of Brookhaven National Labs are providing information or providing analytical support under this particular category.

Here we are dealing with prediction of long-term waste package performance both for single packages and for a suite of all waste packages, assessment of the compliance to the performance objectives for both the subsystem, the waste package and the EBS requirements out of 60.113, support to design optimization studies -- this would be for total system, I'm sorry, for some of the design trade off studies and to provide a source term for the total system performance assessment. This group essentially produces a source term, hands it off the total system group.

In fact I think Bill O'Connell passed out a paper that I believe was provided to each of the board members, a report from the waste package group dealing with the source term developed for the PACE-90 exercise.

[Slide.]

DR. DYER: 1.2.1.4.3, repository performance assessment, this is entirely Sandia

National Lab. This is looking at performance assessment in support of repository design.

Site performance assessment -- virtually everybody is involved here -- Sandia, Lawrence Berkeley Labs, PNL, U.S. Geological Survey, Los Alamos, Lawrence Livermore.

This is -- among other things the ground water travel time assessment falls under this category.

One thing I'd like to point out here. These are all things that have to be done but they're not all things that we can do right now, as you'll see whenever I talk about how the budget is split up for this year.

[Slide.]

DR. DYER: Since we're playing a zero sum game we have to pick and choose which of these items we can put our resources to at this time.

This is a category that you might not think of as being performance assessment, geochemical modelling and database development. This is all done at Livermore and this is primarily the development of the EQ 3/6 code database is funded under performance assessment.

[Slide.]

DR. DYER: This is a slightly misleading category here, the development and validation of flow and transport models, and the keys words in here are validation and this is models. We're not talking about verification of computer codes here. We are looking at validation of the fundamental underlying models that would be embedded either explicitly or implicitly in some computer code.

We have a major effort underway in this category, Sandia, Lawrence Berkeley Labs, U.S.G.S., Lawrence Livermore, Pacific Northwest Labs, Golder, Los Alamos. Virtually everybody is involved in here because the results, of course, are only as good as the model that you are using. The validity of the model.

These list some of the things that we're looking at within this general category.

[Slide.]

DR. DYER: This is a very generic category, supporting calculations. You'll see that we spend a lot of money in this category because this is where we are doing the calculations in support of various task forces. Virtually all of the support for the four task forces was funded under this particular category and of course it involves virtually everybody involved in performance assessment.

The description is fairly straight-forward. This is the category in which support is done.

1.2.1.4.8, performance confirmation, is I guess "dormant" is probably the best word for this particular category now but whenever this part of the program becomes more active, its task will be to assess and ensure the compliance of the performance confirmation program with the requirements in 10 CFR-60.137.

[Slide.]

DR. DYER: The last item in my shopping list of WBS elements is development and verification of flow and transport codes. This is a little more traditional view of verification of codes, not necessarily the models but here we are looking at the codes. This includes, in here we include the software quality assurance requirements that we must satisfy. These are things that show where these codes are used.

[Slide.]

DR. DYER: Now how does this break out asset-wise and resource-wise?

Well, for 1991, fiscal year '91, this is how we broke out the budget: total system gets about 23 percent of the budget; waste package about 14 percent; repository, 4 percent; site, about 8 percent; geo-chem, 4 percent; validation, 14 percent -- as much as waste package; supporting calculations, 24 percent -- this is the largest part of our PA budget is in supporting calculation; performance confirmation is just something in here, just a tiny little piece, less than 1 percent; and verification of codes is at about 9 percent.

[Slide.]

DR. DYER: The final thing I'd like to talk about is to clarify where the document hierarchy, what controls our work since the reorganization occurred. And we've got a couple of themes running on here. We have a Who side and a What side. At the Yucca Mountain Project Office the performance assessment program flows from the project management plan and we have a subtier document called the Performance Assessment Management Plan which details who is involved in the program and what the duties, the scope of each participant are.

On the programmatic side, the program flows from the Site Characterization Plan or now the controlled version of that, the Site Characterization Program Baseline through the Test and Evaluation Plan that Felton talked about out to individual work plans that are prepared by the individuals actually accomplishing the work. The Site Characterization Plan or Site Characterization Program Baseline is tied to the long-range plan which in turn feeds our Planning and Control System which Albin Brandstetter will talk about. All of which is connected to our general strategy for resolving issues for preparing documents to justify closure of issues. That's laid out -- part of it is laid out in the Technical Support Document Management Plan.

Since the reorganization of the program there are two documents that have become superceded, these are listed down at the bottom as obsolete documents; the OCRWM Performance Assessment Management Plan has been superceded by the Yucca Mountain Project Performance Assessment Management Plan and the Performance Assessment Implementation Plan has also been superceded. We found it difficult to use a yearly published document to guide a program that needed to be highly flexible and fluid so we have decided we are using the Planning and Control System which ties budget, schedule and scope of work together in a relatively responsive system to provide our guidance and tracking for the performance assessment program.

That concludes my overview of the programmatic aspects of performance assessment.

Dr. North, any --

DR. NORTH: Dr. Price?

DR. PRICE: On your slide that you show regarding scenarios, I refer to the scenario evolution document that we were provided with before, written by Bernard, Dockery, Dunn and MacIntyre.

DR. DYER: Okay.

DR. PRICE: Where a scenario is defined as a single continuous path through an event tree from the primary event for a process to the release of contaminants in that specific area they were talking about release of contaminants. But I think they tied directly the definition of scenario to the event tree methodology. That's a very narrow definition of scenario. Is that definition what was meant when you presented the slide to us?

DR. DYER: I'm going to try to tackle that then I'm probably going to have to rely on Felton to help me out. Right now that's one way that we can approach scenarios, is to develop scenarios from screening development from features, events and processes. Not necessarily the only way that scenarios can be developed.

DR. PRICE: What is -- I think rather than pursue that, what is the purpose of the development of scenarios? Is it to cover all sources of performance degradation, meaningful performance degradation? Identify and provide available to us all sources of meaningful performance degradation that needs to be looked into, is that what the object is in developing these scenarios?

DR. DYER: Yes, sir. And using the features, events and processes, that takes it down one to two levels lower. What we'll try to do is to roll up to examine many individual things that might happen, aggregate those into a scenario and then if it's possible, lump those into classes of scenarios and analyze those classes of scenarios to determine degradation of the performance of the system.

Is that -- Felton, do you care to add anything?

DR. BINGHAM. I would -- Excuse me. I do hear here a kind of misunderstanding --

THE REPORTER: Come up to the microphone, please?

DR. BINGHAM. I would like to say a word about that, because I do hear cropping up here a misunderstanding that only in the last few months I've begun to recognize as occurring. It's a problem in communication. That narrow definition of the word scenario is useful in this context because these event trees are being used only as a tool to try to help define what are the features, events and processes that we're going to have to model some day? And that's really the old sense of scenario.

I have a slide back in the Age of Innocence that showed that word. It's important to remember, I think, that that word has subtly but significantly changed its meaning. When you people talk to the WIPP group for example, you'll find that they are using scenario in a way that looks like the same thing, but isn't quite. To them a scenario is something that is modeled explicitly for the purposes of showing compliance with the EPA standard.

This project is not nearly so far along. To us at this point the thing that we are calling scenarios are just paths through the event tree, candidates for modeling later.

DR. PRICE: But it is really for the purpose of identifying those candidates which need to be evaluated because they are significant with respect to performance degradation?

DR. BINGHAM. Yes, that's right.

DR. PRICE: And the reason for my raising the question is that this would appear to me to be at the fountainhead of future analysis and therefore it is very important that you cover all relevant and important scenarios, is that not correct?

DR. BINGHAM. Yes, that's -- that's of crucial --

DR. DYER: We absolutely agree.

DR. PRICE: If that is true, what bothers me is the commitment to a single methodology here for identifying scenarios and that is the methodology of event tree analysis and not using any other methodology as well. The event tree methodology may lead you down paths because it constructs your thinking into paths, but may not reveal all potential scenarios that may be relative. Do you have uneasy feeling about really covering all of the waterfront on all of the potential scenarios?

DR. BINGHAM. Well, that's probably a topic for another half hour harangue, but it doesn't give me an uneasy feeling at this point. For this reason, I don't think that we're exactly doing something with those event trees that's quite as high flown as using an event tree methodology. The reason we picked the event trees is they turn out to be useful tools for communicating with site characterizers.

It's difficult to go to a geologist or a hydrologist and say, hey, I'm here from performance assessment and I want you to help me pick out things. He's like to say what's performance assessment and then why don't you get out so I can do my work. But when he's presented a tree and he's asked questions like, you know, I think it might be possible that water infiltrates largely through drill hole wash. What do you think about that? What would be the consequences if it did? He tends to be a great deal more enthusiastic about sharing his ideas with you because he can see them in front of him in an easy to understand form.

But the idea of using other methodologies for coming up with the scenarios that will be modeled in showing compliance with EPA standards I think is a question we haven't completely answered yet. But it's not a fair statement, it's not correct to say that we are using an event tree methodology to determine the features -- to determine the scenarios that will be modeled for the

EPA standards.

DR. PRICE: Well, I'm more interested in the question covering the waterfront than getting all of the scenarios and would like to raise with you the question of using other methodologies, including -- you know, there's inductive and deductive approaches and you've got one deductive approach, another is fault tree analysis, and as a matter of fact, in this article they indicate basic event or processes are added in sequence -- excuse me, the sequence of the events described in each event tree is initiated by and then they have five things which would be really, they are -- they could easily be cast into a fault tree up at the top levels of a fault tree and at that, examination could be done, for example, by a group blind to the group who are doing the event tree analysis so that you could compare to see if they come up with scenarios, what their cut sets they've got to give it a cut set, they can come up with a scenario from that cut set. Looking at the -- all of the cut sets across for common cause analysis, again they've got a scenario.

So there's another way there to give -- to get at scenarios. Failure modes and effects would be an inductive way looking at the components within the systems that you're involved in and raising those questions and you can have independent groups working blind if it is really critical and essential to identifying all of these maybe then you could consider whether or not you should be flowering at this point. If everything flows from that it might be that that's an essential item to have the confidence and be able to portray to the public that you have expended every effort to identify all scenarios.

DR. BINGHAM. Yes, I would agree. I think at this point we're really just still trying to communicate to find out what the features, events and processes are. The use of the word scenario may be unfortunate now, to define it as simply a path within a tree because it does have this larger meaning in terms of showing compliance. But I certainly agree those methods would probably be useful and to have a few more people around to work on that might be a help.

[Laughter.]

DR. DYER: Thank you, Felton.

DR. BINGHAM. This is not an appeal for funds, of course, because that would be the historian's responsibility.

[Laughter.]

DR. NORTH: Let's see, other questions? Bill

DR. W. BERNARD: Russ, you showed us a pie chart of your performance assessment budget, did you tell us what the total budget is?

DR. DYER: No, I did not. But I will. My budget for fiscal year '91 was $14 million.

DR. DEERE: Fourteen?

DR. DYER: Yes, sir.

DR. NORTH: Seeing no other questions on the part of board members of staff, I'll ask a few of my own. And they all relate to the three bullets on the agenda here. And I would like to draw you out for further discussion on each of those bullets.

First, the iterative use of performance assessment. When we had our meeting two years ago I was asking essentially what have we done since 1986 to pull together an updated performance assessment? And I think the answer I got at that time was "not much." And I'm asking the same question in 1991, where we've had three task force exercises now that have been brought completion and we've had PACE going on and my question again is really, do we have a new iteration, the complete cut at performance assessment that we can use at least as a -- shall we say in the aiming mode to try to figure out what are some of the areas in which the whole effort needs to be enriched and refined? Or do we have a collection of pieces and insights and efforts but nothing where it's essentially all together in one place with a systematic evaluation of where we have come and what we have learned as of May 20th, 1991?

DR. DYER: Let's review some of the things that happened since we last talked to you. Of course, PACE, the Performance Assessment Calculational Exercise occurred and we refined our understanding of the nominal case. The most difficult part of the problem to handle remains the disturbed scenarios. We made progress on addressing the disturbed scenarios. I would say that one of the things that we learned through the iterative process was how to define specifically what things we're going to look at in disturbed scenarios.

It's much easier to answer a question if you know what the question is. So we refined the question. You will hear Rally Barnard talk about some of the progress we've made toward answering that question. We have performed total system performance assessments, we have used some of those performance assessments to provide insight to experts on the various task force that you talk about. We've gained some understanding about some aspects of the program as a result of these performance assessments.

Dave will talk about some of the uses of the information. I will throw out one thing that we've learned and that is the Carbon-14 issue. Although Carbon-14 appears not to be any risk --

to pose any risk to public health or safety it's still under the current regulatory environment we still face a reasonable expectation of not being able to meet the Carbon-14 requirements because of the way the legislation is written.

Rolling things up into a total system performance assessment is one of the things that we were doing as an adjunct to the site suitability evaluation this year. And as we've said, one thing we've learned is that performance assessment cannot stand alone. It must be something that provides input to something else. This was another thing that came out of the test prioritization program. We made a mistake when we went into test prioritization because we looked at prioritizing tests solely as a function of performance, using only performance as the measure and it turns out that that doesn't capture much of the program that must be -- must be on-going. For instance, the environmental program is not captured by performance measures, yet it's a mandatory part of our program.

So, in part of the rescoping of the application of performance assessment, we broadened the scope of things that need to be considered by putting it in the context of a body that considers more things. The performance assessment effort from Sandia this year will provide input to site suitability and it will not be a full blown performance assessment to support a license application, but it will be what we are able to do at this point in time. I think we're going to be able to do probably about two and a half scenarios as well as the nominal case plus some sensitivity studies.

DR. NORTH: One of my concerns is that we figure out what the right scenarios are, what are the issues that really need to be addressed as this program proceeds? Now the Carbon-

14 issue I first heard about from a person that's in this room in very informal communications, very early after I became a member of this board. It's very gratifying to see that issue now has been addressed formally, it's in the analysis and we have a lot more down on paper as to why that's important and what are some of the plans that we need to make to deal with it. But I'm concerned about others that as yet may not be out there on the table that probably ought to be.

I reflect on the WIPP experience, where it's my impression, as a distant observer that the issue of human intrusion in performance assessment might have been identified years earlier so that the kind of analysis that's being done now might have been started much earlier and I think would have had great benefit to the program had it been identified in a more timely fashion so that it was in place when needed.

And I would hope on Yucca Mountain the lesson will be, let's do everything we can early by running some practice sessions. I can think of the problem of moving toward a possible license application for Yucca Mountain, very much in analogy to military preparedness against a possible war. That you don't want everybody off with their own particular skill or weapon system. You need to have some large scale integrated exercises where everybody practices together and you see what works and you see what's important. And to make that as realistic as you can so that you can learn what are the things you really need to know and be able to do when you get into the real war if you get it.

And my concern is if we sit out there with individual groups and individual exercises, and fail to accomplish that integration, there are some very important insights that we might have had, we won't have until we finally do the integration and by then we may put ourselves in a position where we have to play catch up and I think that would be most unfortunate.

So, I'm really urging that the deal we program do the most it can to carry out a full iteration of performance assessment. Put it out on the table where everybody can look at it, criticize it, and learn from it and then put in place a set of plans for how we're going to be able to do it better in the future. My impression is you have a lot of pieces of that and a lot of individual insights that various people in the program have achieved. But what I'm hoping is that you can put it out in one place where we the TRB can take a look at it and all the interested people who have gathered here and others who are not here can also take a look at it and collectively we may come up with a lot of insight for what needs to be done to make it better in the future so that the whole performance assessment program can be a better program and be more responsive to the needs that you've described.

DR. DYER: I certainly agree with you.

DR. NORTH: Well, I'm going on to structure a performance assessment management plan. You've given us, I think, a good feeling for how the reorganization has been accomplished and the various documents and how they fit together, but what I would like to see, that you didn't tell me about, is the set of milestones. What's planned for accomplishments within the next year or so? Now that we've been through the task force exercises, what are the next accomplishments that the program is planning that we can look forward to? I've just given you my vision of what I'd like to see first and foremost on that list.

And then finally, the test and evaluation plan, it seems to me that the analysis that you've done in phase one has come up with some interesting insights, perhaps the Carbon-14 issue leading the pack, but a number of others as well, and what you've just described in terms of the need for some of these tests for purposes of the environmental assessment seems like a very interesting issue.

It seems to me it would be very useful to go back and look at that suite of tests and ask essentially what have we learned about the reasons for those tests, and about the importance of the timeliness of the tests, and how does that fit against the ESF design issues that people have been looking at? In other words, we've seen a major change in the ESF plan over the course of the last year. And an integration job that would seem to me important and that we're going to talk about at a future meeting is the issue of how those tests might be prioritized and phased in time. Performance assessment would seem to be a key contributor to that, but here may be a lot of other issues that have to be integrated as well. So, I'm hoping that we will learn more about this issue either at this meeting or in our coming June meeting.

DR. DYER: To take your last two points one at a time, Albin Brandstetter will give you a rundown on the deliverables milestones for the near term.

On the second issue, right now we're looking at the primary focus group that is rolling up performance assessment would be the early site suitability evaluation group which Gene has talked to you at least briefly and I'm sure we're going to be talking to you again about some of the interim and final results of the site suitability evaluation.

That report is due out this summer for subsequent peer review. And there will be a performance assessment section in -- performance assessment section of input in that report. There will be a lot of other things in the report also which will address some of the concerns that you have just raised.

DR. NORTH: Do we have any further questions or comments.

[No response.]

DR. NORTH: Then we're right on schedule for a break. Let us try to keep that break to 15 minutes and come back in session again at 10:30.

[Whereupon a brief recess was taken.]

DR. NORTH: We will resume our session with Dave Dobson.

DR. DOBSON: Does that sound okay?

THE REPORTER: That's fine. Thank you.

DR. DOBSON: I guess I'll introduce myself. I'm Dave Dobson from Laboratory and Site Evaluation Division at the Project Office and what I'm going to talk about are some fo the results and some of the things that we've learned from several of the task forces and particularly I want to focus on the Calico Hills Risk Benefit Analysis that I participated in.

[Slide.]

DR. DOBSON: The Test Prioritization Task Force, the ESF alternative study and then a little bit on upcoming use of PA in the early site suitability evaluation.

There are several points that I will try and get across and in a sense -- with respect to some of the task forces anyway, the story I'm going to tell is a little bit of a microcosm of the overall history of the program that you heard from Felton this morning. An early phase of great expectations followed by phases in which realization played an impractical or more pragmatic considerations played a more significant role.

[Slide.]

DR. DOBSON: There are several points that I would like to make sort of in general before starting out and as I noted and as you have heard several times already today, performance assessment is one of several techniques which provide very important information into decisions. But the criteria that are appropriate to supporting any individual decision are really dependent on the nature of the problem that you are trying to address and performance assessment is not always the driver in terms of doing things like recommending test programs and in establishing orders.

Performance assessment does generally provide the best way to get scientific -- as close to objective scientific input into the decision that is relevant to things like public health and safety that we have right now and it's extremely valuable in that sense. As I noted, I'm going to go on now and talk a little bit about the use of PA in several different kinds of applications that we've -- that we've utilized. The first is I'm going to just spend about two viewgraphs sort of summarizing what in essences was the performance allocation process that has been described to you in several presentations over the past few years.

But the point that I want to make is that performance assessment at numerous levels is critical to planning a testing program. Then we'll go into talking a little bit about the specific task forces that we mentioned and finish up with a brief discussion of the use of PA in the early site suitability evaluation.

[Slide.]

DR. DOBSON: This viewgraph shows in general the structure of our -- of the overall program really. It starts with phases of test planning followed by phases of data collection and interpretation followed by a process of evaluation of the information that you've acquired.

The first two boxes here, the development of the requirements and performance allocation, site models, test impacts and then the data collection and interpretation phase really represent kind of where we thought we had to go during what Felton described as the Age of Innocence. We hadn't realized at that time all of the, if you will, nonscientific aspects of trying to get a -- trying to qualify a site and get a license. So the early phases of the program really focused strictly on what we thought we needed to know from a geological and hydrological and a modeling perspective. And you don't see anything in here about things like scientific confidence or reasonable assurance or things like that.

When you get to the phase where you're trying to do evaluations and make specific recommendations and you're basing those evaluations and recommendations, at least partly on, on public health and safety, you realize that you have a lot of other criteria that contribute to your decisions. And some of them are summarized here. Russ mentioned earlier environmental requirements as I have on here, the need for requirements. The realities of cost and schedule always enter into decisions. Input from oversight groups, of course, including the Technical Review Board and the Nuclear Regulatory Commission and the State of Nevada and various other overseers of the program.

You have obviously a large component in our program of perceived or real problems with public perception. The scientific confidence which we've talked about before as we defined in it in the Calico Hills analysis, and various other aspects of the decision are important. All of those contribute to making decisions and of course there are always various options that come out of your decision box. You may go back and replan your technical program, you may wish -- or revisit some of your nontechnical aspects of the program, you may wish, depending on what you come up with, you may wish to terminate the whole program.

[Slide.]

DR. DOBSON: Now, as I said, just a couple of viewgraphs on the initial part of the program. We initially relied very heavily on the identification of data in these from performance assessment and design when we constructed the whole overall program that is presented in the SCP.

And I wanted to make the point that the data requirements are dependent, at least in part, on the conceptual models that you select and no single PA model provides the complete and necessary and sufficient of data. It wouldn't be appropriate, we don't think, and I think most people would agree to choose, for example, a performance assessment model to use one of Russ' things, you'll hear more about this from Felton this afternoon, but you wouldn't want to pick just TOSPAC, for example, which is a very good code, but is a one dimensional total system or mostly total system simulator. You wouldn't want to pick that code and list the parameter values and say that's all the data I need. It wouldn't get you very far and it certainly wouldn't get you acceptance in a technical community.

You need to understand the data needs from all levels of the performance assessment models, from the process oriented models to the intermediate models to the upper level models and of course when you start -- when you start getting to the upper level models as Russ said --

[Slide.]

DR. DOBSON: -- you may have only a dozen or a few dozen parameters that you're dealing with and those are -- in general those kinds of parameters are very dependent on a lot of assumptions and conceptual models that are embedded in them and so you have -- it gets to be very difficult to assess the uncertainties when you do that.

So when developing a testing program you have to look at the models and the submodels and the assumptions at several levels at once in order to come up with a comprehensive program.

[Slide.]

DR. DOBSON: That is the point that is made on this viewgraph. We'll come back to that a little bit more in just a minute and I'm sure you'll hear quite a lot more about it later on today from some of the talks from more detailed presentations.

This is really intended to make more or less the same point and that is, everybody has seen this viewgraph, it's derived from about the 1986 version -- the initial versions of the issues hierarchy and the issue resolution process that is presented in the SCP.

The point that I want to make is that this box here that's called performance allocation is -- if you expanded the box and looked at it in some detail you would see iterations of between, as Russ noted, starting with physical descriptions coming up with summaries of conceptual models and physical models and processes, measurable parameters that you can get out of that and then assumptions embedded in those.

[Slide.]

DR. DOBSON: And I put this in just to emphasize that point. In the SCP, in fact, in between the original conceptual consultative draft, excuse me, in the final SCP, as a result of some comments from the NRC they felt we had not done enough explicit consideration of differing data needs derived from differing models. And so we went back and made a very intensive effort in the 1988 timeframe to evaluate all of the alternative models that one might apply to the site and try to determine if those would affect the data needs that you have so you see that we put together tables that include the current representations of models, the uncertainties associated with them and then the parameters that are affected and finally the studies and activities that get impacted by those differing alternative models.

[Slide.]

DR. DOBSON: Okay. Now we're going to move to a fairly brief coverage of the role that Performance Assessment played in various specific activities. All of the task forces utilized Performance Assessment at various levels of detail and to various exclusion of other criteria. And it turned out in the final analysis the performance estimates were only one of the important criteria that affected the recommendations. And in several cases you will see that the Performance Assessment criteria in and of itself did not provide a means for discriminating clearly among options really.

And that's the story that we'll tell in the next couple of viewgraphs. In particular in both the Test Prioritization Task Force and the Calico Hills Risk Benefit Analysis the criteria that were initially set up in order to do those tasks were based strictly and solely on performance related concerns. The Calico Hills started out with the assumption that you could rank tests based solely on how well a test contributes to your understanding of performance. The Test Prioritization Task Force did the same thing.

The ESF alternatives study was always much broader than that. They always had a very comprehensive suite of criteria for coming up with a recommendation. And I'll try and talk through, in just a few minutes, about how those task forces operated and about what we learned and what we didn't learn from that process.

[Slide.]

DR. DOBSON: By the way, I also wanted to make the note that the fact that performance assessment does not necessarily discriminate between one option for the Calico Hills Risk Benefit or for the Calico Hills Testing Strategy in other or the fact that a simple performance model does not have a list of prioritized tests that flow out of it. It's not necessarily a bad result, in fact, we think in many ways it's a very good result and it has contributed greatly to our understanding of what kinds of tests we need to do and why we need to do them. And I hope I can convey that.

Now, this is my version done at about 2:00 in the morning so don't spend too much time with it. About the -- of that pyramid that you saw earlier. And I want to make a few points about it and that is that we have talked about the fact that many parameters are dependent on the models that you derive from it and if you start with a total goal, for example, of evaluating repository performance, well, that you can break down into several subsystems and those subsystems can be modeled by various different processes. And each of those models -- each of the different models that contribute to estimating one subsystem component is then dependent on many other performance, what we call performance measures in the SCP.

Now, I didn't show it here, but you can put models in between each of these steps, between the measures and the parameters and the parameters and the site parameters because at each level you have differing uncertainties.

In the ESF alternative study we did a total system performance assessment with one parameter. We asked the question, is the site okay? That's the ultimate simple performance assessment. It was important for the purposes of that study to get some feeling for that and to get some feeling for how the testing program was likely to impact our understanding of the performance of the site. But, Russ said earlier, that you know the top level total system models will have a few dozen parameters, while the very top level only has one. And so if you ask the question, is the site okay, it's very easy to do a parameter sensitivity analysis. You know which parameter that's uncertain. But it's much more difficult to capture why it is that you don't have -- that you don't understand what's going on.

Now, in the Calico Hills Risk Benefit Analysis, we did a very similar thing at one step lower level of detail. We elicited, we estimated performance of various subsystem components and we did this with a great deal of back up information, you understand this and you've heard many of those presentations about how we went about trying to estimate the performance of the engineer barrier system and saturated zone and the unsaturated zone and they were based on quantitative models that were at lower levels, but they weren't completely -- they weren't rolled up quantitatively in the Calico Hills model.

So we took, for example, we went to all the publications we could find by Paul Kaplan and Scott Sinnock and Rally Barnard and everybody that we could get our hands on. When we said how do we think the unsaturated zone is going to perform and then we tried, using our carbon-based computers to bound that kind of -- that performance range. And we put that then into a quantitative model that rolled up from there. And we used it to try and do performance estimates.

The Testing Prioritization Task Force, when they started out, actually one level of detail lower than that. They started out trying to elicit from experts estimates of important performance measures and in some cases performance parameters, so they were trying to elicit estimates flux and those estimates were based on more subsystem models at lower levels where they assembled all the information they could and then used expert opinion to roll it up.

Now, a point Russ made earlier is one that that's worth repeating and that is that expert opinion always gets involved here somewhere because somebody had got to put the input into the model, no matter which level of the model it is. If it's down here, and you're trying to estimate the tortuosity or apertures of fractures, in fact in a meeting last fall in the -- on sorption that Don Langmuir attended, we were told that, for example, it was important to have a parameter in our model the orientation of clinoptilolites in fractures. Now, that's not in any of our total system performance assessment models, now, but it's a parameter that some people believe may be sensitive.

So the point is, just to repeat, that there are a lot of levels of assumptions in models in here, and from our perspective it's important to have studies on-going at all of those levels because until you can get some consensus that the higher level assessments are supported by the lower level models you haven't got it made yet.

[Slide.]

DR. DOBSON: This just really repeats the argument that I just made and it's taken from a presentation that Larry Rickertsen made to the board two years ago, but it really -- it really makes kind of the same point that the models that the -- in fact it makes another point that I didn't stress which is that the physical models and the calculational models are not always the same either. And you have assumptions that get you from the physical models to the calculational models and then to the things that you can measure.

DR. REITER: I've got a question.

DR. DOBSON: Sure.

DR. NORTH: Yeah.

DR. REITER: Excuse me. Dave, on your previous slide, I just wanted to make sure that I understand it correctly. In the right hand column like in the Calico Hills you said subsystem performance, but your measure of subsystem performance, your way of determining significance was total system performance, wasn't it? You always looked at the impact on the CCDF, that was --

DR. DOBSON: Well, in the Calico Hills what we assessed, the level at which we assessed was at the subsystem level.

DR. REITER: But --

DR. DOBSON: So we compiled different distributions for EBS and -- or for what we called the source term and for the unsaturated and saturated performance.

DR. REITER: But your measure of significance was the CCDF?

DR. DOBSON: Yes. We'll get there in a little bit.

DR. REITER: But, no -- I just wanted to make -- you weren't trying to say that the total system performance was only important in the ESF study?

DR. DOBSON: Oh, no. No. Absolutely not. No, I'm sorry, they all rolled up. In fact, both the TPT and the Calico Hills were intended to be rolled up within the models that were built for those task forces and the measure, you're correct, the measure of goodness or badness of a given test strategy was in terms of how much it helped you with your total system performance assessments.

[Slide.]

DR. DOBSON: I started with a summary on the test prioritization task force and this is it. Basically the priorities that were established on the basis of total system performance alone provide only part of the picture in terms of what's important to your testing program.

It would appear from the analyses in the TPT that site performance is robust and new data is unlikely to change that. The testing program, unfortunately in geoscience testing programs is not perfect, it's not a black and white world out there. And you can not always resolve the questions you would like to resolve by testing.

And other reasons for testing are important. You've heard this from Bruce Judd and Steve Mattson and the members of the Test Prioritization Task Force several times.

The priorities are driven, it's important to note, I think, in part by total system performance and in particular as Russ already noted, gas phase releases were shown to be an important component and the reason that gas phase releases are very important is because our current estimates of possible releases in the gas phase are close to levels that violate the standards. Not necessarily public health and safety, but close to levels that violate the regulatory standards. So performance is a very important driver in that area as long as the regulations are where they are.

The second part is that complex geology appears to be very -- a difficulty to model. A complex geology is a short -- is the short term for a very long description of a concern which is that difficulty with modeling is going to cause you great problems in essentially getting a license because it's a very -- well, I should say it's a relatively complex site and with the unsaturated modeling problems and everything else and in general the problem of unknown unknowns which we have talked about a little bit, it would appear that because of that -- that level of uncertainty that you may get to where you have problems demonstrating compliance. So it does become a performance standard in that case.

Now, I guess I wanted to make the final point that the priorities definitely correlate with, but are much more limited than most of the recommendations to DOE from oversight and other groups. I mean, you won't see, for example, in the testing prioritization task for, and I should mention that that report is out and I don't know if the board has received it yet, but it has been out for a month or so. And if you haven't received it yet, it's on the way.

You don't see on the high priority list from our assessment which were based in the TPT as nearly as we could on performance, you don't see vulcanism, you don't see seismic hazard, you don't see ground water travel time except when you look at ground water travel time because of its regulatory importance. You don't see it in the total system performance sense, you see it in a -- you can't demonstrate a regulatory compliance with ground water travel time. You will hear more about many of these, or at least ground water travel time, I'm sure later.

[Slide.]

DR. DOBSON: What the Task Force really attempted to do -- and this is taken, again, from an old viewgraph by Bruce Judd, from over a year ago, to the Board -- was to assess at the parameter level, as I mentioned, flux and gradient and hydraulic conductivity, to fold those into a model and to see what that told you or what those tests could potentially tell you about how the site was going to perform, and they used something that they called a decision line as a measure of the relative importance of the test.

If a test could tell you that the site was likely to exceed the value, but after doing the test you would know whether it did or not, then that test would be a very valuable test to do.

[Slide.]

DR. DOBSON: If, on the other hand, what a test could tell you didn't tell you about anything that was important with respect to a demonstration of compliance with some standard, then that test was perhaps not all that valuable.

The sort of thought was -- let me see if I can kind of show this -- that we started out with the notion that we would stretch the tails when we tried to assess what the performance looked like and that an initial CCDF might look something like this and might actually cross -- violate some standard or some notion of the standard but that, after a testing program, you would tighten up the CCDF and have a much better understanding of what it looked like, and if there was a case like that, if you initially thought that you had a possibility of violating the standard, because you have high uncertainty out at the tails of this thing, but that after you do the test it would really tighten it up, that would be a very good test, and that was how we tried to measure value.

What we found was, in most cases, it didn't work that way.

[Slide.]

DR. DOBSON: So what we did was we constructed a slightly different measure of benefit and detriment to the testing program, and this has been presented also, and I don't want to spend a lot of time on it, but as I noted, testing benefit was measured on the vertical axis essentially by a measure which analyzed the amount or the approach toward violation of the standard, how close a test -- how close a concern could come to violating the standard when measured according to the EPA standard and then what the test might tell you about it.

I've taken off all the numbers on the graphs here, but the three that clearly are above all others are the ones that I have already talked about, gas phase releases and the complex geology.

There is a whole host of other tests down here or concerns down here about which tests are unlikely to tell you that they are going to cause you a total system performance problem.

So, they have a relatively lower benefit, because the tests are not going to help very much to tell whether you're close to the standard or not.

On the horizontal axis, we've plotted what we call false alarm cost, and the false alarm cost is a measure of the robustness of the test. Many tests tell you good things, but many other tests just get you confused.

For example, I'll use one of my favorite examples. Doing P-wave teleseismic anomalies in the Crater Flat area has indicated broad areas of low velocities in the southern Great Basin.

Those could be interpreted as magma bodies. They could be interpreted as a lot of things, but there's high uncertainty to all those interpretations. So, what value do you gain from having a high uncertainty about a highly uncertain event to model anyway?

This was an attempt to capture some of the robustness of the testing program, and as I noted, many of the things that have high public perception problems don't appear to have high problems in terms of total system performance or don't appear to be a major concern in terms of total system performance.

DR. NORTH: Before we go on, I'd like to raise a comment on that general question.

DR. DOBSON: Okay.

DR. NORTH: It seems to me that what's been developed here is a very useful framework for looking at the question of how to prioritize the tests, but now that I have had a chance to go through your documentation, particularly given the way some of these judgments were generated, essentially as a subjective assessment, without much structure or use of models, from a group of people from within the program, it seems to me that these ought to be viewed as hypotheses to be tested with further analysis, rather than we can be really sure that those dots on the chart are where they ought to be.

What this exercise has indicated, that for the group down there below the three in the upper righthand corner, it would appear that it would take a very large change in the numbers to move the dot into the range where the test benefit appears to be very high.

On the other hand, a number of these issues are quite controversial, and there are a lot of judgments from outside the Department of Energy that some of these things may be very different than what the Department of Energy has been saying and assuming in its analysis.

So, it seems to me that there is a burden of taking this story public and convincing other people that you're right and that you're backed up not just with a group of experts sitting around the table but by that pyramid of models getting down to the lowest depths of parameter assessment and being able to put it all together in a very convincing story, and hopefully, as we move forward, that is going to be accomplished, or where it turns out you can't accomplish it, that will be realized and the appropriate changes will be made.

So, I am concerned over any assumption that anybody might want to make that, having been through this exercise, we have the answer and we're done.

It seems to me what we have developed is further guidance indicating that we have several areas where the tests are clearly quite important for performance and some other areas where the calculations show that the importance, the test benefit is considerably less but that further work might be needed to confirm those insights.

DR. DOBSON: I don't think that I would really disagree philosophically with what you just said. In fact, based on the results of the TPT, the DOE has not recommended terminating a test program.

But I will say is -- I agree with you that it was an important first phase. There's a couple of important points, I think, that I didn't make here.

One is that when you get down and look at what is complex geology and what is -- what are the unknown unknowns that make it difficult, that make it move up so high relatively on the chart, one of the things it tells you is that a testing program that's fundamentally exploratory is important, because it's not what we know about the site that we think is going to hurt us. It's what we don't know about the site.

So, even though you have pretty high confidence about everything you think you know, the scientists in the program, probably like yourself, still share that uneasy feeling of unknowns, and what do we do about that?

So, things that tend -- and you'll see this again when we get to the Calico Hills. Testing programs that give you exploratory information, that tend to confirm your understanding of the site -- and I don't mean to say just drifting to things that you know how they're going to be and show that they are just like you said, but programs that get you information from areas that you can't say you know precisely in advance have some value, and that's why you see things like complex geology and the hydro-geology site coming up pretty high, and I don't think that any of us would present any of these as final total system performance assessments.

That's not the intent, but we did try very realistically to go through the program and see where we thought the big hitters were in terms of concerns or futures or processes that could result in releases that caused you problems with demonstrating compliance, and those are the things that we saw.

I guess I would just say that the next step is do as you said, to go back and, in fact, the early site suitability analysis is going to be much, I think, of what you just asked for.

We're going to have a completely external review of the program in that task, and it will be at a level of detail that, hopefully, some independent people can take a look at it and see whether they concur or disagree with what we've said.

[Slide.]

DR. DOBSON: This is a little bit of a repeat, and I will go through it quickly, but the Calico Hills analysis also initially attempted to define testing value from a narrow performance-based perspective in the value information model.

It became apparent fairly early on -- earlier to the decision analysts than to the technical people on the panel -- that the testing program we had defined, which collected all of the information we needed in all of the cases, was unlikely to indicate -- to cause changes in performance predictions that were big enough to cause programmatic decisions to change, which was, as you may recall, how we defined value in that study.

Because the Calico Hills panel believed the testing had value that was not captured by the VOI, a multi-attribute analysis was initiated to clearly define it.

[Slide.]

DR. DOBSON: I have a viewgraph which is basically identical to the one I previously showed. It shows the initial concept.

The concept was that when we thought that when we did the CCDF, we were going to have tails that got close to the standard that indicated that there were going to be problems that we had. But, after we did the testing program, our understanding of the CCDF would be sharper, if you will. We would have a better understanding of the uncertainties and the values.

What turned out happening, really, was that when we estimated what we thought the performance was going to be like before the testing program and what likelihood there was that the testing program was going to change that estimate, we didn't get very much impact from the testing program, the way that we defined it.

[Slide.]

DR. DOBSON: Then we went to a kind of a softer analysis. You have all heard this in great gory detail, so I'm not going to go through it, except to say that we defined the value in the testing program in as many ways we could think of. And there's a big box here called scientific confidence, which was defined in much more detail.

That scientific confidence box really has a lot of similarities in it to the kinds of analyses that went into the complex geology box, if you will, in the Testing Prioritization Task Force, and that is to get -- I can stand up here all day and tell you that the Calico Hills analysis said that the site was going to perform a six-level, or six orders of magnitude better than the EPA standard. Most of you people will sit there and rightfully say, okay, now when are you going to do the test program? Because it's basically -- without the data to demonstrate the correctness of the views, you have a problem convincing other people, and demonstrating reasonable insurance to a regulator, in a situation like that, would be most difficult. In fact, that parameter is really what drove the Calico Hills recommendation.

[Slide.]

DR. DOBSON: Again, and these are from the meeting we had, I think, in March. Is that right? Was it March -- the last report on the Calico Hills and ESF Studies?

The testing program, as we conceived it, was not really performance based, it was based more on -- was more exploratory. The need for testing is based on programmatic concerns, more than in acquiring information that will really result in changes to your performance estimates.

Again, some of the other conclusions sort of collateral from the study. It appears that the performance of the site is quite good, based on the analyses that we've done. It is a fairly structured summary of them, so it is kind of a useful reference, if you will, to go in and see why we think that the performance is, as we said it was.

[Slide.]

DR. DOBSON: Some of the other conclusions were that it did appear, from our fairly short analyses, that the saturated zone would contribute significantly, and that the potential impacts from characterizations were going to be small, in any event. And that was modeled fairly explicitly in this study.

DR. NORTH: Yes. I think there's some very strong implications, particularly from the first bullet you had there on the saturated zone. I hope those lessons are getting integrated into the further planning.

DR. DOBSON: I guess I would say I agree with that absolutely, but I would look at the other end as well. Our analyses also indicated that the EBS would probably do a pretty good job of moving waste as well. And some people who work on that part of the program, would probably assert that you probably ought to be expending most of your resources studying the very near fuel environment, since that's where the initial things have to happen.

So, it is just -- I guess I would just say that you have to study all levels, and you have to balance things when you are prioritizing the program. You can't -- I don't think you can conclude, at this point, from what we know now, that we should study the EBS or the unsaturated zone or the saturated zone to the exclusion of others. We currently have, basically, studies in all three.

Again, to repeat something that I think many times in the Calico Hills analysis, it's the combination of those multiple barriers that gives you the most confidence that the site has the potential to perform well.

[Slide.]

DR. DOBSON: A few other conclusions from the Calico Hills. One is that you don't need to drift forever necessarily, to get scientific confidence, but it is a kind of a linear, well, maybe it's not linear, but progressive scale. The more work you do underground, the more exploration you do underground, the more confidence you are going to have.

But, the final bullet is kind of the key. And that was that we thought that the underground facility should be designed to be capable of drifting to any part of the repository block, because you don't know what you don't know. And you might need to get information, in some area that you can't predict at this time, and having that capability will really give you the ability, down the road, as you start -- as you conduct the site characterization program, to acquire the information that you feel you need.

[Slide.]

DR. DOBSON: I'm kind of running over. I apologize for that.

The ESF alternative study had five main criteria that you have seen presented by numerous people on numerous occasions, including several that incorporated performance assessment as an important part of the component.

In the overall analysis, the performance assessment-related criteria really didn't provide a clear means for discriminating between the options. And I will talk about that in just a second. The two critical variables in the ESF Alternative Study, turned out to be what we call management viability and the probability of regulatory approval.

[Slide.]

DR. DOBSON: You have seen this decision tree before, and I am not going to spend much time trying to explain it, but Lee Merkhoffer, has in detail. But performance assessment-related concerns are folded into both of the testing categories here. They had to first understand what the likelihood was that the site was going to pass or fail, if you will -- that it was going to meet the criteria; and secondly, the likelihood that the test could tell you something important about that performance.

[Slide.]

DR. DOBSON: But, when they looked at the results, after they rolled all of this stuff up -- and you'll note the viability of -- the programmatic viability and regulatory authorization are on the decision tree. When they rolled up all the results, they found that all of the different options were narrowly spread in the testing-related things. You don't see a lot of discriminating ability in here, in the probabilities that were derived for the testing programs -- things are all in the .89, .91 range.

If you look at the variation in the probabilities and programmatic viability, they go from about .3, I think, to .9 for options 24 and 30. So, the main discrimination that we got on the task force, was in those non-performance related criteria, and the main discrimination between the different options.

[Slide.]

DR. DOBSON: We mentioned earlier, the early site suitability evaluation. So, I'll just spend one minute showing you the structure of that analysis.

What we're doing now is -- in the early evaluation site suitability, is we're doing -- we're evaluating the site against the DOE siting guidelines very strictly tying things into 10 CFR Part 960. And that is this box that you see right here.

The elements of the evaluation, at the present time, include evaluating disqualifying and qualifying conditions and making lower level and higher level findings, as defined in 960, regarding the conditions, and then re-evaluating -- well, we will re-evaluate with updated information through time.

[Slide.]

DR. DOBSON: Now, within that box of evaluating the site, you will find some additional detail. Basically, what the additional detail shows is a block that says assess system behavior. And you do a qualitative, if you will, evaluation of the existence of disqualifiers, and assuming that you don't find disqualifiers, then you move into total system performance assessments, after which you can do evaluations of the qualifiers.

The reason for that sequence is that most of the qualifiers have in them a statement to the effect that the qualifier is -- applies to the extent that it affects performance.

So, then, the question would be: What level of total system performance assessment is that? Is that SPARTAN? Or is that Mike Wilson's total system analyzer, which he will describe, in some detail, this afternoon? Or is that a rolled up version of EQ3/6, with hydrologic codes built in and everything else.

I guess, I don't have the final answer for that. But I would argue that it's a little bit of each of those things, or maybe a lot of each of those things.

[Slide.]

DR. DOBSON: But, the final point that I want to make is that the site selection decision that we eventually have to make, after we come out of this box, with the evaluation against DOE siting guidelines, will also include other criteria that are broader than performance assessment-related ones strictly, and some of those are summarized here.

Again, we will have to be able to make the case that not only can we predict the performance, but that we can do it credibly, and that we can convince, not just ourselves, but regulators and the public, and, in our case, the President, who has to recommend the site.

There are a lot of other reasons for doing testing programs. So, I guess, the final summary of the lessons learned from the task force -- this is on the last viewgraph.

[Slide.]

DR. DOBSON: That is, that PA is a valuable tool that gets applied and must be applied at several levels of detail, and it must be applied consistently and over a long period of time. I think, to address what Warner said earlier, it needs to be applied, not only within that level of detail, but vertically, in the pyramid that Russ Dyer showed.

You have to understand when the data coming out of EQ3/6 are telling you something important about the results coming out of TRACER, and when the data coming out of TRACER are telling you something important about the results in TOSPAC, or the total system analyzer. But PA is not a panacea. It needs to be applied with some intelligence.

That's all I have.

DR. NORTH: If I can tie together some of the things you just said, putting back on the pyramid chart, I think one of the lessons that's come out of these Task Force efforts emphasizes the need to use the top of the pyramid as a way of communicating the insights that you have achieved, that you really do have a very robust assessment of performance with respect to that issue, that you don't have those long tails getting into the abandon reaches, and to use that as a way of communicating with interested parties and with your critics in thrashing out these issues, so that you get, hopefully, some degree of consensus on why the available science supports your position.

Getting a lot more detail down here may be much less important than being able to link all the way up through this pyramid and be able to communicate with people at this level, to convince them that what you've got is appropriate and robust.

So, as you go forward with this, I would urge you to think more in terms of outreach and communication in the age of responsibility, as opposed to we're doing interesting science down here.

DR. DOBSON: I don't disagree with that, but I guess I would want to emphasize, though, that you can't get some kind of concurrence up here unless these people down here are also part of the team.

I mean you're not going to get, at least, any kind of good technical consensus that your simple models are correct unless you can get some consensus that the subsystem models are consistent with the higher-level results.

DR. NORTH: I agree with you completely, and that's why I was raising the question to Russ Dyer, of getting an iteration of performance assessment, where you have all these linkages in place, from the top of this pyramid down into the bowels and the details, so that people can see it as a whole, linked system, where everybody is cooperating, the models are consistent, the data is there to drive the models, you have the sensitivity analysis in place, etcetera.

So, it is a whole entity, as opposed to a bunch of little pieces with a plan to put these pieces together sometime in the future.

I think the Task Force efforts have accomplished a great deal in actually trying this out, but I'm personally not comfortable with as much having been done as I think you will need to do link from this level down to that level.

You're essentially asking that people take a lot on faith, that the judgments of your experts is appropriate, as opposed to being able to link down here into the details of the models and the data and show that you have, indeed, consistency between expert judgement from the members of your team and all the detailed data and modeling that is available.

DR. DOBSON: I guess I don't really disagree with that.

I think, as Felton described earlier on, we had, after we had done the EA, in a sense, done one cut on the total system, assessments that using SPARTAN and Sinnock and Lin codes that we were using at that time, I think a significant need was identified to understand a lot of the subsystem processes, and a lot of the effort in the time, in the last couple of years since then, has been done at that level, and I don't think anybody here would disagree that it's appropriate to iterate back and forth.

I think the viewgraph that Russ showed earlier this morning from Dr. Bartlett is an appropriate one, that shows going back and forth between PA and site, but again, I want to emphasize that that happens at each of those levels. I mean it happens at the EQ 3/6 level, too.

The solubility of actinides -- it may be a generic parameter in a high-level model, but in detail, it turns out to be pretty sensitive when you run EQ 3/6, and it's obviously a data need that we have identified, but we haven't got a real good handle on it now.

DR. NORTH: Right.

What I'm worried about is a situation where this is being run relatively mechanically, and you picked out that actinide solubility from the book and you don't go and talk to an expert who suggests that perhaps the availability of organics that complex with those actinides is the real crucial issue, and what I'm hoping is that iterative use of performance assessment, you will flush out those critical issues by interaction between the experts, including those outside your community, so that you have them early, and you can get the information on those critical issues and be able to disseminate it so you accomplish the objectives on public perception and scientific confidence, not just having a good set of answers that come out of the exercise.

DR. DOBSON: Well, I couldn't agree more. I mean I think you're trying to do what you're requesting, and we're trying to iterate on PA and understand what the critical issues are, and if anybody knows of any critical issues that we've left out, I'm sure they'll let us know.

DR. NORTH: Don.

DR. DEERE: Yes. I would like to comment, in a related line, on your site-selection decisions which will incorporate additional factors.

You have listed there at the bottom, by the dots, quite a number, but I think if you go down to the fifth dot, the scientific confidence, whether it's the Calico Hills characteristics or whether it's volcanism, I think there has to be enough information, there has to be enough exploration done so those people that are carrying it out are able to get some degree of understanding and agreement, and then you have a much greater chance that you have a public perception that the scientists have studied it and this is their findings and there is reasonable agreement.

The NRC views will certainly be interested in the amount of scientific confidence that they have in the findings. The design information will depend on the scientific confidence, and the performance calculations fall right out of that.

So, I think that one of the primary reasons for the additional exploration, in all phases, whether it's groundwater travel time or the currents of faults we know nothing about, it's just a question of getting the information so that those workers have scientific confidence, at that level, and then it will grow out into confidence, I think, in some of the other factors.

DR. DOBSON: I don't disagree. If the DOE went to the President with a recommendation for the site and the President called up the NWTRB and the National Academy of Sciences and said what you do think about these guys and they said they're out to lunch, I think that would have a very profound impact on where we went to out of that box.

DR. DEERE: Yes.

DR. DOBSON: And until we can demonstrate some of these things, we're going to have a tough time doing it.

Sorry for the delay.

DR. NORTH: Okay. Dennis?

DR. PRICE: I just want to open up the issue on expert opinions again, which has already been raised. Does the report that we're going to receive identify the criteria for -- I'm going to run through a list of questions here --

DR. DOBSON: Okay.

DR. PRICE: -- identify the criteria for experts, the method of selection for experts, including how many do you need on a particular issue -- is one enough; the objectivity of the experts, including their independence; the methodology for obtaining opinions?

For example, it's referred to as sitting around a table, and if that's the case, if that's the way opinions are obtained, then the dominant person may get his opinion cranked into the model versus a submissive person not. So, what are the methodologies?

How do you determine the concordance and consensus of the experts, and then how do you maintain the integrity of the applications of these opinions? For example, you speak of performance assessment as being one which supports decisions.

So, if the decision which is to be made is not supported, do you go back to the softest part of it and then prevail upon those experts to reassess their opinion until you get the opinions lined up into an order that does it? How do you protect the integrity of the process?

There's a whole slew of those, which I don't think are entirely new, some of them, and I wonder what comment you have, and is this sort of stuff examined and portrayed in the document we'll be receiving?

DR. DOBSON: Certainly, it is at a certain level. I am not sure the level of detail you're referring to is described, for example, in the Test Prioritization Task Force report, at the level of detail that would allow you to have a complete understanding. You may, if you really want to get into it, there's -- there will be reams of back-up documentation from -- like meeting minutes, for example -- and I'm not sure, the TPT did not transcribe, did they? They produced very thick meeting summaries for every meeting that they had.

But you will be able to get a sense for how the process worked. Certainly, the qualification statements are in -- are in the -- at least in the documentation that goes along with the report. I don't remember now, specifically, whether each person's qualifications are summarized in the report. I don't think that they are in the summary report.

I guess, all I can say is that -- as a general comment, I don't think any experts ever were asked, and even if they were, would never change their opinion, on the basis of something that somebody else wanted. I think that we ask an awful lot of questions and we had an awful lot of long meetings, where we worked out way through some very difficult and sometimes controversial problems.

I think the methodologies that we employed -- well, let me put it this way, the guys that we brought in, in the case of the TPT, was a fellow named Bruce Judd, I think, did an excellent job, in terms of structuring the methodology and acquiring -- eliciting the information that he needed. I don't think you would find very many, if any, places where anybody's opinion was suppressed.

DR. PRICE: Would I be likely to find, in the documentation somewhere, Kendall's coefficient of concordance or something like that, to show that, statistically, you arrived at concordance?

DR. DOBSON: I'm afraid I'm not the person to tell you that. I know who you could ask though. Certainly, we don't -- you know, we were not -- if there's anything you would be interested in knowing about the methodology, I would get Bruce Judd out here and he can talk to you for that study, at great length.

DR. PRICE: The reason for this line, is it appears that the reliance and somewhat heavy reliance, which may be necessary, upon expert opinion, is also the Achilles Heel of the procedure.

DR. DOBSON: No argument.

DR. NORTH: Shall we go on?

DR. DYER: Our next speaker is Albin Brandstetter, with SAIC, to talk about the performance assessment integration with site characterization and design.

[Slide.]

DR. BRANDSTETTER: The previous speakers have covered, principally, work that has been done so far, about a general approach. I was asked to talk some more about integration of PA with site characterization and design. I am concentrating more on what's going on right now and what our long-range plans are.

[Slide.]

DR. BRANDSTETTER: So, the topics -- I was also given some subtopics to cover under that. And that includes are formal system for planning and control of our activities and some specifics of the integration aspects and what our activities are this fiscal year, and what is planned for next fiscal year.

[Slide.]

DR. BRANDSTETTER: Initially, talking about the project planning and control, the process -- the way it works is that DOE, OCRWM and Yucca Mountain Project Office defined what the top-level goals should be for the coming year and the following 10 years, in terms of objectives, budgets and schedules. On that basis, then, the contractors defined -- prepared work plans for the 10-year period, and then descriptions of specific activities they would perform in the following year, and that includes also budgets and schedules for these activities.

Then, based on their input to DOE, the iterations between the contractors and DOE, they come to a consensus on the activities, the budgets and the schedules that meet the overall top-level goals of the project.

[Slide.]

DR. BRANDSTETTER: So, the overall objective then of this approach is to prioritize the activities to accomplish the integration between the various activities including performance assessment, to eliminate schedule conflicts, which means to assure that the input that is needed by one activity -- that it has the output from another activity, in a timely fashion, and also meet the overall long-range schedules of the program, up to licensing.

Then, the objective is to monitor work that is in progress and to institute corrective actions, if any variances are observed.

[Slide.]

DR. BRANDSTETTER: We used a computerized information system to aid us in this process, which is called PACS, or Planning and Control System. And later on, I have some examples of some of the print-outs. It defines who is doing the work, in terms of contractor, principal investigators, overall managers of the activities, what the deliverables are, schedules and budgets. Then, for the current year, it also tracks the cost and tells us any variances, in terms of costs and schedules.

In the long range, we have now -- we have defined out plans for the next 10 years, up to the year 2001, and that is all in that system. The system is also built, however, that we can change it, based on changing priorities and also budget, as Congress allocates funds on a year-to-year basis.

Because of this PACS system, which defines all of our activities, plus the supporting work plans by the contractors, we do not think, which Russ Dyer has already mentioned, that we need to update the performance assessment implementation plan on an annual basis, because we are accomplishing this with the PACS.

[Slide.]

DR. BRANDSTETTER: I just have some examples here of -- I am missing one viewgraph. But I have some examples that are listed in your next -- in the handout that I will show you now.

The first one is an example of a summary account. I should mention, this is the level of information that DOE maintains at the project office. The contractors, themselves, then can go down to a lower level, and split each of these activities into smaller activities, using their own control system.

This, for example, is for Total System Performance Assessment. It's the category for specific activity, it is the total system analyzer, and we will hear about that later on. So, it describes the scope here, and in the real print-out, it will have also the budgets by month-by-month. Since this is only for '91, it will have the budget for fiscal year '91.

[Slide.]

DR. BRANDSTETTER: The second sheet is the same type of print-out, except that it lists, under scope of work, the activities for successive fiscal years, in a summary fashion. And as I explained, again, the contractors, in their own system, have additional details, describing these activities. So, we have fiscal '92, and it goes all the way up to '98, to the year 2001.

This print-out would show the planned budget for each of the fiscal years.

[Slide.]

DR. BRANDSTETTER: A number of these summary accounts are then rolled up into what's called a P&S or Planning and Scheduling Account. So, this consists of a number of activities all supporting Total System Performance Assessment, and describes the overall scope of this activity and then the specific activities are listed under that so that I know, as a manager, what specific activities, like model gas-phase release, or descriptive scenarios, as an example, that feed into this roll-up description of the additional activities, that are on the second page, which I am not showing.

[Slide.]

DR. BRANDSTETTER: In addition, this system includes the so-called long-range plan, and that's an example of the long-range plan for Total System Performance Assessment, which gives a schedule.

So, it lists -- like here, source term model for use in Total System Study, and it lists the duration of this activity, 1 October '90 to 17 December '90, and also shows a supply diagram, and the triangle shows the latest that this activity can be completed without having impact on other activities, and when the activities are actually performed, these boxes are shaded in to show progress.

[Slide.]

DR. BRANDSTETTER: One of the outputs of this system is a long-range -- is a diagram of the so-called Project Master Schedule, which shows the different activities for the entire project and the key milestones. Performance assessment, I have some additional detail.

So, the Project Master Schedule, at present, does not include any high-level milestones, other than the completion of research on model development and the completion of the total system PA for the license application, but at the next lower level, there are numerous milestones that define the details of these activities.

DR. NORTH: Could I ask, on either this chart or the preceding one, could you clarify when is it that we are going to get an integrated performance assessment view of Yucca Mountain?

When is the preliminary assessment of total performance going to be complete in a version we can all look at?

DR. BRANDSTETTER: Well, there is a demonstration of the Total System Performance Assessment that is being conducted this fiscal year, actually two parallel efforts: one at Sandia National Lab in support of the site suitability evaluations, and the early analyses, I think, initial analyses are scheduled to be completed in June, and the report itself for the public is expected to be available in January of next year.

DR. NORTH: So, we're going to have one in June with documentation to follow in January.

DR. BRANDSTETTER: That's right.

DR. NORTH: Does that appear on the chart here?

DR. BRANDSTETTER: Yes, that's correct. And then there is, in parallel, a Total System Performance Assessment, more in a demonstration mode, using Yucca Mountain data by Pacific Northwest Lab that is to be conducted and completed by the end of this fiscal year.

Now, these are not yet the integrated analyses that you are talking -- that you have been mentioning in terms of integrating all levels of models. So, these are more of a demonstration of the Total System Performance Assessment modeling with some at the lower level that are needed for the early site suitability evaluations.

DR. NORTH: I would like to see milestones on the chart as to -- we're going to get an integration every year, and every year, we're going to do more to tie together the levels of the pyramid and go out there and get it reviewed in public to deal with the scientific confidence and public perception issues.

DR. BRANDSTETTER: I don't have the kind of iteration in my presentation that you are mentioning.

It's maybe more implicit in the types of activities that performance assessment has to support in the next few years, and I have mentioned here -- well, two of those have already been mentioned, the PACE test prioritization, and we will perform iterative analysis in support of the site characterization, and I have some examples of what we are doing right now in terms of, for instance, evaluating the tests that are being planned, and the early site suitability has been mentioned, and then the site suitability evaluations will continue all the way until the recommendation of the site, a potential recommendation, to the President.

The ESF alternative evaluations have been mentioned. They have been completed, and we are continuing now in supporting the planning for ESF design, and I will go into some detail a little later of what that includes, and that also includes evaluating the tests that are planned in the ESF.

Another aspect that's part of this iterative performance assessment is support of the repository and waste package designs, which will be done in two phases; first for the advanced conceptual design and later for the license application design.

So, these are performance assessments which includes analysis of the waste form, the waste package. The repository layout is in the example as individual components, but then also what's the impact of the proposed designs on Total System Performance Assessment.

So, I would say that, probably, what you are talking about in terms of an integrated iterative performance assessment includes all levels of models, that the start of the support of the waste package and repository advanced conceptual design will be the real beginning of that, which would be a year from now.

The initial efforts that I listed here for starting early next fiscal year will be mostly in the sense of defining design requirements for the designers in terms of criteria; as an example, maximum temperatures in the rocks, recommendations relative to construction methods to minimize disturbance of the rocks, that type of information.

Then, of course, eventually, in the out-years, we'll do the total system and individual process analyses for the EIS, and I should mention the environmental assessment and then the license application.

[Slide.]

DR. BRANDSTETTER: The next viewgraph is essentially just a repeat of what I have in this diagram, just for your reference of what the timeframe is for the major components of these analyses, so I will not go into any detail.

[Slide.]

DR. BRANDSTETTER: I have selected an activity that is proceeding right now that demonstrates performance assessment's role relative to the design of the Exploratory Studies Facility, and this has already happened, the evaluation of alternative ESF designs, which have led to the selection of one design and also input to the ESF design requirements in terms of water used, for instance, disturbance of the rocks and so on that the designers and the constructors of the facility should observe.

These things are being -- will be updated based on new analysis in the next year.

Right now, we have just completed, again only on the basis of subjective expert opinion, our analysis of alternative portal and shaft locations.

So, the design engineers have proposed three different locations for the south portal of ramp, and I'm not sure if -- I don't have it in my handout, but we'll just quickly put that up, a viewgraph of the current concept for the Exploratory Shaft Facility.

[Slide.]

DR. BRANDSTETTER: The north ramp is pretty much fixed, and a document is being prepared to substantiate the decision for this location.

So, there is a ramp that will go in here, and the drift in the Topopah Springs unit, and then a second ramp from the south that will connect to it for the south.

However, the designers have proposed three different locations: one like that, another that comes in like this, and then the third one actually from the west side of Yucca Mountain, straight into this drift, and then there is another loop that goes down into the Calico Hills and back.

Anyway, performance assessment has contributed to the evaluation of these alternative portal locations and also to five potential locations for the exploratory shaft if the decision was made that one should be built.

This information has been given to Raytheon, the design contractor, and it's being aggregated with information on the importance of testing itself on construction and design aspects and costs and schedules to come up with a final recommendation, which hasn't been made yet, of which shaft -- south shaft location -- which south portal location and which shaft location to select.

We are just now starting the evaluations of test planning packages. The contractors, with Los Alamos, in the lead, are describing the specific tests to be performed in the exploratory shaft, and the USGS and the Bureau of Reclamation -- what surface-based measurements to be performed.

And performance assessment will evaluate these -- these test descriptions to look at test interference and impacts on performance of the repository, and also to assure that the data that are needed will actually be produced by these tests.

This may in fact include some analysis of the tests before they are actually performed, to assure that they are planned properly, that we do get the data we need and also that the disturbance to the repository is acceptable.

We arrive now -- just reviewing the conceptual design that is in the ESF Title I Report for Raytheon -- and the real analysis, however, will start next fiscal year, in October of '92. The Title II design will be resumed, and the plan is that performance assessment will analyze the Title II design to evaluate, again, aspects of testing that is, will the tests be productive, and what are the potential impacts on repository safety. And this will include a combination of individual process modeling and Total System Performance Assessment.

The design, itself, will be conducted over a two-year period, in stages.

[Slide.]

DR. BRANDSTETTER: In this diagram, for instance, you see the numbers from one to 10. That means, these pieces of the facility will be built in phases and one part will be constructed while the next one is still in the design stage, so that they can use the information they gained from the construction in the design of the next part of the facility on the same -- that the data that are collected, in each phase of construction, can be used in each successive phase of performance assessment. So that, as we proceed in this two-year period, we will gradually be able to update our performance assessment with better data and also, I hope, better conceptual models.

And then in our 1985 -- fiscal '85, after the completion of the ESF design, we will actually do a Total System Performance Assessment of the repository, as constructed, of the exploratory shaft facility, as constructed, to evaluate its potential impact on repository safety. I don't have this as a viewgraph, but it just shows some of the mechanics of our planning.

I was going to put this on you wall, but I couldn't bring myself to put pins into your new wall. This is, for instance, a logic diagram that shows all the ESF design and test planning packages for ESF, in one diagram. And each box like this is a particular activity. For instance, this is the revision of the site characterization program baseline. This box is the performance assessment, and then there's little boxes to show how each activity interrelates to the others. It also includes permitting environmental monitoring and surveys. So, everything that's needed, up to the initiation of construction of the ESF in November of '92. They are separate.

Now, this does not give any schedules. We have prepared also, a diagram that show the actual schedules of the activities for the entire ESF activity and then or each sub activity as well. I just brought a little piece, which is still in the development stage, it is not completed, which lists the performance assessment activities, in support of the ESF. This is actually for the next three years. And what's missing yet is all the tie-ins to the design and other activities. But, it does give you a duration of each activity, as well. How it ties into, other than performance assessment activities.

I was going to give the example, that I mention in your next handout. I don't think I'll need to go into this anymore, because Dave Dobson has provided you with quite a bit of detail on these activities that happened in the past, and we are essentially continuing those in the present.

[Slide.]

DR. BRANDSTETTER: I just show this to -- as a hope that, with PA's involvement in the test planning, that this sort of activity may not happen in the future.

[Slide.]

DR. BRANDSTETTER: Just to wrap it up, then, what are we specifically doing this year, in terms of all performance assessment activity? I have already mentioned that we're involved in the preliminary Total System Performance Assessment which PNL is conducting, and which is also being done by Sandia, in support of the early site-suitability evaluations.

The PACE analysis documentation is being completed and you have received the draft report. I've mentioned this -- this has been mentioned. This has been completed now.

PNL, when they still reported to DOE Headquarters, was asked to do a performance evaluation of the borosilicate glass waste form. The evaluation has been completed, and the documentation is being processed right now.

I mentioned that we are continuing the ESF design and test planning work. And what hasn't been mentioned, is that a repository design alternative scoping studies are continuing at Sandia. These will look at the thermal loading of the repository and the effect of different geometries of the repository and waste packaging placement schemes, to determine what the thermal regime of the repository would be. That would be as an example of the type of analysis that's being performed to define the design requirements later on for the waste package and repository design.

DR. DEERE: These are going on at the moment and will continue?

DR. BRANDSTETTER: These are in progress right now. They're also under an evaluation of various seal concepts. And, again, these are ongoing right now at Sandia, and next year, when we start with the definition of design requirements for the waste package and the repository advance conceptual design, this activity will provide input to that.

Then this, of course, QA program and implementation is a continuing process.

One activity that was new this year, which Russ Dyer has mentioned, is the evaluation of waste from transmutation and permutation, to assess potential aspects on repository safety.

[Slide.]

DR. BRANDSTETTER: These were activities, in terms, really, of analyses and evaluations, and are listed separately. The more research and development-oriented activities, documentation of models and codes is continuing, and that includes: Groundwater flow, radionuclide transport waste package -- waste form of models, waste container models, corrosion models, and then the Total System Performance Assessment models, and geochemical models, should be mentioned.

A report that Russ mentioned, on the model validation approach, which is more of a strategy, in terms of a paper trail that you need, and the general aspects of validation, is being completed. It does not go yet into specifics of validating specific process models, for instance.

A plan is being written by Sandia on the field validation of flow and transport models, which is also based on some experimental work. I should mention that, as part of the validation task that we have in performance assessment, includes also lab experiments at Sandia involving flow and transport in fractured and porous rocks, to improve our understanding of the basic flow and transport principles.

We will continue the model development, which goes in parallel with the documentation. I mentioned the experiments -- the international activities of model validation for flow and transport and an intercomparison of benchmarking of total system PA codes is continuing. There is also support to natural analogue studies in Canada and Australia, and the QA again.

A new effort, this year, in development is a near-field waste package model that takes a better account of the near-field environment than the current models.

DR. DEERE: Who is doing that letter work; the near-field waste package work?

DR. BRANDSTETTER: That would be Lawrence Livermore.

[Slide.]

DR. BRANDSTETTER: In 1992, we're more or less continuing this year's activities. So, the site suitability evaluations will continue. The evaluation of test planning packages, since the construction of the ESF will be over a two-year period, so there's time, as that proceeds, to re-evaluate the test descriptions, as we actually -- get actual field information.

I will continue, in an iterative fashion, to revisit the Total System Performance Assessment that we're doing this year. QA will continue.

Because of the expected budget cuts, some of the activities that are listed here, which I've listed before, I expect to be reduced, in an effort so that the primary emphasis or the priority of our work next year will really be the analysis and support of decisions, rather than the long-range R&D.

As already mentioned, the analysis in support of the Title II design will start and input to the design requirements for waste package and repository design.

[Slide.]

DR. BRANDSTETTER: In summary, then, I can say that we have plans in place for performance assessment activities, up to the year 2001 and, of course, because of changing priorities and also unexpected budgets, they have to be revised on a continuing basis. We have integrated these activities with other project activities. And, as I mentioned, that these activities include iterative analysis and support of all these major projects -- project priorities.

Any questions?

DR. NORTH: Thank you. Let's move on.

DR. BRANDSTETTER: Thank you.

DR. NORTH: Thank you.

DR. DYER: Our next speaker is Les Jardine from Lawrence Livermore National Laboratories, who will talk about preclosure performance assessment activities.

[Slide.]

DR. JARDINE: This talk is going to be about preclosure performance assessment, and I don't believe the Board has been introduced to that topic before. But, for this talk, basically, it involves all those operations that go on during the lifetime that the repository is operating, including the shut-down and decommissioning-type operations. I mean, that's what the term is.

And another thing, in the back of your mind, is safety analysis, if you like, but were used in the past performance assessment -- meaning preclosure, to cover what that is.

[Slide.]

DR. JARDINE: Now, what I want to do is go through these five different topics, in the point of the history. What I want to -- the message there -- there's a lot of work that's been done, and numerical results exist, and I'm going to give you those references.

Another thing, is it is extremely difficult to get any radioactive release into the environment and offsite to the public.

The general methodology I'm going to minimize, because I don't want to spend my limited time on the results of applying the methodology. So, bullet three then, I will be showing examples of design weaknesses that were identified as a result of applying a performance assessment methodology to a conceptual design. This is very powerful to apply it to conceptual design. It was some of the first times it had been done, at least to our knowledge; and that the offsite doses that result from accident scenarios, which I'm going to focus on -- accidents, during the preclosure period, are likely to be well below a half rem offsite to the public.

The last -- the third -- the fourth bullet here, about the other applications; I want to give a little bit of information about some more detailed studies that we did, as follow-ons, because I can't do everything. But these resulted with closing the loop and completing a performance assessment within one year, and gave us a lot of insights, an the thing evolved, and we did follow-on studies, including sensitivity studies. There is a real illustration there.

[Slide.]

DR. JARDINE: So, let me move into the first part of this, which is the history. The history really means -- what I want to say is that a lot of work has been done, over the years, going back to the late '70s and all the repository projects, and these have been documented in the reports I'm going to show you, four viewgraphs from now, give you the project documents, which contain the references to those.

[Slide.]

DR. JARDINE: The bottom line is that a large range of external events that have been looked at and consequences estimated, as well as internal events, for all kinds of various repository designs -- the bottom line is that all categories of events that might happen that could lead to some kind of a significant offsite dose, have been identified.

[Slide.]

DR. JARDINE: This is a message that I think very many people don't realize, and I want to convert you from the reactor perception to a repository perception. And that is that the offsite dose consequences that can come out of a repository, are orders of magnitude less than reactors or reprocessing plants, which is, of course, basically, where people derive their nuclear facility experience.

There is less decay heat, there is much less radionuclides involved, as compared to reactors, if that's your frame of reference. But most importantly, there is just no energy available to cause a release of radionuclides to the environment, as there is. What you find is there has to be some kind of a mechanical happening in order to generate the radioactive release during the operating period.

[Slide.]

DR. JARDINE: Now, to get us there, this is the thing that we can use to convert from reactors to repositories. And a reactor, if you plot the fraction of thermal energy in the core, as a function of time after shut-down, you turn the thing off, you've got six percent of the thermal energy is in the core. If it is a thousand megawatt plant, it has over 3,000 megawatts. Six percent of it is still here. Up to one day after shutdown, it just goes down to a factor of 10. A tremendous amount of intrinsic energy, and, of course, that's why you do a lot of things to maintain the cooling systems so that you don't get this potential for an off-site release.

[Slide.]

DR. JARDINE: Now, this is the same plot -- but what I have done is lay on a point in time for reprocessing plants, which could handle fuel that's aged one year. In other words, the same curve time, after shutdown, from a fuel assembly -- out at one year, you're down several orders of magnitude in a reprocessing plant. When you get to a repository, where you're handling fuel that's five or more years old, you're down in this range, and you're down three orders of magnitude.

The real message here is there just isn't -- there's a whole different ball game, in terms of identifying source terms, than what has been in the past, in other nuclear facilities.

[Slide.]

DR. JARDINE: Now, the history here -- what I -- this viewgraph is -- I have to spend a couple of minutes on it. But the key is, I have given you reports that have been published in the project that deal with preclosure performance assessment, focusing on accidents. A lot of work has been done on normal operations, but those are not included, and I am not going to cover them.

There is a history behind this. The very first assessment was done back in the 1984 timeframe -- 1985, within a year, and it came to closure, and it gave a lot of insight, which led to a need to do a lot of other things. Like the first assessment, in order to come to closure, was pretty superficial on the underground facility, so a second analysis was done that focused on the underground facility in the same level of detail.

Then later and more recently, we got into the mode that the ESF had to be brought in as part of how it might fail, if it was converted into a repository. So, there was a progression, over a period of five years, where different kinds of total system preclosure assessments were made on the repository.

There was also identified, in needs to have these ancillary type support things done. One of the first ones, really, back in 1986, was the Q list. The fact that the first performance assessment had been done, provided a basis to develop the Q list for the project. There was a need later on, to update that Q list, based on this ESF conversion, and then that was done.

The very first assessment, in order to bring it to closure, could not focus on the seismic issue or all of the external events. It was recognized and, on the next page, I list all of the seismic things that were taken on in detail, and led to a progression of studies, that led to the conclusion that seismic events were unlikely to be serious accident scenarios in the preclosure period of the repository.

Because we did these studies, we identified weaknesses in data or needs and scenarios that were left out. These reports, on this line, represent topics that were merited additional follow-on studies. Aircraft scenarios, as an external event, which was looked at in two different reports. Criticality was not looked at in the official -- or first version -- that has been looked at in several times.

Nine, here, I am going to use one viewgraph, at the end, to show you that we also got into the question of how certain or uncertain are the doses we're estimating. This was a specific report that was done to take a look at what are the errors that we have and what is the confidence on our doses that we are calculating. There were a couple of other things here that dealt with.

Maybe a message to make is that the conceptual design was put on paper in 1984, and was available at the start of 1985. That formed the framework for the analyses that were done of the preclosure safety -- or performance assessments of the repository; the exception maybe being the ESF conversion, at the later end.

[Slide.]

DR. JARDINE: I think I'm not going to say anything about this, other than that this is done to just give you the references of reports that went and looked in more detail at the issue of seismic events for preclosure period.

And this is a listing of reports only, again. And the seismic cost-benefit study really was the one that pulled a lot of that together.

[Slide.]

DR. JARDINE: So let me move on to try to minimize some comments on the general methodology, so that I can get into how we applied it.

And contrary to what a lot of people think, it's a four-step process, the way I want to use it, in the sense I want to define four terms, in the design process and its relationship to performance assessment.

You construct the design bases from a lot of outside information and sources in parallel with the development of a facility design, and you produce drawings, specifications, and other kinds of reports that support hat design.

That constitutes the facility design, be it repository or waste package. That is available for modeling and performance assessments, as I use that analogy. And you're going to see how we did that, or applied that.

As you do the performance assessments, the things that are on paper or written down, you then can provide recommendations back to the design process as to where you might change things and where there are weaknesses. And I'll show you some weaknesses in that 1984 conceptual design, which are referred to as the SCP CDR.

[Slide.]

DR. JARDINE: This is another busy way to say there's four steps. Design bases and facility design are constructed in parallel, produce a series of drawings that are conceptual design, and go on in different design phases.

Once they are on paper and documented, a separate, independent group came along and did performance assessments of those things and provided recommendations of how that would perform gives you this feedback ideally to the facility designed to modify it or modify the design, or in the case of upfront studies, which are conceptual, and where the requirements up here are evolving, there may be a need to go back and change the basis of design. But the preferred option is to make recommendations back to the facility design.

[Slide.]

DR. JARDINE: Let me focus a minute on this performance assessment. And the way it was used in these reports follows what is used as probabilistic risk assessment techniques, Level 1, 2, 3. That was the thinking that was used, and we were really modifying PRA techniques to apply to a conceptual design.

So performance assessments consist of what I call system modeling, or you heard Russ say system description, which we then developed the radioactive releases that could happen, went to the third level of going through and calculating dose consequences, and probabilities were also calculated along in parallel with these, so that we had an ability to judge how the accidents would perform against regulatory numbers.

[Slide.]

DR. JARDINE: The methodology has six steps. I heard that somewhere. But that's coincidence.

And really, the first step is defining repository compartments. It's part of the system of modeling. You break it up into something that you can focus your analyst's attention to.

Go on and identify within each of those compartments what are initiating event, internal and external type events, from which, when you do this, you have an ability to represent it.

And we chose event trees as a way to do that, and we even did some fault trees for Dennis. And I'll show you that. Or I won't show you that, but I'll tell you that we did 16 of them.

But once you have the event trees, then you are in a mode where you do need to put on the consequence. Again, we're focused on offsite dose consequences, and the probabilities associated with the scenario that comes out of the event trees. And that leads you to what I'm calling quantified event trees.

So I want to save that one, and talk a minute about, the first step was develop repository compartments.

[Slide.]

DR. JARDINE: I am not going to spend any time on that. You've all seen it. You may not have seen it in that way.

[Laughter.]

DR. JARDINE: This is the view from -- well, no comment. But there is underground and surface. And we are going to focus our talk today on the surface facilities. And you have to break down the big system into subsystems or compartments that you can get your analyst focused in.

[Slide.]

DR. JARDINE: This is in your handout, but I got mine that is colored. You begin looking at the surface facilities, and this zone here is where there is no radioactive materials. You can begin to not emphasize attention there.

The red boundary here in the waste receiving inspection area is boundary which radioactive materials are handled. And the waste handling building number two is the major big building with a lot of radioactive inventory. So what you're doing is beginning to zero in and break this up in a systematic way.

[Slide.]

DR. JARDINE: When you look at the waste handling building number two, this is basically a planned view of the building, and you can draw -- well, what I wanted, there's seven different compartments or areas or zones where identified in the building. And a boundary here in the blue represents the cask receiving area.

We also recognize that the radioactive source terms have different degrees of ruggedness associated with them.

The inventory out here is inside of a massive shipping cask, and when you get it into a hot cell, which is represented by here the unloading hot cell or the fuel consolidation hot cells, there are bare fuel assemblies which are handling. So you don't have quite as much.

What I'm trying to say is, here, if you have accident scenarios, you have bare fuel assemblies, accident scenarios out here can involve, they're inside of a large, massive shipping cask.

When we get them outside of the consolidation cells into the packaging cells or the storage vault, they're inside of a sealed container. And the sealed container has the fuel in it, consolidated or unconsolidated. And this is a barrier.

So there is an implicit degree of protection that you are building in in terms of making it harder to get the radioactive material dispersed. And this is explicitly taken into account.

So in this case, I'm showing you seven different compartments within the waste handling building.

[Slide.]

DR. JARDINE: When you go through this process of laying it out, we screened internal and external events, but sticking with the viewgraph, we kept out of the screening process 12 event trees and then developed those into 58 different pathways or scenarios. There are also 16 fault trees developed for those.

In the external events, we screened out 45, but only kept the seismic event, because our mission was to complete this in nine months. And we did do that. For which, then, that seismic event was developed into nine events trees or 91 scenarios, and you add that up and you have like 149 different accident scenarios.

And what we were trying to do is pick, cover all of the repository and pick bounding-type scenarios, and initiating events within each compartment, so that we had a confidence that we were looking at what could happen in different parts of the facility.

[Slide.]

DR. JARDINE: Now these event trees or compartments, as we said, this is once it gets in the building.

This first one is the access area, that when the train is coming onsite across 40-mile wash, there's a bridge. You can have a scenario involving that kind of a situation.

But these are the ones that, as you get into the building, and into the unloading hot cell, for instance, the crane can drop fuel assemblies, a glass canister, or you can have some kind of a failure of the equipment that's in there.

[Slide.]

DR. JARDINE: Again, I think that, basically, what we picked in these nine compartments was a total of about 13 representative initiating events, in order to cover the spectrum, and I'm going to show you some pictures and show you what it is.

I've already mentioned that, in the underground emplacement area, we had 13 different initiating events, and we only picked one to quantify in this first round, and later, I'll show you how we came along and had 129 scenarios in our second pass at this, two years later.

[Slide.]

DR. JARDINE: I guess I had better put this up to remind myself where we're at, in a sense.

What we're doing is going through, identifying initiating events. We want to develop event trees and deal with consequences, which are the doses, and the way I want to do that is talk about the source terms and remind yourself that what we're dealing with is spent fuel in glass waste forms.

The kinds of releases from spent fuel can be gases, volatiles, or particulates when you breach the cladding, or more important, really, is when you break the brittle ceramic fuel and pulverize it.

This forms an airborne source term, which can get into the air, go through the confinement systems, and offsite to the public, and the same is true for the glass waste forms. You do not have gases or volatiles, having run it through the melters at Savannah River, to worry about in terms of source terms.

[Slide.]

DR. JARDINE: The specific isotopes that you deal with in the source terms in these three categories are listed here. You do have some of the shorter-lived ones, because we're talking about the operating period, particularly tritium.

It's not a problem for offsite doses, but I will clue you in: It is a problem -- not a problem -- it is a concern for the operating period of worker exposures, perhaps, but not offsite to the public. It just gets diluted too much, or there isn't much of it, is a better way to say it.

[Slide.]

DR. JARDINE: Now, when you look at the doses that we calculated, and it was basically due to having a radioactive cloud, if you like, of brittle fractured particulates of that, and the doses that were resulted in the offsite, this is a way to show what is the organ at risk, and what they are are bone surface doses.

The scenario is that, three miles away, there is a public -- a maximally-exposed individual who inhales during the duration of the accident. What happens? He doesn't get a lung dose or thyroid doses.

Really, the organ at risk is the bone, and the bone surface dose, and this is just the relative contribution of what they are in terms of contributing to the doses, which I will show you later.

[Slide.]

DR. JARDINE: When you look at what's contributing in terms of isotopes to the doses, this is just the stacking up relative to the 100, what are the specific isotopes, and the plutonium dominates it, there is a minor contribution from the strontium-90, which contributes to the bone dose, or bone surface dose -- let me get that right.

So, there is a source term. It's due to the fact that you have particulates that can get generated.

[Slide.]

DR. JARDINE: Now, the way that we chose to present this information was event trees, and this is the style that was used for an initiating event of a crane dropping a fuel assembly in the unloading hot cell compartment.

You can get into situations where, if it is dropped with this probability, the cladding breaks or ruptures, yes or no.

There is a simultaneous loss of the ventilation system and the filtration system, and you can get into these conditional probabilities, which you can add up, but more important, you can tabulate the doses due to these three contributions, or the total dose, and you can build up scenarios like that.

I think, rather than belabor these, we have them, but the pictures are what I want to get to.

[Slide.]

DR. JARDINE: This is just another one. In the storage vault, the container transfer machine drops the fuel assembly, and with some initiating event probability in another compartment, you can calculate a probability and a dose, and you can do these.

[Slide.]

DR. JARDINE: When you do that -- we had like 149 scenarios -- you can summarize them on a plot like this, where this is the probability, versus dose, and you scatter plot these things, and what you're interested in is these dots, which have probabilities greater than 10 to the minus 9, these being very small and not likely, that have to be considered in your analysis, and what you're, of course, interested in is doses that are higher than the others, and so, we have drawn a couple of lines in here.

This is the one that's a half-rem, or 500 millirem, and there's a couple of points that are sticking out there, and then what are these? What I want to show you is -- let's look at the specific design and see what led to those points.

[Slide.]

DR. JARDINE: This is a way to plot, in the compartments in the waste-handling building, the 13 different scenarios, with their probabilities and their doses, and here's the two points that were sticking out at 1,000 millirem involving the drop of an unsealed container or equipment that was falling on an unsealed container or a seismic event that was causing something to happen.

[Slide.]

DR. JARDINE: What I want to do is -- let's look at that part of the building and see what the weakness in the design was that the designers were not aware of.

This is at the end of the fuel-consolidation cell, and the way the design was, the consolidated fuel is loaded in this horizontally. Six PWR consolidated assemblies are passed into this container through this wall. The crane lifted this from horizontal to vertical to do the welding. That was the way the design was evolved.

So, the scenario -- but there was no welder and no closure of the can. So, the scenario involved the crane dropping an unsealed container with six consolidated rods in it, six times the inventory of one, and it was not sealed, because they wanted to move it to the weld station to do the closure.

When you do that, and you go through the source term, this gave us the big 1,000 millirems relative to other things, and of course, the focus here is, right away, to the designer, and of course, they get very irritated, that's why -- the rule that we said was it had to be written down, because they had many ways to defend why this was not a problem.

But the PA people provided this as insight, because a designer has several ways to fix, including -- you know, he can just use mechanical rotations. You don't have to lift it. We can do the welding in there.

But you're beginning to focus in on what I'm calling a weakness in the design, because you've brought it to closure, and you're focusing it in systematically as to what might lead to releases. Given that, I'm not going to spend time.

[Slide.]

DR. JARDINE: Let's go to the next one that is sticking out, having covered this one, and then say, if this is the next one that's sticking out at 300 millirems and the cask receiving and preparation area involves scenarios of a cask drop.

What is it in the design that was on paper in the conceptual design that led to these kinds of doses that were larger than other ones? And the probability is still in the range that you needed to consider them in the design.

[Slide.]

DR. JARDINE: Now, when you look at the cask receiving area, which has provisions for truck and rail shipments, there is a massive overhead crane that lifts the casks off of the vehicles and lowered them down to a transfer cart down an elevation change of 25 feet.

Once they were on this cart, this is where the unbolting of the lids would take place, and they would be shuttled, if you like, on these transfer carts under the hot cell, where the unloading hot cell was, and then, one assembly at a time was lifted, and the cask was unloaded.

Now, the scenarios were that you suddenly find yourself in a position where drops of a sealed cask exceed this magic number of 30 feet that is used in a lot of people's arguments as to how they can survive transportation accidents, and the facility did not have a lot of confinement capabilities in this area.

It basically looked like a WIPP, in a sense, in the sense it was not a -- it was a sheet-metal-type fabricated structure. So, what this would lead to is potential releases for which the ventilation system wasn't designed to capture, and this is why we had big releases offsite, but you get there by the analyst saying what is it?

Really, the key was that a decision was made by the structural and mechanical people, who probably didn't talk enough to each other, to set this as they're going to do it this way, and there was a major tradeoff, because you can make design recommendations -- I don't want a 25-foot elevation change.

Instead, I can either sink the receiving area here down to the same grade level as that and eliminate a 25-foot elevation drop. Now, you've got two ways to do that. You can sink the whole building, which raises flood scenarios as a potential issue, and we looked at sheet flow and PMFs across the site.

You know, you get flooding, and now, what does this do to your criticality and your arguments for hot cell design?

If you raise the constant elevation up to grade level, then you get into what I think the structural people -- why those this was that now you're raising the center of gravity higher, and it responds differently during seismic events and not as desireable as having a lower center of gravity.

So, there are major tradeoffs that need to be looked at, and I might make the point that, you know, this was treated without a specific design requirement or a design basis statement.

The MRS facility did make this a request or a requirement, that you should have the design so it's all at the same elevation. So, when you look at their facility designs, you will find that there is not this pit concept, if you like, in there.

But again, how do we get here? I was trying to show you, by going through, calculating doses and probabilities, we had a way to rank things and see what were the highest outliers, and I think we've found some things that are significant enough issues to be passed back to the designers and say think some more about this, is this something you want to change, and keep track of it.

[Slide.]

DR. JARDINE: This is an application where the PA, the performance assessment, really gives information back to those responsible for the design, and they are the ones that have to look at a lot of other factors, like schedules, and past work that's been done before immediate decisions can be made. That's a great idea. There are a lot of factors, and the PA people, in this case, can provide back these recommendations, come up with a different alternative to rotating that unsealed container in the packaging hot cell.

There are a lot of ways to do it. Develop this passive design alternative to get rid of that 25-foot cask drop. But if you are going to do that, pay attention to the seismic and the flood effects trade-offs, as you do that.

Those are at least two of the things that were recommended back in that 1985 report, as specific examples of applying a PA to it.

[Slide.]

DR. JARDINE: What I wanted to do here, so we can all go to lunch, is talk briefly about two follow-on studies of more detailed applications. Because what I have tried to do is walk you through a general methodology, show you how we applied it to a conceptual design that was done in 1984, which is the SCP CDR, and we have -- we actually applied it. It gave us a lot of insight, and a lot of other follow-on studies were done and gave you -- on the history -- what those were, accident scenarios, specific modeling of fuel assembly drops, and a lot of other things.

But I want to pick and comment about two other ones; specifically, the first report, or another report which dealt with let's take a specific more detailed look at the underground repository, in the same way that I just showed you for the surface facilities. Because there we only had one compartment representing it.

[Slide.]

DR. JARDINE: In the interest of time, I've given you the references; but -- and I've got my cheat sheet at the top there that there were basically 14 different compartments looked at in the second phase of the ESF. I will put it down. I don't have to tease everybody.

But there were, basically, in this study, what we -- there were -- 14 additional compartments were looked at in the underground: the grouping of the ramps, the shafts, the performance confirmation areas, leading to 21 event trees, 129 new scenarios, or different scenarios, when you build up the event trees, leading, in 66 of the 129 had potential radioactive releases associated with them. These references have those numbers in them, Dennis.

But, when you looked at it, only 33 of the 66 had doses greater than 50 millirem offsite, and only six of them greater that 100 millirem, with probabilities that were reasonable. I have listed those six here.

What this shows you -- it confirmed the initial results that the underground is not likely to lead to a significant offsite release more so than the surface facilities, and they're comparable -- these are the same kinds of numbers, but it was a more systematic type look. In essence, the kind of scenario that was driving it, tended, at least, the one that looked better to me, was the ones that dealt with some kind of a container falling or failing during the emplacement operation, during performance confirmation when, once a month, you're retrieving packages and taking them to the surface. There were some -- you can't get around people wanting to have transporter collision accidents.

[Slide.]

DR. JARDINE: So, a second question became, well how accurate or how conservative or unconservative were you in your -- in these different analyses? This lead to what we called an uncertainty analysis, and I gave you the report reference, and I yellowed it, as one of the three that I was going to talk about. And this was reported, actually, in the meeting in Las Vegas last month, in a paper, but there was also a full report on it.

Basically, what we did there was look -- went through and identified, for 13 different parameters that were associated with calculating doses, assign distributions to them: Normal, log normal, histogram, and ran a Latin hypercube sampling method to do this, and ran simulations, with up to 500 runs, in order to develop a statistical basis to calculate or estimate what the dose was. This was based on the Sandia code that was developed under the NRC contract.

What we were trying to do, or what -- not trying, but what we did was actually come up with a cumulative probability of the doses that we've calculated, in order to be able to answer the question. Your first result, in the packaging hot cell was 1,100 millirems. We don't think you're conservative or you're not.

What that second followed on detailed study showed was that you cannot lay on whatever your favorite number was, the 50 percentile cumulative number was 320 millirem, or the 90 percent cumulative probability of this occurring, limited to the 13 parameters that we assigned distributions to.

Some of those parameters dealt with the choices between BWR fuel/PWR fuel. You know, that makes a difference. What's the height that it could drop from the crane, based on the drawings? You can develop some kind of a distribution, and then the simulation would go through and calculate that.

We also looked at things like wind speed, and there were -- but what we did was pick 13 different parameters and built this up. And then, what we had was an ability to put on an estimate of the uncertainty, or how accurate was this number. And I conclude from that that our first results were within, indeed, are conservative. Let me put this up.

[Slide.]

DR. JARDINE: What it allows you to then do is go back to all of these event trees that have been built up, and there is over 250 of them, and assign some kind of a confidence number to those results. We have done that. And then, what you -- so, we're in a position to get into the conclusions, which are really saying that these results are conservative.

[Slide.]

DR. JARDINE: So, what I want to really end up here with saying are several things. Based on all of these preclosure performance assessments and the follow-on things, which I could not cover here, but for accidents only, and that's what we're talking about, is accidents today, that it's very unlikely that you're ever going to see 500 millirem -- some kind of an accident scenario, during a preclosure period, leading to an offsite dose of 500 millirem. That happens to be the number that's used in the definition to define important to safety. I won't say that -- then -- that's opinion.

Another second point is that probablistic risk assessment techniques can be used in a conceptual design process. I feel like what was done was really to tailor back things that were going on in the reactor business and elsewhere, is to bring it into the Yucca Mountain, to a conceptual design and apply those kinds of analysis techniques, and I've skipped over most of them -- how we build up the probability and I'll probably get a question on that.

But, really, it can be done, and we did do it, and we got a lot of insight, in order to select design alternatives that would help you. It is very important to do it -- builds up now. If you can make -- identify these things now, what you're doing is minimizing later cost and schedule impacts.

The way I say this is that it's much easier to erase these kinds of things, on paper, than it is to start chiseling out concrete. If you've got the -- actually to the construction stage, or even more important, I think, if you go to a Title II, I won't use that word -- but the next detailed phase of design, ACD, and this doesn't get caught, what you've done is invested a lot of man hours and all the other associated engineering disciplines, and you have got a tremendous loss of resources, because you haven't identified it.

There were other recommendations that were there, and I just can't go into them now. But, there's a real powerful thing, and it was done as part of the preclosure performance assessments by Sandia, as you saw Russ Dyer line out, who had that responsibility.

[Slide.]

DR. JARDINE: I put this up to remind myself to comment that we had a pretty good challenge to figure out how to simplify things that were being used in PRAs, officially, to apply it to the conceptual design process. We get into the -- we didn't want to get into these traps, which eat up resources. You know, in your failure modes or your cut sets and fault trees -- and we got into these arguments of how much detail do you need, and we had to back down.

What we think, or what I wanted to say, is it's going to take a little bit of thought process as to what's the appropriate level of detail to bring in of these fancy techniques that are used in as-built plants, or more detailed designs. Because, remember, a conceptual design, or even the ACD will not have a lot of design details, and if it's not in a drawing, you can't go off chasing fault trees.

We did develop 16 of them and they were kind of vague. But it encompassed what the choices were, so to speak. And those were done by the analysts. You have to make decisions as to how you're going to proceed as the design evolves. That is a very important thing that still needs some thought.

The other thing that we'd always maintained was that the design has to -- you can't take a design review; in other words, you do not have to wait till the completion of a design point, like, let's say, the completion -- let me stick to the conceptual design, so I don't get in trouble. You do not have to wait until the end of the conceptual design to do this analysis.

There is a point which is called usually a design review. It may be the 30 percent completion point, where there is available engineering, drawings and sketches, which are a record, which can be passed to those doing the analyses, and they can do them. But the rule is --

my rule is, and I think a good rule, is that it ought to be on paper. That's what gets analyzed. If you do this at the 30 percent review point, you have an opportunity to get back an make a change before that's brought to the hundred percent completion point in the next design phase.

So, I am a large subscriber to the iterative nature of performance assessments. It has been done in the preclosure assessments, and was a key part of the repository design process.

We also learned that by applying these performance assessments in the preclosure, besides finding these alternatives and modifications, there is another class of people out there that fall into the deterministic mindset or, I labeled it, design-basis accidents. The insight you get from running these things give you a better basis to come up with what might be the deterministic set of design basis accidents, which traditional facility designers use.

This -- having done this, provide you an information database, and a knowledge base that dealt with it. In fact, this is why we spent a lot of effort in seismic follow-on studies that I showed you on the one slide. We knew that that would be a crucial issue for discussion, and there was a lot of follow-on detailed assessments done to support that.

The Q-List was one thing. And by running through, in the case of sourceterm calculations, you get into the specific gaps in the R&D database. Things such as tritium were not being measured, and that's an issue that was identified.

The brittle fracture question. The data that exists is only for non-irradiated UO2. It is not for irradiated fuel. But, we know enough from the German programs and the U.S. programs, and the people who smashed up fuel that's been irradiated to know that there is a difference, and that it's probably conservative. But, the question is, if you break up the fuel, what's the airborne fraction say, below 10 microns? There is a difference between irradiated and non-irradiated fuel -- that has not been done.

Those are the specific kinds of things that were identified, as a result of doing this. You also can get into the -- you know, the needs for site characterization. I think the one I will limit it do will be the seismic issues and the fault -- the fault displacements and those kind of things. You will find, on that seismic list of studies, reports that were specificly targeted at -- when there's fault movements under the structure, what happens? That gets you linked, if you like, to the site characterization programs and its needs.

Also, I think when you go through this, you get an awful lot of insight as to what is a good way to put together the story to show to the regulator or to manager on programs, "How am I going to go about getting to the end point?" which, in this case, is a dose calculation off-site for accidents, and what is it, and then, of course, the yet to be resolved issue is what's it's probability and how will that be used? It's still an open case for the repository, and my opinion that how probability will be used along with those for questions of importance to safety still needs to be looked at as the program is implemented.

I think, with that, I'll end.

DR. NORTH: Okay. Do we have any questions or comments?

DR. PRICE: I was going to say I don't know why it is every time there's a topic I really get interested in, it's lunch time.

[Laughter.]

DR. JARDINE: I'm the one on the other end -- I always get it when I'm --

[Laughter.]

DR. PRICE: So I don't know to what extent to go into some things. I'm not going to ask you the question about where did you get the numbers from because that will probably take a little while, but let me ask you this: With respect to what was done in coming up with this conceptual design which was the basis for those numbers, wherever they came from, to what extent did the utilities and their operations and functions, the MRS and its operations and functions, the transportation system and its operations and functions, enter into the conceptual design which was the basis for this?

For example, if, somehow, that system was such that all that had to happen is that you had to load the shipping cask into the -- and I'm not suggesting this, but I'm just saying it as an option that might have been in there -- into the repository, how would this have changed the way things worked?

In other words, supposing there was an overriding systems command to minimize handling, you have quite a bit of handling there, and it reflects, I think, back through the system an awful lot of handling that probably is going to go on.

[Slide.]

DR. JARDINE: Let me choose to make a statement, I guess, and go back to the methodology. The philosophy was that for the analysis that we did, it had to be limited to what was on paper and in the design. So the kinds of things that you're talking about, I would say, would be something passed in one way or another into the design as a constraint. It's a bigger picture system look.

In other words, when the design basis was constructed for the conceptual design, it had documents feeding in and it had slightly different names, but it meant the same thing.

I think it was constrained. It didn't have the opportunity to say, "Give me some systems studies with and without an MRS, or a universal cask, or a waste package." So what I was only able to cover this morning really dealt with the design that got on paper, which there is some traceability for, but it didn't get into addressing the kinds of things that I think you're alluding to as to how might you change the casks that come in to remove the scenario that I showed you.

You know, because of the facility design and the way it evolved, and it was on paper, this exercise found, I call it a weakness in the design, which you have an opportunity to improve.

Part of the recommendations -- nothing wrong, I believe, with these group making recommendations back. "Hey, up here, guys. There's a bigger system you might take a look at." But --

DR. PRICE: Would that really be your recommendation, that there be a big system look?

DR. JARDINE: Well, in a controlled way. It depends. I mean, it depends. If your job is to bring in the design basis and the facility design, you know, and you don't have those horses, your mission as a project manager or a project engineer is control schedule and cost, and that kind of a question becomes, I call it -- you've got to assign who's responsible for that. So I think I would just classify it in my talk here a recommendation, and someone else has to make that call.

DR. PRICE: If there were an overall dedicated systems safety group that had to look after some of these things that you have just now performed, but they were an overall group looking at all of this, would they not have such an overall systems view if they are doing their job?

DR. JARDINE: Well, you know, it sounds like it's a loaded question.

DR. PRICE: Yes.

DR. JARDINE: Because one of the classic arguments that they got into in the development of this is, Is this group doing this independent from the designers, or is it better, this group being part of the designers, who understand the design better?

If you get an independent group that's sitting out in left field, they may not have all the information. So there's a serious question of how you implement it.

DR. PRICE: Surely, we wouldn't advocate somebody doing it without the necessary information.

DR. JARDINE: No. But part of it's communication. But the way it was conducted was that it was within the same building where these two groups did their functions, but they are different people, and the two people don't work together. So what we applied in the Yucca Mountain project was really a hybrid of what you're saying.

It was not an independent group that might be sitting so far away from the design process that they might not have reality in mind; but rather, it was a second set of individuals who could work in somewhat independent ways, but we were sure they had some knowledge.

Now, it's a different question about the schedule that comes in. Remember, this was kind of constrained to be a one- or two-year thing, and I think your kind of question perhaps is aimed at a license application date that Felton Bingham picked a date for.

DR. PRICE: Let me change to another comment that you made. I'm trying to hurry this because of the schedule problems that we have.

DR. JARDINE: Yes.

DR. PRICE: You mentioned the level of detail that you had to go in, and using one methodology, for example, but you did do fault tree. By the way, I would love to see those because I have yet to see a fault tree since I have been on the program.

DR. JARDINE: In my briefcase. I will show you at lunch.

DR. PRICE: I'd just like to see a fault tree from the other side.

But the level of detail that you talk about going into, I would like to make a comment on that. That is, if you're looking at this kind of a problem, at the identification of degrading things like this that really affect performance, can really affect things, and you haven't used the state of the art tools which are available for you, and it ends up in court, as some of this stuff may do, then you have not done all that you need to do, and the consequences of failing to do all that you can do -- that is, that which is the state of the art -- and pursue diligently the options which you have available to identify all of these things can be a lot more than the cost of just doing those.

DR. JARDINE: Yes.

DR. PRICE: You know, I've heard this twice now, and so that's why I'm making the comment.

DR. JARDINE: It might be worth making a comment that the Department recognized something of that. I'll give Norm Eisenberg, who is in the room here, some credit for that when he was in the DOE.

They had a program that they put in called PRAM, or Preclosure Risk Assessment Methodology, and its charter was to do exactly in the long-range the kinds of things you were doing. It was an idea that I think was ginned up by Norm and Tony Eng from the Department. There was a program that was started in the '87, '86, '88 time frame.

Due to the fact, primarily, that the design phase stopped -- I mean, that's the best way to say it -- there was no real design of the repository -- that program came to an end and that group or that function was sort of dropped. But I think the Department recognized that it was an important thing to address, and its purpose was -- at that time, it brought in people from the reactor side, you know, from Brookhaven specifically, who were tied in spades with all of the NUREG-1150 stuff. I mean, they had all this stuff that was used in the nuclear reactor industry.

So I think what I'm trying to say is the Department did have that, but because the design got stopped, that program, rightfully so, was, perhaps, brought to an end.

DR. PRICE: Another quick comment. This was an off-site dose analysis that you did, the accident and the off-site consequence. I just got back from Sellafield, and their most likely severe thing was a worker entering into a cell where there was exposed fuel.

DR. JARDINE: Right.

DR. PRICE: That was the one that they couldn't handle, was the most difficult, at least, for them to handle, and the most likely as well. So this is just one site and only a start in the kinds of analysis that could be performed to ultimately get down to the details of design necessary to provide both off-site and on-site protection. Would you not agree?

DR. JARDINE: Yes, I agree. I'd just comment that Sellafield, of course, is a reprocessing plant. I only alluded that there is -- and I labelled the slides "accidents" -- there is a series of documentation that is quite extensive where operational exposure -- well, that isn't as extensive as the normal releases we looked at, but there is some of that information, and we would agree, or I would tend to agree that it's likely that some kind of an inadvertent worker exposure will be a bigger dose number than something off-site, primarily because the repository is just very difficult to get an off-site release to occur.

It basically boils down to physical energy or mechanistic things, and when you look at the seismic cost benefit, which a lot of that work was done, the experts, the structural people and the dose assessment people, just could not get an off-site release to occur.

Of course, there's always the potential for worker exposures during those kinds of scenarios, which were not included.

DR. PRICE: I'm glad to see some of the analysis.

DR. NORTH: On that point, I think we'll break for lunch. Let's try to keep it to one hour. So if everybody would try to be back here at 1:45.

[Whereupon, the meeting recessed for lunch, to reconvene this same day at 1:45 p.m.]

AFTERNOON SESSION

[1:47 p.m.]

DR. NORTH: Let's reconvene.

DR. DYER: Back by popular demand is Felton Bingham, Sandia National Labs, who will talk to us about performance assessment and development.

[Slide.]

DR. BINGHAM: Those of you who were fooled by my act as an historian should now judge me on my performance as a structural morphologist. This talk has a pretty generic title.

I want to emphasize that this is Scott Sinnock's talk. These are Scott's viewgraphs. The opinions that I'm going to express are all Scott's. I am happy to say, though, that I think I share all of those opinions. I spent two or three hours with him after he made up these viewgraphs, discussing them with him so that I could be an honest reporter of what he wanted to say.

Not once in the entire time did we have a disagreement. I suppose that's what you would expect since we both learned performance assessment sort of at each other's knee. These are the thoughts that he had, and his understanding for the point of this talk was really to present these thoughts to you for your digestion and contemplation.

There aren't any real answers to things in here, and there aren't any results that are going to amount to something, but there are some ideas that may clear up some things, and I'll try to present them faithfully.

[Slide.]

DR. BINGHAM: I have brought a cheat-sheet up here along with me, just to make sure I do represent what he wants to say. Scott wanted to start out with something -- these are some things that may be almost self-evident to people who have worked in this project.

We've got to predict how a repository is going to act for ten thousand years, and that's a formidable task, but it's made a little less formidable, maybe, if we remember something that Russ and Dave, especially, have been hammering on for most of the morning; that the reason for doing all of this is to provide a basis for making some judgments, for making some decisions. These are judgments on the acceptability of the site, which happens to be one of the most important ones that can be made.

Now, since we can't do a ten thousand year experiment, the idea is to use numerical models to give this basis. This is important in thinking about what needs to be done, and how much needs to be done, and how do we handle our models.

What's necessary are models that will give us a basis for judging, and that's not the same as a model that will help us understand all the details of a particular process. It's not exactly the same as a researcher's attitude when he starts out to learn something new about nature.

The problem, as Scott sees it, is how can we convince anybody that the results are any good? Well, this turns out to be sort of an outline of his talk. The way to use numerical models will occupy about the first half of it, and dealing with how anybody can believe the results will occupy something like the last half. This is where he'll talk about uncertainty and about validation.

[Slide.]

DR. BINGHAM: Let's ease into this a little more with things that are probably clear to everybody. I think of this slide as being mostly a definition of these terms -- subsystem, function and process -- that are going appear prominently on the slides that Scott prepared to come along soon.

All of you are familiar with the division of the repository system and the subsystems, and all of you are familiar with the notion that each of those must play some function, as required by regulation to play some function. I don't intend to go through this whole slide, but let's just take the waste package, for example, since it's at the top.

You may think of it as having two things to do. It provides containment and it controls releases, and those two words are picked, of course, because they mimic performance objectives in Part 60. There are other ways you could express that, and the detailed division of the waste package into its parts that's being done as part of the Management System Improvement Strategy, will have a quite different arrangement.

But for this purpose, we can think of it as having those two things. Realizing that it must do these jobs, what are the things about it, what processes go on around a waste package that can contribute to those, or that might detract from them? Scott has listed several of the things up here: the corrosion of the container, the corrosion of the waste form, radionuclide decay is something that contributes to controlling releases.

For each of the other systems, there's a similar division. At the bottom is something that probably shouldn't appear under the heading "subsystem" it's the total system itself. The key point here is, all of the above; that all of these processes that contribute to functions of subsystems also contribute to the way the total system behaves, and the function of the total system is to meet the EPA standard.

[Slide.]

DR. BINGHAM: Now, mostly, Scott wants to focus on the necessity to get to the total system someday, so this slide gives some talking points for the relationship between those subsystems and the total system. The first one is something that I already said; that the total system has to combine what everything else does.

The next one, though, is a little more subtle. It's probably self-evident, but there's a message in it. That is, when you're predicting the performance of those subsystems -- and, of course, you do have to do that, because the regulations require it -- those predictions are going to depend on processes that have time-dependence and they must be modeled in a site-specific fashion.

Now, that seems obvious to us now, but in doing that history work for the first talk this morning, I realized how not very many years ago, probably people didn't expect that this level of detail was going to be necessary. I can't imagine that in 1957 when salt was recommended as a medium that there was an expectation that the detail that's required today would be required.

But as it is, it is required and the key point to remember in it is the one I want to repeat again: it's not an effort to understand those time-dependent processes in their completeness, but to predict the behavior of the subsystems in a way that will allow decisions to be made. I'm going to come back to that more than once as a way to make sure everybody is still with me.

If you disagree, maybe it's a way of hammering you into agreement. Well, to do this, it seems useful and it's turned out to be useful to have a hierarchy of models. The naive expectations that some of us may have had years ago that there was going to be one model that just kind of did everything has not proved out in practice because of the necessity to do what the second bullet says.

For this hierarchy of models, which you have already seen two or three times today, has process models where process means what was defined on that slide just ahead of this that are mostly mechanistic. It has models that model the performance of a subsystem and then something called the Total System Simulator.

I want to avoid a semantic puzzle with this one. The SCP refers to something called a Total System Simulator, and that is not exactly the same thing as the Total System Analyzers that you've seen probably in the recent past and will hear a good deal more about this afternoon. Here, I'll have to use it to mean some kind of model that predicts the behavior of the total system.

[Slide]

DR. BINGHAM: This is Scott's version of the hierarchy. It should be familiar by now. You've seen it in triangular form several times. This was done by somebody who knows how to make an isometric projection, I suppose.

Here we've got the process models, the details of the processes that occurred on that slide two or three back, the models of subsystem and model of the total system

Since these models are closer to the decisions that have to be made, to those regulatory decisions, they become more the province of performance assessment than these models are. These models are useful more for understanding the details of the process.

The heart of this slide, and the two points that Scott really wants most to make with them, are contained in these two arrows.

Now, first let me say what they do not mean. They aren't intended to say that uncertainty increases as you go this way or sensitivity increases as you go that way. The idea is something more like this: advice about uncertainty has to be passed up from these detailed models to the total system models.

Since this thing doesn't model anything in detail, it's hard for the work up here at the top to know just what its uncertainties are.

By working hard with models of processes, detailed models, we can probably acquire enough confidence in the uncertainties inherent in them that we understand them, that we may feel we understand those uncertainties.

That information has to appear at the top, though. And I think the issue has been touched on two or three times already, or at least I heard it echoing in some of the things earlier speakers said. This is an important link to make, that the real information about uncertainty has to be gathered together at the top, but it doesn't come from the top; it comes from the work done at the bottom.

Sensitivity sort of works the other way. This is also mimicked in something I heard two or three people say this morning.

The people who are working down here don't know how important their results are, generally speaking, to those decisions that have to be made. So the advice on which parameters are sensitive has to come, generally speaking, from the top down to the bottom.

Now, that implies a lot of things. It implies close linkages among these models, close linkages among the people who are doing them, and it probably implies iteration.

Well, I don't know that any of those ideas are all that startling, but I think this is the first time I've ever seen them displayed quite so succinctly with these arrows. And if I can assume that all of you understand, now, what Scott was trying to say with those, I'll go on to the next slide.

[Slide.]

DR. BINGHAM: Here is some talk about the models that appear in that hierarchy. This gets a little more concrete, a little less abstract, I suppose.

Let's start with the ones at the bottom. The ones at the bottom of that pyramid tend to treat processes very complexly. They try to incorporate all the details that seem to be necessary.

If we have a -- if the process we're looking at is, say, corrosion of stainless steel, there are a lot of things, I understand, that can contribute to corrosion, and a modeler would like to feel he's covered all the important ones as he predicts how fast the container is going to degrade.

The computer time that's used with this kind of model when it's embodied in a code is involved mostly in solving complex equations.

Scott wants to point out that it's not quite fair to say these are complex codes and these are sufficient codes. Both kinds may take very long to run on a large computer.

The difference is in what the CPU is doing while it's going. And Scott thought he'd like to point that out. Here you're solving equations. Over here are some uses for it. I don't know there's a lot of point in going through it, because I believe you know what they are. But the idea is to predict just what the processes are going to do, to identify the subprocesses within the processes that are most important, and then to define the input for those subsystem models, to tell them what they, in their simpler fashion, have got to mimic.

But up at the subsystem, the process treatment becomes much simpler, because the subsystem models are going to rely on this one.

I wish I'd added another line here. Scott would have, I expect. The solution of complex equations is something they do. But these also tend to use a lot of CPU time in Monte Carlo simulations. Because at this point, some of the analyses need to be probabilistic. They are departing from the mechanistic work that's done down here, the deterministic kinds of calculations. They're trying to combine things together, partly to incorporate uncertainties and partly to incorporate natural variabilities.

Well, they'll predict subsystem performance. That's an end in itself, because there are performance objectives that deal with those. But they are also defining the input to go into the total system model.

Well, that one hardly treats processes at all. It may not do any calculations that have to do with what we call processes, about three or four slides back. Maybe it'll be look-up tables.

Maybe it'll consist simply of distribution functions.

The computer time can be very lengthy with it, though, because it has to do many realizations, sampling as it goes, from many different possibilities for things that may occur.

Here's the word "scenarios" again. I suspect that in this case it's used not simply to mean a path through an event tree, but it is the things that may be important of all kinds, how they're linked together. And the use, of course, is in showing compliance with the EPA standard.

[Slide.]

DR. BINGHAM: Scott thought you might like to see the names of some of the codes that are on here.

Now, after expending all this breath, and your time, on saying how important the hierarchy is, now I have to say it's not really that important. It would be a waste of time to try to pick a code and put it somewhere in the hierarchy, because, of course, the way a code is used depends on where it appears.

So Scott put the names of a lot of codes that you hear of a lot along the side here with arrows, to show that they probably, most of them will span more than one chunk of the hierarchy.

To me, the archetype mechanistic process code is the one called EQ3/EQ6. It deals on really a molecular scale with geochemical reactions. That's a very tiny thing, indeed, to be dealing with. That makes it, because of its complexity, essentially useless in a system code. But it's very important where it is.

On the other hand, the archetype system code would be, I think, the one called Total

System Simulator, in the SCP, which may very well, as the preceding slide pointed out, have nothing to say about processes at all, that it hasn't already been told by models lying beneath it.

It might be worth walking through a couple of these. The ones like NORIA, TOUGH, TRACER, SAGUARO, are all things that are useful, sometimes, in dealing with these mechanisms, but they do come up into the subsystem models, depending, again, on how they're used.

The TOUGH model, for example, I can pick that one out as a particular example, since it can treat two phases and two components, it can be used in a lot of very detailed studies down here. But when some simplifying assumptions are made about a subsystem, it can be used up in here as well, and has been used that way.

The arrow that's widest on here, in Scott's estimation, is the one called TOSPAC, which has been used a little bit in detailed studies, used a lot in subsystem studies, and then, as you will hear later on this afternoon, has been tacked into some systems studies as well. This is a system code, one that uses TOSPAC, that actually does some calculations of some things, in particular, groundwater motion.

We put the EPRI and GOLDER codes, the new ones that are on here. We're not sure, Scott didn't feel sure exactly where they would fit, but our understanding of them is that they are primarily useful as system codes, that they primarily work like shelves into which information is put that's been derived from running codes at the lower levels.

I think this is a little bit useful in dealing with this barrage of animal names that you're going to hear later on, or probably for the next several years.

DR. NORTH: Before you go on there, I'd like to comment.

The multiplicity of these codes, and the need to try to understand the linkage between these different levels, makes this really a very formidable problem in terms of the complexity of trying to inter-relate these various issues and check out that you really know how to integrate across the levels of knowledge from the top of this pyramid to the bottom.

It is easy enough to state in concept this is what we need to do. But to actually do it, and be comfortable, and convince other people that you've done it well, is a formidable undertaking.

That's why I'm going to keep harping on this theme of we've got to iterate. It's not enough to be doing research on this and conceptualizing how we're going to do it. We've got to practice. It's going to take many, many times around the loop, to the point where we really get comfortable that we know how to do this job.

I wonder, for example, how many people there are that know all of those codes well and understand what the differences are between them?

DR. BINGHAM: I can answer that very easily.

[Laughter.]

DR. BINGHAM: Less than a null set.

I might mention one or two things that do help to deal with it. I certainly agree with you, of course, about the iterative requirements to be placed on that. But one techniques that helps with some of these is the use of codes that are close together in linkage fashion.

To run TOSPAC to do water travel and FEMTRAN, say, to do transport, puts them together.

To use TOSPAC along with, well, you hear about simple-minded simulators, it does help to reveal some of those differences, too.

There's another technique that's been used, well, we'd like to do it more. And that is, the definition o f problems to help decide when is the, say the two-dimensional modeling that I can do with these codes, mimicked properly by a one-dimensional code like TOSPAC?

There are specific questions like that that can be answered by, simply by running the codes often.

DR. DOMENICO: What does TOSPAC do?

DR. BINGHAM: TOSPAC is a one-dimensional code. It solved Richard's equation, to get a flow field, and then tacks transport onto the end of it.

DR. DOMENICO: A one-dimensional is a constant velocity system.

DR. BINGHAM: Well, velocities may be different in a stratigraphy that has different hydrologic property scores.

DR. DOMENICO: Well, it's one-dimensional, that's what I'm saying; it's one-dimensional velocities, in a one-dimensional system, the velocities are constant.

I just learned something here. It's not a steady-state system, it's a transient system.

DR. BINGHAM: It does work either in a steady-state mode or a transient mode.

DR. DOMENICO: All right. Then that's fine. Everything is clear.

DR. W. BARNARD: Why is your Total System Simulator dotted as opposed to solid line?

DR. BINGHAM: That's because the thing that's called Total System Simulator in the

SEP has not been built yet.

DR. W. BARNARD: Okay.

DR. BINGHAM: There are other things. These could probably be called -- all of these that are up here near the top could probably be called Total System Simulators in a sense. But the thing described in the SEP is really very complex in concept, and it's intended, of course, to pull together all significant events and processes and combine them in a rigorous way.

DR. W. BARNARD: When do you envision that taking place?

DR. BINGHAM: Well, I think originally, we had planned to do that in a few years. That was sort of the plan in the SEP. But we've been learning recently that the world would like to see it earlier.

[Laughter.]

DR. W. BARNARD: Yes. I think that's what Dr. North was saying.

[Laughter.]

DR. BINGHAM: Now, it may be that it doesn't have to come out right away because these other codes are really doing its job, and I think they're probably doing its job well enough to support the decisions that need to be made now.

See, I've done my homework. I remember. I'm always supposed to answer you by saying, "Well enough to do what?" "To do the decisions that have to be made."

My guess is that -- I wouldn't put a lot of priority into developing that thing that's described in pretty abstract terms in the SEP because I believe these others will probably be good enough to support the decisions that need to be made now.

But there are people who would like very much to do it. If they can be freed up from doing some of the other things, they would like to give it a try.

[Slide.]

DR. BINGHAM: Well, that ends the talk about the hierarchy of models, essentially. That's Scott's idea. He'd now like to swing into that third bullet on the first slide. How can you get anybody to believe the results that you get from these things? That has to do, of course, with dealing with uncertainty.

Now, Scott doesn't have any magic bullets to offer about how to treat uncertainty. He doesn't have the answer. In fact, this talk isn't supposed to be about uncertainty; it's supposed to be about the model hierarchy and how that arrow that was on the left hand side pointing up can be represented in something real? How do you deal with uncertainty in the hierarchy? Here, Scott's title is appropriate. He had some thoughts to offer. He doesn't have the answers.

Scott had several viewgraphs to show in here. He told me take them out because he thought the talk would run too long if he put them in. Now I'm sorry because I don't have them to use as a cheat sheet for talking about each of these four processes. So, I'll do what I can. If I get lost, I'll go look at Scott's notes again.

There are many ways to break down uncertainty into its components. The first one Scott chose is parameter uncertainty. By that, he means expressing a lack of knowledge about the values to assign to the input variables in the models.

Now, there are many sources for such uncertainty, and if this were a talk about uncertainty, and I'm sure you've heard several already in your tenure, it would begin with an explanation of those sources.

Here, it's not so important to notice them or to list where they all come from, as to say that there do appear to be ways of handling them, fairly rigorous ways of handling them.

In fact, the reason these are ordered this way on the slide is that the subjectiveness with which their treatment occurs tends to increase as we go down.

You can be pretty objective about handling uncertainty parameters, and I think that, in a later talk today, when Paul Kaplan talks, he may give a glancing brush against that problem as he goes through the topic that he's been assigned today.

The methods of statistics really are pretty well known. However, Scott points out, in order to use those, it's usually necessary to be able to define something that the statisticians call a population.

Scott's favorite example of what he means when he talks about that is to remind you what happens if you look at the data that come out of a core hole -- it goes way down deep, a stratigraphic column.

Some of the properties in the typical bore hole seem to be grouped into nice discrete units, and that property, whether it's voracity or hydraulic conductivity or whatever else, appears to follow some kind of recognizable, determinable distribution within that unit, but not in the next unit.

When that occurs, it's not too hard to define a population to use your statistics with, but some properties and some bore holes aren't like that. They don't appear to fall into those discrete units. They appear to show trends instead. It's to deal with matters like that that Scott has broken out what he calls scalar uncertainty from parameter uncertainty. It would be perfectly reasonable to include what he means here within that rubric.

What Scott's talking about is not scalar as opposed to vector, but scalar meaning this expresses our lack of uncertainty about the scale dependence of the variables to which there must be assigned values and models.

Since we can't dissect the whole mountain into little millimeter sized pieces and determine all their properties and put it back together again, we have the problem of using data describing the properties of one point in space to constrain the properties we wish to assign to the rock in some other point in space.

How that's to be done is, at least for purposes of this talk, a little bit different from the way you handle parameter uncertainties. It's a little less objective. It's a little less well developed.

Scott mentioned some names of techniques that are useful for that. Geostatistics, of course, is a well-developed field that has a lot of handles on that. It my be -- the jury doesn't seem to be in yet -- that the factual theory and the chaos theory will offer some insights into dealing with that. There do appear to be other, Scott calls them heuristic processes around that could help with that. But the problem is a little more difficult than it was for just the straight problem of, "How do I assign a distribution to a parameter in the face of uncertainty."

Process uncertainty is an interesting one. This, of course, means the lack of knowledge about the way energy and momentum move through a system, and process means just the same thing it did back in the earlier slides.

How do you handle that kind of uncertainty? Well, normally, it's handled by what Scott calls conditioned scientific opinion. The key word there is "conditioned" because that's simply the way all science has been done, that it's a scientific opinion that's based on knowledge, that's based on study. There doesn't seem to be much of any other way to handle it.

In a later slide, I will talk a little bit more about handling this kind of uncertainty, but since conditioned scientific opinion is the way it's handled, basically, the answer is peer review, except for one other thing that I'm going to leave and hope it startles you so that you will stay awake until the next slide, and that word is "simplification." How does simplification help deal with process uncertainty?

Some of you probably already know the answer, others of you may not, and, gee, I see heads popping up all over the place.

[Laughter.]

DR. BINGHAM: The last kind of uncertainty Scott talks about is temporal uncertainty. We have algorithms that will tell us how processes will evolve in time. We have algorithms that will tell us how to express parameter uncertainties. Paul Kaplan will talk your ear off any time you like about how to do that. But we don't have an algorithm that tells us what society is going to be like in 10,000 years, and we can't do the 10,000 year experiment. So we have arrived by now at the bottom of the heap, a heap of, as you go down, increasing subjectivity.

For this one, conditioned scientific judgment or conditioned scientific opinion is still the answer, but when Scott says that, this one, you can hear the quotes around the word "scientific" in his voice. It's very hard to say how you do a scientific appraisal of the future of society for

10,000 years.

[Slide.]

DR. BINGHAM: Those are remarks that Scott made about this sort of thing. They lead into this slide that simplifies things a lot. It's strategies now for dealing with uncertainty through the hierarchy.

The first of these is defense in depth. This brings us back to something all of us have been trained to repeat over and over again since we got into this program, that a geologic repository is a system of multiple sequential barriers.

In fact, you can think of each of those subsystems that was on the second or third slide as a barrier, a barrier to release of radionuclides.

Because they are sequential and overlapping, it's not necessary to have as much confidence about the behavior of each one as you would have to have if that one were the only barrier.

If we can be, say -- if we think the possibility that the waste package subsystem would fail is about 20 percent and we think that the possibility that the EDS would then fail, independently of the waste package, is about 20 percent, and if we feel like the probability that the site will fail is about 20 percent, we have a much better than a 20-percent probability of failure for the whole system.

Now, that's a simple way of saying things, and I'm sure you could drag me around for a long time about common-cause failures and so on, but the basic point is really the simple one, that by this defense in depth, we probably reduce, for the purposes of making decisions, the certainty that we have to have, or we can reduce the reduction in uncertainty that we have to have.

Another step is the one that probably is most obvious, scientific engineering on, to reduce the uncertainty in the parameters, to use site characterization to get after them, cut the things back down to where they belong. I think there is hardly any need to discuss that one very much. Of course that's something you will want to do.

I think Scott's point here would be there are methods for doing that. There are well-accepted methods for doing that, for expressing uncertainty in ways that let you know when you have reduced it, without bias of experimenters and other people showing up in it.

Here is the word "simplification," to use simplified models to come out with what are the sensitive processes going on there. This way of reducing uncertainty Scott likes to illustrate with the example of volcanism.

I suppose the Board has probably had a presentation on volcanism from Bruce Crowe, and you are aware that his method of dealing with the probability that a volcano will come through the site is essentially to go around and count the craters that are out there and date them, and that kind of gives you an idea for how often they will occur and where.

Now, there is a much more complicated way of doing it. That simple model that he has could be replaced by a much more complicated one that studies the deep underground processes that lead to volcanos, the migrations of hot spots, and the motion of plates.

That way would be important if it were likely to yield insights that you had to have to make the decisions that you have to make.

If there can be consensus in the scientific community that Bruce Crowe's kind of approach to treating volcanism is a valid one, we will avoid dealing with all of the uncertainties associated with motion of magma beneath the earth's crust, another way of dealing with uncertainty.

I wish I had a different word than this one. It says "stress uncertainty about compliance rather than about actual" -- "phenomena" is the word I would like to have there.

Let's see: It's probably only for about the three-dozenth time that I've said we need these for examining compliance, for making decisions. It isn't necessary, this bullet is saying, to know everything about every process.

We need to stress our uncertainties about compliance. That's where the chickens will come home to roost. That's where the license application will tend to rise or fall, if it rises or falls on the problem of uncertainty.

Scott points out that it's a curious fact that review panels differ greatly in their assessments of how much you need to know about processes and how much you have to reduce the uncertainty in them.

He says there are some review panels who insist that the real difficulty with the project is you don't understand the microscopic phenomena in detail.

There are other review panels that say you guys seem to know most all about -- you seem to have kind of identified the important processes. You probably know basically how they work, and you've just got to reduce the uncertainties in them.

Scott points out that it's also a singular fact that when an expert in a particular discipline is asked that question, he tends to answer by thinking that the other disciplines are pretty well taken care of; they understand those process.

The one I am an expert in, however, needs a great deal more study in the process before I can say that you truly understand it. Human nature at work, I suppose.

[Slide.]

DR. BINGHAM: The last one is making clear what reliance is on professional judgement. Statements like this seem to provoke screams of outrage pretty often, because it sounds, on the face of it, as though somehow the project is just going to go around and ask experts for their opinions, and that's it, won't have to do any real work.

Well, the idea here is that, of course, as has been pointed out several times already today, professional judgement enters at every one of these steps. Even those detailed processed models involve judgement, because every one of them is a simplification of the actual process that's taking place in nature.

The person who creates the model and the person who runs it and the person who derives data for it all have to make assumptions about what it is that they are going to rely on.

At a higher scale, the selection of subsystem models relies on judgement. Do you pick a representation for groundwater flow that uses the hydraulic conductivity formalism, for example?

All of those things are professional judgement, and Scott thinks it's important, in explaining to people how we're dealing with uncertainty, to acknowledge that, not to say that these things up here are magic bullets of some kind that are going to reduce uncertainty and solve everything.

In the end professional judgement will be required, and that really shouldn't shock anybody, because it's been at work all the way through the entire process.

DR. NORTH: I'd like to reiterate the point about whose judgement.

It strikes me that it's going to be very important, as this program goes toward the possible license application, to have the professional judgement in a very wide community of scientists, not just the people that have been part of the DOE program, and that means there is a very major communication and outreach program that has to go along with this.

It's not something you can accomplish overnight, and it's not something that you can accomplish with a few meetings with the Nuclear Waste Technical Review Board or anything like it.

It means getting out there in the professional society meetings and having a lot of explanations for just what it is you're assuming, why your models make sense, the linkage between the detailed phenomena models at the bottom of the pyramid and the subsystem models and the total system models, so that experts in that community understand what you're doing and acknowledge that it makes sense.

DR. BINGHAM: Scott certainly wouldn't disagree, and neither would I.

[Laughter.]

DR. NORTH: Good.

DR. BINGHAM: Scott has only one viewgraph about validation, and again, there is no magic bullet here. In fact, I have always thought of validation as being something like we used to have in the western movies I saw as a kid.

The scene opens up in a tavern where all the cowpokes from the Lazy J Ranch are sitting and enjoying themselves. One lone stranger comes in and announces I'm here from Circle Bar W Ranch, and immediately, the next frame, there's a fight going on, not just everybody picking ont he man from Circle B W, but even the people who were there from the Lazy J banging each other over the head with chairs.

So, I announce I am here to tell you about validation.

[Laughter.]

DR. BINGHAM: It's to let you know I can duck very quickly.

Scott points out that most of the thoughts about validation -- and you probably have had presentations on this topic, too; I know, at least, it was touched on briefly two years ago -- are directed at those lower-level models, the ones that are more mechanistic, and although nobody seems to be too sure of exactly how to go about answering the questions, the questions that the validation has to ask are probably pretty well understood, and I'll just let you read them, because I think they probably speak for themselves.

I don't think there is anything there that you haven't heard before.

What Scott was thinking of when he made up this slide was mostly what does it mean, what does validation mean for the models in the top of the hierarchy?

In the models at the lower part, the parameters tend to be things that you can measure, and you can do validation by going out and doing experiments, measuring those parameters, and seeing if the models predict the same results, but in the higher-level models, the parameters tend to become things that can't be tested.

For example, a total system model of a simple kind might well not calculate groundwater travel from some more complex formulation. It might just assume something about groundwater travel time that is learned from the other models below it.

But groundwater travel time is really not a validate-able concept. There aren't any groundwater travel time meters around that you stick into the ground and see whether that model is using the right thing.

So, Scott has pointed out here that validating those models really involves asking some other questions.

The first one has to do with something we've talked about, we've touched on, is the uncertainty, at least, in parameters and, probably, in what he called the scalar uncertainty, as well.

The example that he uses for this is that, in the SPARTAN code, one of the ones that's up near the top in the hierarchy, it does groundwater travel time by simple division involving effective porosity and flux.

The question for that model, then, is does it adequately represent the variability in those two parameters? Proving that that's a valid concept is not really the job of the SPARTAN code. It's not a validate-able question for it.

Of course, it's important to take these simplifications and make sure they are adequate. That's something that isn't often thought of in the lower-level models, but it applies especially to the higher-level ones.

Then, lastly are the probabilities, which are very important in those higher-level codes. Are they properly described?

As I say, there is no magic answer here, but I thought, after Scott had explained these things to me, that they tended to put some of the vexing questions of uncertainty and sensitivity and the use of models into a perspective that's useful for thinking about them, that clears up, I think, some of the muddles I have heard in meetings that have lead to those bar-room fights, like the ones in the Roy Rogers movies.

I am not sure I can answer questions anything like the way Scott would be able to, but I will be glad to field them.

DR. NORTH: Any comments or questions?

[No response.]

DR. NORTH: Maybe we'll let you off lightly. I can't resist the opportunity to think about that bar room fight analogy that you've just given, and reflect that scientists disagree with each other, and maybe much as old time cowboys like to have a bit of exercise, debating with each other differences that seem significant scientifically is what scientists like to do.

Maybe this is a process you have to work through in the process of validation, so that, in fact, you see if the disagreements make any difference, given that there are so many disagreements. You tie it back into that last bullet on your second-to-the-last slide, the reliance on professional judgment, and your fight analogy indicates to me just how much exercise and maybe black and blue skin is involved in that process.

It's not something that you can short-circuit and do it in a simple way. It involves a lot of interaction.

DR. BINGHAM: I'm sure Scott would agree completely.

DR. DOBSON: I'd just like to agree and add one clarification. I think the Department would agree completely with that, and I think it's our strategy, if you will, on both an informal and a formal basis, to get all of the issues that we can out into the open scientific literature and have them discussed and debated in scientific forums.

I know you're familiar with -- we've been trying to essentially adopt that strategy in volcanic hazard assessments and seismic hazards with respect to the hypotheses of Jerry Szymanski. In every issue that I'm aware of, that's exactly what we intend to do; is to have it discussed in the open scientific forum, and we think that's the best way to go about it.

DR. NORTH: It seems to me that the sooner you can get that discussion out there, get the brawl started, as it were, the faster we can bring that brawl to a conclusion and get some consensus. But to try to avoid it because there might be some conflict or somebody might come out with some viewpoint that might not be very popular, it seems to me that ultimately impedes the process.

DR. DOBSON: I guess I would just want to reiterate that I don't think that we would ever try to stop the conflict, if you will. In fact, we encourage the debate and we do it both formally as we're doing in things like the ESSE task, and, as I said, informally with respect to a lot of individual technical issues. All I can say is that we encourage that approach.

DR. BINGHAM: If I don't carry the metaphor too far, I think the difficulty now is that it's not that the cowboys don't want to fight; they would like very much to get into doing that, but sometimes they have so many things to do out on the range that they don't get to go into the saloon. Sometimes the management of both ranches forbid their cowboys to go into the saloon as well.

DR. DYER: Jerry Boak is next.

[Slide.]

DR. BOAK: The next series of talks are going to attempt to demonstrate to you how our performance assessment capabilities have evolved over the course of a series of exercises we performed during the past few years.

[Slide.]

DR. BOAK: I'll be providing simply an overview, simply pointing out a few points of history that won't get touched on in later talks, and I will try to make a few points about what it is we've been driving at in the course, primarily of the PACE-90 exercises. That's going to be central focus of this series of talks, PACE-90.

We'll also be talking a little bit about some of its predecessors and also some of the successor efforts to try and demonstrate what we got out of PACE-90 and what it built on in the first place. To talk about the evolution of performance assessment capabilities, you have to think about evolution.

Cosmologists think of evolution as a very ordered, structured directional process, and there are aspects of performance assessment that are like that. These are primarily those that are driven by the silicon sacrament of computation. It's that process which is supposed to absolve us from the sin of subjectivity.

However, there is another aspect of the evolution of our performance assessment capabilities that is much more like organic evolution. It's rough, steamy and definitely contingent. It's directed by internal forces, but it's also very much affected by the impact or external bodies which provide those iridium layers that Felton referred to earlier.

That part of performance assessment is much more general. It -- employs those compact portable carbon based computers that we carry around with us. But it's also those carbon based computers that provide us the critical intuitions and insights into performance assessment that lead us to ask questions like; what's the most ignorant thing that we can say about this site, given all of the data we've got?

I think those are really important aspects of performance assessment. I think you'll see some of them coming out in the course of the discussions that lie ahead. I want to sort of flag those as milestones, things to look for.

[Slide.]

DR. BOAK: To talk about evolution, you inevitably have to go through a little bit of history to look back and see how it is the animal you have in front of you got its prehensile tail and where it lost the opposable thumb. The technical parents to the PACE-89 and PACE-90 exercises, you'll hear a little bit about the HYDROCOIN and COVE 2A which were code comparison, intercomparisons. I won't say anything more about them.

I do want to mention that in the wake of the 1987 Amendments Act, the performance assessment management which consisted of a group which -- both at headquarters and at the Yucca Mountain project, which gave itself the title of the Performance Oversight Group, or

POG, decided to establish a group of technical advisors that they called the TIG, or Technical Integration Group, and these, in turn, defined a series of performance assessment working groups which might be called POGs.

In the midst of this welter of acronyms, the TIG proposed a series of performance assessment calculational exercises which are now known as PACE-90. The acronym PACE-90 was proposed facetiously by Ralls Barnard, and for this sin, he was forced to constantly work on it for the past two years.

PACE, as it was originally laid out, had quite ambitious goals, well beyond the mule and 40 acres, chicken in every pot, new world order, thousand points of light. We were supposed to provide support for suitability determinations, capability demonstrations for performance assessment and ESF design support which Felton pointed out at one point, were at least three and possibly four or more mutually orthogonal goals for PACE-90 to achieve in one fiscal year.

Nevertheless, I think a great deal was accomplished. From the programmatic standpoint, we felt that we demonstrated we could put together an integrated working structure that did center around these working groups. We generally conceived of them as much more loosely knit and much more flexible working groups that we constituted and reconstituted regularly throughout the course of our work.

We also found that we were able to interact extremely productively with site characterization personnel and we actually dragged performance assessment personnel out to the mountain and armed them with rock hammers and other things as part of one of Warner North's field exercises to get us ready for that big war.

We developed, as a consequence of PACE-90, a proposed path to a credible performance assessment. That's the six-step series of events that Russ laid out for you, going through scenario development, probability assignment, calculation, et cetera.

I think we really began to get used to the idea that we were going to have make rapid turnaround assessments that could be used for decisionmaking for the various task forces we supported which, I think, has made the efforts we have worked on for site suitability much more effective. That essentially is a wrap up of what this intermediate step, the PACE calculations, had to do with what went on before, both from a programmatic and technical side, and also what's coming out of it.

You'll hear not only about site suitability, but about this task force that we've put together, a kind of loose knit working group looking at for the first time, what's the connection between relatively homogeneous flux through the repository horizon and that highly inhomogeneous flux that comes in at the surface of the mountain. Heretofore, there has been relatively little interaction about how you get rainfall into some kind of flux through the mountain. This working group this year, I think, is doing some of the first work to try and interact with Alan Flint at the U.S. Geological Survey and really see what the actual connection ought to be.

[Slide.]

DR. BOAK: What you will be hearing is, first, Holly Dockery talking about the PACE problem definition, then Bill O'Connell speaking about Source Term development, Flow and Transport Results from PACE-90 Exercises presented by Maureen McGraw. Rally Barnard demonstrates that he will continue to be in the hot seat for years to come and will give us some kind of an overview of the continuing and future performance assessment activities.

I believe that's the point at which we have a break. After the break, we'll have a discussion by Mike Wilson of the total system analyzer that's beginning to come online and we hope to use, once we've got some results from the consequence calculations of the site suitability effort.

Finally, we'll hear from Paul Kaplan about using groundwater travel time calculations to identify site characterization needs. With that, I'll turn it over to Holly Dockery.

[Slide.]

DR. DOCKERY: I will be talking about the PACE-90 overview and problem definition. I am hoping that I kind sort of set the stage for what will come in the next sequence of talks, both talking about where PACE came from, technically, setting the stage for the actual problem development, and then be followed up by, as Jerry said, Bill O'Connell, and Maureen McGraw, talking about the various stages of the results of the PACE exercises. Finally, Rally will talk about where we went from PACE, as a result of PACE, into the site suitability exercises.

So, I hope we can demonstrate, at least a good faith effort, on performance assessments part, to try to iterate, just as been requested, to implement some sort of an iteration plan, and make progress, if that's not a bad word, in this particular group.

[Slide.]

DR. DOCKERY: You can see, this is where my talk is sitting, with respect to other people's talks.

[Slide.]

DR. DOCKERY: Jerry mentioned that COVE and HYDROCOIN were the precursors to PACE. They were the technical grandparents for PACE. COVE was the code verification benchmarking activity that was one by Sandia, and HYDROCOIN was the hydrologic code intercomparison activity, that was an international effort.

Both efforts, as I said, were precursors to PACE. However, they shared other things in common, as well. They were both code verification exercises. In other words, their objective was to put the same data in the codes -- various codes, and have the same answers come out at the end. That was just -- that was the basic objective.

[Slide.]

DR. DOCKERY: They used the same five-layer geologic model, and that geologic model was based on Ortiz, et al.'s thermomechanical hydrostratigraphy that most of you have probably seen, in one incarnation or another.

But, lastly, these codes only calculated groundwater flow, they did not incorporate any transport of radionuclides. So, what I am going to do is have a very brief discussion on what these codes were and what their conclusions were, so we can see where we went with PACE, as a result of these codes.

[Slide.]

DR. DOCKERY: COVE-2A was 1-D only. The problem domain went from the surface down to the water table, and it had an extremely highly specified problem, in which case you got no variations interpretation. You were supposed to put the input in and see what information came out at the end. So, all you had was a comparison of the code output, not of interpretation.

The organizations that were involved, at that time, as you can -- you'll start to see these code names over and over again. Los Alamos, Lawrence Berkeley and Sandia were actually involved in the calculational efforts using TRACR, NORIA, LLUVIA, TRUST and TOSPAC.

They used a range of infiltration rates; both steady state and transient.

[Slide.]

DR. DOCKERY: HYDROCOIN, in comparison, had both 1 and 2-D aspects to the code, it also had steady state and transient aspects. It went further and explored some parameter sensitivities. There was more of a variation in infiltration rates. It also did some looking at anisotropies, and those anisotropies were mostly in terms of vertical to horizontal ratios of hydraulic conductivities.

Sandia was the only laboratory involved in this particular exercise. The two codes that were used in that case were NORIA and LLUVIA, which had also been used in HYDROCOIN before.

[Slide.]

DR. DOCKERY: Some of the important issues that we felt were identified by these two exercises, included most importantly that the parameters with the greatest sensitivity seemed to be those that would force matrix flow into fracture flow. Those sorts of things included the anisotropy, that I mentioned before, the vertical to horizontal ratio of conductivities, conductivity contrast across layers and any aspect that would form a lateral diversion.

There were also problems -- the transition could be induced by applied fluxes, and then by code artifacts, like the boundary conditions that were implied or imposed.

DR. DOMENICO: Holly, what was the output on these codes? What's the output? Hydraulic head? Waterflux? Mass? What was the output?

DR. DOCKERY: I think it was waterflux. Yes, yes and yes, except for the mass.

DR. DOMENICO: Yes, yes and yes? Yes, yes, and no.

DR. DOCKERY: Yes. Yes, yes, and no. There were no radionuclides transported.

One of the other aspects that became quite clear is that 1-D calculations were not adequately realistic, and that there was a need to move on and look at the effects of dimensionality.

Lastly, again, it was -- became quite clear that you needed to put the source term and the near term environment into these calculations, because you weren't getting a complete picture just by forcing water through the system.

[Slide.]

DR. DOCKERY: So, we came out of that realizing that we had to have a complete circuit, starting with mobilization and release from the EBS, near-field transport to the unsaturated zone transport, which is where the other codes had started, and then, finally, to the saturated zone transport.

[Slide.]

DR. DOCKERY: In the origin of PACE-90, and this might not going to be quite the right word to use, but we continued with the analysis teams that were organized during PACE-89. We had organized certain teams to do certain aspects of the problem development.

There was a working group for total systems, there was a working group for a source term, and there was a working group for hydrology. Those teams were continued into PACE-90.

There was also a very limited suite of analyses formulated, because there was a very quick deadline, as usual. But, the interesting thing about this is that the PI's had a lot of input. The investigators actually had a chance to decide what it was they should be calculating. What things were interesting, and what needed to be done, what was the next step?

So, they decided that the major departure, in terms of data interpretation would be to reinterpret the existing site data and see if there was information that had been added to the database geologically and hydrologically, that would be of importance in these calculations.

DR. NORTH: Was there also an attempt to build on the insight you had from the earlier validation exercises, for example, the lateral flow issue?

DR. DOCKERY: There was a sensitivity study that was done in the three-dimensional code later, but that wasn't actually part of PACE. This was simply changing the site hydrostratigraphy and then using the codes that we had, pushing the different infiltration rates down through the system.

DR. NORTH: So, you were still looking with sort of a one dimensional view of the problem?

DR. DOCKERY: We did have a -- these were run in one dimensions and in two dimensions.

[Slide.]

DR. DOCKERY: The rogues gallery of participants here were -- our let you read them. They thought maybe some of you wouldn't know what these acronyms meant, but I can't imagine who that would be.

The different laboratories were -- not everyone was involved in every aspect of the problem. You will hear from Bill O'Connell, that there were certain people involved in the source term and development, certain people involved in the flow and the transport sections of the code development. But, every one of those people contributed, in one form or another to the actual technical expertise.

[Slide.]

DR. DOCKERY: The objectives of PACE-90 included to demonstrate the participants' computational capabilities and mostly this meant the ability to integrate performance assessment models into the calculational effort.

This is also, once you identify critical elements and processes and some of those processes were within the codes themselves but were important within the code.

Demonstrate the ability to work interactively and this was sort of a two-phase problem, not just trying to work together within the different organizations but trying to get the modelers together with the people that gathered site data to get people together with all the different disciplines to try to work together on one problem and contribute to that one problem.

This is the first time that a lot of people had ever talked to anybody and that a code jock had actually come up against an ash-flow tuffs, you know, one-on-one. I think it was a very scary thing.

[Laughter.]

DR. DOCKERY: The last thing was to perform elements of a total system analysis.

This was only going to be one realization of a total system analysis but we felt that ultimately it would be a preparation for doing more detailed calculations. This was sort of our first step when we knew you had to start somewhere, so this is where we decided to start.

[Slide.]

DR. DOCKERY: So what was PACE not? PACE was not a code benchmarking exercise. This was not an attempt to get all the codes to come up with the same answer. It was not intended to answer conceptual model problems and it also was not -- here's our caveat -- not intended to represent calculational reality at Yucca Mountain, however we never say that this is what we think is going on at Yucca Mountain per se but one realization of what might happen.

[Slide.]

DR. DOCKERY: I will talk about the three elements of PACE with the first being by far the largest contributor in terms of time and calculations and that is the nominal configuration problem.

The nominal configuration problem was a calculation for unsaturated conditions were maintained at Yucca Mountain.

The disturbed case definition encompassed a limited number of specific events that might happen at Yucca Mountain.

The sensitivity studies were to explore the variations in the input parameters and what might happen as a result of that variability.

All were intended to ultimately result in some form of consequence analysis.

[Slide.]

DR. DOCKERY: What we have is a several step analysis approach.

First was to develop the best model parameters.

Then the second was to get a nominal case as opposed to what you might have heard before is expected case. We didn't call it "expected," we called it "a nominal case" problem of flow and transport, and then using this more or less as a baseline move on to calculate perturbations or the disturbed case problems and also to look at and investigate parameter sensitivities.

[Slide.]

DR. DOCKERY: As I said before, the PACE-90 calculations are hoped only to represent one component of expected conditions. The actual conditions are not really comprehensively modelled. We all know that we have a very limited set of site-specific data.

We didn't have any QA control on the codes at that time.

We haven't done a lot of conceptual model validation as a result of not being on-site to do a lot of the testing so that's why we are caveating this as one realization of what -- and not comprehensively modelling these conditions.

What we are saying that it is however is that we did some realizations using a variably saturated sequence of tuffs depending on the hydrologic properties of the various tuffs.

We used a very limited suite of radionuclides and did groundwater transport.

[Slide.]

DR. DOCKERY: So the comparison to the COVE and HYDROCOIN is that in similarity they were all flow problems. There was all an aspect of groundwater flow encompassed in these problems, but the differences are going to be that we developed a new hydrostratigraphy which I will talk about in more detail toward the end of the talk. We also did separate gas phase analysis or I should say Bill Lee did a separate gas phase analysis. He was kind of a one-horse show. Radionuclide transport was incorporated, since that's obviously something that was going to have to be done sooner or later.

[Slide.]

DR. DOCKERY: In the nominal case problem development, all the calculations began with the same input data basically and with the same boundary conditions. The boundary conditions I can show you later. Their sides were no-flow. There was a specified influx rate and zero pressure head in the water table so all those were specified and everybody used the same input and boundary conditions but after that point a lot of interpretation began to creep in very insidiously.

That was not something that we didn't expect. We thought that there would be some data interpretation and that some of the, part of the aspect of this exercise would be that we would see what was the difference in some of these model interpretations.

[Slide.]

DR. DOCKERY: So the fixed problem inputs, the things that everybody used for this, were infiltration rates that maintained unsaturated and of course where necessary locally saturated materials.

There were four long-lived nuclides in the source term and the reason we picked long-lived radionuclides is that the problem was run from 1000 to 10,000 years so the very short-lived radionuclides were not included for that particular aspect but these nuclides were also chosen to show a range of different behaviors, in sorption characteristics and release rates and other aspects of the source term.

These are the actual nuclides that were chosen but I am not going to talk much about those because that is Bill O'Connell's milieu and I wouldn't want to step on his toes, but we did use neptunium, iodine, cesium and technetium and as you can see, trying to look at a range of different parameters for the nuclides.

Steady state flux for 10,000 years and Maureen will talk probably to some extent about how the flux was arrived at and there were several flow field and transport calculations done in each case.

Performance measure at the end was release of radionuclides at the water table.

You can see we haven't gotten to the saturated zone yet and Rally will talk about how we are trying to incorporate saturated some in some of our calculations that we're working on now.

DR. DOMENICO: That is a mass release?

DR. DOCKERY: Yes.

DR. DOMENICO: Not concentrations, mass?

DR. DOCKERY: Yes.

[Slide.]

DR. DOMENICO: Chemical mass release?

DR. DOCKERY: Correct.

[Slide.]

DR. DOCKERY: The participants that actually were involved in the calculational end of things were Pacific Northwest Lab, Los Alamos Lab, and Sandia. These are the flow codes that were used.

Any questions on specifics of code go straight to Maureen.

Same thing on transport codes and you can see the dimensionality of the codes that were used. Both SUMO and the NORIA FEMTRN combination did two-dimensional calculations.

[Slide.]

DR. DOCKERY: The model region that was chosen was in the northeast quadrant of the potential repository block and now I'm going to say the wrong word there but the potential repository block.

What we hoped to do in this area was represent a range of conditions that might be used in a final PA model. We know we can't cover the entire scope and hit every single value but we wanted to try to encompass the range as best we could for an early calculation.

We selected that region for several reasons and I'll show you a picture next, but it extends beyond the repository boundaries and so that allowed vertical flow into an out of the repository.

It is bounded by four drill holes and from those four drill holes, hydrologic and lithologic data were taken in order to make that the geologic hydrostratigraphy. Also it includes a segment of the Ghost Dance fault.

Now we didn't explicitly model the Ghost Dance Fault in this series of exercises but we hoped that as we continued to build on our calculations that having the Ghost Dance Fault at the edge of our boundary whenever we did explicitly model a fault we would be able to more closely compare it to the PACE calculational exercise.

That's what we are trying to do with the site suitability is look at having some explicitly modelled fault but it is in the same region so they can compare them perhaps more easily.

[Slide.]

DR. DOCKERY: Here we have the famous pork chop. The view of the model region. You can see the four drill holes, G-1, H-1, G-4, and UE-25a#1 are the bounding drill holes. And so this is the modeled region within here.

Maureen has a viewgraph that shows you exactly where the different codes will run, in which holes and along which lines.

You can't see it very well, but here's the Ghost Dance Fault going through the repository, so you can see it just intersects, it goes across actually one of the 2D calculation lines.

[Slide.]

DR. DOCKERY: This is the interesting part, where you get to the geology.

In the hydrostratigraphy development, whenever Ortiz, et al. developed their hydrostratigraphy, they used 16 reference units, but that covered a section of 1,250 meters. So it was fairly coarse in its interpretation.

It also didn't incorporate rock mass properties. It didn't try to envision how the rock mass would behave. It was simply based on grain density and on porosity variations.

And so we felt that for the new stratigraphy, what we'd like to try to do is incorporate data that already existed, the hydrologic data, and try to determine what the rock mass would behave like, in a more realistic sense.

So we had 19 units that were defined in a 600-meter interval, so we had a much finer grading, and had zonation that was based on rock mass hydrologic properties. And those included grain density and porosity, but also bulk density, moisture retention, saturated hydraulic conductivity, and fracture characteristics.

So there was a larger suite of data involved in breaking up these units.

[Slide.]

DR. DOCKERY: For each individual zone, the way it was handled was first just to use the lithostratigraphic units that you would see in any geologic column. Those were the initial boundaries that were chosen.

And then the next step was to grade down and look at changes in more or less textural, and by textural, that means vapor phase alteration zones, changes in composition in some cases, changes in the fragment composition and size, and changes, most importantly, in the degree of welding.

Then the last division was made on a change in the mean porosity value. And while we know that within any zone you get a large range of individual porosity values, each time there was a step between zones, there was a large change in the mean porosity value. So this still seemed to be a meaningful change to make at that point. So that's how we divided the stratigraphy.

[Slide.]

DR. DOCKERY: And if you can tell, at least the people in front can tell, this is what the divisions of the thermo-mechanical unit looked like. Here is the top one, here is the second line, here's a line. So these were the zones that were used in the thermo-mechanical.

And PACE-90 had many more zones, especially once you get down into these bedded and non-welded zones. There was a lot of variation down in the lower section to try to look at the effects of the alternating vitric, zeolitic, welded, and nonwelded units.

[Slide.]

DR. DOCKERY: The data, as I mentioned before, were developed from these four drill holes. And we obtained the stratigraphic contacts and the physical characteristics actually from core data.

There was a very limited amount of data, as you can imagine, available from the various cores, and where there was a lack of data, these average properties were applied through the entire model region.

So we had in some places just one or two data values, perhaps, and those were applied throughout. Wherever we had explicit data, then those were averaged for an individual strat. column.

There was one layer that we included with high and low matrix conductivities to look at what would be the change if we took an almost unrealistic hydraulic conductivity change and changed it by several orders of magnitude to look and see what that was like.

There was one data value that supported using that particular value, but only one. So we realized that this was probably more looking at a parameter variation maybe than trying to model reality.

DR. DEERE: A question, please?

DR. DOCKERY: Yes.

DR. DEERE: What was the limit of hydrologic data that you got from the core? The core had been tested in the laboratory, or --

DR. DOCKERY: Yes. It was core data available from the USGS that had done the moisture retention characteristics, they had done the saturated hydraulic conductivity, but not on every piece of core, and not from every hole.

So we only had what was available from those, we only used what was available from those four holes.

DR. DEERE: Was there any in situ permeability test in the borehole itself?

DR. DOCKERY: Not that I'm aware of. I would have to check and get back to you on that, but I don't think there was.

[Slide.]

DR. DOCKERY: So to give you an idea sort of what the nominal case looked like when we started modeling, it was to have an implied flux of 0.1 millimeters per year. And we'll discuss, Maureen will discuss to some extent, why 0.1 was used as a value. But it was basically with this particular section of rock, to maintain unsaturated conditions.

Since that's what was observed at the site, then that's what was our base case, the no-flow boundaries and the water table pressure head, and this was the repository level.

There are not as many layers in here, obviously, as the hydrostratigraphy actually used, but we saw what those looked like from the earlier strat. column.

[Slide.]

DR. DOCKERY: So now, I've hopefully set up for you to some extent what the nominal case problem was. And Maureen will tell you what the results of the calculations were.

But the next two problems I'm going to set up for you are the perturbations and the parameter sensitivities which she will also give you the results on. I'll simply set up the problems for you.

First, I'll talk about the perturbations, and then I'll talk about the parameter sensitivities.

[Slide.]

DR. DOCKERY: In all cases, for the disturbed case development, the problem definitions are what were requested. There weren't any actual calculations done, except for the climate change case. This was simply setting up the problem in preparation for doing a calculation.

However, one thing we want to point out is that it's not just sitting down in a library and figuring out what a neat problem would be to do. There are scoping calculations that are required, there's a lot of give and take amongst the different participants, to decide what is the best way to couch this problem that we hopefully found within the context of one of our event trees.

So that the disturbances that were discussed include a climate change, human intrusion, and basaltic igneous activity.

[Slide.]

DR. DOCKERY: In the case of climate change, the climate change was simply couched as a variation in infiltration rates on the nominal case. And that was a factor of 10 and 50 times the nominal infiltration rate.

Also investigated was just simply the effect of a 50-meter rise in the water table.

[Slide.]

DR. DOCKERY: Human intrusions obviously become an interesting player, especially since it's so important for WIPP.

And what we worked on was developing a scenario for drilling into the potential repository block. And we did use 20th Century drilling technology, and we looked at what would happen as a result of a flooding repository or actually mechanically intersecting one of the waste containers.

We wanted, as a result of setting up the problem, to eventually calculate transport, both through interconnected fractures, a la Tom Buscheck and the people at Lawrence Livermore, and looking also at distribution of the radionuclides in a highly permeable zone, that might later infiltrate into the underlying region.

[Slide.]

DR. DOCKERY: This is a picture -- pretty scratched-up picture - of the drilling scenario, where we were heading through the drift, mechanically intersecting the container, and then either allowing the material to flow back up into the drift and then flow down through the fractures, or to flow down into a highly permeable layer and then be spread out for dissemination downwards.

Rally is going to talk about this drilling scenario a little bit more when we get into the early site suitability calculations, because we have gone farther, and we are actually working on some calculations to look at the defects of this problem here and then down into the saturated zone.

[Slide.]

DR. DOCKERY: In terms of basaltic igneous activity, we understand that Bruce Crowe and other folks are working on looking at the probability of actually having an intrusion in the area, and if you have it in the area, what's the likelihood of it intersecting the repository, and if it intersects the repository, how likely is it to intersect a container.

We're sort of looking at the very last step, assuming that that happens, what would be the consequences of such an event.

So we looked into detail at what the length, orientation, temperature, the overall effect, thermal effects would be of the intrusion in that area. How likely would it be to intersect a waste package? What would be the effect?

It's not clear that there would be a huge effect on an intact waste container. So we're looking at some of the details of what might happen if you actually envelope a container; what type of radionuclide transport could you expect; and how much could you expect as a result of this intersection; and then finally, looking at a consequence analysis as a result of all these things, all these very unlikely things happening.

[Slide.]

DR. DOCKERY: So, in summary, for the disturbed case, the perturbations on the nominal case, we calculated the results for climate change, and those will be available in a report soon, and we also defined problems for human intrusion and for igneous activity, and both of these -- well, I should say certainly the human intrusion activity is being investigated in a calculation, and the igneous activity is being set up for calculations, although the thermal consequences have also been done by Los Alamos National Laboratory.

[Slide.]

DR. DOCKERY: In terms of the sensitivity study, there were several aspects of sensitivity study investigated. One of them was to look at prior work. This was to compare the results of PACE with COVE-A and with HYDROCOIN and find out what sort of interpretation we could make as a result of the comparison. Maureen will talk about that.

We also wanted to look at the relationship between infiltration and observed saturation and sort of do the backwards problem -- given that we have such and such saturation, what kind of infiltration is required to give that value for saturation at Yucca Mountain? So there is the question.

[Slide.]

DR. DOCKERY: So we're using a I-D steady-state flow calculation, how do we match the data? What sort of infiltration rates will give us the data we have at the Mountain? Of course, this is assuming that we're using a certain model and using certain ways of coming up with these answers. So we realize that this isn't the be all and end all of the question.

[Slide.]

DR. DOCKERY: The methodology for the inverse problem was to look at eleven different layers and sample from those layers using 95 data values that were available from the site and running about 300 of the possible million or so realizations, and then restrict those realizations only to real data value and throw out the ones that came up that were not the real data values, and then accumulate the calculated saturations in each layer, and then compare those to infiltration rates at 0, 0.1, .1 and .5 millimeters per year to find out which of these infiltration rates would match the calculated saturations.

[Slide.]

DR. DOCKERY: This is my summary. So, for the PACE-90 problem definitions, we tried to draw on the experience of COVE and of HYDROCOIN, and of other calculations that have been done out there that hadn't actually been published but were within the community to try to formulate the problem that we would actually do.

As a result of that, we did develop a new hydro stratigraphy to use. We tried to integrate the different participants' efforts both in modelling, where we tried to find out what aspects of the various models produce these calculations and results, and in terms of hydrology, what data is available, there seems to be more data available out there than we gave credit for sometimes. So we'd like to really make sure that we use the data that is available to us.

The last thing is to put a more realistic source term in your field environment calculation or aspect into the calculation, and Bill will follow me up next and will talk about the source term and how that's incorporated into the transport calculation.

DR. NORTH: Do you have a question?

DR. DEERE: Yes, a question. Could I see the last one, please?

[Slide.]

DR. DOCKERY: Yes.

DR. DEERE: In the new hydrostratigraphy, in your hydrostratigraphic unit, what again were the parameters that you pulled out that go into your model? In other words, did you have difference in fracture as well as difference in porosity at the given layers compared to another?

DR. DOCKERY: Yes, that's correct.

DR. DEERE: So I presume you found you had lots more fracture flow in one case than you did on another?

DR. DOCKERY: Well, fracture flow is a product of what is your infiltration rate and what is your -- you know, there are other reasons that induce fracture flow.

But the actual divisions, the hierarchy of the divisions, was that after you've looked at the litho stratigraphic units in the Topopah Spring member versus various members, then, to break those down in terms of welding and vapor phase alterations, those were different zones, and then the last gradation was based on changes in the porosity.

But within that second level of the hierarchy was looking at fractured characteristics, so that's how those were broken down. And then once those layers were broken down, then we went back to the data and said, "What are the hydrologic values that have been obtained for those particular zones?"

DR. DEERE: Okay. Thank you.

DR. DOMENICO: So you did have a dual porosity type fuel model?

DR. DOCKERY: Yes.

DR. DOMENICO: And there was coupling between the processes so that when you exceed the infiltration rate, you --

DR. DOCKERY: That's correct.

DR. DOMENICO: Okay.

DR. DOCKERY: So is it Bill's turn?

DR. DYER: Yes.

DR. DOCKERY: Okay.

DR. DYER: The next speaker will be Dr. William O'Connell from Lawrence Livermore National Labs, who will talk about source term development.

[Slide.]

DR. O'CONNELL: Okay. I will be talking about the source term development. I'll put the title in perspective, again recalling the series of talks about the PACE-90 exercise, which was an exercise in developing a problem and in working together, as well as in calculating the results.

[Slide.]

DR. O'CONNELL: Without the problem definition, I'll talk mostly about the source term and a little bit about another subsystem, the transport subsystem.

[Slide.]

DR. O'CONNELL: Again trying to put things into perspective in the total system, I've drawn up a flow chart or a data flow chart for the total system. The engineered barrier system, which I'm going to be talking about principally, is here, and in terms of mobilizing and transporting radionuclides, we have various either partially transmissive barriers in sequence, the engineered barrier system, the near field, which may be different from the far field ambient temperature unsaturated zone, and then there's the saturated zone.

The near field environment either provides a problem or it provides a shelter for the engineered barrier material; so the near field, you know, it's part of the environment as well as being a medium of transporting.

[Slide.]

DR. O'CONNELL: Now, for the PACE-90 problem, we decided to just pick a subset of this total system to analyze, and we left out the near field analysis. So we just used the engineered barrier system and the unsaturated zone, and we also left out the saturated zone. These other barriers can be analyzed in the future. We want to reserve that possibility.

But for a start, we just looked at these two barriers and looked at the far field and near field environments as influencing them, you know, for one nominal scenario.

[Slide.]

DR. O'CONNELL: Now, for the engineered barrier system analysis, Working Group 2 consisted principally of these three laboratories who were doing the technical work and co-authors of the final report and of the conference report summary of it, which I believe is in a package provided to the Board. We worked together on this, you know, starting from the beginning, you know, What do we need to know to analyze the problem?

In terms of the six steps for doing performance assessment, we were starting really with Steps 3 and 4. You know, 3 -- what are the conceptual models that we should use?; and 4 -- what are the appropriate input data to use? So we spent a lot of time discussing, you know, what concepts and data to use, or, for the most part, selecting the most appropriate among existing concepts in data.

We decided to focus on the final steps leading to releases, and I'll show that on the next viewgraph, and look at the earlier stages as data input. We're using available data, and we're trying to be cautious, but not necessarily absolutely conservative, but being on the cautious side rather than a best-estimate side.

In this slide, we used the boundary condition for the groundwater flux, which was somewhat different from the total systems and hydrology groups. So we used what is probably an upper-bound for anticipated conditions or nominal conditions.

[Slide.]

DR. O'CONNELL: In looking at the last processes in a time sequence leading to releases from the engineered barrier system into the host rock, the processes are the waste form alteration from a solid form into water-soluble materials and the waste release from the engineered barrier system, and we grouped the types of inputs that are required into hydrology, containers properties, and the geochemistry waste form interaction, and in terms of the local hydrology, we need to know what water contact modes there are, how many of the containers are contacted by water, and then some of the parameters of these contact modes.

When the container is breached, we need to know the time distribution: When do some containers start breaching and getting wet so that they are available for aqueous transport and how long a time duration does this extend over, and in terms of geochemistry and waste form interaction, we need to know the rates of chemical alteration, the solubilities of some of their lower-solubility radioactive elements, and certain design information and waste form information, as input to some of these inputs and to the waste form model.

[Slide.]

DR. O'CONNELL: Now, taking the first of these input areas, the hydrology, first I'll look at our general conceptualization of the modes of water contact, and then, a couple of slides later, I'll discuss some of the particular types of experimental information that we were using as insights.

Now, in general, if you have water contact, it could be by seepage or dripping, where you have moving water, and there are two different modes, depending on how many breaches there are in the container, whether it -- just near the top, you might have a bathtub mode, or it can flow through and out the bottom.

But in addition to the moving water modes, we have the static modes; in particular, any static pathways will allow diffusion, and this could arise by rubble filling up the air gap, which is part of the design, or by a rock block moving in and touching the waste package.

Now, I'll go over these contact modes in pictorial form.

[Slide.]

DR. O'CONNELL: In the flowing or seeping water, I've shown a somewhat shortened picture of a waste container. Now, the drift, the emplacement drift and so forth are high above here.

So, in terms of parts of the engineered barrier system that are active in transport, there is just this air gap on a pedestal and the container itself. If water does get through breaches, it could trickle over the waste materials, dissolving some of it, and then trickle out the bottom.

[Slide.]

DR. O'CONNELL: Now, showing the bottom part of a container, just to illustrate the diffusion mode, it's conceivable that, you know, over thousands of years, the rock wall could weather or silt could seep in, and somehow part of this air gap could get filled with rubble material, and that, coupled with breaches in the container, could allow pathways for water.

Now, the rubble, like the host rock, would be partially saturated with water, and the rough surfaces of corrosion holes could retain a water film, and you would need water film contacts through to the fuel.

Now, we don't know these fine, gritty details yet. So, we decided just to analyze the problem using just this proposed rubble zone.

Now, the types of experimental information we're using as insight to this problem, first, have been prototype heat-up and cool-down tests, a test in G tunnel at the Nevada test site in an unsaturated tuff of slightly different properties, and there have been laboratory-scale heat-up and re-wetting tests done by Livermore using rock blocks, and at least, in Washington University, there have been some laboratory tests on diffusion coefficients in partially-saturated gravels.

These tests were done both with things like granite, which has essentially zero porosity, and if it's partially saturated, that's represented by our water film on the surface, which can provide a diffusion path, or by tuff, which is porous. It tends to soak up most of the water to interior pores, so you have even less of a diffusion path.

Experiments have been done showing three to five or six orders of magnitude reduction in the effective diffusion because of the reduced path available.

Now, in terms of theoretical analyses, first there's coupled fluid and heat flow analysis using the tuff program from LBL and the V-tuff program at Lawrence Livermore Lab.

This was looking at the heat-up and cool-down and partial saturations, using an equivalent continuum model, and this has been followed on by studies of non-equilibrium single-fracture infiltration by Buscheck and Nitau at Livermore.

If you have, somehow, a concentrated water source, they find that that can penetrate over quite a large distance through fractures before that water is soaked up into the porous component of the rock, and this is getting close to the actual borehole-scale fracture flow calculations that we need to get down to the single borehole scale. So, we are looking for further progress on a finer scale there.

Now, what we actually did in this first round of calculation, as part of the cooperative working groups, is to use simplified models of these two, moving water problems and one diffusion problem with the rubble zone, and we also used the heat-up and cool-down calculation on a repository scale, which shows that different waste packages cool down to the condensation point of liquid water at different times, and that spreads out the water contact times.

Now, water flux variability across the repository, on a spacial scale, will be as important as the temporal variability. So, we want to highlight that as one of the future needs.

DR. DOMENICO: That last statement means that you used .5 millimeters per year as contact?

DR. O'CONNELL: The .5 millimeters per year was the area-wide average downward flux, and near a waste package, we used an area about twice the diametrical cross-sectional area of the borehole as a catchment area. So, it's a little larger than the direct amount of flux into the borehole.

DR. DOMENICO: Okay. thank you.

[Slide.]

DR. O'CONNELL: Now, in terms of the spent-fuel groundwater chemistry, one of the very interesting experimental results that has come up in the last few years through multi-cycle tests -- these were done at Pacific Northwest Laboratory under contract to Lawrence Livermore Lab as part of the Yucca Mountain project, and these tests used successive batches of water on the same batch of spent fuel, and in the early, first or second batch, some surface components of some elements dissolved in high amounts, but in later batches, where that surface material was no longer in the source, the dissolution of highly-soluble elements, such as Technetium, Iodine, and Cesium, did not stop, but it kept up at a fairly steady rate, even though the uranium in the container was reaching an apparent solubility limit.

Now, this is in contrast to expectations from single-batch tests that the uranium reaching a solubility limit might stop the alteration of the uranium oxide fuel matrix and thereby slow down the dissolution of other materials, but so far, this does not seem to be the case over tests which have gone on for over 1,000 days of water contact time.

Now, the conceptual models to explain this continuing release could be either forward alteration of the uranium oxide matrix, which contains the bulk of the radioactive materials, or it could be crack and grain boundary release, and we've used the forward alteration as the more conservative of these two assumptions, and we're looking at experiments at two different temperatures, and we used the higher rate from among those two experiments.

So, this is not strictly conservative. A third experiment could get an even higher rate, conceivably, and I just wanted to mention that, because of this uncertainty about what process is giving this phenomenon, additional tests are planned, and directions and test procedures have been worked out, and we will be going forward to resolve these processes.

An additional new element which was not used five or six year ago, at the time of the environmental assessment, was the solubilities of the actinides. These have been observed now, experimentally, in the spent-fuel tests, and there have been calculations.

The calculations vary sometimes by six orders of magnitude, depending on the input conditions -- oxygen availability, temperature, pH -- but they're all quite low, and so, the solubility is more controlling than the uranium matrix alteration was.

[Slide.]

DR. O'CONNELL: Within this newer concept, the three groups of us settled on this set of concepts and data to run an analysis. We used these representative radionuclides. The technetium, iodine and cesium isotopes are the predominant, highly-soluble materials after a thousand years. Carbon-14 might be added to the list, that could either stay in solution or go into gaseous forms. That was analyzed separately.

But, in terms of being representative, these are the highly-soluble ones, and we have low and high retardations, which make a difference in the diffusion model.

Now, Neptunium-237 was selected as a representative actinide. It's of some interest, although, it is not anywhere near as high curie concentration as the americium of plutonium isotopes. But, since it only has one predominant isotope in the inventory, in terms of mass, the results are easy to interpret. The other elements will be added in the future -- may add some other complicating factors, which are easy for the computer programs to handle, but not so easy to explain graphically.

So, it is just a representative. It has a low solubility, like the other major actinides I mentioned, and it has a medium retardation factor, which is not of critical importance.

[Slide.]

DR. O'CONNELL: Now, we had a few other cautious assumptions in the analysis. First, the container breach, we decided to breach 300 years after getting wet, because we didn't have a -- we didn't know of any good way that it could fail, so we just assumed that it did fail.

We neglected the fuel cladding as a protective cover to the fuel. As I mentioned earlier, the diffusion --only part of the diffusion zone was treated.

[Slide.]

DR. O'CONNELL: Now, here is a representative result from this first analysis of one case. This is for the three different water contact modes for the low solubility Neptunium. After a rise time of one to three-thousand years, they settled down to a steady state, and the steady state is at about 10 to the minus 10 curies per year. So, that's at about 10 to the minus 10 fraction per year of the inventory inside a waste package. That's about two curies inventory and two times 10 to the minus 10 curies release.

Now, of course, this looks steady because we're assuming a bounding value that doesn't change with temperature. Somewhat coincidentally, the values are fairly close for these different water contact modes. I will explain that in a later slide.

The major contrast from this slow steady state is represented by technetium here and iodine-129 show a similar behavior. For the parameters -- input parameters we used, the release rates are a little bit below one part in one-thousandth per year of the inventory -- the peak release rates. And the duration is just of some thousands of years -- about 6,000 years here.

Now, this -- underlined, four waste packages. And I would like to also underline -- we were using fairly high values of the parameters for the water flux and for the spent fuel alteration rate. So, all this tells us, so far, is that a -- you know gratuitous, conservative assumptions tell us something, but they're not good enough. We have to go back and analyze the problem more carefully.

DR. DOMENICO: Wouldn't that be a violation of NRC regulation right there?

DR. O'CONNELL: If all of the packages were wet, and if our parameters -- if our input parameters are correct, then, yes it would be a violation. But, we don't believe our input parameters are accurate. They were selected as being probably bounding.

DR. DOMENICO: If that were true, could you have such a violation and still meet the total release requirements at the boundary, even if you were?

DR. O'CONNELL: At the boundary of the EBS?

DR. DOMENICO: Of -- well, in your case, it would be the water table. We have to consider the water table the boundary.

DR. O'CONNELL: Well, for th EPA's --

DR. DOMENICO: Yes?

DR. O'CONNELL: -- total system requirements, if the unsaturated zone water transport works, as we expect it to, then the releases will be well within the EPA limits.

DR. DOMENICO: Even though we've exceeded the individual releases from the barrier?

DR. O'CONNELL: Yes.

DR. DOMENICO: Yes, thank you.

DR. O'CONNELL: But, you have to keep in mind that, under some parameters or scenarios, the water transport might not work as well, and then you would like to be able to fall back on the engineered barrier system as a barrier.

[Slide.]

DR. O'CONNELL: Now, for cesium, these objective results are the same, but the diffusion result is stretched out over a longer time and its peak is down by about a factor of ten because of the retardation in the diffusion zone in the rubble. So, that's for cesium, with a high retardation.

[Slide.]

DR. O'CONNELL: Now, summarizing these results. This type of source term behavior, and it's expressed in equations, also, this is really information for the total systems simulator models that can just sort of summarize, parametrically, the performance of these different types of radionuclides.

So, for technetium and iodine, the release can be of relatively short duration, and the limiting factors are either the matrix alteration rate, which is a chemical interaction thing, or hydrological factors, such as in the bathtub model, how fast the water turnover time is, due to additional input; or in the flow-through, what fraction of the fuel is wetted by slow dripping of water.

Cesium is a little bit different. And Neptunium is representative of a long, slow release, which is fairly steady-state, and is dependent on the solubility of that element.

[Slide.]

DR. O'CONNELL: Now, here, Holly Dockery mentioned sensitivity analysis. So, I put in one -- the simplest example of how we can do a sensitivity analysis, in this case, by inspection. The low solubility elements, such as Neptunium, in terms of the flow-through models, its release is proportional to the water flux Q. In terms of a diffusion model, it's proportional to a product of the diffusion parameters, the diffusion coefficient, the water-fill porosity and so forth.

The rise time depends on other things. But the plateau here is fairly simple. In all cases, it depends on the solubility limit, C zero, in this notation.

Now, the sensitivity and important parameters in the other water contact modes and for other radionuclides is a little bit more complicated. But, in the conference report, which is in the recent international high-level waste conference, and in the package provided to the Technical Review Board, we wrote down our qualitative conclusions about what parameters are important.

These were derived just from inspecting the models. So, it's almost as easy as the case that appears on this viewgraph.

[Slide.]

DR. O'CONNELL: Now, the waste form alteration rate is important in some of the release modes. That depends on other inputs which were not captured in our first cycle of model. But they were captured in our conclusions and discussion section, where we say we have to look to these other factors the next time through.

So, for instance, the waste form alteration rate depends on the groundwater chemistry, which depends on the rock mineralogy. And it depends also, indirectly on the water flux, because the -- if some of the groundwater chemicals are use up in complexes, then water flux would be the only thing that would keep the groundwater chemistry from changing during the interaction.

[Slide.]

DR. O'CONNELL: Similarly, the effective diffusion coefficient in a partially saturated rubble, you know, we picked the value that looked toward the high end of partially saturated cases but in fact that depends on the degree of partial saturation which depends on the average flux of ground water, which we have learned from the far-field hydrology experts.

These types of dependencies will have to built in either into our model or into models providing input to our final stage models.

[Slide.]

DR. O'CONNELL: Now I was asked to say a few words about the inputs to the transport subsystem. Maureen McGraw will be presenting results of calculations out of this whole mobilization and release to the water table but I want to say a few words about the inputs to this transport.

In addition to the hydrology, which is one of the elements of the transport, there are other inputs required to have a transport.

There are physical or hydrodynamic processes and the parameters of those processes, you know, for set longitudinal dispersion, molecular diffusion and various types of matrix fracture interchange which could move some of the dissolved radionuclides from the fast-moving fracture location to the matrix locations.

There's the chemical factors or chemically-based factors, the retardation of some radionuclides, which would have the effect of slowing down the bulk transport and slowing down these exchange processes as well and we have to look at other processes which might get around to the retardation process such as colloidal transport.

The source term is just a stable input to this calculation and then the output is time histories.

[Slide.]

DR. O'CONNELL: Los Alamos National Laboratory is the primary source for the Yucca Mountain potential site specific transport processes. You know, both the retardation and chemical processes and also the mixing and dispersion processes, at least on a small scale.

[Slide.]

DR. O'CONNELL: In summary, I've talked mostly about how this one group from three laboratories working together has selected the source term concept and results and we have provided the reference source term to the total system people and we have done some sensitivity analysis, much of which is still ongoing and I just said a few words about the other inputs for a transport subsystem.

That concludes the talk. Are there any questions at this point?

DR. NORTH: Let's keep it going. We've got one more speaker before the break.

DR. DEERE: Just one question. You left the breach in curve 300 years after emplacement?

DR. O'CONNELL: 300 years after the container gets wet with liquid, water. For some period the container will be above the boiling point and will be a dryout zone in the rock nearby but after that has cooled and recovered --

DR. DEERE: That's about 300 years?

DR. O'CONNELL: 300 to 1000 years, depending on location within the different layouts of spent fuel packages and then 300 years after that, we're assuming that corrosion has had its effects.

DR. DYER: Our next speaker is Maureen McGraw from Pacific Northwest Labs, who will talk about flow and transport results from PACE-90.

[Slide.]

DR. McGRAW: While he is changing the slide over, I would like to apologize for the copy of the overheads. There's two things wrong with them.

I handed them off to somebody as I was leaving town last week and you actually have a longer version of the presentation than I will actually be giving so I will not cover all the slides that are in here.

The other thing is that they were printed dark. Another copy is being sent and will arrive tomorrow morning. Copies will be provided for the review board and I am not sure that we will be able to get copies for anybody else, but we will try or we can get them to you later.

I do apologize for that.

[Slide.]

DR. McGRAW: In the outline there, now at the results, and I'm from Pacific Northwest Labs and I will be presenting a summary of all of the results from the PACE exercises. I am by no means going to try and cover all of the results.

Part of the results are summarized in the Sandia Report which will be out in June and there will be a second volume that will cover some of the results that I am also talking about. If you want more specific information that I can't answer for you, I would refer you to this document or to the Volume 2 that will come out at a later time.

[Slide.]

DR. McGRAW: This is a general outline of what I am going to talk about today.

I would like to start with one dimensional analysis, go through the results, do a comparison to the COVE exercise and then look at the inverse problem that Holly described.

[Slide.]

DR. McGRAW: Here is the repository and listed here are all the codes that did calculations for these bore holes. These were all one-dimensional calculations.

The one that I am going to talk about today is I'm going to talk about the TOSPAC results but I wanted you to realize that LLUVIA, TRACRN, NEFTRAN also did calculations for these bore holes.

[Slide.]

DR. McGRAW: This slide has a lot of information on it.

What it shows is it shows both the nominal infiltration and the climate change infiltration, which is the .1 millimeters and .5 millimeter infiltration after 10,000 years for two different bore holes, for G-1 and G-4.

G-4 was the borehole located within the repository boundaries.

The slight gray line that goes across is where the repository is located, just to orient you.

[Slide.]

DR. McGRAW: The layer that contained the higher hydraulic conductivity, these are plots of saturation so as we go this way on the plot, we are going towards a saturated system. As we go this way it's more unsaturated. There is less water in the pores.

This layer that we see here in G-1 is an indication that hydraulic conductivity layer that Holly alluded to that we put in. What it did is it simply moved the water out very quickly from that layer.

When you contrast it to G-4, you see that G-4 has a larger layer that has not as high of a contrast.

Template!

[Laughter.]

DR. McGRAW: Well, maybe that's one I should have been pointing but -- so that's what it shows.

Now the red is the nominal infiltration of .01 that was used. The blue is the .1 and the yellow is the .5.

What I would like to point out is that when we get to the .5 millimeters per year infiltration we are nearly at a saturated system. One of the things that comes out of this analysis is if you say that the system is saturated, we are now inconsistent with what we see out in the field. This gives us some indication that there is either something wrong with our model, there is something wrong with the input, whether it is the infiltration or the homogeneous layers or something. This is a signal to us that this is wrong.

[Slide.]

DR. McGRAW: We went ahead and we used those results for doing transport studies. Now I am going to show you several transport results because Bill took a lot of time to explain to you the source term.

This would be kind of what Felton was referring to earlier. We took the information from working group two, which did a very mechanistic model, and we took it in tabular form and we fed it into our models.

Whether our models are a submodel or whatever follows in the thing, we are using that very specific mechanistic input and we wanted to see whether that specific input when we varied it according to the parameters, they varied, really made a difference in this total system calculation.

The red line represents 10,000 years and the kind of purplish line represents 100,000 years and the blue line going across represents the location of the repository.

This figure goes to 800 meters. The water table is actually down at 730.

The scale on the next three slides are going to change. I apologize for that. I scanned these in and I did not have the raw data to replot them on the same scale, so the scale here is 10 to the minus 5th curies per meter, cubed.

What we see is that the source term is released right around the repository and with longer time it extends. Now this is for a base case Moist-Continuous -- Bill actually called it a diffusional case.

Then there was three variations done on this base case.

[Slide.]

DR. McGRAW: The first was a high diffusion coefficient was used. If you look at it, the high diffusion coefficient read into the TOSPAC results. It really didn't seem to make a lot of difference.

[Slide.]

DR. McGRAW: Now, when we change to a high reaction rate, now I told you that the scale is going to change. Now, instead of going to eight here, it goes to four, and it's eight to the minus four curies per meter cubed instead of eight to the minus fifth.

We see two things. The first is that at 10,000 years, we see the peak, and at 100,000 years, we see it kind of spread out. The reason for this is in this case, you get a very fast release into the environment, and so it has more time to diffuse down and move with the groundwater system than in the other case.

When we go to a high fuel alteration rate, again the scale is now getting smaller. It only goes to 1.6 now. We see that the amount released decreases. So the high fuel alteration rate actually decreases the amount of radionuclides that we see released.

[Slide.]

DR. McGRAW: There were two other scenarios that Bill talked about. One was the bathtub and one was the flow through. Again, we get the same sense that we did here, where we get the fast release, and then we see it diffuse over time. Again, the scale is slightly different; this goes to five instead of eight, as in the first one.

But this just gives you a sense of, depending on what the source team people are going to give us as input, we can see different things in our model without having to do the very specific source term analysis within some of the hydrology codes.

[Slide.]

DR. McGRAW: What I'd like to do now is I'd like to take a step back and go back and say; All right: We did these calculations. We got these hydrology results. Well, have we learned anything from when we did the COVE exercise? The COVE exercise was a one-dimensional exercise.

So what I have plotted here is I have the higher infiltration rates of .1 and .5 plotted. COVE was not done at .01, so we couldn't compare it. I'd like to remind you of the figure that Holly showed with the stratigraphy columns where, for the COVE exercise, she showed the Ortiz stratigraphy, which had seven different layers, versus the PACE stratigraphy, which had a lot more layers.

The other thing was is that they didn't span the same distance. This one started -- the

COVE started at 1250 and the PACE started at 1200. So we do have slightly different systems.

Did we see anything really different? Well, we see in the COVE exercise that we have a high hydraulic conductivity layer up here. Well, that layer was not included in the PACE exercise as it would be above what is shown here.

So if you ignored that part of it and look at the rest, you know, this is about 90 percent. That's about 90 percent. I mean, as a rough comparison, we really didn't see a lot of difference.

So the question is, is we went through a lot of trouble to, you know, put together this new stratigraphy. It takes a lot more computational time, a lot more gridding, to do a more detailed stratigraphy, and yet, at the higher infiltration rates, it really didn't seem to make a difference.

Now, I did say at the higher infiltration rates. When you get to the lower infiltration rates of .01 millimeters per year, it starts to make more of a difference on what your layering is, but if we're at the higher infiltration rates, we really don't see the difference compared to what we did before.

[Slide.]

DR. McGRAW: All right. We're talking infiltration rates. Higher infiltration rates -- you know, we don't see any difference. Well, is a higher infiltration rate realistic?

So the sensitivity study that was done by Jack Gautier at Sandia was this inverse problem of, "Can a one-dimensional steady-state flow calculation match the in situ data available?" I mean, are we even in the right ball park with what we're doing?

[Slide.]

DR. McGRAW: So what this figure shows is this is a saturation line. The circles and the triangles represent data from two USGS reports. These are not all the data points that were used in the inverse problem; I just put some of them on there.

What was done is a simulation was run using real data from each elevation. It was sampled, and then a simulation was run to see if we couldn't match the results. Now, this particular blue line is the .01 millimeter per year infiltration. You can say, "Well, you know, you picked that one because that's what you want us to believe, and, you know, so it really looks nice." Well, what he did is he actually plotted them up a different way.

[Slide.]

DR. McGRAW: This is at an elevation of 1,082 meters. So I'm only looking at one elevation that's in the Topopah Springs. He said, "Let me plot saturation versus a cumulative probability for the different infiltration rates. So now I'm going to look at no flow, .01, .1, .5, and the in situ data."

These black lines here found what was observed in the site, and the red line is a plot of the actual data that was measured by the USGS.

If we look at the .5 millimeter per year case, it falls outside of the bounds for what was observed. So based on the assumptions that went into this inverse calculation -- these were all done in TOSPAC, so this is all one-dimensional -- this implies that there is on chance, given current information at the site, that we could have an infiltration of .5 millimeters per year.

Well, that makes me feel a little bit better because our model said that we would saturate at that level, and so, you know, here is a thing that supports that this may not be realistic.

At .1 millimeter per year, if we come over, we see that there's about a ten percent probability of getting an infiltration of .1 millimeters per year. So that is within the range of possibility that we could, based on the assumptions, get that.

When we go to .01, we now see that there's a between 0 and 60 percent probability of observing an infiltration of .01. That means that when we took the sample data and calculated it, it is reasonable to come up with an infiltration of .01.

It is also reasonable, between 40 percent and 100 percent, to come up with a value that says that there is no infiltration, that there is no net flux into the mountain. You could probably carry this further and actually go into negative fluxes, and that also might fall within the realm of possibilities for the site.

[Slide.]

DR. McGRAW: That concludes what I'm going to talk about on the one-dimensional analysis. There are a lot more simulations done on one-dimensional analysis, and I again refer you to the Sandia document.

What I'd like to do now is I'd like to look at the two-dimensional analysis, look at one code's results, and then do a comparison with HYDROCOIN.

[Slide.]

DR. McGRAW: Here is the same repository boundary. There was actually two different cross sections done for the two-dimensional. The NORIA/FEMTRANN calculation looked at the cross section between G-4 and UE-25A, and the calculation I'm going to show you, which was done by SUMO, goes between G-4 and G-1.

[Slide.]

DR. McGRAW: I was told, when someone was looking at these slides, that geologists assign darker colors to welded units and things like that. I'm an engineer, and I don't adhere to that, so there is no color relationship to these.

This kind of tanish unit represents the Topopah Springs' welded unit. The brown area here represents the non-welded.

Now, you will notice that these calculations were done from PACE, and there are only four layers. This was done to test what would happen if one of the layers was non-continuous across the domain. It was a very coarse sketch just to see whether or not it would make a difference.

The other two-dimensional calculation that was done, you know, looked at the tilted beds and included most of the layers.

This layer in here is the Calico Hills, and then this layer down here in the corner actually represents the Prow Pass. The boundary conditions are no flow, and the constant flux was input into the top. The water table is down at the bottom.

[Slide.]

DR. McGRAW: This is a saturation profile. I have the repository highlighted in red. This layer that was chosen had a hydraulic conductivity an order of magnitude less than out of the other layers. The other layers were fairly close.

This was done deliberately to see whether or not in the two dimensional simulation, this would make a difference. What we saw, applying that constant flux across the top, was that we started to get ponding above this layer.

I'd like to say two things about this simulation. The first is that the ideal thing would be to obtain a steady state solution. With a non-linear problem like this, it's very difficult to punch a magic little button that says steady state and get the solution, so what we actually do is we step through time and try to come to a point where we believe the situation is steady state.

What we use as our criteria is that the flux in the top is equal to the flux out the bottom. This causes a problem when we're actually starting to get ponding above here, because then what we have to do is we have to keep stepping, waiting for this water to move around the system until we see the constant influx coming out the bottom.

I would say that this system, although we finally did reach a point where the outflux was equal to the influx, is probably not at a real steady state, because we do pond the water up here beforehand. When we got to a higher flux rate of .1 millimeters per year, this just gets worse, and we just see more ponding and more saturation. It actually goes saturated above this layer, and, you know, pretty much makes the whole system a lot more saturated than is seen in the real data.

[Slide.]

DR. McGRAW: we went ahead and we took the .1 millimeter per year flux, even though it was fairly saturated and more saturated than we expected, and did the release profile into the repository to see. Now the criteria for the PACE calculations was whether or not radionuclides would reach the repository.

I'm showing you an Iodine 29 because --

DR. REITER: You said, reached the water table.

DR. McGRAW: The water table is down here at 730 feet.

DR. REITER: You said the criteria is whether radionuclides reach --

DR. McGRAW: Whether or not we got radionuclides to the water table. What we see is that even at the higher infiltration rates that we had, and the fact that we had the site fairly saturated, we did not get a release to the water table in these simulations.

There's a lot of simplifying assumptions in here, but this kind of gave us a feel for what would happen. Well, you know, we saw this ponding and stuff and we said, well, they did two dimensional analysis back in HYDROCOIN. What did they see?

[Slide.]

DR. McGRAW: This is the HYDROCOIN results at .5 millimeters per year, and now we're looking at something different. These are actually the flow lines in black that all end up down the side.

What they saw in HYDROCOIN was that here's their high hydraulic conductivity layer. They saw something similar. With the higher infiltration rates, whether due to ponding or this layer, that all the water moved to this lateral boundary.

It ponded at this lateral boundary and then it went straight down the side. This has implications in two respects: one is that all these layers are homogeneous.

It could be a factor of the fact that we were using homogeneous layers. The other effect is that we've assumed a no-flow boundary. Now, if this boundary is the Ghost Dance Fault, I'm not going to argue whether or not the Ghost Dance Fault can serve as a no-flow boundary, but it might, or it might be a fracture that you get flow into.

That may or may not be realistic. But if you're simply cutting off your domain and looking at it, this boundary is questionable and maybe we should be representing it differently.

[Slide.]

DR. McGRAW: Now I am coming to the part of the presentation that everyone always wants to talk about, except me.

[Slide.]

DR. McGRAW: I would like to take a moment -- this phrase to me represents what I want to say. "When I use a word," Humpty Dumpty said in a rather scornful tone, "it means just what I choose it to mean, nothing more, nothing less."

I find that in working with travel times, that depending on who you're talking to, people may not be talking about the same thing. So what I'd like to do before I show you any results is, I'd like to take a couple of minutes and I'd like to define for you, how I'm defining travel time.

[Slide.]

DR. McGRAW: I want to define what is the definition of groundwater travel time. The travel time is inversely proportionate to the velocity, and in the saturated zone, it's just the Darcy flux over the porosity. It's a fairly reasonable quantity. In the unsaturated zone, we have a problem, though, because now the Darcy flux is dependent on the saturation and we're dividing it by the saturation.

So, what happens is, the saturation gets really low and our system dries out. Well, you have to apply L'Hospital's rule if you're integrating it and so it's not really clear what happens. If it's steady state, you know, your velocity would go to infinity and your travel time, in a sense, would go to zero.

So, this quantity isn't very well defined. this is, in general, how we calculate it. This is calculated for a particle. What I'd like to do is, I'd like to compare the results of what happens when you measure the travel time for a particle versus when you measure the advective velocity associated with the transport of contaminants.

[Slide.]

DR. McGRAW: This study was actually done at Sandia by Kaplan, Klavetter and Peters back in 1989. What they did is they looked an unsaturated column where they put water in the top and measured the flow through. In their computer code when they did the simulation, if they tracked the particle as I just described the definition as the Darcy flux over the saturation, what they find is that they get a travel time of 1.6 years.

If they measure the advective velocity associated with a contaminant moving through, they get a million years. These are two very different numbers.

The numbers that I'm going to show you in the next slide are particles. They are not associated with a mass. You do have places where if you have fracture flow or something, you might get faster velocities of the particle than you would of the radionuclides that are being measured.

[Slide.]

DR. McGRAW: These are the results for both the TOSPAC one-dimensional and the SUMO two-dimensional runs. For the TOSPAC runs, at the four different wells, they're all approximately either six years or a million years. The variation with the accuracy of the code is, you know, close enough.

When we came to the two-dimensional analysis in SUMO, we also saw that the values were approximately a million years. They were slightly lower than the one-dimensional analysis seen in TOSPAC. It's a factor of two, maybe.

At the higher infiltration rates, what we saw is that the travel time actually dropped an order of magnitude.

Was there a question?

[No response.]

[Slide.]

DR. McGRAW: So now, I'd like to go on to the important issues identified in PACE. I've kind of alluded to them as I've been presenting the different results. The issues identified were the material properties -- looking at the matrix, is it homogeneous or heterogeneous? Some of the factors influence radionuclide transport -- modeling artifacts. What is real to the system and what is part of our model -- the incorporational probabilities.

[Slide.]

DR. McGRAW: This, again, just represents the comparison between the old stratigraphy and the new stratigraphy. We have these two columns and we have several considerations. We have; how long is it going to take us to run this model? How long am I going to have to crank it out on the CRAY? How complex is the system? What are the travel times and what are the precision?

These are often a tradeoff. It goes back to what Felton was talking about. If you want to look at very specific mechanisms, if you want to look at the very small, fine, detail, you're going to sacrifice some of the simulation time and increase your complexity.

But if you're at the higher level where you're looking at a system wide model, you may want to give up some of the complexity so that you can do more runs, look at more variables. There is no easy answer or solution to this, but this did make us aware that complexity doesn't necessarily give us a new answer; that just because we make it more difficult, make it to be more realistic, we don't necessarily get more insight into how the system operates.

[Slide.]

DR. McGRAW: For transport of the radionuclides, you know, the people who did the PACE-90 flow modeling, they picked up a source term from the people at Lawrence Berkeley or Lawrence Livermore, and really didn't take any time to look at it.

Well, I don't know how many of you caught onto what Bill was saying, but they did all their analysis at .5 millimeters per year. So all the source terms were calculated using a flux of .5.

Some of the hydrology results were done at .1 and .01. So we were inconsistent in the information that was being used.

The people doing the hydrology model just took it and assumed, well, this is fine. We didn't give any consideration to some of the near field and the far field hydrology that was taken into account, and that was realized and is being considered in some of the site suitability and other analysis that's going on now.

The other thing is, as we go from scenario to scenario, whether we're doing the nominal case, we go to the human intrusion cases, and the vulcanism case, our source term is going to change. And we have to be aware of that and how that's going to affect our results.

And finally, the retardation and the dispersion also must be consistent with the transport results that we're using. If anything came out of this exercise, it's that we need to communicate more so that the people who generate the source term are aware of what the people doing the hydrology are doing and that the people who are doing hydrology don't have to become experts in the source term, but have an understanding of what it is.

[Slide.]

DR. McGRAW: In terms of geometry, we have done one dimensional, two dimensional, and the work that will be coming out in Volume 2 of the PACE calculations actually includes a three-dimensional calculation done at Los Alamos by Kay Birdsell and Ken Eggert.

They did an interesting analysis of their runs. They used the same computer code, and then they ran it on one dimension, two dimension, and three dimension, and they measured this travel time.

What I did is, I normalized the values to one dimension, so as you go to two dimensions and three dimensions, this means that it's decreased 64 times from the one-dimensional result.

What this brought up in their analysis is that as you go from one dimensional to two dimensional to three dimensional, we really get different travel times. There's different results. The water can now flow around that low hydraulic conductivity layer. It doesn't necessarily have to pond on it if it can go forward or backward into the problem. So that we have to be sensitive to the dimensionality, in that this one-dimensional simulation doesn't really give us the most conservative answer about the system.

[Slide.]

DR. McGRAW: Some of the modeling artifacts that I talked about, we see some unrealistic ponding that's inconsistent with the field data.

One of the problems with the project in the earlier stages is that the people doing the field work didn't talk to the people doing the modeling, as Holly alluded to. You know, there was a trip where they got a lot of the modelers out to the site for the first time. People who had been modeling the site for four or five years, you know, finally went out and saw the site. What are these rocks that we are modeling?

It provided a lot of insight for the people who participated on the site, and it also made them realize that there is some information that's available that isn't numbers, that there is a lot that you can get out of just looking at some of these. Some of the soft data, you know, if we go out there and we look at it, are there things that we can see that we can use as inputs to our models that we can't put numbers on? And the answer is yes.

Some of the things we need to consider, as I just said, the dimensionality. Some of the heterogeneities. This again is getting to that very detailed mechanistic model. If you go into a lot of detail and try to map every little pumice chunk that you get in there, you know, that may not be realistic for a large-scale simulation, but it may be done on a smaller scale.

And then the other thing is the boundaries. You know, do we need to reconsider how the boundary condition is handled?

And, as Jerry Boak alluded to in the beginning, and Rally will touch on further, there is a study going on this year that actually examines the boundary condition, both the lateral boundary condition and the top boundary condition, assessing whether or not this uniform infiltration across the top boundary is realistic.

[Slide.]

DR. McGRAW: The other thing that we realized is that there is information that can be obtained from using probability analysis that we weren't using. We had some limited data that could be used to create parameter distributions.

These parameter distributions, whether this is porosity or hydraulic conductivity, could be sampled and fed into the model to obtain one realization. In a sense, this nominal case that we did was just that we in a sense sampled one parameter from each distribution for the different ones, and plugged it in and ran it once. In theory, you could run this hundreds and hundreds of times. And for certain things it may not provide more information, but other times, it may provide you with insight that you don't have right now because you don't have enough data, or it can provide you with different ways to look at different combinations that you might not be thinking of, that the computer will generate.

The thing that you have to be careful of is if you let the computer pick things at random, is that the computer might generate something that's not realistic, and doesn't exist on this planet.

[Slide.]

DR. McGRAW: And so finally, I'd like to conclude with what we've accomplished with the PACE exercise. We've developed a new hydrostratigraphy. We learned about the effects of multiple layers, and at the higher infiltration rates the new hydrostratigraphy really doesn't help us, or doesn't change the results. It does matter at the lower levels. We realize the value of hard and soft data.

I would like to think that this is one of the greatest accomplishments, you know, to finally get some of the modelers out to the site, talking to the field people, and communicating in that way.

We identified the need to be more systematic about how we did our work. And Rally is going to talk about that a little bit more next.

We did compute one realization. How I caveated it was saying that this is not a simulation, this was not meant to be a realistic simulation of Yucca Mountain, but it was a first cut. It got people working together; it got an analysis done that can be used as a starting point to continue and do more realistic calculations.

We also enhanced computer codes and techniques. A lot of times this doesn't get a lot of credit. You know, people think, well, you can just go use a computer code and you know it's going to give you these results.

By finally getting together and working with the other modelers, we were able to look at the way our program was set up. And while this person was using upstream wading for averaging their hydraulic conductivities between blocks, and they seemed to have faster times, well is that something I could program in real quickly and see if it helps me? There was dialogue like that on very specific ways of well, how do you write Richard's equation; are you solving the theta-based or the H-based or the mixed form of the equation?

And that type of dialogue was really helpful in enhancing the models and the way that the models represented the system.

And that's all I have to say. Are there any questions?

[No response.]

DR. NORTH: No questions? Then time for a break. Let's try to hold it to 15 minutes. Back here at 4:35.

[Brief recess.]

DR. NORTH: Let's reconvene.

[Slide.]

DR. BARNARD: Okay. I'm Rally Barnard. I'm going to talk about ongoing and future PA activities. My talk is divided into three parts: Evolution and development, a summary of what we've head before; a discussion of the systematic methodology; and then some high-falutin' words for what we are going to be doing next.

[Slide.]

DR. BARNARD: I'm going to stress the PACE and the allied areas and talk about the calculational aspects of PA only.

[Slide.]

DR. BARNARD: Maybe I can make shadow birds here also or something.

From Holly's talk and from Maureen's talk and Bill's and so forth, we've learned that the problems that we did for PACE were defined in prior work. We learned that the modeling techniques that we have used similarly came from some prior work. The results that we found in PACE were consistent with those which we had determined before, and we extended our knowledge of performance assessment, both the knowledge and the tools and the skills that we were trying to develop.

[Slide.]

DR. BARNARD: Some of the things that we learned from PACE -- some of the good things that we learned, in a nutshell, I think that we can say, based on the results that we came up with, there are no releases to the water table, i.e. the accessible environment, as we defined it. There are no releases to the water table, without looking at fast paths.

Now, admittedly, we didn't go into -- particularly much, go into the details of what might constitute a fast path, in terms of the mechanics of what was one in PACE, but I think you can see, from the results that we came up with, that we did not get releases, at the water table for what has been shown.

The models that we used, were not particularly sensitive to the number of layers that were used. So, therefore, having more layers in the PACE stratigraphy, compared to the COVE and HYDROCOIN stratigraphy, was not necessarily more realistic. Maybe we better put quotes around that word realistic.

Certainly, having a wide variety of participants, having a number of background strengths in a number of disciplines was extremely valuable in allowing us to be as efficient and to learn as much as we could from each other's experiences.

It's also very important for us to realize that PACE-style calculations, calculational exercises, where we have a problem that we're trying solve, we are trying to exercise our tools, our knowledge, our interactions and all, they're very important. In order to be able to come up with a credible argument -- a technical argument for licensing, it's going to be necessary to continue to do this style of analysis.

DR. NORTH: Could you give me an idea of what PACE costs -- what was the expenditure in running what we've just saw? How much of that was computer time, and how much of it was people time?

DR. BARNARD: We had monthly meetings of -- in Las Vegas, of 20 to 30 people. So, $500 -- $6, $700 to fly people out there every month for nine months. Didn't use the Crays too much, which are several hundred dollars an hour. You can do a lot of that on a VAX. So, there was probably several thousand dollars there.

It was. I haven't bothered to add it up in my head, but you're getting the picture that you're talking about a couple of hundred K probably.

DR. NORTH: So, the computer time cost was a very small fraction of the cost of assembling all these people and getting them to work together? Is that the picture I should get?

DR. BARNARD: That, I think, is correct. Yes.

However, in contrast, we could always talk about HYDROCOIN, which burns $750 K, on those analyses which Maureen showed. That was only done by one person. You didn't have a lot of travel involved there. He was able to spend that all by himself.

DR. DYER: Let me comment a little bit on that, Dr. North.

From the DOE perspective, I'm guessing that I probably spend about 30 percent of my budget last year on this -- probably three to four million dollars.

DR. BARNARD: You should have told me that.

DR. NORTH: Within that, could you give an estimate for how much of that was for people and how much was for computer facilities and the like?

DR. DYER: No, it's all buried in the overhead, from my perspective, whenever it gets to me. I can't give you the breakdown there. I can find that out for you, if you want.

DR. NORTH: I think it would be interesting. The benchmark I found useful in my kind of consulting business -- is that with all the talk about computer time, it's usually somewhere around the travel budget, or less, and usually less than 10 percent of the overall cost of when you've added everybody's time up. However, I can see where, if you get into Cray computers and lots of runs, you might easily switch into another mode.

DR. BARNARD: Some of the lessons from PACE, on the negative side, were that the way the analyses were structured, they didn't particularly stress the models that we were using. When you're talking mostly in the matrix flow regime, you find that the differences in the codes, many times, which are reflected in their handling of fracture flow, didn't get exercised, in this case.

So, we weren't able to particularly differentiate among the different codes and the different models that we used, with the PACE nominal case. The analyses weren't particularly tightly structured. To a certain extent, this is related to number four down here, in that in order to get the job done, people took -- used as their models, for analyses, prior runs. So, one group would do things this way, and another group would do things the other way. And it -- things were not as tightly controlled as they could be.

I think, when you look at the PACE SAND document, this will be abundant evidence, because you'll see that each person's -- each group's presentation of the results is completely different. We were unable, combining these and these, to be able to put out a consistent presentation of saturation versus elevation and so forth, like that.

The other thing is that we did use deterministic parameters. We made one run. We recognize the necessity of reflecting the uncertainties -- our expression of uncertainties in the parameters. So, we recognize the need to change that and do better in future work.

[Slide.]

DR. BARNARD: Being a positive sort, here are some recommendations for improvement.

The first is that one of those minuses was that we weren't too structured and so by using a systematic methodology which looks at problems and takes into account more of the aspects that it's necessary to look at, such as making sure that not only the far-field but also the near-field interactions are included. That is an example of a better-structured problem and in order to do that we had better be --

DR. NORTH: Could you go into a little more detail on what went wrong there? How much time was spent at the front-end defining the systemization of the methodology and structuring the cases that would be run?

DR. BARNARD: Not a lot of time was spent in doing that. What we were presented with on December 14th, 1989 was a request to produce an expected case calculation, disturbed case calculations and sensitivity studies. We were requested to produce the expected case calculation -- notice my language, please -- the expected case calculation by April of 1990, the disturbed case by July of 1990 and the sensitivity studies by September of 1990.

We had four months then to start from square one with a problem for the big part and then three whole months to do three more perturbations of the problem and then three months to tidy it up with yet more perturbations.

Therefore, the amount of time we allowed ourselves in contrast to the way we are working now was very minimal.

DR. PRICE: But you didn't do the expected case?

DR. BARNARD: The expected case was redefined in view of our feeling that we would be unable to produce an answer for an expected case but we did feel we could produce an analysis of a nominal case, which is a component of the expected case.

DR. NORTH: I think one of the take-home messages from that experience is that you need to define the order of 25 to 35 percent of the project in time to really structure what it is you are going to do and how you are going to do it before you start trying to run a particular case.

DR. BARNARD: Very true, yes. In fact, I may have to give you five dollars as a straight man again the way I do many times for other people, because I think you will see that coming out of some of the future things.

I want to step back to this slide and point out that you have hit exactly upon minus number four here, which is the aspect which caused us to get going on PACE the way we did and essentially have it turn out as we have illustrated to you.

Well, moving along to the next slide, talking about the improvements, in addition to coming up with a systematic methodology which will address exactly the point you were raising, we need to expand the analysis scope to make sure that the models that we use are more complete, that we reflect the fact that we do not have a good handle on the parameters that we are using and reflect the fact that computers are changing, people are getting smarter about algorithms and so forth and we need to take advantage of that in our development of performance assessment analyses.

[Slide.]

DR. BARNARD: Okay. Now I'm off to Section II. What I want to talk about is a method for use with these calculational exercises, PACE-style calculations of a six-step method. It would be six except for a head -- thank you! Now you can go back the other way!

[Laughter.]

DR. BARNARD: I'm going to go through these in detail.

[Slide.]

DR. BARNARD: Step 1 is scenarios. Now before I push anybody's "hot button" on using this particular word, I will define exactly what I mean by "scenarios" and what I mean by several of the terms that I use in here.

The discussion that went on this morning was quite important and quite illuminating as to some of the differences and some of the misinterpretations that people can raise by incorrectly using terms.

What these things called scenarios are, are going to be the consequence of identifying the features, events and processes which describe a system such as a repository system in unsaturated rock in a place something like Yucca Mountain, for example.

I have abbreviated these as FEPs and that's what Felton and others will be calling them.

What are these things? A process I am defining as an ongoing something-or-other, it continues like groundwater flow through unsaturated rock.

In contrast, an event has a defined starting time and a stopping time, like a volcanic eruption.

A feature could be something like the bedded nature of the tuffs there, which cause diversion or lateral flow or some other aspect modifying the behavior of the processes and events.

These need to be represented in some kind of systematic fashion and there are a lot of ways of doing it.

One that I am going to talk about are what I am calling and defining "event trees" but there are binary trees, fault trees, matrix methods and so forth like that.

Some of these other methods rely on different principles to achieve the same display of the logic. You might be looking here at barriers or residence time to describe the interaction of FEPs with one another.

You might have parameters -- one type of event tree might severely lump together the parameters and in another they would be fairly explicitly described.

What I am going to talk about are event trees which are representing a process which is really quite young. We don't know a lot about the features, events, and processes going on at this site, so our method to try to address this is to include in detail as much information as we can in our event trees to do the minimum amount of lumping that we can to try to put it out on the table so that we can discuss it and find out what is right and what's wrong, because the essence of doing this is iteration.

With any logic diagram, event trees or anything else, in order to be able to arrive at something which represents consensus or some recognition that it is the right way to do it, you have got to review it. You have got to redraw it. You have got to consider alternative processes. You go back to square one and you continue doing it until you decide that you aren't going to improve it any more.

[Slide.]

DR. BARNARD: What happens if you start with a premise and you go to a consequence is that every logic diagram which starts with a premise and it goes to the same consequence eventually, if you do enough iteration, should arrive at the same consequence, although it might express the intermediate steps in some different fashion, so our deciding to use event trees and our using a great deal of detail in them and our including both features and events and not separating out the consequences and so forth like that is the way we have chosen to do it. However, it does not represent the "right" method nor necessarily the only method that we are going to use but it represents mostly a tool, a talking point to try to elicit from other folks, from experts exactly how the features, events and processes are to be considered.

Scenario identification involves taking an event tree and using one event tree to express one class of features, events, and processes. For example, nominal flow is an example.

[Slide.]

DR. BARNARD: If you look on the wall over there you can see the entire nominal flow event tree which is part of the Barr and Barnard report, a Sandia internal report, setting out the initial information that we have on these four event trees, four classes of scenarios that we are initially addressing.

The nominal flow is one, basaltic intrusion is a second, human intrusion, and tectonic events. A nice thing about this is you don't have to separately consider "disturbed conditions" because these three classes of event trees address disturbed conditions.

You have the nominal flow and you have disturbed conditions and this isn't a separate case. It's just another group of event trees.

[Slide.]

DR. BARNARD: Here is an example of an event tree. This one is for basaltic volcanism. That one is posted on the back wall over there in its entirety; this represents just a fragment of it. I'd like to point out that in your handouts, this is Slide 11 and it resides just where you would expect, between slide 23 and 25.

[Laughter.]

DR. BARNARD: If you turn to between 23 and 25, --actually, if you look at 11, you'll see No. 24, so when we get to there, I'll tell you to look back, okay? So, that's where you will find a better picture -- for those of you in the back of the room or with weak eyes -- of what this is. It starts when basaltic volcanism occurs, and the path through here, which is illustrated, follows down here. There are dead ends shown, but that is only for the sake of artistic license. For every one of these boxes with an ellipse, there is yet more event tree which is illustrated on the back wall over there.

What this shows is the basaltic intrusion occurs, the intrusion acts directly on the repository, in contrast to acting indirectly. There is no mechanical waste interaction with the dike. Over here, there is mechanical interaction where the waste is enveloped by a dike and so forth like that.

But the process is to try to identify and put down on paper, every single feature and event and process which is necessary to describe the condition of a basaltic volcanism leading to the release of radionuclides at the accessible environment.

[Slide.]

DR. BARNARD: Step 2 in the six-step method is to talk about probabilities. Here we want to estimate ranges of probabilities of occurrence. The important word here, particularly at this stage of the development of performance assessment is the word estimate.

We don't necessarily have good models that we're using, and so it would be unreasonable to expect that the values for the probabilities that we have are going to be any better. But we have several resources for trying to determine what the estimates of probabilities can be.

This might be the back of an envelope, this might be a reigning expert and this might be information from the SCP, for example. There are a couple of concepts of probability which I want to stress.

In addition to talking about an absolute probability, the probability of basaltic intrusion occurring, for example, which is a number which conveys the sense of probability the same as that of throwing dice or an occurrence, you can also broaden the sense of probability for those cases where the probability of its occurrence is one, for example, groundwater flow, and say, let us now use the concept of probability to represent the partitioning between competing events. So, the probability of occurrence in this case represents the partitioning among competing events for the occurrence.

For example, flow through fractures or lateral diversion through bedded tuffs might be two choices which occur at the same level in an event tree, and you need to determine what's the probability or what's the partitioning of that occurring in either one of those cases. This probability estimation is a way of doing that.

[Slide.]

DR. BARNARD: The first two steps are going to result in event classification. After the scenario and probability screening steps are done, at the first cut -- and I want to emphasize that this is the first cut -- we should be trying to determine which are high consequence events -- and example might be an igneous intrusion entraining nuclear waste, or we might also be looking for high probability events, for example, the nominal groundwater flow condition.

Why should we want to do that? Well, because we have to prioritize how at first we are going to be using our time. Eventually, all branches in an event tree are going to have to be addressed, all probabilities are going to have to be assigned, and we're going to have to, quote, "know everything about the site."

Eventually, the information will have to be contained in some comprehensive fashion. For starters, we need to figure out what we should be expending our energy on, and, clearly, high consequence or high probability event seems like the best place to start.

But there's another category and that is high public perception events. These are ones where you might not be able to justify it on its consequence or you might not be able to justify it on its probability, but it's made a few lines in the Las Vegas Journal Review or something like that, and so it's worthwhile to address.

[Slide.]

DR. BARNARD: Step 3 of the six-step method is to look at conceptual models. Here again, because there can be some misunderstanding about what is meant by a conceptual model, I wanted to define my terms.

Conceptual models, I am considering to be those which express processes. You can globally express a process, for example, the conceptual model for one-phase water flow through unsaturated tuff.

You can get more detail. You can say that the conceptual model I want to use is that of the composite model for the saturation relationship for unsaturated flow in a fractured medium.

In addition to expressing your processes, you need to identify the assumptions that you're talking about. Here, I've classified three in a descending hierarchy. The site assumptions are possibly the most global where now you want to say that here you are going to say that you will consider diffusion and you will consider advection when you are talking about the transport of radionuclides.

A physical assumption that you better not take for granted is that Darcy's Law obtains, because it is quite possible that you can come up with regimes where you would be less than confident saying that. Lastly, a modeling assumption is getting down to using a code and you are making the assumption that you can get away with using uniform zones or that you can get away with a certain boundary condition and so forth, but, boy, it's necessary to state that.

In addition, remembering what an event tree looks like, it's possible to describe a process by more than one alternative. So, it's necessary to identify exactly which alternative you are using in the process of setting up the event tree and the path that you are going to use.

At this point, I would like to give you my definition of what we consider a scenario to be.

Given the fact that we have an event tree which describes features and events and processes starting from an initiating state to a final state, we define a scenario to be a path through that event tree for which we have provided enough detail that a calculation can be made.

A calculation may either be analytical, back-of-the-envelope, or it may involve 30 hours of Cray time. But in order to be able to do that, we need to have sufficiently describes out conceptual models, our parameters, and all the processes going on, and when we do that we consider that we have a scenario.

[Slide.]

DR. BARNARD: Step four are parameters.

I don't think I'm giving away any great secrets to say that all natural systems, even those which are well-characterized, are going to end up being variable.

When one takes a measurement, several measurements, they are going to fall within a range of values. That seems like a silly thing to say, but if you turn the statement around, and say, if I take a measurement, and I don't have any information to the contrary, there's no reason that I should not believe that that measurement is not a valid measurement, and should be retained in my set of data. Therefore, it falls in a range, and it helps to define a range. However, no finite number of measurements is going to absolutely define the entire range. The range that you observe with your measurements will be less than some absolute range which you would be more comfortable in using in describing the data, in other words, the entire variability of the system.

If you feel like trying to pick an expected value, or a best value, it's going to be uncertain, but the only thing you can say is that it better be within the range that you are establishing for your values.

What to do about a situation like this? Well, you have a range, and you have an expected value, and you may have a feeling for the variation of these values within the range. And the best thing that can be done is to use a systematic method to try to quantify your uncertainty, to try to be able to make a maximally unbiased statement about the uncertainty of your data.

And the way this is done is by methods which describe parameter values and distributions of them, and by methods which will allow you t o sample from the distributions in a method which maximizes your expression of the uncertainty in your parameters.

[Slide.]

DR. BARNARD: Okay. It's plug and chug time in the six-step method. Here are the calculations.

One thing that may not have been done yet, but which is necessary in any analysis, is that the modeling assumptions, such as boundary conditions -- we've seen no-flow, we've seen other statements of boundary conditions -- these must be stated, too, because, as Maureen pointed out, they can have profound effects on the results that you get, and if you don't realize what you've done, you can draw some pretty silly conclusions, in other people's eyes, I guess.

What are calculations? Calculations are going to generate the outcomes from the inputs and the assumptions that we have. This is the important thing to realize, that this isn't necessarily an answer, this is merely an outcome. The outcome reflects on the inputs and the assumptions

Now, if you have used parameter distributions, it's going to be necessary to make multiple calculations. Because of having distributions, you'll have multiple realizations, and because of making multiple calculations, you're going to have multiple outcomes.

This is certainly of use, because the multiple outcomes are going to allow you to get some information about central tendencies of the outcomes, and there may be certain measures of outcomes, such as groundwater travel time, for example, which are highly sensitive outcomes, and can give you information about the parameters which you have used, and the assumptions, the models that you've made to determine which of any of those are important and which are unimportant.

[Slide.]

DR. BARNARD: Lastly, no job is complete without doing the paperwork. And that's the interpretation.

What we need to do here is examine the outcomes in terms of the inputs.

At the stage of performance assessment, that performance assessment is at for the Yucca Mountain project, it is very difficult for us PA people to say that we are giving anybody answers, that we are giving anybody statements of performance of the site.

The kinds of things that we can say are that the outcomes that we've come up with are significant in terms of indicating that certain assumptions and parameters that we have made are important to the sensitivity of those outcomes. It looks kind of circular, and it is at this stage.

The effects of parameters and conceptual models are some of the things you can examine, looking at your outcomes. And you can find out information about parameter sensitivities.

The best thing that we can do is to tell the guy who gave us the data exactly what we did with those data, and what results we got.

If nothing else, it will make them happy to know that their information has been used; but even more importantly, it might provoke them to look at something in a different light, come back with more data, stress a different aspect of further data-gathering, and it will all help to develop yet more information for us.

If we do repeated analyses, using recognizable assumptions, or changes from assumptions, and we come up with consistent answers, we're going to increase the confidence that we have in our analyses.

And interpretation is essential for model validation and code verification. This is the essence of doing that.

[Slide.]

DR. BARNARD: Some of the benefits that you're going to get from doing iterative PA analyses, I picked three of them here, but I could probably put up many more slides. But we can use this for evaluating design alternatives, providing measures of site suitability, prioritizing testing, issue resolution for the SCP, site suitability, and so forth.

A lot of things are all going to depend on calculations of the type that I'm describing, and the systematic method that I'm describing for doing it.

[Slide.]

DR. BARNARD: Okay. The last part of this is to talk about the ongoing, what we're going to be doing next.

The six-step method is being used for several items. I've listed four of them here. And of those four, I'm going to talk in detail about two.

I'm going to talk about detailed site suitability calculations and the boundary condition study. The other two are necessary to make the slide look balanced.

[Laughter.]

DR. NORTH: I would particularly like to put on the record that we will be very interested in hearing about the other two, at the appropriate time, particularly number three.

DR. BARNARD: Number three, ESF design, and test and evaluation prioritization. I agree.

This is an ongoing item, the site characterization plan issue resolution. That will be with us forever. And sometime if we run out of fire drills, everybody will get back to that one.

[Slide.]

DR. BARNARD: Okay. Site suitability analyses, in a little more detail.

There are five items here. Three of them represent analyses that we're doing: the fast paths due to nominal flow; human intrusion; and basaltic igneous activity.

In addition, two others are, I won't necessarily call them ancillary, because Bill O'Connell is sitting in the audience, but the near-field interactions and the saturated zone modeling are both analyses that we want to develop which can stand along and can actually possibly act as modular analyses to be fed into yet different analyses here, which require information from the near field interactions and from the saturated zone.

[Slide.]

DR. BARNARD: How does the fast-path problem interact with what is going on in the larger world?

Here are the calculations that I'm going to be talking about. And the immediate issue is the early site suitability evaluation.

To address the early site suitability evaluation, several key geo-hydrology issues have been raised. These issues are being addressed by the detailed analyses and, in turn, these issues are going to address the early site suitability evaluation.

But in addition, what we plan to get out of this, primarily because we are using the systematic six-step method, is we're going to enhance our PA techniques, we're going to know more; we're going to be able to work smarter and better, or whatever they say in the ads, about doing PA calculations in general based on this experience that we're gaining here.

In addition, the key geo-hydrology issues relate to SCP issues, and many other T&EP, many of the alphabet soup items will benefit from the knowledge we get from the geo-hydrology issues.

[Slide.]

DR. BARNARD: Let's look at the fast-path analysis. What is listed here are four of the six steps that you should be able to identify in the six-step methodology.

We have used the nominal flow event tree, as it is posted over there. We have traced through the nominal flow event tree to identify a specific fast-path analysis or to -- a fast-path path. How is that for a good statement? A path through the event tree describing fast-paths. We have talked to some of the experts and started to get information on probability of occurrence. In this case, it's the partition nature. But, we have developed some of this information. We have chosen four conceptual models to look at, and we have done a pretty rigorous job on selecting parameters, which -- the ranges for which reflect our uncertainties in their values.

[Slide.]

DR. BARNARD: Here's a picture of the problem. If you turn back to slide 11 you will see this. The problem has been set up, for the benefit of the numerical modelers, to be roughly 300 meters tall by 10 meters wide. Within it, the primary structural feature is a fault, which is similar to the Ghost Dance Fault. There are four layers used. As I pointed out, one of the lessons was that complex is not necessarily better, and again, to help the Cray jocks, we cut down a little bit on the computation that will be necessary by using four layers.

The water table is here. We have a highly welded layer here and then a couple of others. This fault here is fed by an interconnected fracture system. Immediately above the problem domain is where the repository lies, so the implication here is that contaminated water from the repository would flow into the interconnected fracture system, pass over to the fault and run down the fault. This is our conception of a fast-path to reach the water table.

Incidentally, the problem doesn't stop at the water table. We hand-off, at the water table, from the unsaturated zone calculations, to the saturated zone module, and that will carry it the five kilometers to the regulatory-defined accessible environment.

An example is shown here of one nuance in the data reduction that we did. We discovered by looking in hole G-4, that by and large, the ratio between vertical fractures to horizontal fractures was three to one. So, we have shown here, and we're modeling this with three vertical fractures to every one horizontal fracture in the interconnected fracture zone.

[Slide.]

DR. BARNARD: Some of the conceptual models that we looked at -- I mentioned that we looked at four of them. This will list them. The problem was done in three basic ways. Looking, first, at the interconnected fracture network, everybody has to do -- everybody who's doing the problem does a baseline case, as was illustrated in the prior slide. But, then when we get to doing sensitivity studies, people are going to be looking at different ways of expressing the interconnected fracture network. One of them is to explicitly model the fractures.

In this case, that means that, if we decide to 10 micron fractures, they are going to model in 10 micron fractures -- they're going to grid up 10 micron fractures. If they're going to do three of them, as shown, they'll grid up three of them. If they're going to do 10 of them, they'll do 10 of them.

The other method is going to use one gridding of the fracture network, and just vary the fracture hydrologic properties, as expressed by, for example, the composite model or the dual porosity model. Express the fracture hydraulic properties, in that fashion, for one gridding.

The second, fundamental conceptual model difference is going to be, instead of using an interconnected fracture network, in that same zone where I showed the interconnected fracture network, use a heterogeneous matrix zone, where now, that zone will contain a region in which there are randomly assigned values for the saturated -- for the hydraulic conductivity, and to see whether that is going to result in a fast-path which will feed the fault.

Both of these one, two, three, methods are going to be done numerically. They're going to chug away on the VAX and the CRAY for large amounts of time and presumably come up with their results, based on numerical simulations.

The last method down here, the nonequilibrium fracture matrix interactions method, draws upon the work that has been done at Lawrence Livermore, but makes sufficient simplifying assumptions that it can be done analytically.

This method will consider continuous fractures through that problem domain. It will consider, similarly, through the saturated zone, continuous fractures, and look at a bounding worse case calculation, using this conceptual model for trying to describe what would happen, as a fast-path analysis that way.

[Slide.]

DR. BARNARD: Well, that detailed the fast-path calculation, but there are some other aspects to it. One issue is the source term, which is going to use the PACE-90 radionuclides, which have been cited before. But, in addition, we're going to add two isotopes of plutonium, two isotopes of americium. This being for the amount in the inventory, and this being for the mobility of americium. So, now we have eight radionuclides instead of four being transported.

We're making quite certain that the near-field interactions are consistent with the fast-path model. So, the near-field people, who produce the source term for us, have used information and conceptual models, which result in a source term, which has the same conceptual model -- same assumptions in it as are being used for the far-field transport and the far-field flow.

To do that, the waste packages were considered to be exposed to varying amounts of fracture water and this was the mobilization method.

Well, I mentioned that the saturated zone does need to be treated, in order to do a complete performance assessment analysis from the repository to the accessible environment. What we hope to do is to produce a module, a stand alone calculational feature, which will allow us to plug in, at the front-end of this module, the output from unsaturated zone calculations, and do the transport to the accessible environment and come up with an answer which is consistent and for which we have a good understanding of the processes, so that questions related to the saturated zone maybe can be suppressed, compared to those of the unsaturated zone, for example.

The module -- the work that we're doing for the fast-path analysis, is going to cover -- to start with two different regimes of the saturated zone. One is in the tough saturated zone, and the other is in the carbonate aquifer. In each case, we're considering transport, including retardation. The results, as I speak here right now, are being chugged-away at at various laboratories.

I guess I forgot to mention, but the participants in this -- in keeping with the cooperative, interactive work that we have done in the prior PACE and in COVE, the results are being done by Sandia of Pacific Northwest Laboratories, Los Alamos, and Lawrence Livermore

Laboratories. Let's see, I think I remembered everybody. Oh yes, at LBL also, excuse me -- Lawrence Berkeley Lab.

These results will be available. The numbers and the graphs will be available by the end of the month. The write-up will be starting soon thereafter, and it's our intention, by the end of June, I hope, to have our first narrative out, associated with this.

As I said, as one of the minuses for the PACE, this time, our presentations of the graphs are all going to be consistent, because we're going to plot them all at Sandia.

DR. NORTH: When you have had time for the interpretation, I think we would be very interested in hearing what those results are.

DR. BARNARD: So would we! So would a lot of people.

[Laughter.]

[Slide.]

DR. BARNARD: Okay. Now taking a great leap forward from the fast path problem to the human intrusion problem, this one is a different animal, in that we are not doing as intensive calculations on it. In fact, we are barely doing calculations at all, but I will get to them.

Our strategy for handling this problem is threefold. The first thing we do is, we say to ourselves: "What's the probability that anybody would go out to the site and mess around for any reason whatsoever? What could they be looking for?"

So step 1 is to do a resource evaluation. This is underway. Is it's very preliminary, and I don't have anything to report in the way of an answer to that.

The second question is: "Assuming somebody is there outside messing around on the mountain and is drilling holes, what's the probability that the drill bit is going to hit a waste container underground?"

This is done by just comparing the ratios of number -- the area of the holes that are drilled to the locations of the waste containers underground and figuring out the probability.

What we so far have had to base our analysis on is the EPA estimate of the number of bore holes which is likely to have been -- is likely to be drilled at a site such as Yucca Mountain. There is some open question as to whether that number is a valid one, but based on that number, we have come up with an estimate for this, and so that part of it has been done.

Once we determine that people are out at the site and they've drilled a hole and they've hit something, what's the consequence of what's happened? What's the consequence of their having this interaction?

And what we're looking at is two particular analyses in this case. Both of them revolve very heavily around the source term only.

In one case, we're assuming that we exhume -- that's hard to say those two words together -- the contents of one waste package straight to the surface. Somehow the contents of one waste package has been entrained, taken up, dumped on the surface. That defines release to the accessible environment -- boom! So the source term people are going to report to us what the consequence of hauling that one waste package -- the contents of that one waste package to the surface is.

The second analysis is going to say: Let's suppose this driller did his thing, drilled down, due to Murphy's Law, just absolutely skinned past the container, but went all the way down to the watertable, decided that this wasn't a place for him, and pulled out. And in the process of pulling out, you know -- swoosh! -- gets a little bit of suction, and the contents of one waste container now fall down this bore hole and go -- plop! -- down in the saturated zone, so you have bare metal, bare waste, sitting in the bottom of bore hole directly in contact with water in a saturated zone.

What are the consequences of that? That can be directly modeled by using our saturated zone module to see how much is going to be hauled out to the accessible environment. So that's the second consequence analysis.

These two are going on, and they will all be put together to contribute to the early site suitability evaluation for the site suitability core team.

[Slide.]

DR. BARNARD: The other main problem that is being addressed is igneous activity, and this consists of three considerations of probabilities and one of consequence.

Now as we have mentioned before -- and I believe you folks on the Board have actually been addressed by Bruce Crowe -- the reigning expert on probabilities of igneous activity has talked to you guys, and we, too, are going to take his information and try to come up with probabilities of occurrence for igneous activity intersecting waste packages.

We feel that this is going to be a pretty small number, and so the consequence analysis will not be as high on our list as some of the other ones would be, but we have prepared, as part of the PACE analysis, a consequence analysis, and so it came be done. But we hope to dismiss this problem by showing its low probability.

[Slide.]

DR. BARNARD: Well, the analyses we've done really can't be done in isolation. In order to come up with a total system evaluation of the site, which will tell us whether it meets EPA standards, we have to include all available analyses. This is the total system analysis.

These three that we have done here, we will have consequence, and we will have probability for them, but we will have those independently. It will be necessary to combine the separate outcomes to get a measure of the total system performance.

The other issue that we are going to try to do as part of the early site suitability is work is, by the end of the calendar year, to come up with a total system analysis and to give a report or some kind of document that illustrates our estimate of the total system performance of the site.

It will be necessary, in order to be able to do this in a reasonable amount of time with a reasonable amount of effort, to simplify these results, and I'm not telling you anything you haven't seen previously today, that the detailed calculations have to be simplified in some fashion in order to be able to roll them up.

This issue will be talked -- or will be addressed by Mike Wilson next.

[Slide.]

DR. BARNARD: The one other thing I want to talk about, the second great bullet on the slide of what we're doing as ongoing PA, is to talk about the boundary condition studies. The boundary condition studies are going to draw upon field data. They're going to draw upon alternative conceptual models. They will be looking at the physics of the site, both in terms of seeing what the site tells them and what we can predict about how the site should behave, and it's all going to be rolled up in a great interactive fashion, and hopefully from that, we will at some later time be able to do more realistic calculations. Hopefully, it will be the building blocks for doing these more realistic calculations.

[Slide.]

DR. BARNARD: Well, okay, to summarize what I've talked about so far, our PA effort has been demonstrated its ability to learn from prior work. We don't run around in isolation. We aren't a bunch of guys in white smocks or white suits -- maybe that's a better choice of terms -- doing silly problems, but we try to apply them to where they are most needed and to try to draw upon our previous experience.

We now have a well-defined methodology for doing calculations, and we certainly have well-defined near-term tasks, which we feel will be very rewarding for us and for the sponsors who have requested them.

The methodology that we have developed will be useful for future PA calculations, and I think it will form the basis for most of the calculational efforts that we do from now on.

Thank you.

DR. NORTH: Dr. Price?

DR. PRICE: Is no consideration being given to a scenario, a certain-conditions scenario, involving any aspect of the Szymanski hypothesis?

DR. BARNARD: Yes, it is. Actually, I should have mentioned a couple of other things.

There were two scenarios that you might have picked up that were missing. One is under the field of tectonism. I left it off there completely. Tectonism is being investigated. We are going to develop a report similar to that which has been produced for the nominal case, basaltic volcanism and so forth. And within the tectonism event tree, the question of seismic pumping is directly considered in as many nuances as we can think of.

Secondly, what was also missing from anything I listed was a scenario, quote, for climate change, because we aren't handling climate change as a separate scenario. We consider it a boundary condition on every other scenario that you see there.

You can have a basaltic intrusion, and you must take into account that the ambient conditions might be considerably wetter or considerably drier at the time that the basaltic intrusion occurs.

So we can't separate that out. It's part of what has to be considered when we consider every other scenario, every other event within the event trees.

DR. DOMENICO: I have a comment, I think.

I think in regard to PACE-90, which was your ongoing and future activities, I really think PACE-90 has got one major flaw that suggests that your ongoing and future activities may not be -- well, let's say, may not be useful, the results may not be useful. And that major flaw is the assumption that all the flow is taking place in the matrix.

Now that may not be an assumption that you've made, but that's what's happening in the model, because when you get --

DR. BARNARD: Correct.

DR. DOMENICO: -- infiltration greater than 10.5 millimeters per year, it goes to saturation.

Now I feel like I've stepped in a time warp and I'm back six years when I was fighting with USGS over travel times on the order of millions of years, but that's basically what you have built into that model, whether you like or not.

Now the advective transport should be a first-order effect. Retardation should be a first-order effect. Dispersion is small. And the source loading term should be a first-order effect, but you've taken the source loading term right out of the picture, because you will not permit the material to move to the watertable over the 10,000 years.

So I think, when you talk about fast path analysis being a special case you're going to look at, I think that should be the general case you look at, and you should replace that equilibrium flow model with a non-equilibrium flow model that considers the interaction between the matrix and the fractures.

Some of us think that the one good thing about Yucca Mountain is the very low infiltration rate. Some of us think you may have very large travel times, but that may not be as important.

You have not given yourself any chance at all to demonstrate that, because you insist upon using that flow model that keeps all the flow in the matrix, so you will never demonstrate that, and no one is every going to believe it. I won't.

DR. BARNARD: I can't deny what you have said at all for PACE, but the site suitability part that we are doing directly addressed that, and only as special cases has it considered the composite model.

I don't know if I can get back to the slide -- I won't bother getting back to the slide -- but I will tell you that we have gone to great lengths to exercise many different models of fracture flow by many of the different participants. That is the stress of this, that we are looking at fracture flow.

So it is really incorrect to assume that we're mired in talking only about the composite model as the main mechanism for water flow. That just is not true any more, particularly in the ongoing work, particularly specifically in the site suitability work that we are doing.

DR. DOMENICO: Yes, but you the fast path was a special case. I think it's a general case. I think it's fast path, low infiltration.

And I think a more suitable flow model would attempt to incorporate that, to give this project the chance to demonstrate that the whole key is the low infiltration rates, and not necessarily the speed at which it reaches the water table.

But we could argue about this forever. I just, I've been listening to PACE-90 all afternoon, and I thought I had to make that comment, because I've stepped into a time warp. I heard this six years ago.

DR. BARNARD: I agree with you. And you will find, if you look at the nominal flow event tree, that fracture flow is not considered a special case, it is right up there in the same color ink as any other flow process, and it happens to be the one we're modeling.

I'm actually sorry if you considered, if you interpreted my statement that it's a special case. It is the pre-eminent case that we're looking at now.

And what I did say is that I consider it to be an element of high public perception, not necessarily of high consequence.

So maybe that is where you might have misinterpreted what I said.

DR. NORTH: Let's go on.

DR. DYER: While Mike is getting set up here, let me go ahead and introduce our next speaker. That is Mike Wilson of Sandia National Labs, who will talk about total systems analysis.

[Slide.]

DR. WILSON: I am supposed to speak very briefly about total system analysis, what it is, why we do it, and the sorts of things that we are working on right now.

[Slide.]

DR. WILSON: This is where we are on the afternoon's program.

[Slide.]

DR. WILSON: All right. By total system analysis, what I mean is an analysis that includes all of the subsystems of the disposal system -- the waste form itself, the waste canisters, other aspects of the engineered barrier system, the geologic system, both saturated and unsaturated.

Many times, more specifically, by total system analysis, we mean a comparison against the EPA total system requirements in 40 CFR 191.

Now, the EPA requirements in 40 CFR 191 are stated in a probabilistic manner. They define a performance measure, which I will call the normalized release or the EPA sum, and the limits on that performance measure are stated in a probablistic way. They say likelihood of less than such and such.

So the basic idea of that is that you have a lot of input parameters and models and whatnot, and for each one, there isn't some particular value. You have a distribution of possible values, because of uncertainty, and that uncertainty translates to an uncertainty in the outcome, which is the normalized release.

[Slide.]

DR. WILSON: The uncertainties in the input parameters are of several different kinds, that I have listed here.

You have spatial variability; you have time variability; you have uncertainty because of having only a few measurements of various quantities; and you have uncertainties in some cases about what the proper process models are.

[Slide.]

DR. WILSON: This is the old modeling hierarchy back again. It's been pretty well beat to death so far today. But I wanted to reiterate a couple of things.

Down here at the bottom you have the very complicated big codes, and those are the best kinds of simulations that we can do right now, but unfortunately, it's not uncommon for them to take 20 hours of Cray time for a single realization. And that makes it rather difficult to explore your parameter space, and get the kind of probabilistic information that you need for comparison to the EPA standard.

Because of that, we're forced into the more simplified categories, and which one of these simplified categories you use depends on the tools that are available and also on what your objectives are for a particular study.

For some kinds of processes, we have a lot of sophisticated tools available, and by cutting these very complicated models down to lower dimensions and single-phase and that sort of thing, you may be able to get something that will just run in a couple of minutes, instead of in a lot of hours.

So some kinds of problems you can address using still the sophisticated models, but in a restricted way. Other ones have not been studied as extensively, and so you are in a situation of doing something that might almost be a back-of-the-envelope kind of calculation.

Now, I want to emphasize, or at least state, that so far we've never done a truly comprehensive, systematic, total system analysis. The event tree analysis that Rally talked about earlier is the beginning of such a systematic approach to studying the total system at Yucca Mountain, but so far, at the present time, as far as total system calculations, we're in more of an experimental mode in which we try out different things, trying to determine what the important processes are.

[Slide.]

DR. WILSON: For the rest of the talk, what I want to do is go through an example of total system calculation. What I did is I made three different kinds of calculations, and for each one, developed a probability distribution of the outcome, the EPA normalized release, and then combined together into a total probability distribution that represents the effects of all three.

Now, obviously, this is a restricted set of assumptions. It doesn't include all the things that we know might be important. But, it will serve to illustrate some important points. One is the concept of alternative conceptual models. The nominal conditions means conditions more or less the way they are now at Yucca Mountain, i.e. no volcanoes erupting or anything like that; but perhaps allowing for increased infiltration because of weather conditions or that sort of thing.

That has been split into two conceptual models of how the flow takes place in Yucca Mountain now. I'll talk more about it later.

The other process that is being included in the example calculations is a simple model of exploratory drilling. It is important to note that this is not meant to be mutually exclusive with nominal conditions. You have nominal flow conditions and you have drilling and they're both ongoing at the same time.

Another important thing that you can see from this example, is the modeling hierarchy. Because the calculations are done at different levels on the modeling hierarchy. We have a lot more sophisticated tools available for the composite model than we do for the others. So, this calculation here is a relatively sophisticated calculation, using detailed flow and transport codes. This is -- and these two are more simplified models.

[Slide.]

DR. WILSON: All right. Let me start with the composite calculation. This is what the results look like. Let me talk a little bit about what this means. The composite model has been alluded to already. Basically, what it is is an equivalent continuum type of model, in which the flow, in the rock matrix and in the fractures in the rock are in equilibrium, so that they move together through the two media. You can see that, with that model, you have pretty low releases.

One thing I should -- I don't want to forget to say is that none of these calculations include the gaseous releases. From what we know, the Carbon-14 gaseous releases are probably up here somewhere, and we're working on including that in the next go-round. But, on these here, it has not been included.

So, anyway, with the composite model, you can see that there is low releases.

[Slide.]

DR. WILSON: Let me elaborate a little bit on the problem set up for this. The problem set up has Yucca Mountain represented by four one-dimensional columns, which are distributed more or less randomly around the repository area. The stratigraphy was taken from the three-dimensional model of the thermal mechanical units that was developed at Sandia several years ago. That's what's been referred to variously, as the COVE stratigraphy, or the HYDROCOIN stratigraphy. Anyway, it's the older one, and not the more recent PACE stratigraphy. The main reason for that is that it's simpler.

There are six distinct units that are used in the calculation. There are a number of parameters that have to be defined for each unit, leading to a total of something like 150 variables that are sampled for the Monte Carlo simulation. So, that's a lot to deal with already.

The red line here, by the way, is the repository location. The water table is at the bottom of each column.

All right. For this calculation, each column was modeled by running TOSPAC, which is a one-dimensional flow and transport code 200 times. The total amount of time that it took to run this was about 35 hours of computer time on a Sun workstation, and that does include a crude saturated zone calculation. The saturated zone was handled by a two-dimensional numerical integration of a semi-analytic method. So, we, at the time this was done, we didn't have a detailed saturated zone model handy, but we didn't want to leave out that aspect of the system.

[Slide.]

DR. WILSON: So, moving on to simpler things. This is the results from the weep flow calculation. Now, what I mean by a weep, is a localized area of a fracture in which you have water flowing. The idea is that it flows down the fractures with essentially no interactions with the matrix. So, it is not exactly the opposite of the composite model, but more or less.

The -- in the composite model, you have a very tight coupling between the matrix flow and the fractured flow. In this weep flow model, you have no coupling at all, it all goes down the fractures.

The calculation is based on a simple model, in which we determined the number of weeping fractures, based on parameters, specifying the sizes of the fractures and how much infiltration there is, and then the number of canisters contacted by weeping fractures, is calculated, based on geometric considerations. The releases from the canisters into those weeping fractures is calculated and it is transported down to the water table. Then the same saturated zone model as in the previous calculation was used to extend it onto the accessible environment.

You can see that the releases are quite a bit higher for this case. It does have a lot of conservative assumptions built into it. There's no retardation at all, in the weeping fracture flow.

Something I meant to mention in the last one, when I was talking about the composite flow model, by the way, is that as low as the releases were in that one, it also had conservative assumptions in it for that model. The -- for example, the mean water flux in the composite calculation was taken to be two millimeters per year.

From various studies that have been done, including the two-dimensional HYDROCOIN calculations, it seems likely that within the composite model, you would not have two millimeters of flux at the repository, because it tends to run off transversely above the repository.

That's not the case in this one. It is certainly possible that if weeping fractures are controlling the flow, that it would not run off horizontally, so you could have a higher flux and, in fact, I think, if I remember right, the mean flux assumed for this calculation was five millimeters per year.

[Slide.]

DR. WILSON: And those calculations, by the way, took something like five or six hours of computer time on the Sun workstation. And that time is almost all because of that saturated zone calculation. It's a real simple calculation, but still, it's not trivial.

Now, this one here, the exploratory drilling calculation, that is essentially trivial. There's no flow or transport calculated at all in this. And so this calculation only takes a couple minutes to run, even though there are 10,000 samples.

Basically, in this calculation, drilling is treated as a Poisson process, and as Rally said earlier, the drilling rate is based on the EPA guidance in 40 CFR 191, which suggests a value of three boreholes per square kilometer per 10,000 years.

For the area of the repository block at Yucca Mountain, that means something like 17 boreholes expected in the course of 10,000 years. And for each of those boreholes, then, the probability of intersecting a canister is calculated from geometrical considerations.

And you can see on this graph that with the values that were chosen for this calculation, about 8 percent of the time a canister was contacted, 8 percent of the realizations there was a canister that was contacted by a drill.

And then the calculation at that point is very simple. A random fraction between zero and one of a canister is taken to the surface, if a canister is contacted.

So that's just the simplest approximation I could think of for the releases. And you can see that with that assumption, you have some pretty high releases.

Now, I would expect that that is pretty conservative. I imagine that most of the time it would be some rather small percentage of a canister that would be released in a drilling incident, but I don't have a model of that to work with at the moment. It's something that hasn't really been developed yet.

[Slide.]

DR. WILSON: Now, let me go back to the, if I can find it, to the little tree, and talk about putting these together.

The assumptions are different, and that affects the way they're combined into a single CCDF.

For this calculation, these two alternative conceptual models were considered to be mutually exclusive, so it's either a weep flow or it's a composite flow, and for the sake of simplicity, they were just given equal weighting, so it's 50-50 that it's one or the other.

It doesn't have to be done that way. You could have assumptions about both of them going on in different proportions in different parts of the repository, and things like that. But this is a simple way to do it to start with.

So, to combine these two, what you do is you basically just add the curves together with the appropriate weighting, which, as I said, is .5 for each, in this case.

And then, to combine this with the combination of those two, the assumption was made that drilling and flow are independent. That is, drilling a borehole in the mountain doesn't significantly affect the flow and the flow and transport in the mountain don't significantly affect what releases you get when you drill into a mountain.

Obviously, there will be some interactions, and in the drilling problem that Rally talked about earlier, they're going to calculate some of that. They're doing some more complicated calculations of some various possibilities of things that might happen when you have drilling. But, as a first cut, this seems like a reasonable thing to try.

And to combine these, basically, what you'd have to do is another Monte Carlo simulation. You take a drilling consequence and you take a flow consequence, and you add them together to get the total consequence; and that's one point in the distribution over here. And you do that a lot of times to get the probability distribution, which turns out like this.

[Slide.]

DR. WILSON: This curve, the CCDF for the overall suite of calculations, is basically dominated by the weep releases up here in the part of the curve; and down here in this part of the curve, it's dominated by the drilling CCDF. That's not a general conclusion, it's the way it worked with these assumptions, but with more sophisticated models, it might not turn out to be that way at all.

[Slide.]

DR. WILSON: Finally, I just wanted to leave you with sort of a cautionary note. This is a saying I've always liked. I have it taped up on the wall above my computer, to always remind myself that we make a lot of simplifications in this business, and I have to be careful, number one, to make the simplifications reasonable, as much as possible, and number two, I have to be careful not to believe them without a lot of time having passed.

DR. NORTH: Comments or questions?

[No response.]

DR. NORTH: I will put one in here.

I have a deep suspicion of getting into Monte Carlo analysis too quickly.

I think in Step Number 6, interpretation, it is very important to try to understand the sources of uncertainty in the inputs as they relate to the outputs, and to understand what the structure is, what might be independent of what, and where can you get dependencies that may be very important in dictating how your answer is going to come out.

So, where I appreciate what you've shown us as an example of methods, I'd like to put on the record a very strong cautionary note that Monte Carlo analysis is not a substitute for careful consideration of the uncertainties.

DR. WILSON: Absolutely.

DR. NORTH: It's sort of the corollary to your last slide.

DR. WILSON: Yes. We absolutely do need to explore sensitivity and uncertainties, and it's something that's down the road. We have not really engaged in it very much so far.

DR. NORTH: Let's go on, then.

DR. DYER: The last presentation today will be Paul Kaplan from Sandia National Labs, who will talk on, the title is "Using Groundwater Travel Time Model to Identify Information Needs from Site Characterization."

[6:00 p.m.]

[Slide.]

MR. KAPLAN: Based on your last comment, Dr. North, I'm in big trouble now.

The talk is divided into three parts. We are going to very quickly review the framework for the work.

We are going to then look at a comparison and a contrast with some calculations that were done for environmental assessment and then we are going to look at some calculations, some simulations that were done within the framework I am presenting to try and help Russ and Jerry and the others at the project prioritize needs or information needs from a site characterization program.

[Slide.]

MR. KAPLAN: I would like to start the presentation by asking a number of simple questions. I have found that the obvious questions that I ask myself don't always have obvious answers.

The first thing I want to introduce is what question are we answering when we run a performance assessment model?

The question I am going to try and answer in the framework we're looking at here is what credible circumstances or conditions exist that lead to failure to meet a regulatory or a technical criterion at the site?

The histogram is a prediction. It is the consequence unfortunately of a Monte Carlo simulation. I said that with a smile on my face.

[Slide.]

MR. KAPLAN: Why are we asking the question about failure? Precisely to identify information that if obtained is likely to change the prediction. We have a prediction. We get information. The prediction changes.

[Slide.]

MR. KAPLAN: This is the last framework slide. The three slides we're gone through summarizes what I normally give as a two-hour presentation. How do we ask the question?

Probably the key principle at least in the paradigms I follow is I define uncertainty as Shannon's informational entropy and certainty in the framework I am working in is a quantity.

By phrasing the problem that way I can ask the following question: What are the consequences of my ignorance as an analyst? As Jerry alluded to earlier, one of the jobs performance assessment has is to plumb the depths of our ignorance. A consequence is defined as a failure to meet a regulatory criteria.

If I can generate the failures, what I want to identify is what caused them.

[Slide.]

MR. KAPLAN: Now we're going to skip ahead, compared to what you have in the handout.

We're going to call this framework I laid out a hypothesis testing approach and we are going to ask the question, if I take this approach, is there a material difference between an old performance assessment that we might have done let's say for the EEA and something I am doing now. The case we'll compare is a half millimeter per year one dimensional steady state.

The calculations that were done in 1986, they were done stochastically. There was an expectation of 40,000 years.

[Slide.]

MR. KAPLAN: For the simulation we'll compare it with, there is an expectation of 16,000 years. This is not a material difference in a regulatory sense. The criteria calls for a 1000 year groundwater travel time.

But here is what we are asking now, this is what we are trying to calculate -- the probability that we'll fail to meet the criteria. Based on the old calculation the probability was about one in a million. For the way we're doing it now there's a material difference.

If we put up the two simulations we can see some of the things that are happening.

[Slide.]

MR. KAPLAN: The white histogram is the recent simulation. The blue density function is the EEA calculation. The failures of course are occurring here. This is just -- it's a frequency histogram. This is number of outcomes.

Probably the most important thing to illustrate with this overhead is, one, if we fairly incorporate into the model what we don't know is we are likely to get outcomes that are unfavorable to us. We know very little about the site. We know very little about the processes. This should not be a surprise.

There is something else that is important. The process we are modeling is non-linear. The expectation is telling us very little about details of the distribution and we are interested in details.

This was another driving mechanism I think and it came up during PACE and you are seeing it expressed now and you saw it expressed by Rally's and others' that we cannot ask this question deterministically because we are asking information around a central tendency. When we do that, when we use the concept of nominal, this is the information we want. This is the information that Pat is concerned about.

There is nothing in our current information base that precludes these events from happening.

Now how do we use this to identify again information needs from the site? We have generated this. I argue that there is again nothing in our current information that precludes this. Let's take a look at why some of the simulations fail.

[Slide.]

MR. KAPLAN: And let's talk about root cause reasons, not fundamentals or model coefficients. Let's talk about something that is common to almost every model we have ever run.

The root cause of most failures is because of an assumption in the models that we have interconnected fracture pathways.

Let's ask a simple question. Given our current state of knowledge, how reasonable is this assumption?

Let's test that. Let's ask that question and then let's ask what information would change our degree of belief in this assumption but let's ask right now, given what we have, is this a reasonable assumption?

[Slide.]

MR. KAPLAN: To continue on with the exercise I am going to make another assumption. I am going to assume that connectivity is a function of fracture density. I don't know if that is true or not. I certainly didn't at the time I made the assumption as part of a PACE exercise.

What I am saying is that I am more likely to have connected fractures the more fractures I have. It seems like a reasonable assumption. It is plausible and testable, which is one of my criteria for making an assumption in a model.

One of the reasons I am trying to relate the model performance to density is because density is a function of two pieces of information that can actually be obtained as data. This is one of the things that we have to be sensitive of in performance assessment. We have to ask for information that it is possible to collect.

The two pieces of information are fracture frequency -- that is when you go down a hole you count the number of fractures per linear unit in orientation.

Connectivity can be related to this information through a test. This is an important concept. Too many times in PA we ask somebody else, somebody to go out there and count something. We don't ask them to relate what they counted through a test to the way the system performs and this is a very important concept.

[Slide.]

MR. KAPLAN: The first step, now, tying the pieces together is, we have to ask the question, how many fractures per cubic meter are likely given the currently available data?

We're going to do this for one of the holes, for the non-welded embedded units in the Calico Hills. Based on the information in Rick Spengler's open file report, I get a probability density function that describes how likely so many fractures per cubic meter are.

So this describes my uncertainty as an analyst as to what is the appropriate number to use in the model. That's what this model is.

Now, this does not tell me whether or not the fractures are connected. Now we have to do the test.

And I want to thank Larry Hayes, even though he isn't here. One of the things the USGS is doing now is they are sending is draft copies of some of the open file reports prior to their publication.

[Slide.]

MR. KAPLAN: At the time I started this exercise, I did not know that Art Geldon was out there pumping the C-holes. The units are dipping. He's pumping many of the same units that I'm looking at in the unsaturated zone, he's pumping them in the saturated zone.

So step two: we now want to relate data that we have on frequency and orientation in G-4 to the tests that were done at the C-holes.

Well, God bless Art Geldon. I made an assumption that, again, fracture density was a predictor of connectivity. Art knew nothing about that prediction, but in the open file report, there's a statement in there by him that of all the information he could obtain, the best predictor of whether or not he would see water in that interval was fracture density.

What he did, they went through cross-hole tests, packed-off sections, and what he found is that until he reached a density of 2.8 fractures per cubic meter, independent of lithology, he did not get any yield.

So now I can relate this data from the C-holes to this. What this is saying here, this is 2.8 fractures per cubic meter. This model tells me how likely I am to have less than 2.8 fractures per cubic meter at the Calico Hills at G-4. It's the area under the curve here. The curve is normalized to one, so it's 72 percent of the area.

What it's telling me is, the assumption of interconnected fracture pathways, at this location, if I put it into my model, it has a probability of occurring of 18 percent, based on the data.

It tells me again, it gives me a degree of belief in the assumptions I'm making in the model.

[Slide.]

MR. KAPLAN: Now, the whole point of this was to show that we can use this information in performance assessment after we get it. Information change is a prediction. Here was our prediction.

This particular prediction, and there's no unique prediction -- I can give you any prediction you want -- this particular one we had a 20 percent chance of failing, given that there are fractures, because that assumption is in the model.

We looked again at a model of belief in the assumption that there are fractures. We would out it had a probability of 18 percent at that location. We now asked the question, was the probability that both continuous fractures occur and the site fails to meet the criterion. It's the intersection of the two. It's about 4 percent.

DR. NORTH: Why should those be independent? This is my basic problem with Monte Carlo analysis. It seems to me the site failing to meet the criterion might be a lot more probable if I know that there are lots of continuous fractures.

MR. KAPLAN: It is very much related to that. Yes.

DR. NORTH: So I think those two events might be highly dependent, and I would not want to multiply those probabilities as a way of getting that intersection. They have to be considered as conditional probabilities. What's the probability of B, given A, and multiply that times the probability of A.

MR. KAPLAN: This is the probability of failure. This here, 20 percent, is the probability of failure, given the fractures occur. This is the probability the fractures occur.

DR. NORTH: Given the first condition.

MR. KAPLAN: No. Probability of failure given the fractures occur. Or in other words, there's a 100 percent probability in this model that the fractures do occur. This is the probability of having connected fracture pathways there. So it's the intersection of the two.

[Slide.]

MR. KAPLAN: But basically, the point of the talk is to say first, PA tools can be used to identify information needs. The information I've got changes the prediction.

I've run into some criticism on the way I approach the problem, because I do generate failures. I don't do it on purpose. But I do it.

I'd like to dismiss at least my attitude towards that with the following headlines. I don't think the fact that we can generate failures at this point in time is big news.

[Slide.]

MR. KAPLAN: So in summary, basically I want to re-emphasize a point that's come up several times.

PA calculations do not carry a thousand-year warranty. They represent, hopefully, the best of your information at the time you use it, and that information is going to change.

What they should do is provide a plausible basis for a current decision. You do not know whether or not the prediction is true or false; you do not know if those probabilities are the actual likelihoods. All you know is that hopefully you've made plausible assumptions, assumptions that can be tested and shown to be false if they are wrong.

The simulation, or the exercise, should be constructed in such a way that it identifies information that can actually be obtained. And then it should use the information as soon as possible, again, to update the basis for the decision. And then you go through the process over and over again.

And that's basically it.

The other overheads in your thing are just hopefully to fill in a little bit of the logic of the process we went through.

DR. NORTH: I can't help commenting, I'm sorry you left out the law of mythical numbers.

[Slide.]

MR. KAPLAN: I have it right here.

There were a couple of places I could have used this.

I could have used it at the beginning in terms in the areas where I was telling you about framework.

One of the things I've been working on is again a formalism or a process to process information without putting my bias or anybody else's into the process of transforming that data.

The other thing is, when I actually started working on this fracture exercise for PACE, one of my first concerns was again, what value of fracture density do we put into the models.

And what I ran into was a value for fracture density that we did use in the most recent PACE simulations that referred back to an '87 report, that referred back to an '86, that referred back to an '85, '84, '83, finally tracked it back to an oral communication in 1975.

Between 1975 and now, while we're still using that number, somebody actually went out and drilled four or five holes, at no small expense, went out, counted fractures. The actual numbers you get for fracture densities, the earlier estimate was very good, but as you can see from the probability model, close, in this case might make a big difference. Just nobody had looked at the data.

Then, when I reviewed the data, in one of these large PACE meetings, one PI actually was rather upset that I used the data, and not the oral communication that was from 15 years ago.

His argument was, how can you trust the data? And my argument was, if you don't use it, why pay for it?

So anyway, that's the law of mythical numbers. And I wish I could take credit for it, but again, it's a well-known phenomenon.

DR. NORTH: I think the date on that reference is wrong. That book was published about 15 years ago.

MR. KAPLAN: They've reissued. This is the last --

DR. NORTH: Okay.

MR. KAPLAN: -- they've just reissued it.

DR. NORTH: I might add, this has been observed by a lot of people in a lot of different contexts, who are out there using probabilistic analysis. And there are a number of celebrated failures where, shall we say, this mistake cost a tremendous amount of money.

MR. KAPLAN: There's another overhead where I quote a paraphrase of Ian Hacking, in his book. And I didn't bring it this time. But it's basically: chicken guts and experts are prone to corruption and flights of fancy. I enjoy that one, too.

DR. DOMENICO: Paul, if it'll help you, we just did the study of the five pavements at the Yucca Mountain, and we did a connectivity study. And I can send you all those results, where the densities have actually been determined by hand, on the five pavements that are available at the test site.

MR. KAPLAN: I would like to have that.

DR. DOMENICO: Yes.

MR. KAPLAN: Thank you.

[Slide.]

DR. DYER: Let's see. If I talk real fast, we can get out of here by 5:30. No, I don't think so. If we set the clock back.

Before I get started on wrapping up today's presentations, you've heard some allusions from some of the previous speakers about the PACE results, the infamous PACE report.

This is in fact, it's cleared review, it is in printing even as we speak. We brought a couple of copies with us to this meeting. We've made some copies available to the Board and to some other people. I know all the Board members will be on automatic distribution for that report.

If anybody else in the audience would like to get a copy mailed to you, either see myself or Jerry Boak. We'll put you on the mailing list for that report.

Volume II of that report is currently in technical review, should be out, oh, next couple of months, I hope.

[Slide.]

DR. DYER: Let me try to wrap up some of the things that you've heard about today.

First off, I hope we've demonstrated that we do have performance assessment models and computer codes available to support near-term decisions. They are not a panacea, they are not the end-all and be-all, they're not where we need to be eventually. We know that. But there are things in the works, things in existence and things in the works.

While we're on this topic of decisions, since we started out very strongly on that early on this morning, let me revisit it just a little bit, in that performance assessment is, at least for the decisions that have faced DOE recently, is a necessary, but it's not a sufficient consideration. It must go into the decisions.

Model verification is in progress. You heard a lot of people talk about the importance of constructing, using adequate models in the various performance assessments.

A model validation methodology has been drafted and is in review. We didn't talk about that very much today. But there is a systematic process by which we hope to demonstrate how one can approach the validation of models used in performance assessment.

Experiments involving model development and validation are currently underway. We heard just a little bit about that today. I suspect the next time we talk to you, we'll have more to tell you about this.

Software and data quality assurance are being implemented. I suspect that you're picking up information on this in some of the software, or some of the quality assurance briefings that you've had.

The last item here, integration of Yucca Mountain performance assessment contractors for complex analyses accomplished. This was one of the more important things that came out of our 1990 activities, fiscal year 1990 activities. And that was the integration, if you will, of the performance assessment capabilities between two formerly parallel organizations. So now we have all of performance assessment talking to each other. But even more important, we have lines of communication established between the performance assessment organizations and the testing community, and also the management community.

So there has been, I think, tremendous strides taken in bringing performance assessment into the day-to-day operations of the program.

That doesn't mean we're, like I said, we're not as far as we would like to be.

Some of the positive things that came out of PACE, Paul just talked about.

A systematic review of the data and the assumptions that underlie some of our previous modeling gave us a considerable "bang for the buck," if you will, by re-examining, tracking back to the source, some of the assumptions that we held to be sacred.

The consistency of results from the various participants gave a feeling of confidence to the various participants.

What about the downside? It wasn't all a bed of roses here. There were a few thorns in there, too.

It was, as Dr. North pointed out, very resource-intensive. We put an awful lot of effort into the PACE exercise.

And one of the things we learned was that we need to have a little better delineation of things that people work on. We don't have the luxury to have everybody doing the same calculation for every problem.

One of the things that came up, as you saw from PACE-90, was that we did a Total System Performance Assessment, at least of some parts of the total system, but it was only carried to the level of consequences. It needs to be carried further. Rally talked about the incorporation of probabilities, carrying it through to a Total System Performance Assessment, which would be out this year.

[Slide.]

DR. DYER: What are the things that are on our plate?

Well, go to the next slide.

And this is the big one on my list, on your list took apparently, is a Total System Performance Assessment with sensitivity and certainty analyses will be done this year.

Some of the things that I hope have been pointed up time and again by the speakers today is that the credibility of modeling is severely hampered by the lack of site data. In many cases, we're carrying multiple conceptual models forward, and I think a relatively modest input of site data, the appropriate site data, would allow us at a fairly early stage here to make an intelligent choice between alternate models.

On our scenario development front, we're screening models and codes to identify which to continue to support, which to improve, and what new development is needed. This is part of the iterative, evolutionary nature of performance assessment.

We have a list of codes and models that we're using now. Those will almost certainly not be the same suite of codes and models, well, I hope not codes. I hope that the models we're looking at include the models that will still be in vogue ten years from now.

Several performance assessment issues need to be resolved with respect to the adequacy of the current models and the relative importance of phenomena and parameters.

This gets back to the issue of model validation, or if -- I know you've seen it before, but

I'll pull it out -- the model pyramid.

[Slide.]

DR. DYER: What are the important things that we need to look at in the top-level models that incorporate, that derive from these fundamental processes down here at the bottom? How do we need to aggregate or roll up information, observable down here, into things that can be processed in a top-level performance model?

[Slide.]

DR. DYER: Finally, performance assessment issues.

At the top of our list of things that need to be addressed, 10 CFR 60 and 40 CFR 191 need further interpretation for implementation.

You've heard just a little bit of some of the controversy, some of the edges of the controversy about both 10 CFR 60 and 40 CFR 191 today. We have recognized a need to carry on dialogue regarding several things that are implicit in here. How does one construct a CCDF? How does one make a meaningful, reasonable CCDF?

Scenarios is another item. Is there a particular preferred method for developing scenarios.

These are all things that are either implicit or explicit in both Parts 60 and 191, that we need to explore in the relatively near term, in order to implement performance assessments under these criteria.

Right now, you've heard several people say it, performance assessment is limited more by the credibility of conceptual models than the ability for mathematical modeling.

We can make a lot of mathematical models, but not many people will stand behind any one of those models.

Scenarios and scenario probabilities need further study. That is linked in part to this first bullet.

Current site parameter uncertainties are large. No big secret there.

And considerable improvements in models and computer codes are needed for final site suitability evaluations and for licensing support.

This we hope will be resolved over the course of time, through the evolutionary nature of this program.

This concludes my presentation, and the presentation of the Department of Energy this afternoon, this evening.

Do we have any questions from the Board, Dr. North?

DR. NORTH: Not from me.

I'd like to conclude by thanking all the speakers and the members of the audience for hanging in here until 6:30 in the evening.

[Whereupon, at 6:30 O'clock p.m., the meeting adjourned, to reconvene at 8:30 O'clock a.m. the following day, Tuesday, May 21, 1991.]