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NUCLEAR WASTE

TECHNICAL REVIEW BOARD

OPEN MEETING

Panel on Structural Geology and Geoengineering; and the

Panel on Hydrogeology and Geochemistry.

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Atlanta, Georgia

Tuesday, July 24, 1990
8:00 a.m.

PRESENT

Nuclear Waste Technical Board

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P R O C E E D I N G S

CHAIRMAN DON V. DEERE: Good morning. This is a joint meeting of the panels of the Nuclear Waste Technical Review Board: the Panel on Structural Geology and Geoengineering; and the Panel on Hydrogeology and Geochemistry.

We have invited the Department of Energy to present to us the updated information on the studies they have been making on the SAS--Studies on Alternative Strategies--and other items. These have been covered in part in our previous meetings. I believe this will be the third meeting that will let us know what the progress is.

Mr. Brocum, I will turn it over to you.

MR. BROCUM: Good morning. The Department of Energy is pleased to be here at the meeting of the Panel Review Board to present the status of our four major activities: surface-based testing; the Calico Hills cost/benefit analysis; the Exploratory Shaft Facility Alternative Study; and the Alternate Life Strategy.

My name is Steven Brocum. For the record, this slide is incorrect. You have had a reorganization, which I think you heard about yesterday. I am now the Acting Director of the Requirements, Analysis and Verification Division of the Office of Geologic Disposal. Paul Gertz is the Director of the Office of Geologic Disposal.

Today we have a quite a few people here from the approximately six or seven months. They all started late last year, early this year; and they are all due to be completed by the end of this year.

This is a status presentation. We don't have all the answers, but I think you will note we have made a lot of progress.

OVERALL MANAGEMENT OVERVIEW

Mr. Maxwell Blanchard

MR. BLANCHARD: By way of introducing the topic I would like to provide you with a status of each of these four activities. Mr. Brocum described the sequence in which we are going to discuss them.

These Task Forces had two roles. One as to provide input to some technical issues that were raised by the Commission staff, this group here: the Technical Review Board; as well as the utilities. So that centers definitely around the Calico Hills Risk Benefit Analysis and the Surface-Based Testing Prioritization.

In the systematic basis for program decisions, all of these have a need for a more systematic basis for folding them together. As a consequence, we have been using decision analysis as a guide to help develop a more systematic approach to this.

We will be discussing that more as we progress into the topic.

The objective of the Surface-Based Testing Prioritization was, first, to develop the methodology for early tests that influence site suitability. This methodology, we felt, should fold, like hand in glove, into a methodology for evaluating site suitability.

We have been looking for quite some time and trying to evolve, as a matter of fact, a methodology to fold early testing into the management on an annual basis so we can be assured we had that data and the sensitivity analysis that went with that data to merge that with the funding to prioritize our funds for the year in the nature of the tasks.

We are moving in that direction very nicely.

The Calico Hills Risk Benefit Analysis Task Force was to evaluate the benefit of these tests. This is a collective view of a number of tests, not specific tests. We will tell you more about that later on for ways to explore the Calico Hills versus the impacts on what will happen as a consequence of exploring the Calico Hills.

Looking at the Exploratory Shaft Alternative Study and its systematic evaluation of options for configuration and construction methods with a goal to recommend that the third option is considering input from Calico Hills.

Finally, we will discuss the evaluation of alternatives to the top-level licensing strategies that are now on the Site Characterization Plan that, at the outset, appear to improve scheduling costs or performance, or have the likelihood to do so.

The current status of each of these: For the Surface-Based Testing Prioritization methodology is nearly complete for prioritization. The models for the sensitivity studies are well under development. You will see they are moving down the path, and are very informative and useful.

The suitability methodology is now being defined; and it is being defined in a way that is very compatible with this methodology so the two fit in a way in which it will be not at all difficult to prioritize the funding based on things you can begin to perceive about the Site Characterization Plan from a suitability standpoint.

For the Calico Hills Risk Benefit the preliminary Task Force recommendation has been made from that for characterizing the Calico rock unit. As you recall, it is the rock unit beneath the Topopaw Spring and above the water table. So it is one of those principal barriers to radionuclide retardation.

That input has been given to the exploratory shaft people. They have been waiting for it. They would like to have had it quite some time ago; but they have it now. Then the draft report is now under preparation for the Exploratory Shaft Alternatives. The options have been selected for the analysis.

The scoring of options began in June. The input from Calico Hills was not scored. That was held up. The sensitivity studies will be continued into mid-September. The expected draft recommendations will be provided to the Department of Energy in the December time frame.

For the Alternative Licensing Strategy, the Task Force report is in draft form and it is undergoing internal review now.

All of these tasks, I might say, have gone through our procedures for identifying items and clearly support safety. They all went through grading, and they all have a full set of documentation.

How will the Department of Energy respond to these recommendations?

First, we will initiate management reviews of these recommendations. We will be looking at how they all fit together as well as the recommendations within themselves. From a programmatic standpoint, we will be holding interactions with the Nuclear Regulatory Commission.

Two of them--the Calico Hills and the Exploratory Shaft--are alternatives. The Department of Energy has given, at previous times, a promissory note to the staff of the Nuclear Regulatory Commission that we would meet with them and discuss the results of those studies before we made any decisions.

Then in order to determine an appropriate course of action we will have to develop an implementation plan. In the course of doing that a number of things will be considered. One will be, certainly, to subject the Task Force recommendations to peer review before the implementation plan can be finalized.

The peer review can be in several areas. It could be a question of the extent to which each of these methods actually met the classical approach to decision analysis. Was that process followed? Were we set up to do it? Did we do it?

Another one could be to review the actual data that was available prior to now: all the available data, whether it was by the project or published on the open literature. Was that appropriately considered by the experts?

Another approach could be to determine whether or not the confidence levels and the actual values used by the experts could be agreed with by another independent group of experts.

So we have a broad spectrum of possibilities for peer review on these recommendations.

Also, decisions to implement these recommendations may involve reassignment of staff as well as reallocation of the Fiscal Year 1991 budget. Any impacts of staff or budget reassignment on other Department of Energy commitments will, of course, also have to be addressed as we best fit these into our overall program.

There are some issues that are relevant to implementation. One is the Nuclear Regulatory Commission's acceptance of the Department of Energy quality assurance program.

As you know, the Department of Energy has been trying for quite some time to get all of our participants up to what we call a Gold Star Art.

The availability of permits? We still assume that January of 1991 is the first realistic potential start date. Of course, it assumes that we would have budget to conduct expanded field programs, which are suggested in some of these Task Force recommendations; and that would start in 1991 because we need the money to do it.

Then, since some of these Task Force recommendations involve some major changes in strategies, some of these strategies cannot be changed without outside involvement: with the Nuclear Regulatory Commission as I mentioned before. So it is not a unilateral action by the Department of Energy by any means.

I would like to share with you our perception of the manner in which these Task Force activities fit together.

In many ways, they all start with the strategies that are described in the Site Characterization Plan prepared several years ago. We described how we would demonstrate conformance with the regulations, and then identify program activities for performance to design so that the Site Characterization could be conducted by useful information: the demonstration of compliance.

Starting with this center line, beginning the part of Chapter 8 that describes the exploratory shaft, we developed the Alternative Task Force to look at other ways, different locations, other construction methods and other layouts.

Eventually we will have preferred Exploratory Shaft Alternative configuration. Before we could achieve that we had to have the Calico Hills Risk Benefit Analysis.

As you recall, there were differences between the Site Characterization Plan consultation draft and the statutory draft, the nature of which being how we would go about running tests at the Calico Hills; and it took some time before we could provide an input that the experts working in the Calico Hills Risk Benefit Task Force thought would be sufficient to bound the nature of the exploration program so that whatever preferred configuration was selected by Exploratory Shaft Facility they would have full cognizance and understanding of how extensive that underground test program could get.

We feel the input that came into this group from the Calico Hills Risk Benefit Study did appropriately bound the largest meaningful underground test program that one would expect to conduct in that rock. So the design with be considering that as an upper bound.

After that configuration is selected we will be moving on with Exploratory Shaft Design.

The Surface-Based Prioritization Testing started with the Characterization Test Program: that is, the program in geology, hydrology, geochemistry and so forth. That is described in 31.

Then using decision analysis we developed a method for prioritizing those activities relative to the regulations and the strategies for demonstration and compliance. Then, once the methodology is developed, the methodology is applied to the test program to prioritize those things that are more sensitive to waste isolation using sensitivity and uncertainty.

Then, once that is done, the Underground Test Program will be visited with that same methodology, and then that will provide input to the design; and this Underground Test Program will consider the inherent tradeoffs between the Calico Hills Underground Test Program as well as the Underground Test Program at Topopaw Spring.

Once this was developed, this methodology for prioritization, it was clear that we had a good start to begin developing the methodology for suitability analysis. As that methodology is evolved--and it will be described to you by our speakers--it is the type of thing that will be applied vehemently.

I don't mean to suggest that this is not also applied vehemently. We are looking at the methodology and the sensitivity, and certainly the analysis that goes with prioritization to be the very manner in which those of us who are doing the Site Characterization will be applying that information to make decision about: first, where to put our funds--how much emphasis, level of effort to place; as well as, as we move downstream and get in test information, to help us answer how much is there.

We are also considering the description of the basic program as the Alternative License Application Strategy. We considered that and looked at different ways to conduct the same program.

To use a common phrase, it could be considered every better ideas to do the program differently. At some point this becomes an input. It was not meant to be the answer of basis on which we worked. We then changed the program because it was meant to get input. I will conduct other inputs that go along with that.

Eventually these kinds of concepts will be incorporated in the mission plan; and there has to be some degree of credibility for all of these to fit into these strategies.

Indeed, I think you will see the day that these things are moving along in a direction very similar to what some of the recommendations are for the Alternative License Application Strategy.

We have taken some steps to insure that these efforts are integrated. We used similar decision analysis approaches. We held monthly or more often coordination meetings among the Task Forces. Some staff were shared by multiple Task Forces; some of the experts were, in fact.

There were also common-influence diagrams at a very high level because they relate to the approach to waste isolation.

Some issues? What is the appropriate test program from the Calico Hills project? The Study compared different exploration strategies rather than specific tests because there is not enough knowledge and understanding involved about the tests that have already been conducted.

However, the specific tests we evaluated using the Surface-Based Prioritization methodology is very elusive. So we asked another question: What is the best program that should be conducted during construction as we go down through the rocks? Then: What is the test program that should be conducted at the mean test level?

If an option for more extensive exploration of the Calico Hills is chosen, then the testing strategy for the Topopaw Spring may require a re-evaluation. Again the prioritization methodology will be used to focus on this program.

How will the changes in the plan testing impact the requirements and restart a tactical design? Our view is that the options under consideration are thought to be possible to accommodate the range of likely changes in the test plan. We have options for more extensive testing in the Calico Hills. There are a number of other options for exploring the Calico rock outside of that.

There will be, or could be, a restart of the design or changes in the program scope at any time.

With respect to the Underground Test Program, it is certainly possible that the test program at Topopaw Spring could be refocused in a manner to enhance testing on the constructability and thermomechanical issues. This are topics where the test program in the Calico Hills rock would be focused on those things that most affect the radionuclide operation of retardation: hydrology and geochemistry.

Can we develop a method for evaluation of site suitability that will allow major changes in program strategy should they develop in the next few years?

There are a few things we think are encompassed in these methodologies that support that. One is that, right now, these methodologies allow favorably conditions to compensate for the potentially adverse; and we assume that there will continue to be the multi-barrier approach that is inherent in the regulations.

What role does expert judgment play? As you can see--and our subsequent speakers will show--they have played a very large role in these Task Force deliberations. There really is no other way to do it at this stage.

Probably, in the long run, with respect to License Application and Demonstration Performance there is still no other way.

Predictions about performance that impacts on that performance are currently based on large-component expert judgment; but they use the data in the models that exist now. There is a lot of information about the geology, the hydrology, the rock characteristics and the geochemistry about that site.

Therefore, I don't think the experts felt they were on very difficult ground with respect to the acquisition of information that was published. The question was: How much confidence do they have in the values as they apply the decision analysis process; and what range of values would they consider, and what levels of confidence do they have in those ranges of value?

Of course, all of them felt they would feel more comfortable if more information were available. That is definitely a Site Characterization procedure for the judgments to be updated and refined. It is presumed that if some measures of central tendency will be nearer the confidence level will be higher.

That may not be the case, but most people would like to see it that way.

How does the sequence of testing enter into the Task Force consideration? Well, testing that provides information that is important to identifying the suitable conditions will be done as soon as possible.

From a management standpoint, we are going to get into the trade-offs. If we, early on, wanted to identify those characteristics to build confidence that you knew very well in a barrier--like the Calico Hills rock--the barrier having orders of magnitude differences in hydraulic conductivity and distribution of radionuclide-retarded minerals from, say, there south you would want that information very early to confirm that either that information was as good as your experts thought it was or as bad as your experts thought it was; and if it were outside the bounds perhaps the conclusion might be on the ragged edge.

You would like to have that information early.

The trade-off is: If you follow a classic program it might take two or three years to do the tests and construction to get down to the Topopaw Spring or down to the Calico Hills. So there is a trade-off in going fast and getting this information that you might think is critical to understanding how good is the Calico Hills waste isolation, versus doing something a second time or doing something that may not give you the record-keeping that you would like to have.

So it is not going to be easy to go through this and reach a conclusion.

These trade-offs will relate cost and schedule, and the value of the information will come right back to expert judgment on that. I think that is what are after?

What is the appropriate level of surface properties to assume now and in the future? All the Task Forces were confronted with this. In the technical opinions it is apparent in the way they drew their influence diagrams. It is also apparent in their human distributions for the values they chose: 10 percent, 50 percent, 90 percent confidence.

So conservatism is shown and uncertainty is shown in their opinions; and that will be described as the decision analysts get up here and explain to you the process they followed.

Reference for managers with regard to conservatism also, in some cases, are encompassed or will be encompassed in the studies. Future competence conservatism will be an issue that needs to be address at the highest level of the Department of Energy.

It is clear that whoever the director is of OCRW he will be confronted at should point with: Should I or should I not support a license application? When he does that, he knows the process is one which calls for a very rigorous and systematic critique of what is in that license application.

When he is in the process of doing that things happen to the confidence in the data and analysis. Oftentimes they don't come out to be as good as people hoped they were.

It would not be surprising if that director decided he would like to have more conservatism than the Environmental Protection Agency released. He would probably be too anxious to move forward with the Environmental Protection Agency release on that and an SAR on that that was 3.1.

If he had his druthers or was king for a day, it would be two or three orders of magnitude below that. He would also like to have a very high confidence that he was orders of magnitude below that.

How much you characterize the site and how much confidence you want is the trade-off in cost and scheduling. That, I think, is what the Department of Energy had to begin to focus on because that costs money; it takes time; and it is not altogether clear, always, whether or not that confidence will be the conservatism required for a reasonable cost.

That ends what I was going to say about an overview. The Task Forces will be described later this morning and tomorrow. I would like to go on and discuss the status on Alternative License strategies. If you have any questions on the overview, I will be glad to deal with them.

CHAIRMAN DEERE: Are there any questions at this stage?

DR. DOMENICO: On a slide, maybe 9, where you show the diagram on Surface-Based Prioritization Testing, even with those running parallel they will actually be done in sequence, I believe. Is that right: the VS exploratory shaft will not be started until the Surface-Based Prioritization Testing program is completed?

MR. BLANCHARD: No.

DR. DOMENICO: That is not true?

MR. BLANCHARD: Under the strategy described in the SEP, they would both be parallel. Under the current program, the Surface-Based Prioritization Testing program, we believe we will be ready to start January 1 with our quality assurance program, with all the plans, procedures and everything.

The Exploratory Shaft Facility surface disturbance work cannot start until the SF design is finished. So there will be information coming in from the Surface-Based Prioritization Testing programs, beginning in January, that can help us; and perhaps, in some ways, provide usable information to help define the design.

DR. DOMENICO: I think that is what I said: they are going to run in sequence rather than in parallel.

If I see that line connecting the Calico Hills Risk Benefit Analysis to the Exploratory Shaft Alternative Study, does that mean the Calico Hills will be investigated by way of the Exploratory Shaft?

MR. BLANCHARD: Yes.

DR. DOMENICO: Strictly?

MR. BLANCHARD: It is not the only. Subsequent speakers will describe Surface-Based Prioritization Testing programs that examine the Calico Hills, and other ways that we use the combination of a Surface-Based Prioritization Testing program and the Underground to characterize the Calico Hills: some of which are inside the area where waste emplacement will occur in the mountain, some of which are outside that area; and some of these strategies show an augmentation of certain ways to explore in addition to the Surface-Based Prioritization Testing program.

For instance, angle drill holes into that rock and outside the waste emplacement area.

DR. DOMENICO: If you did start the Surface-Based Prioritization Testing program in January of 1991, are you looking at two years, three years for its completion? What sort of schedule are you looking at, more or less, if things go well?

MR. BLANCHARD: A lot depends on how much comes out of the Surface-Based Prioritization Testing program; but, in my view, it is approximately a five-year exploration program: the Surface-Based Prioritization Testing. It could be longer.

A lot depends upon the rate at which we can drill holes; and that is a very costly activity. If one wanted to drive it as quick as you could, you would want to run drilling rigs 24 hours a day. That gets very expensive: it consumes lot of money.

Some of these rigs, as you know, are unique rigs. We are trying to drill dry and core dry. So they are expensive rigs.

Not all our holes are like that, but the ones where we wanted to treat core and the ones where we want to place instruments and measure the properties without having to subject to the large perturbation are.

That requires a big budget. The 1991 budget does not look like it would support that kind of activity. So that will tend to lengthen the program.

DR. DOMENICO: Thank you.

MR. BLANCHARD: Sorry, I misunderstood your question.

MR. McFARLAND: That prioritization for sub-surface testing does not start until after you have a preferred configuration.

MR. BLANCHARD: That is true. There is some risk inherent in everything you do, and that is one of them.

On the other hand, we want to make sure the design can accommodate. It is better if you design something a little bit bigger at a small delta in cost and then decide, a year or two later, we don't really need to do that. You have to keep building in that case.

If you went the other route, from down scope to minimum cost, minimum time, and the you decided you wanted something bigger you would be in a really difficult situation. It would be very expensive. The retrofit would be very time consuming.

The strategy we have here is that the current Underground Test Program in the SCP is geared toward the total cost spread. It is not likely to get any larger.

So we think that, from a design standpoint, that scope is not likely to get much larger, except for more drifting in the main test log area.

The same is true with the Calico Hills. When we add the Calico Hills in it and say "Do this extensive drifting," it starts from the northeast sector and it crosses and goes to three different places. It goes all the way down to the southwest.

We think that is also an good program, which would be an underground test program: thousands of data points, hydraulic conductivity, and so forth.

That is a large underground test program that we considered in our configurations. I believe that those of us who are familiar with both the test program and the engineering program needed for the Exploratory Shaft Facility will have found the design requirements so the design will move ahead, assuming the large underground is at Topopaw Spring as well as at Calico Hills.

That is probably not at all reversible.

CHAIRMAN DEERE: Is there a potential for stopping the testing? For instance, you have priorities for developing methodology to identify early tests that could influence the site suitability decision. This was a point brought up, I think, by various people in the past.

As you recall, the Board felt it might be necessary to get the shaft down before you would really find the adverse things; but let us say your surface testing does find some questionable things.

Do you have a procedure by which these are re-evaluated or do you have to run through a certain four years of program of five years before you get a cut-off point? When do you say, "We have something that needs to be looked at completely different than we anticipated"?

Can you do that?

MR. BLANCHARD: I think we can do that. I think our current Surface-Based Prioritization is geared towards doing that. Steve and Bruce Judd and Russ Dyer talked about the manner in which they would go about doing that.

I think you will recognize it is inherent in that methodology to pull out those things that are values that come out of our Surface-Based Program, that exceed the 90 percent level or way off the 50 percent confidence level, which would cause you to ask the question: Do we have to rethink this in its relationship to waste isolation potential?

I hope you will ask that same question to each of those speakers and they will give you convincing evidence of the capabilities.

CHAIRMAN DEERE: They probably know that I shall.

[Laughter]

CHAIRMAN DEERE: I failed to introduce Dr. Domenico, who is the first appointee we have had from President Bush to our Technical Review Board. I want to welcome him publicly at this meeting. He also will be a member of the panels that are meeting here.

You all know him, from his past attendance at the meetings, as a consultant in hydrogeology for the Board.

MR. BROCUM: Max had mentioned he had made certain commitments to the Nuclear Regulatory Commission about making presentations on Calico Hills and the Exploratory Shaft Facility.

For everybody's information, there is a meeting on July 31st where we will set up the next six months of schedules with the Nuclear Regulatory Commission.

The second is one to which I think Max alluded, but it did not come out very clearly for me and I would like clarify it.

When we published the Site Characterization Report the program we had for investigation was not only for site suitability but to get all the information we need for construction and design.

Since then the Secretary has issued a 60-day report to Congress which is evaluating things really early. When Max talks about the studies at Calico Hills versus the Topopaw Spring, the value of that information to each of those units has to be considered.

If you define "suitability" as the ability to accommodate waste, the value of the Calico Hills increases relative to Topopaw Spring.

These are all the issues that are kind of swirling around as we do our studies. You have to remember the original program was not only for suit suitability: it was for constructability and to get design information.

The third thing: In our reorganization we have set up a new office that is responsible for systems engineering and regulatory requirements. It is their charge to make sure that all our requirements are in place before we proceed, and they are reviewing all the requirements.

I just want to make those points clear.

Our current schedule is to enter design in March if 1991 and to start constructing the shaft in November of 1992. Those have not changed.

DR. BLANCHARD: Thank you.

It would seem appropriate now to discuss, for a few minutes, the preview to the current Alternative License Application Strategy. I would like to give you a summary of the status and some examples.

Perhaps you will remember that the purpose and scope was really a management scoping study. It was to identify possible alternatives for management and conduct of the Repository Program. It was not intended to be a basis for justification of programmatic decisions.

This is an input to Dr. Bartlett, the Director of OCO. There are a number of other inputs that are happening at the same time.

For instance, his boss, Ed Watkins has some views to be considered. The National Academy of Sciences' report on the Santa Barbara meeting is one that Dr. Bartlett is considered. Tom Isaacs has a strategic plan with this he has been involved for over a year now. That is another input.

There are conversations Dr. Bartlett has had with the Commission, with the Environmental Protection Agency, with Congress, individuals on the Hill and with the utilities who are all provided input to him so he can make his mind up and prepare the different strategies.

I did not want to present the results of this atlas and leave you confused. I want to make sure you understand this is one of many inputs he is considering; and that we did not compare it as a basis for the justification of major program changes.

There are three tasks in this effort. One is the Identification Policy and Alternative Studies. Another one was the evaluation of the renting those strategies according to what people perceived as benefits in terms of cost to their schedule. And the difficulty it would take to duplicate.

Is it within our control, almost within our control or way out of our control in the Department of Energy?

The preparation of a summary has gone with this document for the things we are recommended and some that will not be considered because of what has gone before. Graphically this represents the activities.

We started out by identifying everybody's better ideas of how to conduct this effort. We had two workshops which included project participants, consultants and people outside the program. The utilities were included, also.

We identified and described the strategies: broke them up and then did an analysis on them. A core team analyzed these and helped prepare them in a more readable fashion, and then divided them into three different categories. These were the categories of the difficulty with implementing them: high meaning very difficult; low meaning easily accomplished by the Department of Energy.

Within these pages of strategies they then proceeded with the core team to evaluate. They took these three levels and ranked them based on facts: what they would cost, the schedule and what amount of sense they seemed to make.

To additional workshops were held with project participants and interested parties at headquarters, as well as consultants. Then what we had was an upgrade from the identification of the strategies and the descriptions to prioritize the list and prioritization of the effects related to cost and schedule.

The core team prepared a draft report. There were four categories; and I will discuss these with you now.

The difference in the strategies were basically those things which were felt to be of low difficulty were basically things that were within the Department of Energy control in changing the surface if the Underground Test concept or the nature of the tests.

The things that fell into the medium difficulty category were those plus the addition of one big thing: regulatory influence. There the Department of Energy would have to meet with the Nuclear Regulatory Commission or the Environmental Protection Agency, or others, to make any changes in the program: items that fit into that level.

When we go to the high difficulty strategies, it is these plus the legal framework and the fundamental relationship in Department of Energy, the Environmental Protection Agency and the Nuclear Regulatory Commission areas identified in the waste policy area.

This one would require Congress or courts or other outside bodies. I think those were reasonable classes into which to subdivide these ideas.

Here are some examples of the strategies. Low level: increased reliance on the geochemical barriers. As you remember, the SCP does not consider the geochemistry barrier in forming their calculations.

The travel time through the Calico Hills rock is sufficient so one does not need to rely on the geochemical barrier as a primary barrier. It is easy to shift that: just simply say so.

Of course, what goes with that is an Underground Test Program where you know the three-dimensional character of all those minerals that would be radionuclide stored; and, indeed, we have laid out a Site Characterization Program the purpose of which is to acquire a good three-dimensional understanding of the abundance of radionuclide stored minerals in the Calico Hills as well as in those rocks that are in the path underneath the repository all the way out.

Another one would be to complete the correlation between performance allocation and the Site Testing Program. That is relatively easy to do; and, in fact, we are doing that. We have paced exercises going on now in our testing program, and we also have this Surface-Based Prioritization, which in effect does the same thing.

These are two examples that are identified there. When we get down to analyzing them, we actually are doing them in the process of conducting our current program.

In terms of rulemaking, here are some examples. We take the initiative in rulemaking to try to resolve licensing issues as early as you can. Another one is to resolved disposal issues as part of Performance Conformation.

This one would suggest that you begin leaving waste a little bit earlier, and that you begin conducting programs to see whether or not your predictions are as reasonable as you had hoped they are.

Then you get into a high level of difficulty and introduce concepts like using a test and evaluation facility, as it is described in the Nuclear Waste Policy Act, or convert the Exploratory Shaft Facility to a demonstration facility which has high level waste. Both of these follow the Nuclear Waste Policy Act; but I am not sure.

I would have to read the writing very closely. We may have exceeded the time line on that one. But this one is definitely not consistent with the Act as it was written.

So these would require some significant changes, but they might be some interesting worthwhile programs.

That kind of gives you a picture of what is coming out of the atlas.

MR. BARNARD: Max, in the last two bullets you mentioned three kinds of facilities: a test and evaluation facility; a demonstration facility; and an Exploratory Shaft Facility.

What is the difference between those three?

MR. BLANCHARD: When one looks at the report and reads the description of the scenarios, you get a better understanding of how different they are.

I have not looked at the report for quite some time that supports the current draft; but my perception is that when they used the words "test and evaluation facility", they really meant it was that paragraph in the Nuclear Waste Policy Act.

When they are talking about upgrade Exploratory Shaft Facility, they are referring to doing things which are not current subscribed to in the program: like bringing in fuel elements, putting them in a waste package and running some underground tests, putting them on mechanical and so forth.

There could be other demonstration facilities encompassed in this contract: for instance, we could do it somewhere other than at Yucca Mountain.

All of those are not a part of the current program, and they all require other bodies to get into the picture and start thinking about how useful it is and how well can they be transferred.

It is not altogether clear what the benefit would be. You would have to look at the whole program to try to understand the scope.

Before long we will have this report available. I am sure Dr. Bartlett will be pleased to discuss it with you or have someone discuss it with you.

Do you have any other questions?

MR. BARNARD: In the first bullet you place reliance on geochemical barriers. Do you have a strategy that increases your reliance on long-lived canisters?

MR. BLANCHARD: Yes. It turns out to fit somewhere between here and here. If you said, "Gee, let's have a very robust canister. Let's have a goal a design goal of a 10,000-year waste canister," the current regulations of any of those would lead you to believe that the waste is assumed to fail at 1,000 years.

So if you were going to change the strategy to rely on a very robust waste canister and you were going pay a lot of money to get some materials that were going to last a long time, then you would have to do something to the regulations which would allow you to shift your strategy to take credit for that.

Maybe it is just a question of something like rule-making and the interpretation of the applicable paragraphs of NCR-60. On the other hand, it may not be that easy.

However, it is there. It is one of the scenarios.

I think it is 04, but I am not sure.

MR. BARNARD: How many strategies are there: a couple dozen? You have six listed there.

MR. BLANCHARD: The draft report is about that thick; and my guess is there must be 50 or 60 different scenarios.

MR. REITER: Max, I realize your report was written before the Academy report on rethinking came out; but I see there are certain elements here in which there is a bit of an overlap.

I wonder to what extent the ideas expressed in that document appear as ultimate strategies, in particular the whole concept that the whole legal regulatory framework, because of the uncertainty, is unsuitable for the task at hand.

I think the scenarios in here bound many of those that are identified in the National Academy of Sciences report, but not all. Of course, they don't go into the more philosophical nature of the Academy report, which talks about are we, as a country and as a group, using modeling and statistics applied for the purpose of which they are best intended, or in the process of trying to manage a program like this and made long-term predictions.

Are we misapplying them and generating, as a consequence, false confidence in predictions where there is significant inherent uncertainty in understanding the processes that shape the earth?

That seemed to be one of the fundamental themes I read into that report as I read it. I think that is very thought-provoking and very profound, and one that causes some search through this program to decide, "If that is the case, what do we do now?"

I don't think the atlas was that through-provoking.

Does that help?

MR. REITER: Thank you.

MR. BLANCHARD: Then I would propose to introduce the first speaker on the Surface-Based Prioritization Testing activity, who is Russ Dyer.

SURFACE-BASED TESTING PRIORITIZATION

Introduction

MR. DYER: As Max said, I am Russ Dyer. I work for the Rate and Site Evaluation Division at the Project Office.

I am going to run through a quick introduction to the Surface-Based Prioritization task. Let's start out with a program of what you are going to hear and from who: the cast of characters.

I will spend 5 of 10 minutes giving you an introduction, going over why the study was initiated, and showing you goals, participants and schedule. I will be followed by Bruce Judd who will talk about the decision analysis framework we are using in the prioritization. He will cover an overview of the methodology we are looking at, modeling-building and data assessment, and some assessments and analysis.

Currently we are scheduled at the lunch break at this point: between Mr. Judd and the following speaker, Steve Mattson. Depending on how the schedule goes, we may want to adjust that.

Steve Mattson will pick up next looking at possible methods to assess site suitability: looking at the suitability assessment and decision-making, and the relationship to the services testing prioritization effort.

Finally, I will be back to wrap things up and give you a Department of Energy perspective on the prioritization effort.

As you are probably aware, in November of 1989 we reported to Congress that we were refocusing our scientific investigations: specifically we were refocusing the investigations whether or not the site has any features that would indicate it was not suitable as a potential Repository Site.

This Task Force, this effort, was initiated in order to prepare this change in our charter.

There are three primary goals we are pursuing to reach overall objective. The first goal is to develop an explicit decision analysis method to prioritize the existing Surface-Based Prioritization Testing Program during the initial phases of side investigation.

This is in response to criticisms and comments from industry, the State of Nevada and the general public. This is an example document of what we would hope to come out with. It is a prioritized list of tests that we would initiate early on during the Site Characterization Program.

I would stress that, right now, we are only looking at the Surface-Based Testing. A later phase will look at the whole testing program.

The second goal of the Task Force is to recommend methods to re-prioritize testing at any point during Site Characterization: an aim that would allow us at points during the program to re-examine the testing program in light of data that has been acquired and, if necessary, re-prioritize the testing program.

That is what we see here in this block.

We have completed two tests. We have acquired some data from a couple of tests. That is what the check marks indicate. Based on that data, we have re-examined the existing tests and we have re-prioritized test four: we have decided it needs to be accomplished this test that used to be number three; and we have completely eliminated Test Number Five. This gets to Dr. Deere's question earlier.

The method also will give us a tool for deciding when to stop testing. We usually spend a considerable amount of time talking about the approach we are using.

The third bullet was to recommend a draft method for suit suitability at any time during Site Characterization. This should be consistent with what we are using for the first two objectives.

This is a decision tree that Bruce and Steve will lead you through several times. The decision was to proceed with licensing, continue the testing program or abandon the site.

Just for a quick overview of the personnel involved in this effort to date: Steve Mattson has been the anchor of the team; Bruce Judd, with Precision Analysts Corporation, provides the expertise on decision analysis; Scott Sinnock of Sandia Laboratories, performs assessment input: he has been a common thread through may of the Task Forces ensuring we have continuity between different Task Forces; Bob Williams of the United States Geological Survey; Martha Pendleton of SAIC; Bob Game of Weston; and Augie Matthusen of SAIC.

Bob, Augie and Martha have recently been tasked with working on the suitability methodology.

Doing the oversight is my staff: Jeremy Boak of the Yucca Mountain Project Office; Jeff Peter of headquarters; and Bill Haslebacher of Weston have been involved in providing oversight for the project.

The input today is based on existing site data and expert judgment. We have made extensive use of expert judgment so far, as Bruce will be explaining to you.

There are different site data from existing site-data bases and from prior studies, reports and such that are already in existence. The technical experts we have been using come from out National Laboratories and other participants: Los Alamos, Lawrence Berkeley Laboratories, Lawrence Livermore, Oak Ridge, Pacific Northwest, SAIC, Sandia, University of California at Berkeley, West GS, Weston Consultants, et cetera.

So far we have involved over 60 technical experts in the exercise to date.

This is a rundown of some of the schedule: the activities and deliverables. We initiated this project in January. Right now we are down from this level. We have pretty well developed the prioritization methodology. We think we are well along in defining the suitability methodology.

The next phase is data defining and analysis, working toward the final draft report on prioritization in late September, and the final report in October which will cover both prioritization and a recommendation on a suitability methodology.

The involvement of our expert panels has been focused along the topic of working groups, if you will. These are the topular workshops we have held to date.

Back in February we had sort of a kick-off meeting where we identified critical concerns and uncertainties. You will see the word "PAX"---we potentially had those conditions-- coming into the suitability issue.

We also wanted to identify any other concerns among our technical staff that might not be explicitly mentioned in the PAX. So this workshop was to identify other concerns and uncertainties that we needed to filter through the system.

This was held as a Performance Panel Workshop: unsaturated zone, saturated zone, migration, container and gas transport, and on. We are not through yet. We still have a few workshops to do.

This concludes my introduction part. I will be back to talk to you after the comments of the others. If there are any questions about the introduction I can address them now. If not, Bruce Judd will follow with the framework of the Decision Analysts' framework of the task.

Any questions?

MR. ROY WILLIAMS: I notice your list of sources of experts does not include any universities.

MR. DYER: That is true. We are mostly working from within the project right now.

We think of this as a scoping study. Perhaps in the later phase we can expand the list of experts we poll to take in more participants, more experts.

MR. ROY WILLIAMS: How many hydrogeologists do you have there listed as experts? I notice some of them.

I was curious since one of the problems here is the hydrogeology problems.

MR. DYER: I am going to have to--

MR. ROY WILLIAMS: There is a slide of that up there.

MR. DYER: Oh. I would defer to Steve on that. My guess is probably about a dozen.

MR. MATTSON: In terms of the core team, there is a little bit of difference.

MR. DYER: In the core team?

MR. MATTSON: Yes.

MR. DYER: Bill Wilson would be the only person in there that I would characterize as a hydrogeologist; but this is just the core team.

MR. ROY WILLIAMS: Thank you.

MR. McFARLAND: Early in the program--when it was introduced, I believe, in January of February--this particular study was introduced as the prioritization of all the studies.

The introduction you gave on page 4 indicates that scientific investigation would be prioritized. We have evolved now to just Surface-Based Testing.

Could you explain the rationale that led you to truncate the tests from all to just those that are surface-based?

MR. DYER: Okay. The original charter and instructions from headquarters was to focus on the Surface-Based Program because it looked like the Surface-Based Program--

Well, as Max and Steve mentioned earlier we think the Surface-Based Program will be underway a couple of years before the Underground Program. There was a need to examine prioritization of the Surface-Based Program with a higher priority than the total program.

Our initial charter was to focus on the Surface-Based Program. The method technique that we are developing can be applied to the whole testing program. In our second phase of the application of this method we intend to filter the entire testing program through the methodology.

MR. McFARLAND: Were the workshops advertised?

MR. DYER: Within the project.

MR. McFARLAND: No observers were invited?

MR. DYER: No.

MR. McFARLAND: You had a requirement on the participants, I would assume, that they have all had some exposure, background or association with the program?

MR. DYER: That is correct.

MR. McFARLAND: All from within the program.

MR. DYER: All from within the program.

There is an up side and a down side to that. The up side is that there is not much education involved: we don't have to spend much time bringing people up to speed. The down side is that we are using people who have an intimate familiarity, relationship with the program.

DR. DOMENICO: This seems like some progress from stuff I heard before. I hope we get around to hearing you present them when you have sited the tests: one, two, three four; and I hope some part of the program will put some emphasis on just what you are going to be testing for that you think is important in, quote, suit suitability as opposed to gathering information ultimately for license.

I think there is a big difference between what you are doing and gathering logged information for licensing. We would hope to hear just what your clinical concerns are how you are going to test for them.

MR. DYER: We agree completely. That seems like a perfect lead-in for the following speaker: Bruce Judd.

CHAIRMAN DEERE: While you are preparing, I will add one statement. I think we should go on record that we agree with you completely, Russ, that there is an up side and a down side to the use of only inside programs facilities.

[Laughter]

MR. BLANCHARD: Dr. Deere, could I help discuss some of the points that, I am sure, Russ was considered?

Each of the Task Force leaders was confronted with producing some early results which we could identify as worthwhile, and which would assure us we are moving in a positive direction with the tasks; and identifying those people in hydrogeology, geochemistry and rock mechanisms who have been working with the program, and who have authored a number of the papers that were cited in the SCP and who have been working with our PA people in doing that, who recognize we were working internally and not seeking a lot of outside expertise; but, at the same time, we felt we were working with people who had the most knowledge about those properties: about hydraulic conductivity and the values for radionuclide retardation for the individual elements in that spent fuel.

We felt that would give us a shot in the arm to get started fast. It has not been our intention to not include in these processes outside people. As you will see, some people who are not in the program were used as consultants where we could bring them up to speed easily, or where we knew from our own understanding of that individual they did have a lot of knowledge the properties of a bent tuft or a welded tuft.

In my introduction I talked about: What is management going to do to get these recommendations. It is the Department of Energy's intent to apply, where we judicially dutifully need to, peer review on these recommendations of these Task Forces.

Under the provisions of our Quality Assurance Program, a peer review is defined as people who are not within the program and who do not receive money from the Department of

Energy for working in the program.

Therefore, the Quality Assurance definition of peer review is outside experts. That is the direction in which we are going. Right or wrong, it seemed to be the best route given the charge we had at the time.

CHAIRMAN DEERE: Thank you.

Let's take a short break at this time.

[A brief recess was taken.]

CHAIRMAN DEERE: We will reconvene.

We know there is going to be lots of interest in this subject, as well as the past and the future discussions this afternoon. I would, therefore, ask that the questions be asked only by the Board Members, their staff and consultants during presentation if they feel they would like to interrupt.

At the end of the presentation again we will have questions from the Board. At that time, we would like to ask those in the audience who have a technical question to be sure an take opportunity to bring it up. I would simply ask that we save to the last the questions from the audience.

Position Analysts' Framework

MR. JUDD: I will be providing a discussion of the methodology, the analytic methods if you will, we will be using in the prioritization of Surface-Based Testing. I will be providing first for an overview of the methodology.

I should say that many of the elements of this method you will see in the other Task Force reports. So think of this, to some extent, as some of the methods used in all three.

This is the "what" if you will: What are we trying to do? The second part of it will be: How are we doing it? In particular the development of models that are used to provide quantitative inputs to the decision-making process, and also the data-assessment process that provided the quantitative inputs to the models.

Finally, I will give you an illustration of how it all fits together for Surface-Based Prioritization.

I would like to start out with a glimpse of the final product. Like any unveiling, I would like your first impressions, and I would like you to see what is the first thing you notice on the next viewgraph. As I say, this is a glimpse of the final product.

What was the first yellow box that you noticed?

[No response.]

MR. JUDD: We were at the Water Slide this weekend. We asked a cut little 5-year-old girl, "Take out of a hat some tests for and prioritize them for this viewgraph." So there is absolutely no analysis that has gone into the selection of priorities, tests or reasons, or anything on this viewgraph; but it is intended to illustrate the type of product we intend to have at the end of this process.

As I say, these are illustrative results.

Down the left hand side is a list of priorities: Priority One, Priority Two; high, medium, how. However you want to characterize them.

The next thing I illustrate are some, quote, tests. I think this is an important point.

You have already heard Russ Dyer using the word "tests". You will hear me use it. Each of us in the room may thing of different things when we think of the word "test".

Those most familiar with the Site Characterization Plan will think of the specific tests that are identified in the Site Characterization Plan; and yet you will notice that when I use the word "test" here I seem to be implying something perhaps different or more highly aggregated, or cutting across different activities or possibly even cutting across different study plans in the SCP.

I think the most consistent interpretation of the word "test", when I put it down, will be categories or test groups in the Site Characterization Plan; but even there will be some variation on those. Let me give you some examples.

For example, a test might be ground-water flow time in the unsaturated zone. I call that a test because it is work that is done to resolve or provide information about a factor or a parameter that is highly uncertain.

In this case, the uncertainty is: How long would it take water or a radionuclide to migrate in the saturated zone? Another example might be the retardation rate of Carbon-14, historical climate change, or matrix versus fracture flow in the unsaturated zone.

All of these are things that, throughout our discussion, I will be referring to as "tests"; and, yet, they are in many cases cross-cutting some of the tests in the Site Characterization Plan.

So if this is a glimpse of our final output there would be a list of priorities, a list of these categories or groups of tests, and then some reasons why these are more highly rated in terms of priority than some of these down here.

We have to keep thinking back to the charge we had: to identify tests that could provide early detection of unsuitable site conditions. These are not conditions we know about right now. These are uncertainty.

These tests will help resolve uncertainty or provide information about those uncertain factors. Therefore, in each of the reasons I list here "uncertainty" is a key word.

DR. DOMENICO: Those look more like issues.

Will you have actual testing prioritized under each of what I would call issues and you are calling tests: what you are actually going to do about the issue of ground-water flow time in the saturated zone from a testing perspective?

Would that be part of your product, too?

DR. JUDD: There may be a mapping from the items that are in the product straight to issues in some cases.

DR. DOMENICO: We are looking for the tests to resolve those issues, I think: at least in my view.

The question is: Under Priority Test Number One, would you have a suite of activities--like further tritium testing, Chlorine-36 testing, pumping testings--and priorities under those? Would that be part of that product?

DR. JUDD: The initial pass at this prioritization would take that entire suite and say: That suite of activities because they provide, perhaps, partial resolution about the uncertainty and travel time receive a higher priority than maybe another suite down here related to historical climate change.

Does that answer your question?

DR. DOMENICO: Yes.

DR. JUDD: Okay.

DR. NORTH: Let me follow up on that a little bit.

Do you have a methodology for grouping tests at the level of what is in the SCP to your tests, in quotes, suite, or "issue" in Dr. Domenico's terminology?

DR. JUDD: We have a mapping.

DR. NORTH: Can you tell us about the mapping or illustrate it in some fashion, like the ground-water flow in the saturated zone related to drilling specific bore holes, or using the tritium or chlorine data as that is available from existing data?

DR. JUDD: Do you mean an example?

DR. NORTH: Yes. What I would like to see is an example of the mapping so we get some idea of what is involved in doing it.

DR. JUDD: Let me give you one example. I will jump ahead to number 44, here.

The uncertainty that was being addressed at this point in our analysis was the characterization of flow in the unsaturated zone: frecht [ph] flow versus matrix flow. We asked one of our expert panels to provide a list of the categories of tests that would help resolve that uncertainty. This is the list they provided.

This is the distillation of about an hour's discussion, some of which provided very detailed lists of actual tests in these areas, and other parts of the discussions saying "Those can be characterized at a higher level like this."

So it is definitely a suite and it cuts across many different types of investigation.

DR. BLANCHARD: Mr. Judd, let me help ask the question that was asked by Dr. North.

DR. JUDD: Thank you.

DR. BLANCHARD: You may recall in, I believe it was, April when we were talking about the performance assessment links to the Site Testing Program we mentioned something called Paratrack, which is an extensive computer program that takes the design and the performance issues, works them all the way down to parameters that are coming out of the test program, and then shows from a performance or a design standpoint where those common parameters are used and how they are used.

That helps us assure that, if there are five or six different people or groups that are using hydraulic conductivity, we know which ones are using it for hot-ground-water travel time versus which ones are using it for drainage and, thus, a slope on the repository layout.

The Paratrack is part of this mapping Bruce mentioned where we have a road map which shows how these individual test categories that went into how this logic Bruce is about to tell you about works its way back and which parameters are more important for what reasons.

I cannot remember whether or not we gave the presentation on Paratrack. Did we? A brief one.

Oh, yes. There was one. It was at the close of the meeting: the last talk.

DR. NORTH: Yes. We have heard about it briefly.

The point I would like to raise here is that this map is not simple.

DR. BLANCHARD: That is right.

DR. NORTH: These are rather complex relationships, and the documentation of them is going to be quite important. In other words, when you really translate this into practice the interested parties need to be able to see how this map works in detail.

DR. BLANCHARD: You are quite right.

DR. JUDD: Thank you, Max.

DR. DOMENICO: That was a long glimpse.

DR. JUDD: I guess we ought to look at it from every angle, here: see what it looks like behind.

High level, high priority items help resolve uncertainty more than some of the lower priority items in this list. You will see throughout at least my discussion this morning that the concept of the resolution of uncertainty and early testing can help resolve some uncertainty about the unsuitable site conditions.

How do we approach it? As Max said, there is a very tight linkage between test priorities and site suitability. As the theme here illustrates, our analysis tries to identify tests that will significantly influence the decisions about site suitability.

I have now used a chronological sequence of parts of a decision tree here to illustrate the sequence of decision making, and then we will talk about how early testing can fit into the sequence.

Let's start with a decision--the decisions are colored in red--an early decision to conduct a particular suite of tests--again I abbreviate by saying just "tests"--early or do the status quo: two alternatives.

Status quo, in this case, would be the Site Characterization Plan as it is currently written.

What happens next? There is a series of things. I will spend a few minutes on this rather than jumping over it quickly.

First there are test outcomes; and I have said test outcome positive, test outcome negative. I don't mean to imply here good and bad. A positive test might be purged water was found. A positive test result might be retardation for Carbon-14 was quite significant.

I don't mean to imply good and bad with positive and negative. It is just that the test outcomes can come out one way or another; and we are not sure, early on when you decide to conduct the tests how the tests will come out. So there is uncertainty about that potential result; and that uncertainty here is indicated in blue. There is another one coming down there.

We feed that information from those test results into a decision about the site suitability. Hang on. I'm sorry, forgot an important factor.

You get the test results for the test outcome, but you also do some evaluation of it. You take the results and put them in a performance assessment, and see what those particular results imply about the overall performance of the site according to various criteria.

That evaluation is pretty important. There is not a single test result that you would get that would say "This suit, automatically, categorically, is unsuitable." You need to analyze that or evaluate that before you can conclude it.

That information--both test outcomes and their evaluation--feeds into a suitability decision which, as Russ mentioned, we represent using some simple alternatives here: recommend the site, abandon the site, continue testing. These are all possible decisions--and, of course, there are others a well--that might come out of the site suitability assessment.

But there is still uncertainty. Even though we have tested and gotten some positive and negative results, there is some residual uncertainty about whether or not unsuitable conditions either exist today or might exist in the future. So Even though we have done our testing we remain uncertain about them.

This is a chronology. So that says that this site suitability decision has to be made before we know for sure whether or not the condition actually exists. Testing and these test outcomes may reduce that uncertainty, but in may cases it will not eliminate it.

So our prioritization methodology looks at those decisions, and the possible results that the early tests might produce and tests that have results--positive or negative--that can influence that decision are given early priority.

How do they influence that decision? What is the mechanism for influencing a site suitability decision?

As point number two here states: Those tests that can influence this decision probably reduce the future uncertainty out there; and, hence, improve our ability to make a decision back here.

Therefore, these tests are reducing uncertainty about the future and, thereby, influencing our decisions. That is a conceptual picture of some of the features of the methodology. We will take more time now to link this together and give a more complete discussion.

Before I do that, are there questions about how that fits together?

[No response.]

DR. JUDD: I said these were parts of a decision tree. In this next viewgraph I have now connected some of the parts of the decision tree to construct a very simple decision tree: an idealized situation, if you will.

This shows how test outcomes might affect a site suitability decision. You have the same decision at the beginning: to conduct the tests early or conduct the tests--this was the status quo--in the planned sequence.

If you test early, I have illustrated a couple possible outcomes of the testing and the evaluation. You discover some results that, once evaluated, show there is a major problem for the repository. What, then, is the site suitability decision?

It may be. if this is a major problem, that the decision would be that branch down there: to abandon the site. This is an illustration of a preferred decision path. Up here, if there is a major problem discovered by an early test we abandon the site. If that outcome does not happen--in other words, if there is no major problem discovered--then there is a decision illustrated here.

Let's say the decision at that point is to continue with the planned sequence of tests. Just as if you did not take that early test up there and you continued on this trend, then you would continue with the pattern of testing.

So the decision that is made here and here is the same decision: to continue with the planned sequence of tests. It is only up here, if you discover a problem, that you would abandon site.

The point of this is that tests that have these possible outcomes that can change the preferred decision or change the preferred course of action--tests that can affect this decision and change it from what decision would have been made otherwise--those tests are said to have high value of information.

You will hear that phrase a lot today and tomorrow. They are said to have high value of information and, therefore, they would be given high priority in the analysis we are doing.

That is the technical description of value of information. We will give you some additional illustrations of that later in my talk as well as in the Calico Hills and Exploratory Shaft Facility talks that follow.

That concludes the intent of our methodology: to identify those tests that can affect that decision. I will now start talking in a bit more detail about how we go about that.

Any questions before I proceed?

[No response.]

DR. JUDD: The question at hand is: Which tests can be early indicators of an unsuitable site? In order to answer that I think you need to ask and then answer a couple more questions.

Those two questions are: What do our existing data say about the site? If we had to make the decision today, do the data indicate the site is suitable or unsuitable?

Then you immediately follow those questions with: How likely are the tests results--the things we might find from early testing--to change the conclusion about the site?

If those outcomes are unlikely to change the conclusion about the site, there may be little technical value for further testing. "Technical value": I will refer to that frequently as will at least one of the other Task Forces.

The technical value for testing came into consideration because we said: Is the site suitable or not based on what we know now? And can test results change that? If they can change that decision, then there would be some technical value in terms of reducing uncertainty. If they cannot, then there is not.

But let me be very quick to add something to that notion: There may be many other reasons for testing besides simply gathering information that affects site suitability decisions, such as conducting tests or groups of tests that will facilitate: other test-drilling boring holes, initiating long-duration performance conformation testing, gathering information that will be used for design or construction.

That information may not affect a decision on site suitability, but it may be very important to have: for instance, the geochemistry may be an important issue in choosing materials for the waste package. So there may be some significant benefit to testing that is not reflected in simply this relationship to site suitability: building scientific consensus; providing additional information that is required for licensing.

All of those are reasons for testing. So if we come up with a conclusion that says there is no technical value for testing because it is finding an unsuitable condition there may be other reasons for testing, as well.

When these are a factor management may need to revise priorities, and our Task Force may need to revise priorities based on each of these considerations.

There are some caveats to that technical value of information, but let's recall that our charge is to identify tests early on that can provide early detection of unsuitable conditions.

How do we go about that? You can always answer a question with more questions. In order to answer the two major questions I just listed I think you have to answer some additional questions.

By the way, this does not get any more detailed: I am not going to have any more lists of tests beyond this list of list of lists here.

When I am trying to answer, Do existing data indicate whether or not the site is suitable?, the thought process is something like this: What is the projected performance of this system? How well will this system perform? How certain or uncertain are we about the performance? How much confidence do we have in the performance of the total system: the site, the waste package, et cetera? When you put them all together, do they met our criteria for being a good system? Yes or no.

That is an indication, coupled with the waste package, of whether or not they are suitable. So I claim you have to go through that thought process in order to answer this first question; and you will see an analysis that steps through each of those in sequence, and I will get to that in just a minute.

Let me first introduce the kinds of questions you have to ask in order to answer this question: How likely are test results to change the basic conclusion about the site?

First, what are the major uncertainties at the site? What do I mean by "major"? That they can have a significant effect on the performance and they are highly uncertain. If they are highly uncertain, then maybe testing can help resolve them. If they have a significant effect on importance, that means they are important.

What are the major uncertainties? What tests can be done or what suites of tests to resolve these uncertainties? Very important consideration: How accurate are the tests? If they are perfectly accurate, that is great. Most tests are not perfectly accurate. That factor needs to be considered.

Finally, you ask: What specific outcomes of the tests? If we examine the mineralogy of a particular sample, what kinds of results from those tests can change our decision or could be put into a suitability assessment or a performance evaluation?; and the results of that change or decision.

Finally, are those outcomes likely or not?

That is a long list of questions to be answered. Yet that is the thought process we went through to develop the analytic method.

It is also a boring viewgraph because it has only lots of words on it. I am going to stop using words now and go into pictures. Before I do that, are there questions?

[No response.]

DR. JUDD: Okay.

How do I measure the performance of the system to get a what do existing data indicate about the site? One useful indicator of an unsuitable or unacceptable performance of the site--and I will refer not to post-closure: after the repository has been loaded and the depths have been sealed--is a measure of unsuitable performance of the site.

In our first application of the methodology we have been using the release to the accessible environment: cumulative curies released over 10,000 years. In our methodology that serves as a proxy for other applicable post-closure performance measures and, as you all know, there are several others; but we will this as a proxy in trying to rank tests.

If there are some tests that are not related to total system performance, post-closure, then the priorities would have to be re-evaluated; but in our illustration now and in our first application of the methodology we are using that.

That measure gets at this first question: What is the projected performance of the system?

The second question is: How uncertain are we about that performance? How confident are we in our current estimates of the performance? Let's do that in a picture.

The uncertainty in that measure is illustrated or represented using a probability distribution. The way I have drawn it is a complementary cumulative distribution, but let's start with the variable first.

The variable is how much is released to the accessible environment. I have done that as a ratio of the cumulative curies released for a particular radioisotope to the standard in the Environmental Protection Agency appendix to the regulation.

That ratio, if it is one, says that the total cumulative release over 10,000 years exactly equaled the number that was in the appendix of that standard. If it is ten, that means the actual release over 10,000 years was ten times what was in the table: .1 would be a tenth.

On this axis is complementary cumulative probability; or, in other words, the probability--let me pick a point here; the point is coming down to .01--that the release will exceed this amount, be greater than this amount. In this case, there is a 5 percent chance the release will exceed that.

Notice it does not say the release will be one times the table or ten times the table. It says the release is uncertain: we are not sure what it will be; and, in fact, it could be very low as a percentage of the table, it could be higher, and now there is a probability distribution drawn on those releases.

This curve or this probability distribution reflects the level of confidence that we have in the post-closure performance; and we talk about confidence, knowing about the site and confidence about knowledge about the site. This curve reflects that confidence, it reflects that knowledge.

If this curve heads way out here at sort of a 45 degree angle it says we are highly uncertain about the releases. If this curve plummets at some point, comes straight down, it says we are not sure what releases will be in this region, but we are pretty confident that they will not be above this level.

That curve reflects our confidence.

Questions on that?

[No response.]

DR. JUDD: I am sure you have seen this in many ways and could give me the lecture on this as well as I could explain it to all of you. That is the curve.

If we are trying to get a, first, what is the performance of the system we need a performance measure and we are using cumulative curies. How uncertain are we about performance? This illustrative curve illustrates that.

The third question is: Does the system meet performance criteria or not? As I mentioned, we are going to use the Environmental Protection Agency standard--or we will use, perhaps I have not mentioned that--we will use the Environmental Protection Agency standard as one possible criterion for judging post-closure performance.

The standard has a couple of points measured. It has, there has to be a 90 percent chance that you will be within one times the table or, on my scale: complementary cumulative, 90 percent chance that you will be within that means a 10 percent chance you will exceed it. That is this point here; and then there is another point here.

You have to have .999 probability that you will be within ten times the curies released that are specified in the table, of .001: you have to be less than .001 probability of exceeding that.

Does this curve, the purple one, meet the standard or not? The way I have drawn it, it does. We are on the left side, which says that our cumulative curies are less than the Environmental Protection Agency standard. For instance, the Environmental Protection Agency standard said that we had to have no more than one chance in ten of exceeding one times the table; and for this purple curve we have .1 chance in ten of exceeding a fraction: less than a tenth of the table.

So that curve meets the Environmental Protection Agency standard.

Is the site suitable? I have not said that. I have not said formally that this site is suitable, but I have said: Here's is a criterion that, if we applied it and it were our only criterion, yes, this site would be suitable: if this were the only criterion. In our first application of the analysis that is the first one we are going to use.

MR. ALLEN: And if the purple curve were correct.

DR. JUDD: Yes.

If this purple curve accurately expresses the degree of information or the confidence we have in that site, and if this purple curve makes it look like we know for sure it will be less than that when, in fact, we don't know that and we are highly uncertain, then I think this whole thing would be out the window.

Good point.

MR. CORDING: And you could have several purple curves, depending on, for example, how one laid out the canisters or avoided faults and things like that. Is that possible?

DR. JUDD: Yes, that is possible, of course. That is a very good point and let's illustrate it.

If we have a team of analysts, site experts and waste package experts working together and they say, "If we make some assumptions about this site we get a curve down here. If we make some assumptions about this site," about how the models are or about what parameters we assume about the geology, "we get a curve out there."

So you can have multiple curves there depending on what you put into that analysis.

What should the curve reflect? If we are highly uncertain about those factors you mentioned, then this curve ought to reflect that uncertainty. For instance, if the waste package material depends on ground water chemistry and we are uncertain about the ground water chemistry and, therefore, we are uncertain about the materials, that set of uncertainties should be reflected in a single curve when the decision is made about suitable or unsuitable site.

MR. CORDING: It could be a situation where it is not just uncertainty, but a fact that you say, "We know that we can achieve a different curve if we do something else." It is not uncertain that we do that, but we would then actually have a different curve.

DR. JUDD: Good point.

For instance, changing the materials in the waste package might pull this thing back or push it out. Very good point.

MR. ALLEN: But also it is true that you could have a given curve for a given model, assuming that model is correct.

DR. JUDD: Yes.

MR. ALLEN: But if you try to estimate the uncertainty of that model being correct, you could have quite a different version.

DR. JUDD: Let's assume this purple one reflects a particular model and you are assuming, in your analysis, that model is correct and someone says, "Here is an alternate model." You run that one through and you find out that it is over here.

Technically to have this curve represent the uncertainty you would need to have someone say, "Okay, I am going to bite the bullet and create the curve; and I assign a probability to the first model being correct and a probability to the second model being correct. I cannot rule out that second model. I think there is one chance in a hundred that it is right."

You put those two factors together and you wind up again with a single curve. That is a part of the analysis that is hard because people can defend the various models and it is hard to get somebody to say, "I think there is a 10 percent chance that model is right," versus 1 percent or something.

Excellent point.

So this curve, once it is constructed, shows the degree of understanding we have about performance of the site. That is the purple one. The red one represents a standard that can be held up, and it actually specifies how confidence we have to be or how confident we must be.

We have to be to the left of that curve.

MR. REITER: I would like to relate back to the concern raised earlier about invoking outside expert judgment.

I think the concern--and we have seen this in other kinds of studies--is that if you would go, let's say, to the State of Nevada or EPRI or some other group and ask them to convene some experts, they might have different weights as to which model is correct.

Therefore, it may not be possible to reduce all the models to a unique known distribution. I think the concern about making sure you reflect in your curve a wide range of opinion would express itself in knowing whether or not that curve is correct, or which way it is.

DR. JUDD: If you have a group that says that alternate model has only one change in a hundred of being right and another group that says, "No, it has only about a 50 percent chance of being right," then you can draw the two curves.

The philosophy we are pursuing in this methodology is, when we get that significantly different set of opinions, do the analysis with one set of judgments, do the analysis with the other set of judgments, and see how it changes the ranking of those early tests.

So when we have a difference in assessment of probabilities, we analyze both sets and ask: Does it change the ranking on which tests should be done early?

MR. REITER: So you are planning to go outside the program to solicit expert judgment to provide alternate weights?

DR. JUDD: What I said was: If we get alternate probability assessments. Our current set of experts have been those that were described earlier; and those people don't all agree either.

So we are getting alternate sets of opinion from them and that is how we are treating it analytically. I will let the answer stand on going inside, going outside.

DR. DOMENICO: I would make a point here.

Whether or not you make that standard in this particular case, assuming you can keep the package intact for the first 1,000 years, is going to depend on a few things: critically ground water travel time, critically the flux of the mountain, the critically the retardation capabilities if that is built into your model, and lastly maybe the solubility of the individual nuclides.

Does that not tell you, already, something about your prioritization of testing? Does that not now give us some clue as to the priorities of testing in the sense that that curve is going to depend basically on those factors?

DR. JUDD: Yes.

You are saying that the curve--in other words, the performance of the system--depends heavily on those factors; and now the question is: How likely are those tests to affect that curve?

You are saying because there is a strong dependence of that curve on some of these basic uncertainties therefore the testing will affect that curve and affect the suitability.

DR. DOMENICO: I am saying more than that, I think. I am saying it is almost dictating your priorities in testing, what you have to test for, if you are looking for failure.

I have the feeling that this would be a great exercise if we were ten years in the past and we knew nothing about the mountain. But we have a billion dollars worth of information and we do know something about the mountain.

I think the position of that curve depends on those three or four factors that I just mentioned. My point is that that should guide, somehow, in the establishment of priorities.

DR. JUDD: You will see all of those factors identified--What are the major uncertain parameters?--and then about those we will ask: What tests can be done? How accurate are the tests? Et cetera.

But, yes, you will see those factors in there.

MR. BROCUM: I want to make one point here.

In fact, those points are guiding much of the program today. For example, we have an extensive geochemistry program. One of the major programs we would like to start on the current plan testing is studying unsaturated zone hydrology.

I think the points are, in fact, reflected in the Site Evaluation Plan, and reflected in the ongoing investigations and the investigation we plan to start when we can get permits to get on the site.

DR. JUDD: Thank you, Steve.

I am now going to back away from this specific measure of performance to a more general one, and back away from the Environmental Protection Agency standard to a more general concept; and proceed with that general discussion.

We are not limited to a particular performance measure or a particular standard for what is a suitable site or not. In fact, in concept, we can construct a decision line on the graph to indicate where we would judge the site unsuitable or judge the site suitable.

The Environmental Protection Agency standard, let's say, is over here somewhere; and, yet, the Department of Energy might want to be more conservative than that. We want to be a little bit this side or an order or two of magnitude this side of the Environmental Protection Agency standard because there are other performance criteria or for whatever reason.

Therefore, conceptually you could draw a decision line there and say, "Now, if our performance is worse than that we will walk away from the site. If our performance is better than that the site is acceptable and we will recommend the site."

Here is that same performance curve, but notice now that I have not given it cumulative curies released relative to an Environmental Protection Agency standard: it is just a general performance measure; and I will speak in these terms.

Max mentioned in his discussion how the Department of Energy might choose to build some conservatism into drawing that decision line at some distance back from the Environmental Protection Agency standard.

So I am going to refer now to this as being the point at which you change the decision. It is not necessarily the Environmental Protection Agency standard.

The basic conclusion would then be: If your performance assessment shows you are on this side of the line, the site is suitable. If there are some early tests you can do that would show you this site is unsuitable--in other words, throw that line over to the other side--those tests might receive high priority.

That gets us into the topic of testing. You recall the issues we wanted to address here are: How likely are the test results to change the basic conclusion about the site up here? What are the major uncertainties? What is the accuracy of the tests? Et cetera.

Let me use that graph to illustrate how that would work.

We evaluate the effects of testing, or the outcomes of those, on estimates of total system performance. The purple curve is the one we had before.

Let's say that one of the uncertainties in the problem is flux, and flux is highly uncertain. That is one of the factors contributing to the uncertainty in that performance curve.

Now let's conduct some tests that have to do with flux, that provide information about flux. We might learn two things from that testing program.

The first of the two things is we may get some indication that flux is on the high end of the range. We might get some indication that flux is on the low end of the range. If you put high flux into the analysis it will shift this purple curve to the right where the green one is; if you put low flux into the analysis it shifts it back to the left.

One thing we might learn is flux can be high or flux can be low.

The other thing that happens--and this is an important fact, also--from the testing we gain more certainty.

Notice that the purple curve went from this point all the way over to this point; it is highly uncertain; and the green curves are narrower reflecting greater certainty in both cases.

Where the flux is low the green curve is still more narrow than the purple curve was. If flux is high the green curve here only covers this much of the horizontal axis whereas the purple one covered that whole axis.

So we learn two things from testing: We know something about flux; and, of course, we might learn it is very close to our initial estimate and, therefore, this green curve is right on top of that one. We learn something about the flux and we reduce our uncertainty.

Therefore, our estimate on performance is more certain after testing; but, of course, is either better or worse--it can be on this side or this side--relative to the original performance curve.

You will see in the example I give you a bit later that the first step in our method is to determine the sensitivity of performance to what you might learn from the various tests. Then, of course, the next step is to draw that decision line, the red line in this case, and say, "Is it possible that something we will learn from testing," like the green curve, "will get over here to the right side of the curve?"

That will be a case where performance is now unsuitable--we are on the unsuitable side of that curve--so what we learned in the test detected an unsuitable condition and made abandoning this site the better of the two alternatives illustrated here.

Therefore, this would be a case of the test changing a decision or affecting a site suitability decision; and that would be a primary factor that goes into the priorities.

Other factors that need to be taken into account later are cost and schedule; but first, before we look at cost and schedule of the testing, we need to figure out: Are those tests capable of providing information that would show the site is unsuitable?

That was our charge: early detection of unsuitable conditions.

That is a graphical description of the methodology, if you will. It identifies tests that can change a decision from the decision that would be taken without the additional testing.

There are some caveats. First you need to recognize what our results will show is that testing has value when it has the potential to affect a repository decision; and we will recommend conducting tests early if they have a significant chance of affecting that decision.

We will recommend stopping testing, at least from a technical point of view: technical value of the testing, when the costs of doing the tests exceeds its value. That is where the "Stop" sign comes in.

As soon as I mention I need to go to this next point that says, "We are focusing only on the value of information provided by the test relative to post-closure performance," there are other benefits of testing besides this one that is listed.

But by focusing on what is the value of information we avoid a problem. The problem we avoid is also listed on the slide here, and this gets back to the Chairman's question earlier: something about when do you stop testing?

By focusing and asking the question, "What value do I get from the information?", it puts a pragmatic spin on the testing program. From a management point of view it says, "Can we, at some point, stop the testing because we are not learning anything further about the site that will change our decisions about its suitability?"

So we avoid to some extent that quest for the last decimal point on a particular parameter, that often futile quest for certainty.

That is an overview for you of the methodology. This might be a good point to see if there are other questions.

MR. PRICE: Do you have any comment on the gradeability of scientists to take the test results and magnify from that uncertainty instead of reducing certainty, which is sort of the general tenure of what you have presented to us?

Oftentimes you conduct a test and the next thing you know you are more uncertain than you were before.

DR. JUDD: Yes. Exactly. Good question. Let me use one of my curves here to illustrate that.

That happens all the time: You conduct a test and you learn something that had not been considered beforehand. You are saying that after we conduct the test we are more uncertain than we were before.

Therefore, it is as if we were drawing this green curve and, after we do the test, this thing stretches wider than the original curve did: we are more uncertain after testing. Was that it?

MR. PRICE: That is the gist of it. I did not know if that would change your purple curve.

DR. JUDD: I think you need to change the purple curve. It is sort of irrelevant to change it at that point because it is after the testing and green is the color for after testing.

What happens when you test and wind up more uncertain after you test is there are some important things that you did not think about when constructing this curve.

How does this prioritization method handle that? If there are alternate conceptual models, alternate ways of thinking about the problem that people can identify early in our discussions or during our discussions and get the quantified and into the purple curve, that is great.

If there are things that people cannot think about because there are things out there that we don't know--and, by the way, we don't know what they are: we cannot identify them, we cannot put them in the analysis; the unknown unknowns, what is lurking out there--I would challenge anyone to put those into analysis if they cannot be identified.

So we can go halfway, I think, in meeting your concern.

Those results that can be anticipated or considered possible can be brought into the analysis. Those that simply cannot be identified cannot be brought into the analysis.

Very good point.

MR. ALLEN: The very fact that, very often, increases in scientific knowledge lead also to an appreciation of greater uncertainties should lead us all to be, perhaps, more conservative in our estimation of uncertainties on the basis of the limited data.

DR. JUDD: Yes. We need to be a little more realistic initially when we state just how uncertain we are. The psychologists who do research in this area show that when we estimate probabilities we think we know more than we do.

We are too narrow. So our probability elicitation techniques try, as best they can, to get people to think of broader uncertainties than they might normally have done. In other words, would try to get them to be more realistic in exactly how uncertain they are.

I cannot guarantee we succeed in that, but we at least recognize it and make an attempt.

MR. ALLEN: I think that at least my experience in trying to understand earthquakes on the west coast is that very often the problem, it turns out, is that we assume a certain model with a great deal of certainty and the problem is with the model: that there were other models that should have been considered.

That is where the uncertainty is.

DR. JUDD: Exactly.

In the Exploratory Shaft Facility analysis you will see some explicit representation of that uncertainty both in the parameters of the model and in the model itself. To the extent we have been unable to take that into account and identify it, we have. You will hear more discussion of that later on.

That is a summary of what we are trying to do. I now need to add it is great to say this and say that is the intent. It looks relatively simple when you put it on a graph. Implementing it is difficult; and it requires some very careful development of models and assessment of data. That is what I would like to talk about now.

Our project comprises five tasks. The first is developing the methodology. In everything you have seen so far we have worked out how we are going to do it. There are some other issues related to how to draw those decision lines and some other factors on which we have not yet finished our methodology.

Developing the model and assessing the data is mostly what I am going to be talking about in this discussion now. We are well on the way, as Max pointed out; but, as I say, it is a difficult and large task. So there is still work to be done.

Analysis and review, and report preparation. We do some of that as we go along. As we get a portion of the model completed we do the analysis associated with it; but we have not done very much to date.

Here is a schematic of some of the steps and components, if you will, of our framework. We are trying to take lists of tests or categories of tests and set priorities. So we are trying to get from here to there.

In order to do that we need an analysis model. I will be describing that and how we build it. Out of that model comes these purple curves; and input to the model is the red decision line. Out of that process comes priorities.

I gave you the flavor of how we get from there to there, but did not give you too much of the specifics.

I am going to focus, in this discussion, on how we get to the model, the structure of the model and the data that are assessed to feed that model. Of the diagrams you see here this one is a probability distribution. You are familiar with those and you will see a lot of discussion of it in this talk as well as the others.

Then this is a representation of an influence diagram. Max and Russ both mentioned those. I will use that as a technique for creating the model: in other words, identifying the parameters that ought to go into the model and the relationships.

We also use this influence diagram as a guide to which data to assess when and how to assess it. So this influence diagram is an important piece of the analysis.

Each of the groups you will see will be displaying these influence diagrams. So if you will bear with me I will take five minutes to explain a little bit about that influence diagram because you will see a lot of those.

We are going to use these influence diagrams to identify the key parameters that ought to be in the model; and, also the relationships among factors or variables in the model.

For example, let's say an issue--if I can use that word--or a parameter or an uncertainty in the model is the travel time for Carbon-14 in the unsaturated zone. If I wanted to make an estimate of that--let's say I were an expert in that topic; and, of course, I am not--I might say, "I could make a much better estimate of that if I knew the flow time of," let's say, "an inert gas in the unsaturated zone; and I could make a better estimate of this if I knew what retardation," if any, "there is for Carbon-14."

If I knew these two factors I would make a better prediction of how long it would take Carbon-14 to be transported through the unsaturated zone.

The influence diagram illustrates the factors that are important to making estimates on these high level bubbles, if you will; and we tend to illustrate only the uncertainties in these bubbles.

For example, if I want to estimate travel time if I knew gas flow time that would be helpful; but maybe I don't know that and maybe I need to know some other parameters. One of the parameters I need to know for the gas flow time is the thickness of the unit. Maybe that has been measured to many digits of accuracy. So that is a known parameter, and it might not appear on the diagram.

As you see these influence diagrams you may say, "Some factors are missing." They may be missing because they are known. These things are used primarily to illustrate what is unknown: in other words, what is uncertain.

You tend to construct these influence diagrams from the top down, and the arrows have a special meaning. They have to do with probablistic dependence and independence; and I will illustrate that as we go along. I don't want to mention it here.

You will see these bubbles. You have, I am sure, used something like these in various ways if you have been doing analysis. These may have some different rules than the ones you are used to, however. So every now and then that pops up and surprises you.

It surprises you that the influence diagram seems to have a different set of rules from another bubble kind of chart you might be using.

As Max mentioned, we have developed a common set of influence diagrams. Maybe they don't use exactly the same words, but the importance factors are there in each of them. We have developed these for the three Task Forces that are listed here.

For instance, if what we are interested in projecting is the post-closure performance of the repository system as a whole measured in terms of dose or health effects, we might start an influence diagram at this level and then say, "In order to estimate that what things do I have to know?"

One thing you need to know is how much is released to the accessible environment, and then how that is transported.

For our study--Surface-Based Prioritization--we are focusing on this level, release to the accessible environment, because we are going to use the Environmental Protection Agency standard which sets a criterion at this level. So we have not done the translation up to other levels.

You will see the Exploratory Shaft Facility group is working up at this level because of the nature of their problem. They are considering cost and schedule, and other things that need to be traded off with post-closure performance; and, it is, in the judgment of that group, easier to work up at this level for those kinds of trade-offs than to make trade-offs that involve this level.

So you will see different levels of analysis in these influence diagrams, but the influence diagrams are consistent at least at these upper levels. As you push to lower levels, some of the influence diagrams go off into more detail in areas that other diagrams for the other Task Forces do not.

What do I need to know in order to estimate releases to the environment? If I am going to develop a model that produces an estimate of this I need to know about direct releases, waterborne releases and gas-phase releases. So this is the way we divide that up.

I am going to take that middle yellow bubble, waterborne releases, and begin to break that down. If I wanted to estimate the waterborne release, I need to start with a container; how much is released from the container; and how much crosses the engineered barrier system to the unsaturated zone.

The next important question: How much of the waterborne release is transported through the unsaturated zone to the saturated zone, moving down for example. There also could be mechanisms for moving in other directions; but this one says if I am going down this is the next logical step.

Finally, how much is released to the accessible environment.

In developing the models--I am referring now to our model development--we begin to break these things down at that level. Then, of course, each one of those can be broken down.

Let's take what happens in the saturated zone. There is the flow time of ground water, and there is also retardation: both important factors that need to be looked at; and then we continue down to the point where we can assess probability distributions from experts. I am not there yet.

We could ask somebody, "What is ground water flow time in the saturated zone?"; but that individual might say, "If I could break that up into flux, porosity and distance I could make a better assessment."

Then you begin to ask these experts about flux. That could be broken into the saturated zone, the gradient, and hydraulic conductivity. That, in turn, can be broken down into the various factors.

When we did the assessment of flux in the saturated zone, the influence diagram was broken down to this level and then we assessed probability distributions on these variables.

Boy, is that a technical term. What did we assess?

We asked the scientists how much the don't know. "How much don't you know about flow principles?" "How much don't you know about transmissivity?" In other words, what is the degree of uncertainty in these factors.

Yes, Warner?

DR. NORTH: Bruce, would you comment on the use of performance assessment models in this process? Are you dealing only with expert judgment or are you dealing with experts supported by all the models and the data they have available to them?

DR. JUDD: It is the latter; and, in fact, we asked people when they come to the workshops to bring with them printouts of results, et cetera.

There are modeling activities going on in the performance assessment area to calculate release at the upper level from parameters such as these, and also to calculate release at the upper level from much lower parameters.

Sometimes the individuals doing that performance modeling are different from the experts to whom we have been speaking in order to assess these probability distributions.

So sometimes the people who have given us these distributions may not be totally familiar with some of the modeling that is going on in the performance and assessment area.

Am I coming close to the answer to your question?

DR. NORTH: Yes, and it leads me to comment again on the need to document this kind of mapping. The way you get the data and the judgment that goes into it is not a simple process. It is extremely complicated.

For those who are not so familiar with influence diagrams, you might want to comment on the relation of this sort of an exercise to more traditional sensitivity analysis in systems engineering.

DR. JUDD: Okay.

The construction of the model from the factors is, I think, basically the same in systems engineering as we on this Task Force are doing it. That part of it is the same.

Let me make sure this is the question you are asking.

When I am trying to determine the sensitivity, for instance, of my upper level performance output to uncertainties in the problem, when we conduct that sensitivity we reflect the assessed probabilities down at this level and then flow the up through the model and show the effects of that assessment on the upper level parameters.

What is often done in systems engineering is to construct a model that has maybe these variables in it and then change the variables by 10 percent or 1 percent, and determine how much the upper performance measure changes.

This method does not do it that way. It asks, "What is our degree of uncertainty in these parameters?", and "Let's determine sensitivity of the upper level to that range of uncertainty," rather than just an arbitrary 10 percent variation.

Does that get at your question?

DR. NORTH: Yes. Thank you.

DR. JUDD: You will see other Task Forces talking about assessing probabilities at a different level in the influence diagram: in many cases, at higher levels, assessing them up there rather than down there.

The reason for the difference is due to the nature of the analysis that is being done.

We are trying to set priorities on tests and it is down at this level where there begins to be a fairly explicit concrete mapping between the actual tests being done and the factors in the influence diagram.

MR. ALLEN: What if it turns out that you very accurately assess the uncertainties of flow thickness, transmissivity and porous fracture flow, but it turns out there is some other element, some other bubble you have forgotten about, which is really dominating this thing and somehow you have not included it on the influence diagram?

DR. JUDD: Yes. We are going to miss it.

If it is not put on the influence diagram it does not get put into the analysis.

MR. ALLEN: Is that not a very real possibility?

DR. JUDD: It seems like a real possibility.

Each of our workshops--for instance, our workshop on the saturated zone--had a table this size and maybe two-thirds of the number of people sitting around the table. The reason for getting all those people there, even though not all of them were expert in each piece, was to try to get most of the things on the influence diagram, at least those that group knew about and was familiar with.

If those groups are not familiar with something it will not appear in the influence diagram; and if there are major tests that ought to be conducted on parameters that are not being analyzed here, that represents a potential shortcoming.

On the other hand, the groups with which we have met have taken a look at these, helped us develop them to some extent, and then at the end of the workshop said, "Okay, you have the major factors. Now let's worry about what the uncertainty is in them and what the tests are that we can do."

Warner brought up the important issue of modeling. This describes the factors, only. This is not yet a computer code.

We are constructing a series of models that can be used to take those judgments, down at this level for example, and calculate what we are interested in at the top, which is performance or curies released over 10,000 years to the accessible environment.

This model is a performance assessment model, if you will. The one we are using has less detail than the ones that are being developed for the site suitability assessment, et cetera. It has less detail because what is important to us is to get from issues like these or factors like these up to performance rather than greater detailed issues below this up to performance.

So our level of analysis is more aggregated.

I mentioned simulation here. There are so many variables in the problem we are using a Monte Carlo simulation technique rather than a decision tree; but we can go back and forth between those techniques depending on the complexity of the analysis for a particular radionuclide.

But for most of them we need a simulation model, and that is that indicated there. So the model is a representation of these factors that allows us to get from the lower level assessments up to the top.

When there are alternate models--when some has said, "You have those variables and that is the standard way to think about the problem, but you should recognize there is another way to do it"--out intent then is to be able to reflect both of those models in this one and be able to do the sensitivity analysis to say: Does it matter to a calculated performance result which if these we use?

MR. PRICE: I think Clarence's comment struck to the validity of the models that when we are dealing with a 10,000-year framework we are in danger of creating elaborate models and elaborate processes that end up, when we put all the numbers together, as really a form of numerology.

I think the Academy of Sciences' report was somewhat directed in this area.

DR. JUDD: Good point.

MR. ROY WILLIAMS: By a simulation model, are you talking about deterministic type models?

DR. JUDD: Deterministic?

MR. ROY WILLIAMS: Are you talking about deterministic simulation models?

DR. JUDD: No.

In the model there would be a set of equations in which, if you plugged in one value for each of the parameters in the equation, you could compute one output number, which would be one level of cumulative curies of release. Maybe it is 890 curies of Carbon-14 over 10,000 years.

The equations in the model are deterministic, but the inputs to the model--for instance, the flow thickness and saturated zone--are probablistic and so you take this model, which itself is deterministic, and run it three times, a hundred times, a thousand times picking points off the probability distribution for each of these uncertainty variables and, therefore, computing a probability distribution on calculated performance which is the purple curve.

Did I answer your question?

MR. ROY WILLIAMS: Yes. I just have one more.

So the expert judgment comes in in deciding what probability distribution is acceptable, defensible? I am trying to tie it up with what you have already said.

DR. JUDD: Yes. Good question. Let me twist it around a bit and see if this answers it.

What is a good probability distribution on flow thickness? Is it one that is defensible? Is it one that is conservative? Or is it one that is, yes, prudent and defensible, but one that accurately reflects how much we don't know about flow thickness?

It has to be wide enough to reflect the true degree of uncertainty in the parameter; and if it is too narrow then it is, in my opinion, a biased probability assessment: biased toward more certainty than we really have; and we are going to get bad results if we all assume we know more about these variables than we do.

By the way, testing will show up to have no value if we are highly certain. I think the important thing is getting people to represent their true range of uncertainty on this, and to do it in a prudent and defensible way.

In our workshop panels we often hear the phrase, "To be conservative, I will tell you . . ." these numbers; and our response is, always, "Don't be conservative. Tell us how uncertain you really are."

"Well, there are things that could lead us to that end and there are things that could lead us to this end."

"Good. Now we are getting at that true uncertainty."

This model is a key. What it computes, then,--calculates--is the purple curve; and then we take some additional steps--which I will not illustrate here, but I will in the next section--to say if we did testing how would that curve shift down or shift up?

If we did some testing related to the flow thickness can it shift our uncertainty low or high to the green curves? Of course, it may make the uncertainty more narrow or the range of the performance measurement more narrow.

Therefore, that model is critical in computing both this purple curve and the green sensitivity curves. As I said, the decision line we are using in the model initially is the Environmental Protection Agency standard; and you will see that in an illustration which is the next part of the discussion.

I am going to give you an example related to gas release.

Are there questions? We are at a good question point.

DR. NORTH: I think I would like to ask a general question both to you, and to Dr. Blanchard and others, getting at what are we trying to do here, and how does it relate to recommendations this Board made in its first report and in the just-released National Academy Study?

I am going to read some excerpts from page 29 of that study under the heading of "The Elements of a More Flexible System". The Academy reports starts off:

> In a program governed by this alternative approach, change would not be seen as an admission of error. The system would be receptive and responsive to a continuing stream of information from Site Characterization.

Then I will skip down to the first bullet, which they label "Iterative Performance Assessment":

> The basic approach outlined here would start with a simplified performance assessment based on known data and methods of interpretation. Given the inherent uncertainties and technical difficulties of the process, the present system may well expend large efforts on small risks and vice versa. An iterative approach, on the other hand, could allow characterization to give priority to major uncertainties and risks while there is still time and money left to do something about them.
>
> As in probablistic risk assessment, analysis focuses on efforts to reduce the important risks and uncertainties. In this case, that means acquiring information on the design features and licensing criteria

that are most likely to determine whether the site is suitable or should be abandoned.

My question is: Is what you are doing consistent with this statement?

DR. JUDD: I would say it is. Let me ask Dr. Blanchard.

DR. BLANCHARD: I certainly believe so. We tried to structure this with Bruce in a way which would allow us to take the available data and to get the experts to give us their view on the probability of what are the conduct hydraulic conductivities, and give us 10, 50, 90 confidence values and even values way out at the 99 percent level; tie those, through the influence diagrams, to radionuclide releases; and then put this in a system where we have the capability of using this methodology in near real-time so we can look, as new information comes in, at what the impact would be on these performance predictions made by the experts and substitute for the old data the new data set; and do this in a time frame, in a managerial construct, which allows us to make changes as we fund activities.

Our goal is to put this in place for Fiscal Year 1991.

DR. NORTH: In other words, this is an exercise not to be done once and put on the shelf; but, rather, this is an iterative management tool to be used again and again and again as different questions are raised or different data becomes available.

We can then take it back to this structure and iterate the performance assessment yet one more time to derive insights with respect to which uncertainties or risks may be more important while there is yet time to do something about them, in the words of the Academy.

Is that what we are saying?

DR. BLANCHARD: You are quite right. Our perception is that we will be able to do this. Dr. Judd has worked hard in order to set up a structure and a methodology which will allow us to be able to do it with not a great amount of difficulty.

Wouldn't you say that is true, Bruce?

DR. JUDD: The hard work, thank you.

MR. ALLEN: Warner, does your tone of voice imply you feel otherwise?

DR. NORTH: No. The question, I think, of where we are going with all this complex decision analysis methodology is an extremely important one. I think it is important to clarify the use of this methodology as an ongoing management tool for dialogue and discussion about this problem area as opposed to we do it once, we get a number and, based on that, we made a conclusion; and then we are over and done with.

I think the Academy study is recommending the iterative approach. I think that is consistent with what you just presented.

However, making sure that we all agree on this point seems like a very useful thing to do at this time.

DR. JUDD: Thank you for making that point.

Dr. Steve Mattson, when he is talking about the Site Suitability Assessment, will refer again to that iterative nature of the analysis and the decision-making that goes along with it.

I would like, now, to illustrate this with an example taken from an analysis of gas-phase releases. This is an analysis we put together for this briefing. Some of the data you will see are data that we assessed from our workshop panels; some are data we provided on our own; and some are data we had from the workshop panel and we changed them somewhat for the purpose of the illustration. Please don't take this as anything other than illustrative.

In order to do an analysis of gas-phase releases we begin to break down the problem into release from the waste package to the unsaturated zone--this is going up, now--and then from the unsaturated zone to the accessible environment.

The last part of that--from the unsaturated zone to the accessible environment--is similar to what you have seen in other discussions: You need to know about the flow time of not only radionuclides, but an inert gas, let's say; and then add in the retardation, if any, for the particular radionuclide of interest here.

In order to get at what is released from the waste package, we can begin with: What is the inventory? That is uncertain. What is the fraction of that inventory that is volatile and released rapidly as the container fails? Then we have a more general question: What is the overall release rate from the package?

Those are factors that are all uncertain. They need to be analyzed in order to determine test priorities in this area.

What about this one? I have three factors down here. The condition of the package and how that influences the rate of continued failure and the rates of failures of the cladding in the fuel that is inside the containers. Not all the containers contain spent fuel, but for those that do what is the rate of failure of the cladding?

These package conditions are further modeled by the site condition, the properties of the host rock, the flux or the hydraulic conductivity of the host rock, and the flux.

I said we are talking about gas, and yet notice that we are quickly into some issues that relate also to the waterborne releases. So here are a couple of factors that influence, eventually, gas releases. The same factors are put into the analysis of the waterborne release.

There are a variety of factors there that needs to be considered. We have assessed probability distributions on eight of these parameters. The assessments here are also used, as I mentioned, in the waterborne; but we will just take the gas-phase part of it.

There are eight factors there listed as uncertain. In our workshops we assessed probability distributions on those factors. Let me give you an example.

The rate of failure of containers is indicated by this diagram to depend on the conditions of package. Containers will fail more rapidly under certain conditions than under other conditions.

There is a gross characterization here of the conditions of the package as wether dry. Conceptually you can imagine assessing a probability that the conditions will be wet, a probability that they will be dry.

In fact, this is a continuum, but for the example I will make it a discrete choice: either it is wet or it is dry. We had a third one in the analysis: moist condition; but some of these data looked similar to the wet condition so I have simplified the example.

Let's say conditions are wet; my container-failure rate depends on that fact; and that is illustrated here. How do I quantify container-failure rate?

In this example, with this workshop, the mean time to failure, the average time to failure for all the containers in the repository was the measure these people felt was the best way to quantify the failure of the containers. So for a wet condition we assessed a probability distribution on how long the mean time to failure would be.

This group was highly uncertain about that mean time to failure. There were many parameters that are not shown on the page anywhere here that influence this uncertainty. What is the material of the canister? What is the chemistry of the environment in which that container has to exist?

All of those were factors that were discussed in the process of assessing this curve and, to the extent possible, they were reflected in this curve. The low mean time to failure might be a real mismatch between the chemistry and the container design. High time to failure might be a good matching: even though it is a good match, we have a good match of the chemistry of that environment to the materials.

The reason I am going through this is to illustrate there are a lot of things you need to think about when assessing a probability distribution. Here is an example of an illustrative distribution that was assessed. We did it also for dry conditions.

You will see me, in subsequent viewgraphs, use three points to characterize that distribution: the 90th cumulative percentile point, the 50th percentile, and the 10th percentile point. Those three points are all the information we carry forward into the next two or three pages. We do the same thing for the dry conditions.

We could have assessed probabilities on wet and dry, but in fact we computed them from flux hydraulic conductivity types of calculations.

Here is a page of other illustrative assessments that were needed for each of those variables. Let me pick out one of them here because I will be referred to that later.

Let's take gas-flow time in the unsaturated zone. This illustrative distribution had a 10 percent chance that flow time will be ten years or less: rapid transport through the medium; a median, or 50th percentile, point of 50 years; and a 90th percentile that the time would be 300 years or less. We use that we go farther.

If you put this together in a model it computes the purple curve. In other words, it computes a probability distribution that takes into account all of those eight uncertainties and computes cumulative releases for all the possible combinations of those releases.

Initially we did this with all possible combinations, then we simplified it somewhat for this graph. You get about the same picture in either case. So we get the purple curve computed and, for reference, we drew the Environmental Protection Agency standard on this curve.

This is illustrative, created for this discussion. We are not trying to make comments about where we are relative to the standard; but to illustrate the methodology we wanted a curve that was somewhat close to the standard.

MR. BARNARD: Is that real or hypothetical, then?

DR. JUDD: It is a real calculation based on these inputs; and these inputs came some from the annals that we talked to and some we provided ourselves. I put more credibility on the ones that came from the panels we talked to, although our intent is to do this iteratively: to take these initial results, compute curves like this, find out which of these assessments are most critical to these curves, and then go back and talk further with those experts.

MR. BARNARD: In some of your previous examples you had green uncertainty curves, too. Do you have some green curves for that one?

DR. JUDD: I have some green ones coming up.

MR. BARNARD: Okay.

DR. NORTH: Bruce, before we leave this I would like to observe in passing that I am very skeptical about how fast that curve falls off: how steep it is as we get down there.

I realize that it may have come out that was as an artifact of taking 3 point representations of the kind you just showed us. That is fine as an illustration of the concepts.

However, as this gets refined you are going to want to focus on the tails of the distributions that lead to these relatively improbably outcomes involving large releases; and that does not show on your illustration.

DR. JUDD: That is good. Let me ask Scott.

How far out do we get before we are releasing everything?

MR. SINNOCK: That is approximately representing the release of the entire rapid-release fraction.

DR. JUDD: This roughly, here.

MR. SINNOCK: So the steep plunge is a physical maximum based on the input distributions of the total rapid-release fraction available for release.

DR. JUDD: As the uncertainty increases on that rapid-release fraction it is going to pull this out to the right.

DR. NORTH: So you are saying there are some basic physics which limit the release quantity such that it is inconceivable that you could get it above one.

MR. SINNOCK: Given out assessments on what the total Carbon-14 inventory was, what percentage of our inventory was in the rapid-release fraction, which means that as soon as there is a hole poked in the container that is accessible for vibration from the outside.

DR. JUDD: Notice we only had the upper end of the probability distribution, 3-1/2 percent, available for rapid release. If that were up to 30, 40, 50 percent it will scale this point to which you are referring.

DR. NORTH: So that would sound like the distribution that one really needs to explore and document in great detail: that it is really very unlikely for all reasons we can assemble--expert judgment, model runs, et cetera--that that number is going to be significantly above 3.5.

DR. JUDD: Or so.

MR. ALLEN: Is this not only model-dependent in the sense that, for example, your model here does not include the probability of an earthquake rupture through the canister? However low that may be, that is simply something that has not been included here.

DR. JUDD: Thank you. Yes. In about four viewgraphs I will get to that; but I should have mentioned that this has today's conditions as best reflected in those judgments, and not some of the disruptive scenarios.

Thank you.

With those provisos this curve, then, represents, when it is done not for illustration but done for real, the degree of confidence. Let's get some blue curves first, and then we will get some green ones.

One of the first things, then, we are doing is analyzing the sensitivity of that purple curve to the uncertainty in some of the important parameters. That gets at a question Dr. North raised earlier.

Meantime, regarding container failure, we had long-lived containers on the order of 100 or 20,000 years. Those were the high points, the 90th percentile points on the probability distribution; short-lived containers on the order of 100 or 1,000 years being at the other end of that probability distribution.

What happens when you change that parameter from its low to its high value so this curve is plugging in those values instead of admitting there is uncertainty? Assume we knew that it is those two values that are appropriate for the model, that would pull this purple curve down to here, and we would push the purple curve up to the blue line if the lifetime of those containers were very short.

This shift in the purple curve, that shift in performance, is an indicator of how much you might learn from testing confidently if you could resolve the uncertainty about whether container failure looks like those numbers or looks like these numbers.

In other words, if we could do a test program, unambiguously, with no uncertainty, that these were the values that is what happens to that curve. Notice that it has a tendency to shift down more than it has a tendency to shift up primarily because in the expected case or the middle case the container failures were relatively quick. Therefore, a lot of the material is getting out.

It is only when you shift the parameters to very long-lived containers that you find it makes a significant shift downward.

Here is another one. This was the issue of Carbon-14 retardation. Notice it also causes some shift in the purple curve, but now another interesting thing happens at the top.

Carbon-14 was quantified as a multiplier on the gaseous flow time through the unsaturated zone. In the assessment you saw in the earlier table it went from 1 to a factor of 50 to a factor of 500. It is this row right here.

If we knew it were 500 it not only pulls the curve to the left, but pulls it down. Why is that?

The probability of there being zero release is 1 minus the probability that you read on this axis. This says there is about a 25 percent chance of having a positive release and a 75 percent chance of no release.

This is the case where the multiplier here is 500, the gas-flow time in the middle case was 50; 500 times 50 is 25,000 years; the cumulative curies we are computing are only over 10,000 years; and so there is significant chance, when the multiplier is 500, that there would be zero release within the period. That is why this curve is shifting up and down here.

We chose these two curves because the sensitivities work on different dimensions of the graph. On this one the biggest change is up in here: the probability of no release. The other one was simply scaling the curve left and right.

Notice again we don't get a violation of the Environmental Protection Agency standard here, but there are two things that have been left out. One is the faulting and other disruptive scenarios; and the other is that these sensitivities here assume you could know for sure what the multiplier is on Carbon-14 and, in fact, we are uncertain about that.

Our testing will produce results that might shift the curve back and forth--and this is important--and it will shift it by less than the brown curves here because the tests are imperfect. If we get a test result that says the multiplier looks high that test result might be wrong.

So you don't shift this thing all the way down to the degree it is illustrated here. Testing will cause a shift in that curve, but it will be less than what you see here.

DR. NORTH: Bruce, it occurs to me that if you looked at rapid-release fraction, which we were just talking about, it could very well come out the other way.

For example, if I hypothesize a scenario with a 1 percent chance of a rapid-release fraction approaching 100 percent and then you had the opportunity to test, perhaps within the engineered barriers, whether in fact that could happen, that could lead to a high value of information.

DR. JUDD: Yes, because the possibility of a high rapid-release fraction on the order of 100 percent instead of 3.5 means that the right hand curve for that sensitivity could be way over here.

DR. NORTH: What you would then do would be to spread that lower tail out instead of dropping it. If I propose 1 percent you can see where that would come on that cumulative.

DR. JUDD: Yes. Good point.

So testing needs to be factored in. I will go, fairly quickly, through how we are quantifying that. I showed this viewgraph earlier.

When there is a major uncertainty the next thing we do is identify the categories of Surface-Based tests and assess their accuracy. The accuracy is assessed the same way in all three Task Force discussions. So what I am going to say now will help speed the presentations a little bit later because we are all using the same order of the assessment.

I will use an example from the unsaturated zone so it is shifting from gas over to waterborne release here. Assume, for a minute, it is fracture flow in the unsaturated zone. What is the likelihood that the testing program and the interpretation of the results will follow the testing will correctly conclude fracture flow or the likelihood that it will make a mistake and inaccurately conclude that it is matrix flow when, indeed, it is fracture flow?

There is, in the illustration, a 25 percent chance of making an error in the assessment of what the true flow conditions are. Here is a case where it is really matrix flow and the conclusion is fracture flow: again an error.

So these probabilities of making an error are incorporated in the analysis. If this is a continuous variable, there is a related way to do it which is described on the viewgraph, but I will not take time to go through that right now.

What this does in a Carbon-14 example is: If we had the brown curves before, when we take into account the tests and their inaccuracies the brown curves shift to the inside to become green curves, for those related to testing, and notice there is a more narrow band between those.

So the sensitivity of a performance calculation to testing results will always be no greater than the sensitivity to the basic underlying uncertainty; and this case shows it coming in quite a bit.

I did not calculate these numbers. I just drew them with a pen.

That is how testing is taken into account. The last issue I want to discuss is the disruptive cases, which are not in the analysis.

Here is a set of the factors that influence gas release from the waste package. We have discussed each of those issues, but these disruptive cases down here--either climate change or faulting--have not been discussed. There are other conditions that need to be considered: water table rise, vulcanism, et cetera; but let me just pick the example of these two.

These two possibilities of future events can affect the factors that are in the analysis. So we need to go through a process of asking how that effect occurs.

Here is a list of questions. I have covered up some of the questions to make the list a little easier to deal with. I will expose those in just a minute.

What is the disruptive case climate change? What is its likelihood? An illustrative value here, that we assume for the sensitivity analysis, is a 10 percent chance of a pluvial condition. So that defines a degree of climate change.

How do you measure the magnitude of the climate change? There are many ways: precipitation, et cetera. Let's pick one of those and measure it using net infiltration. Then what are the next questions we need to ask?

If we did have the climate change--in other words, if this occurred and we are in this pluvial condition--then what is the uncertainty in the magnitude or the assessed probability distribution on net infiltration given that a climate change occurs. This is the next step in the analysis:

the probability that expresses, hopefully, the true degree of uncertainty in that infiltration.

What parameters does that affect? Let's say that one of the parameters is flux. How big is the effect? I wish I had a blow-up of this so you could see it a bit better, but assume we have a probability distribution on flux, but if you assumed high infiltration it increases the flux dramatically over to this curve here.

I am about ready to do a sensitivity analysis that says if infiltration is at its highest level, or at a high level here, and we incorporate that high level into our assessment of flux, which shift the flux distribution over, what happens to performance?

Here is an illustrative calculation of, basically, rerunning the model and finding a shift in that curve for that pluvial condition, for that increase in flux; and the average increase in flux was, I think, a factor of 15 that we did as a sensitivity analysis.

This is intended to illustrate the process of doing the sensitivity analysis to incorporating those conditions. It is our intent to incorporate those conditions in our base case along with the probabilities of those conditions occurring.

However, as you pointed out they are not in the current base case in the purple curve so we illustrate it with a sensitivity, and this shows it is shifting back to the right.

It says here these are illustrative not computed. We actually did some computations since we drew the viewgraph and the shift was something like that.

That is the last factor I wanted to mention that needs to be taken into account.

Let's summarize then.

These influence diagrams that the probability distributions are key to producing a quantitative model. A quantitative model is key to assessing the priorities of tests.

The process works by looking at performance computed using the model and the assessed data, comparing that to a decision line; and those tests that can cause us to jump over that decision line--in other words, those tests that can be detectors of unsuitable site conditions--would receive high priority.

Other factors, such as the cost and the schedule, will be factored in; but only after we have identified which ones affect the decisions, first. The process of building this is difficult and time-consuming. As Dr. North points out it is more than just a one-time exercise.

This type of analysis can be used iteratively, sequentially for management decision-making. As long as we are careful to document the process I think it will serve that need as well as the needs of the Surface-Based Prioritization Task Force.

That concludes my discussion of the methodology.

CHAIRMAN DEERE: Are there questions from the panel of Bruce?

DR. DOMENICO: I see one problem in applying this in the real world: well, I see a lot, but probably one in particular.

[Laughter]

DR. DOMENICO: It probably has to do with the Environmental Protection Agency standard.

Correct me if I am wrong, but you have a number for an Environmental Protection Agency standard for a single nuclide if it is released. If you have more than one nuclide in the environment, you have a ratio where the sum has to be equal to or less than one.

Therefore, in order to say something about Carbon-14, for example, you have to say something about what happens to the rest of the inventory, which would shift, I believe, the Environmental Protection Agency standard to the left: that is, you can release less than what that number is if you have other ones out into the accessible environment.

In the sense you are using that as the criteria, I see a problem in the application of this to your problem.

DR. JUDD: Yes.

DR. DOMENICO: I may be wrong on that standard, but I don't think so. Maybe you can comment on that, Steve.

DR. MATTSON: To have Scott comment on it might be better.

DR. JUDD: Scott Sinnock, Sandia National Laboratory.

MR. SINNOCK: I am not sure I understand.

Yes, in the standard you sum all the nuclides ratioed to the given limit. Therefore, an actual comparison of the standard has all the nuclides in it.

DR. DOMENICO: I think what you have used in here is the number they give for, let's say, Carbon-14.

MR. SINNOCK: Yes. Exactly.

DR. DOMENICO: I don't know how many curies.

But when you have other ones in the accessible environment it must release less than what that number is. So in order to say something about one nuclide you have to say something about all of them.

MR. SINNOCK: If that were true for Carbon-14, the other nuclides would add to and push that curve to the right.

DR. DOMENICO: And it may indicate closer failure, in other words.

MR. SINNOCK: Yes.

DR. DOMENICO: That is the problem I see in its application.

MR. SINNOCK: Unless it turns out we have not done the others: that Carbon-14 was dominant, in which case we are only talking about a percent or two for the other nuclides and then you could not see it on the width of the pen.

But exactly: the other nuclides would shift that curve to the right.

MR. CARTER: But I think, fortuitously, you picked Carbon-14. You may be home free with that.

DR. JUDD: Our intent is too model the other radionuclides as well. We chose gas because of that simplification.

MR. PRICE: What you have shown has a shoulder at the probability of one-tenth, or a moment, around about there; and the Environmental Protection Agency standard has a step that goes down there and makes that look like that is the critical part of the curve for evaluation.

Any comment on the realistic aspect of the Environmental Protection Agency standard? Do you know what I mean?

DR. JUDD: Until you said the last part of that I was with you.

MR. PRICE: Page 49, any one of those curves will show the Environmental Protection Agency standard.

DR. JUDD: When you said "realistic" you lost me.

MR. PRICE: At one-tenth you get closest to the Environmental Protection Agency standard, at that probability, which creates one area of greatest concern about the way things are acting.

That is really an artifact of the Environmental Protection Agency standard, is it not?

DR. JUDD: If the Environmental Protection Agency standard were more than just two points, it would be more of a curve.

MR. PRICE: If you smooth out the Environmental Protection Agency standard you would have a lot more room.

[Laughter]

DR. NORTH: Given that the Board has raised some questions earlier, as has the National Academy, about the Environmental Protection Agency standard it seems to me we might want to restrain ourselves from asking too many questions at this time of this group of people.

MR. PARRY: Your attempt here is to prioritize Surface-Based Testing or in situ testing. What you have shown, it appears to me, for the Carbon-14 is that the critical characteristic is the rapid-release fraction.

Would that not, then, suggest testing programs outside of the site itself, but looking at what the effect is going to be of the actual release fraction?

DR. JUDD: Yes. The analysis you have seen here will identify those issues or those factors, such as rapid release. The question of how they might best be analyzed is, as you say, a separate or second question that needs to be asked.

When we assess from our panels of experts what the tests are that are going on they may be tending to focus on the ones inside the repository block: Surface-Based Testing; and you are right, there may be others.

MR. PARRY: Also your calculations, which are very interesting and preliminary I understand, also suggest there is not too much advantage to a long-lived canister.

It is unfortunate Dr. Varink [ph] is not here.

DR. JUDD: Yes. I think we have to be very careful drawing any conclusions off of one illustrative set of calculations that focused only on Carbon-14.

MR. PARRY: Right. I fully agree; but it is still an interesting phenomenon.

CHAIRMAN DEERE: Are there any questions from the audience?

MR. ROBERT WILLIAMS: First let me compliment the group. I think you are making very good progress in addressing what the utility industry was concerned about. This methodology, I think, is very responsive in terms of what we have been arguing for and proclaiming for.

I think it is very important, though, to underline that even a probablistic methodology is doing what the Academy report protested against. In the present level of illustration, it is giving you, I think, a too optimistic feeling of the degree of certainty in where you stand.

So I just want to endorse Warner's and Jack Parry's views that we view all of these results with caution. I think may of them are counter-intuitive.

DR. JUDD: Okay.

As mentioned, the next discussion has to do with possible methods to assess site suitability. Dr. Steven Mattson, the Chairman of our Task Force, will give that discussion.

DR. MATTSON: We are ahead of schedule so we propose having one more presentation of about 15 minutes.

CHAIRMAN DEERE: That sounds good.

DR. MATTSON: But there may be some discussion that needs to go with that.

Possible Methods to Assess Site Suitability

DR. MATTSON: As a third component the first part of the Task Force was to look at ways of prioritizing tests, especially early during Site Characterization, that could look at the potentially adverse conditions or other concerns that people may have about site suitability; and that would be the iterative process.

In this third step we have also been tasked with recommending possible methods in which we could directly assess site suitability.

A site suitability assessment helps the Department of Energy make decisions about the site. Here I have shown two decision trees. The first place where it helps us make decisions is whether to continue testing or to stop testing.

The continue-testing has options. It could be as-planned in the Site Characterization Plan or it could be some revision of the plan presently in the Site Characterization Plan, or it could even include an altered strategy towards reading the licensing regulations; and the other options, obviously, is to stop testing.

The site suitability decision helps us make decisions about whether to recommend the site, to abandon the site or to use an altered strategy; and that altered strategy could include either licensing changes, design changes or other things in the overall strategy that has been put forth in the Site Characterization Plan.

In these types of decisions you will note there is similarity between what we have presented before in the Surface-Based Prioritization and this present analysis.

Our evaluation of possible site suitability methods comprises three primary tasks. The first task is to identify possible suitability assessment methods to be evaluated. That includes what criteria should be applied.

That requires some management input from the Department of Energy. Timing options: When do you do this type of analysis and how often? And what kind of techniques are to be applied?

The second part of this task includes the evaluation of the feasibility and defensibility of the methods that might be recommended. Third is to recommend promising methods to the Department of Energy management.

For the present time we have begun the elements under the first bullet. I would like, next, to go through each of the bullets under step one.

In terms of performance measures that could be applied, under this methodology we have talked about the Surface-Based Prioritization Task Force and we have chosen for this first go-around to use a post-closure performance measure and total system releases.

As all of you are probably well aware, there are several other criteria under the post-closure framework that also have regulatory guidelines, such as the 1,000-year ground water travel time. Those could be incorporated into this methodology, as well.

There are also, which we are not considering at the present time in the Surface-Based Prioritization Task Force, other pre-closure radiological safety issues; there are feasibility types of analyses, such as siting and construction or operation in closure; we have not included costs and schedule; and there are other elements that could be incorporated into here, as well as there are other pre-closure impacts which include concerns about the environment, socioeconomics and transportation.

These types of criteria, those that get thrust into this methodology, will need to have a strong dependence on management as well as other people such as the Nuclear Regulatory Commission.

DR. NORTH: To expand on this point a little bit, were most of these criteria not involved in the 1986 analysis using all the attribute utility that was carried out in connection with selection of three sites out of five?

DR. MATTSON: There are many elements of that in this, yes; and certainly one of the options is to use that as a blueprint, if you will, for other types.

Here we are trying to incorporate the widest frame of what could be incorporated if we need to look at suitability of a site.

Does that answer your question?

DR. NORTH: Yes. Thank you.

DR. MATTSON: In terms of timing, suitability assessments could be carried out at various times during Site Characterization. It may be appropriate at this point in time or soon to carry out that analysis, or it may be appropriate after a major testing program has ended or it may be appropriate again to do that very near the completion of site suitability.

So there are different options which could be applied. It could be done at regular intervals or it could be done continuously, or it could be associated with major testing programs.

DR. NORTH: Lest there be any uncertainty with respect to my previous comment, I have clearly voted for the "continuously" option at the bottom.

DR. MATTSON: Each suitability assessment would involve two types of decisions. As I said before, there are options on whether to continue testing or recommend the site, or alter strategy or abandon the site.

In this type of analysis if we start with the present time, or some time in the future, we have our prior base of information. Any recent information we have collected, which at the present time we are uncertain about and that is going to help us base our decisions in a way similar to the way the Surface-Based Prioritization methodology was applied to make decisions about whether it is appropriate to stop testing or continue testing--

If we continue testing, we will get new test results and there is some uncertainty associated with that; and this will allow us again to make the decision to continue testing or stop testing much as we had the decision down here.

Based on that decision, in turn, either we will continue testing or we will make decisions about recommending the site, abandoning the site or altering the strategy. Obviously this decision would be up here, as well. So that tree would sort of continue one in that continuous manner.

These are methods that could be used to look at suitability; or, as Bruce put up before, this could be the accumulative probability curve. One measure is the Environmental Protection Agency standard.

There could be another performance measure up here, as well, in which we have decisions where management feels comfortable seeing probabilities up above that line should be areas in which we should think about abandoning the site or making that recommendation; areas that fall below a certain probability are areas in which we would end up recommending the site; and there may be a grey zone in between in which we might decide to continue testing or alter our strategy in one form or the other.

Another alternative method that could be utilized is a multi-attribute utility analysis about analyzing decisions about the repository. This is similar to a viewgraph I put up before, except that rather than the value of testing it is the value of utility measure, if you will, that would be assessed by not only technical people, but also management.

Outcomes of decisions are quantified using selected performance measures. Those decisions are then analyzed and evaluated using a utility function which incorporates both management and other judgments which are important.

We will evaluate suitability assessment methods and recommend those that are practical and defensible. Benefits from these recommendations or methods will assist the Department of Energy about the site on aspects of when to continue testing and when to stop testing, and decisions about the site itself: about whether to recommend the site, abandon the site or to turn to an alternate strategy.

This methodology we are presently looking at is to be developed and to be consistent with the Surface-Based Prioritization approach that Bruce discussed with you previously; and we hope to build on existing models and information we already have within the program as well.

Are there any questions?

[No response.]

CHAIRMAN DEERE: Any questions from the audience?

[No response.]

MR. BROCUM: Since we are ahead of schedule, we may as well finish on the Surface-Based Prioritization Testing before lunch. Russ Dyer will present the summation and the Department of Energy perspective.

Summation and Department of Energy Perspective

MR. DYER: Let me follow up on the talks of both Bruce and Steve, here, and stand back a little bit and give you a perspective, and summarize what we have been through here in some pretty fine detail.

This effort has three components to it, three goals. The first is to examine the current Surface-Based Program as outlined in the Site Characterization Plan--by Surface-Based Program we also include laboratory tests--and prioritize those in some rank order to get a list of things on which we want to concentrate, focus our assets in the initial phases of the Site Characterization Plan.

Part and parcel of this is to develop a tool, a method, which can be used iteratively to re-examine the testing program at any point in time; and that is the second bullet on here to which we alluded earlier: Given a future state of knowledge based on testing that is accomplished some time in the future we can re-prioritize, re-examine the testing program at some point in time.

This method, this technique, also gives us the ability to determine when to stop testing.

The third component of this Task Force is developing a draft method for assessing site suitability. The prioritization effort focused on a rather limited performance measure: the cumulative releases. Site suitability is a more complex issue. We are still grappling with all of the things that need to go into a suitability evaluation.

To summarize from Bruce's talk, the test prioritization approach quantifies the current level of uncertainty and how well it can be resolved through testing. This quantification right now is based on the judgment of the Department of Energy experts: the people involved in the testing program to date.

The site suitability approach, which Steve introduced you to, can address broader criteria and quantitative performance measures such as cumulative curies released, the 191 standard; the ground water travel time from 10 CFR 60.112; pre-closure rod safety; or other criteria we may wish to build into the evaluation.

These approaches can produce significant insights as far as justifying tests or defining the sensitivity of decisions to technical and value judgments.

All together the two approaches--the prioritization and the site suitability approach--provide defensible methods for determining the value of tests: as Bruce pointed out our methodology for the prioritization effort is based on a value of information philosophy; deciding whether or not to continue testing: again based on a value of information philosophy; and finally deciding whether or not to recommend the site: this would be in the site suitability decision.

This concludes our presentation about the Surface-Based Prioritization effort. Are there any questions from the panel?

MR. McFARLAND: A point of curiosity, Russ.

You have 107 test plans that will eventually be developed. How many of those tests are surface-based?

MR. DYER: That is a difficult problem to answer.

There are 106 study plans. If we look at it from the parameter level, categories of information to be gained, as I recall there are approximately 2,000 parameters. My guess is that probably about 1,200 to 1,500 of those parameters are gathered in the Surface-Based Program; some of them are also gathered in the sub-surface program.

That is my estimate.

MR. BROCUM: We have categorized study plans in other ways. Some of them are primarily Surface-Based, some of them are primarily analysis, some of them are Exploratory Shaft Facility based. I think we had 12 or 15 Exploratory Shaft Facility, 50 primarily Surface-Based, and the rest were analysis and modeling and stuff.

That kind of gives you an idea of the scope. You can debate because some of these parameters have gotten two ways or more.

MR. McFARLAND: Thank you.

MR. DYER: Any other questions?

MR. CORDING: Russ, included in the Surface-Based Testing are there angle holes included, presently?

MR. DYER: I am going to have to defer to Max.

I am not aware of any.

DR. BLANCHARD: The current program described in Chapter 8 of the Site Characterization Plan does not have angle bore holes identified, although it has an intent to do so wherever it is clear we have the technology.

As you know, we have been developing an air-drilling, air-coring system with an LM-300. Although we have done off-site tests successfully in Utah and Arizona, we have not yet done the tests on the test site with the rock types that we really have there at Yucca Mountain.

To a large extent, how well we can do angle drilling depends upon some success first at Yucca Mountain. We are considering some design approaches that Lang has looked at for second LM-300 model which would allow us to do angle drilling.

We are not yet able to say we have the technology to do dry angle drilling and dry coring on the Mountain.

MR. CORDING: Max, do you feel that with further testing, whether on or off the Mountain, and development of that it is just a matter of getting it done, not so much a matter of whether or not it is possible to do it?

Do you think, in other words, it can be done with further development?

DR. BLANCHARD: I am not in a position to hypothesize on that.

MR. CORDING: I think it is clear that your other program has come quite a ways. You have done quite a bit already with the vertical holes and there has been a lot of progress in what has been done off-site.

It seems that at least there are some concepts that the can begin to investigate the possibility of the angle holes and come up with a program that allows them to at least investigate that and go in with a prototype of some sort somewhere.

DR. BLANCHARD: You are quite right: we would like very much to be able to do that; but at the same time we feel that drifting in the Topopaw Spring and the Calico Hills to the features of interest, especially those that might represent anomalous values that might fit into models so we could improve our understanding, will give us much more useful information than angle bore holes.

So we are counting very much on in situ test programs in both those rock units; and think that that will give us thousands of data points rather than few, and those thousands of data points will allow us to have a better understanding of the values of things like hydraulic conductivity and, at the same time, develop a more meaningful measure of central tendency so we know what we want to use in a calculation with respect to a mean value or a standard deviation.

MR. CORDING: I would certainly agree with that emphasis on the underground drifting.

DR. DOMENICO: Max, probably you can address this better than anybody.

I know models like this are hungry for quantitative numbers: travel times and things; but is the Surface-Based Prioritization Testing Program going to include the indirect methods of determining ground water velocity and travel times?

I am very keenly interested in the continuation of tritium and chlorine studies which I think are the best indicators of travel time. They are indirect and they may not lead to some quantitative description.

I would hope that sort of information would not be filtered through the cracks because of the data-hungriness of this approach.

DR. BLANCHARD: You are quite right.

As you know, in what we have laid out in the Site Characterization Plan on topics like this we have a number of different redundant ways to approach realizing what ground water travel time is likely to be.

Using isotope measurements to determine the age is just as important as more mechanical or statistical methods. We intend to use all of the tools we have at hand.

This prioritization, as Bruce mentioned earlier, will try to look at these different sub-techniques under one topic so we can insure that we give the proper prioritization to this suite of tests that are applicable to that subject and not just to one.

MR. BROCUM: As a real-world example, we are modifying the Chlorine-36 study plan to make sure we can do Chlorine-36 evaluation from the bore holes in addition to the Exploratory Shaft Facility for the shaft of the ram.

That is being modified today even though we have already submitted it to the Nuclear Regulatory Commission. One of their first and formal comments was: Are you going to be able to do these from bore holes?

We decided to modify that study plan. That is being modified right now.

CHAIRMAN DEERE: I think the importance of the angled bore holes increases as there is a delay in the shafting. This was what got us interested a few months ago in looking at it again.

If the exploratory shaft stays on schedule, you will get better information perhaps.

DR. BLANCHARD: One of the cases in the Calico Hills study is using angled bore holes, which I think you will hear about this afternoon.

MR. BROCUM: That is right.

DR. BLANCHARD: One of the eight different ways of studying includes angled bore holes.

CHAIRMAN DEERE: Thank you.

MR. DYER: Leon?

MR. REITER: Russ, this is sort of a follow up on some of the previous questions: the question Bob Williams raised about the modeling.

If we look at the study there is a whole range of kinds of conclusions you come out with about prioritization of tests and about site suitability. It seems to me that people indicated that really one of the key assumptions in the modeling uncertainty depends on your group of experts.

Do you have any feeling that if other people convened groups of experts, which kinds of conclusions would be the most robust? By "robust" I mean where would the various groups tend to agree that, yes, this is a good conclusion?

MR. DYER: There seems to be a common ground. I am the Department of Energy representative on the EPRI efforts. There are several efforts going on right now.

MR. REITER: Yes. The Nuclear Regulatory Commission has completed a Phase One performance assessment.

MR. DYER: That is right.

There seems to be some common ground which most groups recognize, as Dr. Domenico did: things on which emphasis needs to be put. Then there are some outliers that are identified. Different outliers are emphasized by different groups.

We are still trying to figure out how to bring in those outliers: how to incorporate them in the program. We think that the way we have it now, as Bruce pointed out, the methodology allows us to process through virtually all the alternate models we are examining.

So we could take a model suggested by any group and see what the implications might be of that model. If you follow it to the end result, in the simplest measure if there are no implications on performance it may not get as much attention or deserve as much attention as some other issues that do have a strong impact on performance.

MR. REITER: Could you give us a 30-second summary of where there is agreement where there is disagreement? What are the outliers, what are not?

MR. DYER: Agreement? What seems to be the case is that the series of questions had to do with hydrology of the site: whether we have fracture flow; what the flux is.

MR. REITER: Is there any disagreement?

MR. DYER: I have not heard any disagreement with that.

MR. REITER: You said there were differences in different outliers.

MR. DYER: They mostly fall into the category of what Bruce referred to as disturbed cases, disturbed scenarios; and it would what would be the consequence or the importance of a low probability, high consequence event.

Where we seem to have some range of disagreement is assigning probabilities for these high consequence, low probability events.

DR. JUDD: One thing I might add is that the methodology will be there for the Department of Energy to insert other judgments, other opinions, other models--

MR. DYER: That is right.

DR. JUDD: --and look at the implication.

MR. DYER: I hope I addressed your question, Leon.

CHAIRMAN DEERE: Are there additional questions?

[No response.]

CHAIRMAN DEERE: What time would you like to reconvene after lunch?

DR. BLANCHARD: At your convenience. Right now the schedule shows the Calico Hills would start at 2:15. Because it is a very sensitive subject there may be some extra discussion during the presentation.

So you may want to reconvene prior to that time.

CHAIRMAN DEERE: Or would it be better to wait until 2:15?

DR. BLANCHARD: I think it would better to start a little early, which would allow us to have some extra discussion time should it be needed.

CHAIRMAN DEERE: Okay. That is very good. Is 1:45 okay? Then we will start with Dave Dobson's presentation.

DR. BLANCHARD: That gives us plenty of time.

CHAIRMAN DEERE: Fine. 1:45.

[At 12:15 p.m., the meeting recessed to reconvene at 1:45 p.m.]

AFTERNOON SESSION

CHAIRMAN DEERE: The proceedings on Calico Hills Risk Benefit Analysis.

DR. BLANCHARD: David Dobson from the Department of Energy's Yucca Mountain Project will be the first speaker.

CALICO HILLS RISK MANAGEMENT STUDY

MR. DOBSON: Thank you. As Steve mentioned, my name is David Dobson. I am with the Department of Energy in Las Vegas. I am going to give you kind of a two-fold presentation to begin the discussion of the Calico Hills Risk Benefit Analysis.

In this analysis, I played parts of two roles. I was a member of the Task Force as a geologist; and, of course, I work for the Department of Energy. So in the introduction I will give you the initial constitution of the Task Force and what our goals and objectives were, and then I will do the first part of the presentation which is a discussion of the development of the alternative strategies.

This is the structure for the first 10 or 15 minutes of my talk: as I said, the introduction. We are going to start with a one-slide summary of our geologic orientation and then discuss briefly the rationale for the study; the objectives and methods that we used in the study; the composition of the Task Force: who were the people who did this--and I will introduce some of them who are here in a few minutes; finally, I want to give you, sort of up front, the results of the study; and then we will conclude with a quick picture of the structure of the presentation: what you are going to hear for the next few hours.

Most of you are familiar with cross sections that look something like this cross section of Yucca Mountain showing the principal hydro-stratigraphic units, actually showing primarily the lithologic units with the focus on the Calico Hills hydrogeologic unit, which was the subject of our analyses.

The Calico Hills non-welded hydrogeologic unit consists of unwelded tufts of the Calico Hills member of the paint-brush tuft. Of course, it lies below the repository horizon, below the welded tuft of the Topopaw Spring and the non-welded, and through the canyon members. It overlies the crater flat tuft and the top most of the crater flat tuft is the pass member.

As most of you are familiar, Calico Hills is identified in the Site Characterization Plan and we believe it to be the principal barrier to potential migration of radionuclides. I want to make a couple of extra points about the Calico Hills unit.

The most important of these points is that there are really two very different rock types contained within the Calico Hills. There is a zeolitic face which is exposed primarily in the northern and eastern portions of the proposed repository block, and there is a vitric face which is exposed best in the south and west.

This is a schematic picture of that contact, which is somewhat erratic and the zeolitization is concentrated in discrete beds.

Again, we will focus the remainder of our discussion on the performance of the Calico Hills non-welded tuft. We are talking about that portion of the Calico Hills that is above the water table. We are talking about the unsaturated zone performance of the Calico Hills.

Why do we do this?

When the Department of Energy released the original consultation draft of the Site Characterization Plan it contained an activity which did not have a whole lot of detail in it, but it contained an activity which proposed in situ drifting in the Calico Hills unit below the main test level.

During their comments one of the objections identified by the Nuclear Regulatory Commission is summarized here. They stated that, "The need had not been established to extend the shaft into a drift horizontally from ES1 in the Calico Hills unit." They also stated that, "Potential adverse impacts on waste isolation as a result of penetrating the Calico Hills had not been demonstrated."

Because of these concerns the Nuclear Regulatory

Commission recommended a three-fold strategy: One, consider characterizing the Calico Hills without penetrating the barrier between the repository horizon and the water table.

They also suggested a detailed discussion was needed by the Department of Energy to show why the benefits would outweigh the potential adverse impacts of penetrating the Calico Hills rather than obtaining information by some alternate means.

Finally they stated that if alternate means could not be developed then, ". . . justify destructive testing in the Calico Hills and include the consequences of connecting pathways for potential radionuclide migration from the waste emplacement areas to the water table."

In response to the Nuclear Regulatory Commission objection the Department of Energy made some changes to the final Site Characterization Plan. What we did to respond to the objection primarily, in late 1988, was defer the description of how to characterize the Calico Hills pending the completion of a Risk Benefit Analysis which considered those things the Nuclear Regulatory Commission recommended we consider, including specifically the needed data, alternate means of obtaining the data, the benefits of obtaining the data, and finally the risks to site performance by obtaining the data.

We committed at that time to consult with the

Nuclear Regulatory Commission before we took any action.

I did want to make one additional comment. The Nuclear Regulatory Commission did not say, "Don't do in situ testing"; and, in the Department of Energy's response, we did not say, "We will not."

They said, "Before you do in situ testing, you have to demonstrate you are not going to compromise the integrity of the site." For the reasons I have summarized we then set up the Calico Hills Risk Benefit Analysis. Today we are reporting to you the preliminary recommendations.

A quick summary of the objectives and methods.

The study, as Max mentioned earlier this morning, is being conducted in accordance with all of the requirements of the Yucca Mountain Project Quality Assurance Program. That has certain implications.

It means things like we ensure everybody is qualified before we start, which is only good practice. It means we have a plan and that we follow that plan. And it means we have products we specify we are going to generate, like interim products and a final report.

In addition to that, the Department of Energy decided we would conduct the study using the principles of decision analysis; and I will get into the reasons for that in just a second. Finally, the Task Force was instructed to base the evaluation primarily on the two criteria that were identified in the Nuclear Regulatory Commission objection. Those I have summarized here as the benefit from testing versus the risk to performance.

You will hear a lot of the discussion today--in fact, you have already heard a lot this morning--about how you measure the value of testing. Bruce, this morning, talked about something he called technical value and whether or not that is directly related to things like performance. You will hear, certainly, more discussion about that.

Risk to performance is relatively easy to quantify. You can estimate, at least, what kinds of releases you think you are going to generate, but doing a comparison with the benefits is somewhat more difficult.

That leads into what I said I was going to summarize, which is why it was we selected a value of information technique of decision analysis for this analysis.

There are several reasons which are probably all basically common sense, but I wanted to write them down to I remember them. One is that we wanted to structure the process so there would be a clear definition of what the decision criteria were. When we got done with this we wanted to understand what was driving the decision and what was not.

Secondly, the decision, as I mentioned, required some consideration of available quantitative data and model results, but it really had to be considered in light, basically, of expert judgment. We recognized we would not be able to set up a quantitative model, run it through, and the answer would fall out. That was not a realistic goal, and probably will not be for a long time to come.

Finally, the objective was to compare benefits of testing to the potential for adverse impacts on site performance. Again, you will hear a lot more today about what that means in terms of the value of the testing and our ability to reduce uncertainties about how the site is going to perform.

This is a schematic of the structure of the analysis we did. I want to say a couple of words about it.

In order to meet the objectives the Nuclear Regulatory Commission had given us and that we had identified in setting up the Task Force, the first two features we had to do were define the information needs and identify testing techniques which were applicable.

From those we had to compose a set of possible strategies to be evaluated. Then we developed a screening process to do that. You will hear more about that in a few minutes.

In parallel with the development of the list of possible alternate strategies, we were working on developing decision-aiding methodology--Hollis Call will talk about that in great detail--at more or less the same time, starting later in the process.

In order for us to quantify our assessments, if you will, quantify our judgments of how we were going to evaluate these different strategies we had to have models for how we thought the site was going to perform and how we thought the tests were going to do at telling us how the site was going to perform.

So we had to develop what I have called here conceptual models of site behavior. This required the work of a good sub-set of the panel members. You will hear more detail about what assumptions we made, when we set up the models, to estimate how we thought the site was going to perform and what the waste isolation impacts would be.

As you will hear, after we set up the conceptual models we essentially assessed our expert panel for their opinion on how they thought the site was going to perform in terms of the total system, in terms of the Calico Hills itself, and in terms of two other components of the total system which included something I will call source term, in quotes.

It is not source term in the sense of waste package EVS or waste term that you have heard before, but available inventory to the Calico Hills unit.

Then finally we assessed how we thought the saturated zone performance would contribute to the overall performance.

We then performed an evaluation essentially using the decision-aiding methodology we developed and using all the technical inputs we developed, and produced some results. We reviewed those results and finally developed a recommendation.

We are just near minutes from the recommendation now.

[Laughter]

MR. DOBSON: Before I tell you that I want to tell you who did it.

The Calico Hills Task Force was not a large Task Force. We intentionally set this up to be a small group of people. We tried to get sufficient technical breadth of knowledge to cover all the major program areas in terms of hydrology, geology, geochemistry, performance assessment and significant input from engineering.

However, as I said, the Task Force was not designed to include everybody in the program or all possible fields of expertise; but we did empower the Task Force to go get expertise where they felt they needed it.

We used this ability in a couple of specific examples I can think of. We used some of the Los Alamos people to help us out with some of our estimates of retardation, and we used some of the Sandia people to help us out with some of our models for waste isolation impacts.

This is a list of the people who did the work you are seeing here. Several of them are here today and I would like to introduce them in case anybody has any questions they would like to ask them at the break of later on.

Elizabeth Browne and Hollis Call from Applied Decision Analysis were our decision analysts. Hollis is sitting at the front table; Elizabeth is in the green dress in the first row at back. As I said, they helped us set up the decision-aiding methodology and helped us recognize a lot of what our technical assessments meant.

Bruce Crowe is not here today, but he provided part of the geological input and acted as a translator for the geochemistry program for us. Ernie Hardin, who is also sitting at the front table, was the task leader. Ernie is a rock mechanics geophysics modeling type who had a lot of work to do to get this to the point where it is today.

Barnie Lewis from the United States Geological Survey is the section of the Unsaturated Zone Section of the Nuclear Hydrology Branch. He is not only speaking, I think, presently on but will be the principal investigator for Calico Hills until we can get someone on staff who will be writing up study plans and taking some of the next steps, and then Barnie will be that person's supervisor.

Jack Robertson in the second row back there is a hydrogeologist. The question was asked earlier about whether or not we had anyone independent involved in any of these Task Forces. Jack is independent of our program and he was brought in because we were aware of his knowledge. He was, until the early 1980s, Chief of the Hazardous Waste Program for the United States Geological Survey. He left that and went into consulting in contaminant transport with Weston and, now, has his own firm: Hydrogeologic. He provided a lot of the input you will see on hydrology.

I might mention that prior to Barnie Lewis' participation Bill Wilson was a member of this Task Force in the very early formulative periods.

We have two more: Scott Sinnock, who almost all of you know, is sitting way in the back. Scott did provide a significant amount of performance assessment input we utilized in this Task Force. We also have Charlie Voss, also back there somewhere, who has had a considerable amount of experience in reviewing this program over the past eight or ten years. His expertise is in rock mechanics and mining engineering, but he is familiar with most of the Site Characterization performance assessment aspects of our program.

I have two pages of viewgraphs that summarize the results of the Calico Hills Risk Benefit Analysis. The first one--and we will go into this, as I said, several times in considerably more length later on--is with respect to predicted performance.

The analysis we did suggests that the Yucca Mountain site is like to meet total system performance standards by a wide margin. I don't want to get too narrow about that, and I don't want to go into too much detail about what that means.

You will see we did not do a comprehensive total system performance assessment in this analysis. We did sort of a limited total system performance assessment so it is not really total, I suppose.

We did not consider some disruptive events, such as, for example, human intrusion. We did consider natural disruptive events, such as climate change. You will get more detail on this later, but I want to make it clear that that is not a licensing assessment I just gave you: it is a focused assessment of performance that we did to support this activity.

DR. NORTH: Can you expand on this first bullet?

I read ". . . is likely to meet by a wide margin . . ." to mean that you cannot rule out some scenarios where it would not be met by a wide margin and might even not be met at all.

What kinds of scenarios like that exist, and to what extent did you look at them?

MR. DOBSON: Warner, you are going to get a lot more detail on what we did in terms of the technical assessments. I think you will see that statement captures it pretty well: that is, we are several orders of magnitude below the standard.

However, that is not to say that certain disruptive scenarios have been considered that could conceivable result in some violations [sic]; but we will get back to that. I am sure you will have opportunity to ask many more questions.

A corollary or a second aspect of that is that because the expected performance is very good and partly because the tests vary in their ability to characterize the site, test results are not likely to change that view of performance assessment.

That has implications with regard to the variable that Bruce described this morning as technical value. That is not to say that testing has no value, but that in terms of the part of the model we set up specifically to reduce uncertainty with respect to the performance of the site the value is low.

MR. REITER: This is a performance assessment included all factors, not only Calico Hills?

MR. DOBSON: It is not a comprehensive, as I said, complete total system performance assessment. It does include --and you will get a description in some detail of this later on--assessments of releases from the engineered barrier system through the Topopaw Spring. It includes an assessment of that.

It includes an assessment of performance in the Calico Hills non-welded unit and it includes an assessment of performance in the saturated zone. That includes a rolled up summary of assessment of released to the accessible environment.

It does not include all possible scenarios. For example, you heard something this morning about gas-phase releases. The Calico Hills is not expect to be a barrier in terms of gas-phase releases. We are talking about aqueous releases through the Calico Hills.

So it is rather broad, but it is not everything; and we would be perfectly happy to spend as much time as you would like to discuss what it does and what it does not cover as we go through the presentation.

DR. NORTH: Again, my concern is if you just looked at likely scenarios I am not sure you looked in the right place.

MR. DOBSON: I don't think we just looked at likely scenarios. I don't think we looked at all scenarios. But we did consider explicitly things like climate change. When I say "explicitly" I don't mean that we modeled them, but we had them in the model.

We considered things like tectonic changes resulting in changes to ground water flow in the model. You will hear Hollis and Ernie describe in some detail what things we modeled using the decision process and what the components were of that model.

DR. NORTH: You heard me ask the question of Bruce Judd earlier about the tails of the distribution: the unlikely outcomes which might change this conclusion of meeting the total system performance standard by a wide margin.

If you just looked at the 90 percent to 10 percent part of the curve you might miss some very important phenomenon. I want you to assure me as we go through this that you looked systematically at the extreme outcomes and the potential for tests to be able to determine very unlikely or unexpected conditions that might imply failure to meet the total system performance standard.

MR. DOBSON: I hope that we can assure you of that throughout these presentations. I think that is the intent of our presentations.

We did try to capture those tails of the distributions. That was an explicit part and I know Hollis will talk about that in a little bit.

I think it was recognized from the very start that the point you just made is a very valid one: that you need to be aware of those low probability, high consequence events and the effect they could have on your understanding of the system.

DR. NORTH: I am concerned especially in the question of evaluating tests that you were, in fact, exhaustive.

The usual way this kind of methodology fails is the question Dr. Allen posed to Bruce Judd earlier: Did you include everything in the diagram? It is relatively easy.

There is some history in the decision analysis and psychology literature I could point to that very well qualified people sometimes forget to include an obvious category of failure mode.

So we all want to be assured you have done an exhaustive search.

MR. DOBSON: I could not agree more. In fact, we are looking forward to the meetings and the interactions we will have with the Nuclear Regulatory Commission in the near future to lay all this on the table and let people look at it.

That is part of the reason for our wanting to do these presentations and wanting to go back to the Nuclear Regulatory Commission.

I think we have been exhaustive, but we cannot promise there is nothing we have missed. That is the intent of continuing this.

Impacts. Another rather fundamental conclusion, I think, that we came up with is that all the assessments we did indicate that the likely impacts to performance in the ability of the site to isolate waste are very small for all the characterization strategies you will hear described.

We went through a set again and we considered disruptive events in that set as well in terms of in what conditions could the excavations cause releases. We basically concluded the impacts are going to be small for all these strategies.

Finally, under the category "The Benefits of

Additional Testing" there are a couple of points I would like to make.

One is that the analysis does indicate significant differences among the different strategies we considered in terms of their ability to correctly, if you will, predict hydrologic conditions.

You will see more discussion of both what the strategies are and our assessments of how well they are likely to do at determining existing conditions.

Finally, we believe the testing is likely to improve the understanding of site conditions, and increase confidence and performance predictions. This is a little bit like the last bullet that Bruce Judd showed this morning.

Outside of what he called technical value, there is value to testing in terms of demonstrating that you did not miss something fundamental when you did your initial assessments early on.

So the recommendation is the following: "The Calico Hills Risk Benefit Analysis recommends that the Department and the Exploratory Shaft Facility Alternative Task Force should plan for the characterization strategies number 2 and 5 [which you will hear about in just a minute], both of which involve extensive drifting in the Calico Hills within the repository block. These two strategies [which you will hear described, as I said] involve the potential to do on the order of 4 minimum of about 12,000 feet of drifting to access various structural zones and lithologic zones within the Calico Hills."

That summarizes the first part of the introduction.

MR. BARNARD: I think your conclusions and recommendations are quite significant. I noticed on one slide you said your analysis considered available data and model results combined with expert judgment; then, on another slide, you list the personnel who worked on the Task Force.

Is the Task Force your expert judgment?

MR. DOBSON: Yes.

Unlike what you heard described this morning, we did not have a core group that went out and solicited panels. The group you are hearing from is the group I described and that is listed on that page.

MR. BARNARD: Dr. Robertson is the only independent person?

MR. DOBSON: Yes. Jack was probably the only person who was certainly completely independent. There were a few others, including our decision analysts, obviously, who are not project participants; but the majority of the Task Force was composed of project participants.

The remainder of the presentation today is shown on this simplified graphic. The first two parts include the summary of the information needed and the alternate strategies considered. I will do that part of the presentation in the next 20 or 30 minutes.

Following that we are going to go into a discussion of the framework for evaluation and a description of the expert assessments we did. As I mentioned before, Hollis Call will do those presentations.

Finally, following that, we will have a summary of the geotechnical inputs: the results of the model evaluations that I just described and the assessments of the subsystem performance elements we considered. Ernie Hardin from Science Applications will do that.

After that, Hollis will come back to talk about the results of the evaluation model and what it means with respect to our decision-making; and I will be back at the end to talk about the conclusions and recommendations for where we go from here.

It was obvious to us from the start, and to the Nuclear Regulatory Commission as well when they wrote their original objection, that prior to defining what exactly the testing strategy ought to be we needed to consider what kinds of information we needed to get from the Calico Hills.

The first thing we did when we formed the Task Force was we formed a subpanel which consisted of mainly our hydrologists and there was some geological input. We asked them to define the information needs from the Calico Hills non-welded unit, including three kinds of categories.

The first was what kinds of information do we need? What are the parameters you need to measure? Is it conductivity or transmissivity, or what properties. Second was the locations for which that information was needed. We recognize that those properties are especially variable and you need to know them not only in the matrix, but in fault zones and anomalous zones.

Finally, if we could were there any specially correlations that we could establish between the information needs?

They came back with a summary of the information needs that is shown schematically on this viewgraph. They had eight categories of types of information: six of what they called rock information needs including matrix properties and conditions, single-fracture properties and conditions, the same needs for fracture systems, fault-zone properties and conditions, fracture-fault system geometries, and anomalous but not fractured rock properties; and also we needed characteristics of fluid conditions in the rock: liquid and vapor.

DR. DOMENICO: Are any of those variables in the Calico Hills known today with regard to information needs?

MR. DOBSON: There is existing information in all of these, but it is limited as you aware. There are a few measurements of matrix properties. Actually I am not sure that

I can say there are any measurements of properties in the fault zones of the Calico Hills. There probably are not.

We have on the order of half a dozen or a dozen drill holes through the Calico Hills now so we do have a set of matrix properties, some saturation/moisture content measurements and a few.

DR. DOMENICO: To my knowledge, there are two measurements of permeability, both of which is horizontal, one of which is high and one of which is low. Is that correct?

MR. DOBSON: I am sorry, I don't know.

MR. HARDIN: There were 11 core samples taken that have been reported in a Sandia report that I think is in draft.

DR. DOMENICO: I was referring to in situ tests, hydraulic conductivity in situ tests.

MR. HARDIN: I cannot comment on that right now. I would have to think about that one.

DR. DOMENICO: Well, I can. I think I remember.

MR. DOBSON: Okay.

DR. DOMENICO: There are two. I think, from that matrix there, that is about it.

MR. DOBSON: There is no question that, as you will see, the current uncertainty is a significant problem with completing assessments like this. The data set that exists is extremely incomplete and we need a lot more information.

Our subpanel came back and said, "Not only do you need to know those kinds of informations, you need to know certain characteristics of that information, things like what are the values, what are the statistical characteristics of the values like the mean and standard variation, and the spatial distribution. If you can determine, directional variability. Representativeness, if you can get some kind of a measure of that. Finally, any correlations between parameters you can establish."

The subpanel came back with a rather extensive list. It is a table 10, 12 pages long of information needs that probably will show up, if I had to guess, in the study plan when we get around to writing that.

I wanted to show a graphic that summarized some of what I said about the need for information in different areas. This shows, in color, some of the reasons you need to have information from different areas of the repository block.

Most of you are familiar with the conceptual perimeter drift diagram. In the three colors this shows the contours of the approximate thickness of the zeolitic units in the Calico Hills. In the south and western portion of the repository there is very little zeolitic at Calico Hills. It is primarily vitric; but there may even be a small chunk of the repository where there is no continuous zeolitic horizon underneath the proposed repository.

There are also a number of structural zones where we feel it is important to have information of various sorts, including the Solitario Canyon Fault Zone on the bounds of the repository block on the west, the drill hole wash structures which may or not be faults but certainly constitute major lineaments on air photographs, the Ghost Dance Fault which cuts the repository, and the Inregut [ph] Fault Zone which is present to the south and east of the repository.

We felt we needed information from all of those areas.

The next step, after having identified the information needed, was to evaluate the various techniques one could use to acquire the needed information. We put together some tables in which we summarized possible techniques for acquiring all that information.

This is an example of one and it contains some qualitative letters there we don't need to talk about too much; but basically the purpose is to show you that the various techniques we have considered have included surface-based techniques, including mainly mapping which is what is shown on the far right, over here; including geophysical techniques, which can be conducted either from the surface or from the sub-surface; including bore hole drilling, and that includes of course both vertical holes and angle holes that we added as a separate category because of the hydrologic value of the multi-well clusters and also underground bore holes from the main test level; and finally we added excavation and we separated, we had three categories: the shaft, drift and ramp type information.

We qualitatively evaluated the information needs in terms of how well each of the techniques could provide that kind of information as an input or a tool to developing the strategies we were going to use.

I am through the first two parts of this. I am now going to move to the strategies we did consider.

As I said, given the definition of the information needs we then went and composed a set of variables or options that we could use to develop different testing strategies. The various options were identified such that they had different characteristics with respect to at least two important things.

One is their ability to provide different types and amounts of testing information; and second is their potential impact on the performance of the site. Let me see if I can give you a little more clarification on that.

The strategies differed in the types and amounts of surface-based testing they had. We included as a base the current program described in the Site Characterization Plan, which is primarily the vertical drilling that you are all familiar with, in all of the strategies.

Some of the strategies included expanded vertical and angle bore hole drilling from both the surface and the main test level, and we did have a drilling engineer on the Task Force to help us put in only things we thought were technically feasible.

Finally, some of the strategies included the addition of a small underground facility in zeolitic Calico Hills rocks at Pryle Pass, which is north of Yucca Mountain. In other words, especially in cases where the strategy did not contain in situ excavation near the repository block, it was felt that in situ excavation in an area away from the repository would be a minimum necessary requirement in order to support the ability of this technique or strategy to provide all the information needed.

For strategies that had underground excavation in or near the proposed repository block, the principal variables I mentioned that we developed are shown here.

The first one was the amount of excavation: whether it was a limited facility versus an extensive facility. The second was whether it was connected with the main test level Exploratory Shaft Facility. The third was the location of the initial penetration in the Calico Hills unit; and that includes locations both inside and outside the exploration block.

I want to point out that we felt that each of these things was sensitive to one or both of those criteria: the potential impact on the site, and the potential amount of information you get out of it.

For example, with respect to the first category: amount of excavation, a limited facility in general could be expected to have lower impact. It could also be expected to provide less information.

A facility that is outside of the exploration block, of course, would have lower potential impact, but it would also provide less representative information.

A connection with the main test level shaft facility was viewed largely as something not that would provide more or less test information, but that conceivably you could construct scenarios where there was greater impact if you had connected pathways from the main test level.

A couple of other constraints on possible locations for underground facilities were identified by the group. These were selected with some rationale, but not in any quantitative performance assessment sort of sense.

We felt we needed on the minimum of on the order of 100 meters of thickness of the Calico Hills from the base of the Topopaw Spring to the water table. To consider an area for a strategy less than that, we felt we were going to get into problems with capillary effects from the water table and not having enough room to do an adequate test program.

Secondly, for outside strategies we adopted a minimum of a 2,000-foot setback from the exploration block just to get that far outside the repository block. It was selected somewhat arbitrarily, the rationale being that that far away from the block there would be little or no impact as a result of the excavations.

Given the options and the constraints that I have described, we identified six general areas as possible sites for Calico Hills test facility access. These locations were then combined with other variables, primarily the other variable being: How much surface-based testing do you combine with how much in situ testing when you constitute something you call one of your strategies?

This map shows where the six general areas were. It also shows the sketch, the 2,000-foot setback, for the outside zones. The areas you will hear us talk about include north and northeast locations both inside and outside of the block, a central location, south and south east locations: one inside and one outside, and a west location.

These were, as I said, tentatively identified as places where you might put the access for the Calico Hills.

DR. DOMENICO: Is there a line someplace where you could say Calico Hills is unsaturated totally? Where would that line be?

MR. DOBSON: I could not tell you exactly, but it would probably be somewhere around in here. There is just a little bit of Pryle Pass on the west edge of the repository.

You saw that color graphic I saw before?

DR. DOMENICO: Yes, I did.

MR. DOBSON: At one point I had in the presentation an isopack map of the thickness of the Calico Hills.

The total thickness of the Calico Hills' unsaturated unit in the repository block goes from about 300 feet, here, to about 1,000 in the southwest end. So the minimum thickness of the Calico Hills is at the northeast end, and it is 300 or 400 feet there; and it gets thicker to the southwest.

DR. DOMENICO: Let me ask it again.

Five of those locations will encounter unsaturated Calico Hills and one will encounter saturated?

MR. DOBSON: No. All of these will encounter unsaturated Calico Hills.

DR. DOMENICO: All. So it is all unsaturated.

MR. DOBSON: Yes. All of these locations would be in unsaturated Calico Hills. I am sorry, I misunderstood the question.

MR. HARDIN: In the northeast the unsaturated part is the thinnest.

MR. DOBSON: You will see in a couple of minutes that we eventually screened out the northeast option because it was so thin. That was exactly the reason we screen it out; but it met the minimum standard.

When we first drew it up there was roughly on the order of 70 or 100 meters, about 300 feet, of thickness of it out there; but because it was so small we eventually screened out that option.

DR. NORTH: Could you review for us the vitric versus the zeolitic areas?

MR. DOBSON: Sure. They overlap, Warner, as I am sure you have seen.

DR. NORTH: Yes.

MR. DOBSON: The contact is kind of gradational going from northeast to southwest. On the extreme southwest edge there is a few hundred feet where the Calico Hills has little or no zeolitic facies. You start to pick up thin beds of zeolitized Calico Hills as you move more to the northeast. By the time you get to the northeast the entire thickness is zeolitized.

The previous viewgraph I showed that had the colors on it had contours of the thickness of the zeolitic Calico Hills so that if you put my pointer about like this you are looking at about the 100-foot thickness of zeolitic, and here would be about the 300-foot thickness of zeolitic. So the thickness decreases along contours to the southwest.

In most of the block there is some zeolitic and some vitric, but the thickness decreases dramatically to the southwest.

I mentioned that when we combined all these things we came up with 24 possible combinations for the underground portion of the strategies. This shows them summarized according to the variables that I gave you a few minutes ago.

The area, of course, is the first. North or northeast are lumped together here; south or southeast are lumped together here; then central and west; and then there is the option of being inside the block or outside the block for the north and south options.

There is the option of having an extensive facility or having a limited facility so that permutes into two more chances. Finally there is the option of integrating the facility with the Exploratory Shaft Facility or leaving it separate.

We numbered those and then we began to screen the options.

MR. BARNARD: Dave, in that center column, "Extensive Operational Facility", can you describe the difference between an extensive facility and one that is not?

MR. DOBSON: Yes. You will see that in a minute, but if you want a quick summary of it the extensive facilities were facilities where we planned to have the ability to access virtually any structural feature in the block. So they were designed such that you would need to plan for 12,000 feet or more of drifting in the Calico Hills.

The limited facilities were planned such that they would require 5,000 feet or less, approximately. That is drifting, now; that does not include shafts and/or ramps getting into them.

MR. BARNARD: 5,000 feet of drifting is a limited operational facility?

MR. DOBSON: Yes. That was based, actually, on some input from our mining engineers on approximately where the transition would be in terms of ventilation requirements and additional support shafts. That is where we came up with the threshold: somewhere between 5,000 and 10,000 feet.

You would get sort of a quantum leap in support requirements.

We learned that after having composed these 24 possible underground configurations we got the full Task Force back together, and screened and aggregated the options with the goal of producing a finite set of strategies we could evaluate.

As I said earlier, we did a pre-screening phase. Before we composed the strategies we took the components of the strategies--whether it was extensive or limited, whether it was limited, et cetera--and we did a kind of pre-screening by qualitatively evaluating them, as I mentioned, with respect to potential impact and test utility or amount of testing information provided.

These then go qualitatively ranked, as I said, such that a facility inside the block that had extensive drifts and which was integrated with the Exploratory Shaft Facility would potentially have the highest impact. Extensive facilities inside the block also ranked high in terms of test utility.

You would get the most information from a lot of drifts in the repository block. On a relative scale, an outside facility that was small, not extensive, would provide the least testing information, but would also have the lowest impact.

So we developed this sort of pre-screening set of information, again to help us compose the strategies. Given that information we screened out some of the possible options. I have a few examples here of the ones that got screened.

We eliminated outside options that were not integrated with the Exploratory Shaft Facility. We felt that would extend the boundary of the working facility, and there was not really any rationale for doing it; and all it would do was potentially add waste isolation impacts to the site.

We screened out inside options that were not connected with the Exploratory Shaft Facility partly because, depending on where it was, it could potentially reduce useable area of the proposed repository; and partly also because, in most cases, you are going to require an extra penetration anyway to get down to the Calico Hills. So we did not feel we were saving much by leaving inside options that were not connected with the Exploratory Shaft Facility.

Relevant to the question Dr. Domenico asked a few minutes ago, the northeast outside option was eliminated because the available thickness of the Calico Hills was right on the margin and we felt it was not enough to really leave us a lot of flexibility.

The west outside option was eliminated for a couple of reasons. One is that because of the down-through of the Solitario Canyon Fault you have to go pretty far west to get to Calico Hills back in the unsaturated zone. By the time you are that far to the west you have what we felt were fairly significant questions about representativeness; and we really did not know anything about what the rocks look like out there.

Finally, the central inside option was eliminated partly because of potential reductions in useable area, and partly also because, as you will see, we felt the information that was provide by that strategy was provided probably even better by some of the other strategies we came up with.

The remaining options after the screening were combined, as I said, with surface-based testing options to create eight strategies. We think the strategies we came up with represent an appropriate range of the possibilities in terms of maximum information provided by testing versus minimizing potential impacts.

There is one last important slide before I start describing what the individual strategies were. There was some question earlier--and Max mentioned this morning I think--that we did not do a detailed study plan for these investigations: We have not said that at latitude such and longitude such you should do a percolation test.

We did, however, define what we thought the basic testing program would be for the underground portion of the Calico Hills test in any of these cases. This is taken, at least in part, from the original Site Characterization Plan. These were the techniques we were planning to use.

We still feel that all of these are appropriate and applicable so I wanted to go over them.

Each of the strategies, of course, gets a mapping program. In our current program, that is a photogeometric mapping program combined with lots of apa-phase mapping. We have extensive mapping of the wall rock. This supports the studies of the geochemistry, the geochemical retardation potential.

DR. DOMENICO: David, which areas are left? You eliminated all of them, didn't you?

MR. DOBSON: No, no, no.

DR. DOMENICO: What is left?

MR. DOBSON: The north.

DR. DOMENICO: The north is left?

MR. DOBSON: The north inside, the south inside, the southeast outside.

DR. DOMENICO: Okay. Thank you.

I kept marking things gone.

[Laughter]

MR. DOBSON: On that map there is one comment I did not make that I intended to make.

Of course, we were considering the Calico Hills, and drifting and testing within the Calico Hills. We did not explicitly consider where a shaft at the surface would start. We did not consider if an access was in a valley or on a ridge.

We have just provided that we needed to get access here, and that those other aspects of the analysis--many of which are being considered by the Exploratory Shaft Facility--were not explicitly considered by the Calico Hills.

We just considered where we needed access to the Calico Hills.

MR. McFARLAND: Dave, could we back up a moment?

MR. DOBSON: Sure.

MR. McFARLAND: The central inside option was eliminated because of potential reduction in useable repository area. Would you clarify that?

MR. DOBSON: The main reason is that if you have an opening going down through the proposed repository horizon the tendency of our 60 requirements is that you leave space around anything. You have to leave a pillar around any opening, whether it is a drill hole or a shaft.

MR. McFARLAND: You are talking shaft apart from the

Exploratory Shaft Facility?

MR. DOBSON: Yes.

We do have access to the central part of the block in several of our strategies. That is why we eliminated that particular option.

There was also a question in the central part with that testing. I did not go into all of these. For the central option you are limiting your ability to get to structural areas. You are kind of in the best, biggest block of the repository and you cannot get over to things like the Ghost Dance Fault and Solitario Canyon Fault without more extensive drifting than we planned for.

MR. McFARLAND: All options are connected to the Exploratory Shaft Facility, right?

MR. DOBSON: No, all are not.

MR. McFARLAND: All inside options.

MR. DOBSON: All inside options are, now. They were not when we started.

We started with the assumption that they need not be, but during our screening we eliminated those inside options that were not connected to the repository.

MR. McFARLAND: Mainly the central one.

MR. DOBSON: That is right. Exactly.

MR. CORDING: But you are not eliminating the possibility of a drift through the central area.

MR. DOBSON: Not at all. You will see a drift through the central area on at least two of our strategies.

DR. DOMENICO: You are saying if you do have access to either the north or the south areas it will be through the Exploratory Shaft Facility?

MR. DOBSON: If it is within the block, yes.

You will see strategies that have access to the southeast outside the block that are not connected to the Exploratory Shaft Facility; but for our strategies that are in the block they are connected to it.

MR. ROY WILLIAMS: Dave, what happened to your fracture mapping up there?

MR. DOBSON: Fracture mapping is part of the mapping program.

MR. ROY WILLIAMS: Is that an oversight, or it just doesn't do anything like that?

MR. DOBSON: To get back to the information, you will recall we had four different categories of rock information related to fractures. Those are the principal components.

I fully expect that the mapping program is going to concentrate primarily on fracture mapping. I probably should have been more clear about it and put down "fracture mapping".

In addition to the mapping and standard sampling programs we anticipate will be done during the underground exploration period, or would be done, we also have plans for pilot bore hole type tests that could be done. Some of these were described in connection with other tests for the main test level.

Hydrologic testing in whatever the access facility is tests like the radial bore holes tests that are already being planned; exploratory drilling from underground openings for features that we suspect might be there or know might be there from other information and hydrologic tests of any major features intersected by the drifts.

In other words, if we find faults in any of these drifts we are going to stop and try to figure out what the characteristics are of that fault.

In addition, for the extensive drifting strategies you will see we assumed that some additional kinds of experiments would be done since there will be so much more room available, basically. Those include bulk property experiments: things like bulk permeability; we have some described in terms of pneumatic tests in the Site Characterization Plan now, but we could do them hydraulically as well; and some percolation transport experiments could be run.

There are something not too different than from what Allen Flint is probably planning on the surface now, but being done in the Calico Hills.

That summarizes the test program. I am now going to quickly go through the strategies. I am going to do them in order from least excavation to most. Unfortunately there are not in numerical order. They just came out this way.

Strategy Number Six is a Surface-Based Testing Program only. It represents the minimum program that the Task Force felt was appropriate to consider. It is a bigger program that what is currently in the Site Characterization Plan.

In this we added some of the things I described to you before. We have angle holes on the Solitario Canyon Fault, on the Ghost Dance Fault and through the drill hole wash structure; and include a deepening of the multi-purpose bore holes through the Calico Hills, and it includes some angle holes drilled from the main test level through the Ghost Dance Fault underground.

It also includes the Pryle Pass test facility that we described. That would be a facility that would be on the order of a 200-foot long added which could be drilled in from Pryle Pass into zeolitic Calico Hills, and would allow access for tests of whatever sort we wanted to do.

That is the minimum strategy that we considered.

MR. ALLEN: What is the rationale for two vertical holes adjacent to one another?

MR. DOBSON: In the current Site Characterization Plan the multi-purpose bore holes are drilled solely to analyze impacts from the shaft, and are not drilled through the Calico

Hills; and we drilled to the bottom of Topopaw Spring.

So this includes deepening those holes. That is all.

DR. BLANCHARD: Clarence, they are drilled prior to construction of the exploratory shaft so you get a set of baseline conditions.

MR. ALLEN: Related to the Topopaw shaft.

MR. DOBSON: Yes. This just says we will make them characterization holes, drill them through the Calico Hills.

I am not going to summarize the word-sides that go with each of these in the interests of time, but they are there for anyone to ask questions if you would like.

DR. DOMENICO: In terms of hydraulic testing, will you do any of that in saturated Calico Hills?

MR. DOBSON: Yes, I would anticipate that most all of these would be drilled--well, no. You will not encounter saturated Calico Hills unless you drill sort of west of the center line of the repository; but then you will.

DR. DOMENICO: You will encounter saturated Pryle Pass.

MR. DOBSON: That is right, somewhere around the central part of the facility.

DR. DOMENICO: So basically hydraulic testing is out in this program, in this phase.

MR. DOBSON: Remember, I am just describing the testing in the unsaturated zone. I don't think that would imply that the guys doing the characterization program in the saturated zone are not going to test the Calico Hills. I think they are.

DR. BLANCHARD: Dave, there is one point I think we want to make clear.

The Surface-Based Program that has been described to you previously for both anomalous features as well as the geostatistically-based program assumes a combination of drill holes to acquire a core as well as drill holes to place sensors to make hydrologic measurements in both the saturated and the unsaturated zone.

All of these test strategies for the Calico Hills assume that program is carried out as planned. These are deltas to that program.

MR. DOBSON: I guess I would reiterate that.

This program assumed that the current unsaturated and saturated zone drilling zone programs that are planned and that are in the Site Characterization Plan are a fundamental part of this analysis.

All of those do collect information on the Calico Hills.

DR. DOMENICO: The Calico Hills saturated zone?

MR. DOBSON: And unsaturated zone in places; but what we are talking about here is the activity that was originally in the Site Characterization Plan that proposed doing in situ testing in the Calico Hills, and an increment on top of the program in the existing Calico Hills.

So everything I am talking about here does not represent the only testing we plan to do in the Calico Hills. It represents the additional increment of testing beyond that: the primarily vertical drill hole program that is described in the current Site Characterization Plan.

The next strategy was an attempt to get some information from in situ testing without getting too close to the repository. So we added a small facility in the southeast, on the order of a couple thousand feet of drifting, with access to some of the structures that exist down there.

There are some faults down there including probable extensions of the Ghost Dance Fault. It also added some angle drill holes on the northeast in the drill hole wash section, in the Solitario Canyon Fault and Pryle Pass test facility I mentioned earlier.

Therefore, this was slightly more information than we got out of the last one, including an in situ facility, but not within the repository block.

The next was Strategy Number Seven. It was basically similar to the previous one, but had an extensive program of drifting in the southeast unconnected to the Exploratory Shaft Facility.

It appeared the southeast was the best area for getting information that was closely analogous to, but not from the repository block. As most of you are aware, there are a number of faults and fractures in the southeast section. So it would be an area where they information would give us sort of structural information on how the Calico behaved in a structural zone.

It also included some additional angle holes on the Ghost Dance Fault and the Solitario Canyon Fault and drill hole wash structure because, again, of the fact we were not going to get any in situ studies of those within the block. So we threw those angle bore holes into the strategy as well.

Strategy Number Three is one we refer to sort of as something like what the original base case would have been in the consultation draft of the Site Characterization Plan. It includes a small facility in the northeast that does have access to the Ghost Dance Fault, the Inregut Fault and the drill hole wash. It is pretty much the same facility as our current configuration in the main test level except that it is done in the Calico Hills.

That was a kind of a reference case for us in terms of the analyses.

Strategy Number Four is essentially the same concept as Number Three except that it went to the south end of the repository. It is limited drifting with access to the

Solitario Canyon in this case and some of the faults on the southeast edge: a small facility.

Obviously the difference between these two is that Strategy Number Four has good access to the vitric tufts; Strategy Number Three has good access to the zeolitic; but neither of them has good access to the other.

Strategy Number One was an attempt to solve the problem of getting representative information together with getting lots of information . So we came up a strategy that involved extensive drifting in the southeast together with a limited facility in the northeast, a confirmatory facility almost.

The drifting in the southeast gave us access to lots of structural features, also to the vitric/zeolitic transition. The northeast would give us some confirmation that the information acquired was consistent with that we had measured in the southeast.

We also added an angle drill hole on the Solitario Canyon Fault and the southern end of the Ghost Dance Fault because, again, they would not be accessed in situ; and we included the Pryle Pass test facility in the strategy as well.

So this one is kind of a summary of all kinds of stuff.

Finally we have Strategy Number Five, which is essentially identical to Strategy Number Two, which is the next one you will hear about. This strategy includes extensive drifting within the repository block; it includes access several places into the Ghost Dance Fault, access to the Inregut Fault zone to the northwest trending structures and drill holes and wash, and access to the Solitario Canyon Fault.

As I said, we initially calculated this strategy would require a minimum of 12,00 feet of drifting. I think as drawn here it has something like 18,000 or 19,000 feet.

It is an extensive facility that gets information from all the lithologic units. You get the opportunity to go through the vitric/zeolitic transition in one or more places as required, and you get lost of structural information as well.

The only difference between this strategy and Strategy Number Two, which I will put up in five seconds, is that this strategy has its access in the north and Strategy Number Two has its access in the south. That was done partly because of integration needs for the Exploratory Shaft Facility group.

If we used only one strategy and they had a south facility it was going to be very difficult to integrate; but from the view of the Calico Hills Task Force the information provided by this strategy and Strategy Number Two are identical.

We were interested in getting extensive exposures in the Calico Hills and it really did not matter, because the facilities were so extensive, whether the access came in the north or the south.

To confirm that statement, there is Strategy Number two with the access in the south.

That is all I have to say for the first part of the presentation.

DR. DOMENICO: I have a hard question to phrase.

MR. DOBSON: Okay.

DR. DOMENICO: Drifting requires you have an exploratory shaft. That is number one. A lot of us are interested in early assessment of the site: site suitability.

Which parts of your strategies will serve those needs? Drifting is going to come before you can give an early assessment, assuming things are done in sequence.

Will your selected strategies serve the program of early assessment of site suitability? Did I state that clearly?

MR. DOBSON: Most of it.

DR. DOMENICO: Okay.

MR. DOBSON: You might want to join in, too, Max.

I guess I would say that the Task Force did not explicitly go about prioritizing the Calico Hills tests with respect to the other tests that are being done. I think someone mentioned this morning--and it might be a logical next step--the in work that Bruce and Steve described, and Russ, they might want to go back and rank essentially the underground tests versus the surface-based tests.

I don't think what we have done here would prevent you from making this a high-priority item, but we did not explicitly address that. I guess that is the main thing I have to say.

We were not asked to rank the Calico Hills tests versus some others.

DR. DOMENICO: No. I do not want a ranking. The Calico Hills, you said, is your main barrier.

MR. DOBSON: Yes.

DR. DOMENICO: If you are going to get an early site suitability analysis you have to investigate, somehow, your main barrier.

MR. DOBSON: I agree.

DR. DOMENICO: How do your selections meet that need or do they? Maybe we have to depend on other things.

DR. BLANCHARD: Dave, can I help?

MR. DOBSON: Sure.

DR. DOMENICO: I said it was a hard question, Max.

DR. BLANCHARD: As Dave talked through these sequences you can see there are different degrees of impacts to the Calico Hills. Each one of these causes more drifting, which is an impact to the Calico Hills.

It is not necessarily much of an impact on performance; and presentations that come subsequent to this will describe the perceived nature of the magnitude of the impact on performance as a consequence of that drifting.

The real benefit of drifting in the Calico Hills allows you to gain thousands of test points about hydrology and about radionuclide retardation properties. The earlier one does that.

In the Calico Hills the earlier you either learn that your experts' judgments were wrong because they did not have enough information on which to base a good premise, or that you confirmed they are in the right trends.

Therefore, the timeliness with which you conduct these underground test programs in the Calico Hills within the area of the waste emplacement area--in other words, inside that boundary--the better off you are with respect to acquiring information on early degrees of unsuitability or early disqualifiers.

The timeliness issue, rather than being addressed by this particular Task Force, is being addressed in the Exploratory Shaft Facility Alternative Strategies because timeliness has to address schedule and cost, and other factors, in addition to the test program that would be outlined in the Calico Hills.

So if you had your druthers--if cost and time were not very important--the first approach would be to go for it as fast as you could: have access to the Calico Hills at the range of properties or exposure, allow testing in the range of properties where you have order of magnitude changes in hydraulic conductivity and zeolitic-type minerals.

That is not necessarily the way you would manage an engineering program or the way you would choose to spend your money if you had a more ordered process.

Therefore, in the end, I think, it is going to turn out to be very much a management judgment and depend a lot upon the amount of dollars available.

However you are right that if you go in a very step-wise methodical process one might spend a year or two or three years, as you construct your exploratory shaft slowly, going through each one of the rock units until you get down to the Calico Hills, and then not start your Calico Hills test program for three or four years from now.

The point you are trying to make about the timeliness of the acquisition of this information to learn, early on, about the performance characteristics of that rock unit is very important, and it is one that we are watching very closely.

DR. DOMENICO: But I am trying to see a connection between this program and the Surface-Based Prioritization Program. This morning we learned they are all integrated.

Drifting is going to come too late for an early site assessment?

MR. BROCUM: Probably.

At the beginning of Dave's talk he said these things were changes and additions general to our Surface-Based Drilling Program, which has an extensive component of drilling down to the water table: 18 or 20 holes.

We have a major drilling problem which will happen irregardless of whichever choice in the Calico Hills drifting we make.

DR. DOMENICO: So we have 20 holes in the Surface-Based Prioritization Program in addition to these?

MR. BROCUM: I don't know the exact number. Ernie Hardin is the expert. We have a large number of holes going down to the water table, which will be extensively cored and tested.

DR. DOMENICO: That is right. I did not know that this morning.

MR. BROCUM: I am sorry.

MR. DOBSON: I am sorry if I did not make that clear. There is one other fact that maybe I will throw in.

This is all an increment on the currently-described bore holes, the unsaturated zone drilling program, which has I think 18 holes in the repository area, and the systematic drilling program, which has 12 in the first phase.

Those are 30 bore holes, all of which go through the

Calico Hills into the saturated zone, and do testing throughout the unsaturated zone and in the saturated zone. In some cases we will use the same holes for that.

All of this testing that I am describing is an increment on that: specific characterization of the Calico Hills.

I would add one relevant piece of information. You will see our assessments of how good the tests are at providing the kind of information you need. We recommended that we thought Calico Hills drifting should be done as soon as practicable partly because of the fact that the uncertainties with drilling only strategies remain high.

We felt you get a considerable benefit with in situ observation of conditions in the Calico Hills. That is just a relevant observation to your point; but I think there is no question that we plan a big drilling program, and that that will be followed up with a specific characterization program in the Calico Hills.

Our recommendation was, as you saw, as I mentioned earlier Strategies Number Two and Five, extensively.

MR. ALLEN: Will a significant number of those holes in the ordinary plan be within the repository block?

MR. DOBSON: Yes, a dozen, something like that.

CHAIRMAN DEERE: It would seem to me, Dave, that Number Five would have the advantage over Number Two that you have an earlier access to the Ghost Dance Fault, unless you are really after what it is like at the very far end and then the one you have, Strategy Number Two, gets you down and over but still involves a lot more drifting and maybe six more months of time than others.

MR. DOBSON: If the Ghost Dance Fault were your primary target. If, on the other hand, you chose the Solitario Canyon Fault you might give benefit to the southern strategy.

DR. BLANCHARD: If one wanted to do it as fast as possible, you would actually want to go in from both the southwest and the northeast at the earliest possible time with either ramp or shaft and connect in the center, which would allow you to start your underground test program at both ends where the access was.

CHAIRMAN DEERE: I agree with that, certainly.

You say this is an add-on to your existing program, but certainly you must be getting benefit out of the new program that should cause modifications in the original program, like reduction.

MR. DOBSON: Perhaps.

You saw, when I first described the Surface-Based Testing Program, that we considered some of the things you might want to add given various strategies for underground exploration, for in situ exploration.

The current program we have in the Site

Characterization Plan is pretty specific, though. I suspect we would not want to delete much, if any, of those things.

The systematic program gets you spatially distributed information that we think we are going to need in almost any case, although some small number of those holes might be deleted if, in fact, you had a lot of drifts there. That certainly is a possibility.

The site vertical bore holes in the unsaturated zone drilling program are feature-specific. Therefore, this program has clusters of bore holes on the Ghost Dance and clusters of bore holes on the Solitario Canyon.

I suspect you are probably still going to want to do that because it gives you access for various kinds of hydraulic testing.

CHAIRMAN DEERE: A lot of which would not be necessary if you had access to it.

MR. DOBSON: Perhaps. Those things would certainly be reconsidered.

CHAIRMAN DEERE: Yes.

MR. McFARLAND: At one time in the past mentioned there was a constraint, real or implied, that drill holes would not penetrate the repository structure: that they would be located in columns and regions outside of actual repository drift and tunnels.

MR. DOBSON: Yes. That comes out of 10 CFR

15(c)(1)? I cannot remember the number; but it says to the extent practical locate bore holes in shafts and pillars.

Of course, we felt that would be more important with shafts than with bore holes.

MR. REITER: You can answer this later on. That is fine.

What is the significance of the properties of the saturated Calico Hills with respect to performance assessment as compared to properties of the unsaturated Calico Hills?

MR. DOBSON: I think I would like to defer that to Ernie for later on.

You will see that we did assess how we thought the performance would be of the saturated zone; and that is a significant part of our analysis. But I would probably best not go into that now.

MR. REITER: We are going to see what the effect would have of particular assumed properties in the unsaturated Calico Hills on the forms assessment?

MR. DOBSON: Yes. You are certainly going to see what contribution we felt you got from the saturated zone. That includes not only the Calico Hills: it includes all the saturated zone.

MR. REITER: What about the unsaturated: in other words, the range of properties that it is possible you could find in the unsaturated Calico Hills? What impact would that have on the performance assessment?

MR. DOBSON: I think I am a bit confused.

We did the unsaturated Calico somewhat separated from the saturated. We did the saturated zone independent of the unsaturated zone assessments: not unrelated, but not in the same model

MR. REITER: Are we going to see that?

MR. DOBSON: Yes, you will see what we did with the saturated zone.

I will not introduce Hollis Call from Applied Decision Analysis. He will tell you about the framework that we used in the expert assessments.

Framework for Evaluation and Description of Expert Assessments

MR. CALL: I feel like an FBI agent.

My name is Hollis Call. Elizabeth Browne and I have been the decision analysts on this project for the last three or four months we have been in the process.

I am going to describe for you the methodology that we developed, implemented and used over the past four months of conducting this analysis. There will be a lot of similarities with what you have heard this morning in Bruce's talk, but there will be one important difference, at least: this is what we have actually done, not what we are planning to do.

Some of what we have had to do to fit this into the time constraints and so on just to get our analysis completed will account for some of the difference you are going to hear about.

To give you a quick look at the road map, this is the discussion of the framework we used to evaluate the strategies.

Bruce talked a lot about some of the basic principles of decision analysis and information analysis. I am going to review with you again a few things. Those of you who are familiar with this please bear with me. I think it will help to go through this.

I have used a very simple, what I hope is a relatively intuitive, example to help illustrate some of the basic principles.

The idea is that you have a decision to make about buying some stock. It is not relevant example for me because consultants don't make enough money to buy stock, but for many of the rest of you maybe this will be more meaningful.

[Laughter]

MR. CALL: The decision you have to make is whether or not you are going to buy some stock. At the same time, somebody offers you the opportunity to buy some research to help give you some information about how that stock might perform; and performance here is defined in terms of how much money you might make or lose.

So you have a decision about an irrevocable investment of some money in the stock, and now somebody has complicated the problem for you by saying, "You can buy some information."

How do you decide whether or not to buy the information? And, more generally, how do you decide how much information to buy? It can be a very complicated problem, and it appears in a lot of different settings. As you will see in a few moments, it is exactly the nature of the problem we are talking about for Calico Hills.

The basic framework is very simple. It is the same framework we used in the Calico Hills analysis. You have a decision about buying some research. The research is going to tell you something, here in quotes, about if the stock will perform low or high. And then you get to make a decision about buying the stock based on the results of this test.

Down here if you don't buy the research you simply make the decision based on the information you have currently.

In either case, you have the uncertainty about how the stock is going to perform. You cannot reduce that uncertainty. It is something that is going to happen in the future. All the information is going to tell you is something, imperfectly, about the performance of that stock.

Then we have all the values associated with the decision, a result, a decision about buying the stock, and ultimately the performance of the stock.

The next slide is the last one of this simple example: same structure, but now we have supplied some of the probablistic inputs that we are also going to supply to the Calico Hills evaluation framework.

For the moment I am not going to go into the explanation of how all these probabilities are developed, but you can, for example, look at the priors you might have on how the stock is going to perform: what you believe the stock is going to do now based on your current information, 40 percent chance low/60 percent high.

You have also developed some probabilities about what the test results are going to tell you. Those help give you an updated probability distribution on how the stock is going to perform given a test result.

The is all pulled together in taking expectation on all of these events, we roll true back and we take optimal paths for the buy or no-buy. This framework tells us, very importantly, that if the test result is low the optimum choice is not to buy; if the test result is high the optimum choice is to buy.

Down here if you don't buy the research it says, "Go ahead and buy it."

All that is the same is that the information has changed your decision: that with the information you make a different choice than you make without the information. It is a very simple example, but very same principles.

This is one way to describe decision analysis. It helps you make decisions based on what you can do, what you know, and what you prefer: very simple ideas. They can get very complicated depending on the nature of the problem.

How do we implement this relatively simple set of principles for Calico Hills? Three basic steps.

First, identify major decisions: in the first case it was whether to buy the research, whether to buy the stock: identify the key uncertainties and probablistic relationships; and identify all the outcomes and values.

I am going to take you through those steps for Calico Hills.

Step One: Identify the major decisions. We categorize the decisions in two ways for this analysis. One we are calling immediate decisions: the things you have to do right now; the things the Department of Energy is going to decide on in the near future and, in fact, decide on on July 29th, at least at some level.

That is the question of if they would test, or conceivably not test; and if they test, there were eight possible options you heard Dave describe.

In addition, what are the future decisions the Department of Energy can make? A simple example, again: whether or not to buy the stock. In this case, it is far more complicated.

There are decisions about siting: Is this the right place to put the repository? There are decisions about design: What are all the engineered barriers that will go into this? Do you rely on a race-package design of a certain type or another?

Finally, there are decisions about emplacement: Do you put the waste in in the actual engineer-designed repository?

Now that we have identified those decisions we can talk about one of the simplifications that we had to make to make this analysis manageable in the time frame we had available.

This is a very complex set of decisions. You can see with a test result of low or high in this case you have all these decisions to make; and there are probably many more than just siting, design and emplacement. We have just simplified them to those three basic types of decisions.

What is it that is common to all those decisions? There is something that has to do with how the repository is going to perform; and performance we have defined in terms of releases.

One of the outcomes we were told is critical for this analysis is releases from the repository. So we said, "What is it that is common to these?" If it is releases, there is essentially a basis for all of these in common. That has to do with a behavioral assumption, in effect of: How do you behave?

Do you behave as if the repository releases are going to be high or low, or someplace in between? We can imagine some examples of how that would take place.

You observe a test result. If it is low and you believe that and you act as if it is low, then your siting decision might be to go ahead and put the repository at Yucca Mountain; to perhaps take a particular design path; and, ultimately, to go ahead and put waste in it.

Alternatively if releases are high that decision process might be cut very short. It might, in fact, not be the place you site the repository if you site any place at all.

The idea in this--and this is fairly key to the rest of the framework--is that we would boil down this fairly complex decision process into a fairly simple decision. It is going to be a little more complicated than this, as you will see; but in principle it is exactly like this.

We call that "act as if releases are at some level." That is going to be the basis of all the other decisions you are going to make.

That was Step One: identifying the decisions.

Step Two: identifying the key uncertainties and probablistic relationships.

In the simple example we used it was just a matter of what the stock performance is going to be. In the case of Calico Hills and the repository, of course it is far more complicated.

What we wanted to know is something about system performance; and we are uncertain about system performance because we don't know about transport through the Calico, which runs through about a fourth of the Calico; we don't know about performance impacts of testing: How much does drifting in the Calico actually increase the level of releases?; we are uncertain about the source of the Calico, Calico flow conditions; and ultimately we are uncertain about test results.

As Bruce described the logic of the influence diagram earlier today, the idea is that the arcs indicate probablistic dependencies. In this case, Calico flow conditions, for example, condition the test results. That is what we are going to try to measure.

We want know something about Calico flow conditions, and we are going to detect those through the types of tests we conduct.

Calico flow conditions are a condition of transport through the Calico, the source behavior, and performance impacts of testing. So Calico flow conditions turned out to be a very important variable in our analysis.

In fact, because of that importance--this is the same influence diagram with the variable Calico flow conditions--we, in fact, modeled that in quite a bit more detail than simply Calico flow conditions.

As I have shown, we had separate conceptual models for slow matrix, fast matrix, concentrated fracture and distributed fracture through the four major flow conditions we defined and on which we did additional modeling.

By the way, Mr. Hardin, the next speaker, is going to give you a much more precise definition of all these flow modes: a quantitative definition that we developed in the course of our workshops. For now I would like to continue with a high level presentation of how all this fits together.

DR. NORTH: Before you go on, at this point I think many of us will be very interested in more than just the high level overview, from which I am getting very little because I know the decision analysis and I have been exposed to a lot of the concepts.

It comes as no surprise, for example, that you disaggregated the way you did.

I am interested in just how did you do it? I would like to see a lot more detail; and I would like to lodge a request at this point that if those details are not part of the presentation we definitely want to see them as soon as they can be gotten to us.

MR. CALL: The details will be forthcoming in the rest of this presentation and also in Mr. Hardin's presentation. The nature of the presentation is such that it is kind of hard to throw it all out of whack.

DR. NORTH: It is not going to be good enough to give it to us just at this high level because we cannot judge it. You are asking us to take a lot of things on faith.

I made comments at the very beginning with Dave Dobson's overall conclusions reacting to the word "likely". I want to see the details. I want to see how you dealt with very low probability combinations which, in my judgment, should be driving this whole analysis.

I want to see how you dealt with them. Just showing me the influence diagram is not enough.

MR. CALL: Okay. I appreciate that. I am sure you will ask me if I don't answer that question.

DR. NORTH: Yes.

[Laughter]

DR. BLANCHARD: Dr. North, at the end of the presentations on the Calico Hills if we have not provided enough information we will be glad to supplement that with information at a greater level of detail.

As you know the Task Force is in the process now of preparing the draft report. All the information to which you are referring should be in that report at that level of detail.

DR. NORTH: I think it is very important that your draft report provide, not just for us but for all the other interested parties, a description of just exactly what did you do.

I read you the quotation from the National Academy of Sciences report about this being iterative. I suspect I will have a number of iterations that I would like to see in terms of different emphasis: different ways of disaggregating and the like. There may be a many good questions the people in this room can give you and people who are not in this room who may be present the next time this is presented.

I think you need to address all those questions and convince us that the insights coming out of your analysis, which you have given us: your conclusions, are very robust with respect to different ways this analysis might have been done and different expert judgment that groups of people, other than the ones you had involved, might reasonably apply as their judgment for this situation.

DR. BLANCHARD: Okay.

MR. CALL: I believe we are going to address those points, at least to some extent.

Let me tell you a bit about one of the conceptual models we used for the assessment process which I will be describing to you in just a moment. This is an influence diagram, or conceptual model, that the expert panel used when we did our assessments on fracture flow conditions: both concentrated fracture and distributed fracture.

I am going to go into the details of how we actually did our assessments, but this was one of the conceptual models that panel used. They had to justify and explain all their judgments in terms of settings of all these variables.

So when we did the assessment and we asked someone about the probability of a particular type of fracture flow condition that had to be explained in terms of these variables. This provides a very explicit model in a very explicit piece of documentation that we used, that--

DR. NORTH: That is fine. You are showing me that there is an analytical machine over there and that, if I look at it, it looks like the kind of analytical machine that I want to see.

But in order to review it I need the blueprints, I need the exact specifications, I need to satisfy myself that a lot of good engineering design judgment has been applied to the building of the machine. Just looking at it is not good enough.

MR. CALL: Okay.

MR. ROY WILLIAMS: Excuse me. Could you just define a couple of terms for me? I don't what "fracture hydraulic properties" are. I don't know what you mean by that.

The only hydraulic properties I know of are saturated hydraulic conductivity, unsaturated hydraulic conductivity, store activity, and moisture content and moisture tension.

Fractures don't have those hydraulic properties.

MR. CALL: We divided up the presentation this way for a reason. Ernie is, next, going to talk about the underlying geotechnical inputs to this. So we have a fairly long discussion that is going to be coming up next where all this is going to be spelled out.

We are not going to remove this from discussion. Ernie is going to use this as part of the basis of his presentation.

MR. ROY WILLIAMS: But you are not talking about fracture aperture or something like that? You don't know what you are talking about.

Okay, we will wait for the next speaker.

MR. CALL: We have discussed Steps One and Two: Decision and the Events. Step Three is Identifying the Outcomes and Values.

Again, this is fairly simple. In the stock example we had just the profits and losses. In the Calico Hills study the outcomes that we valued were the costs of the testing strategies, the benefits and risk of the act as if decision compared to the decision based on pre-releases, and finally the impacts of testing on waste isolation.

I am now going to show you how all this fits together.

In the influence diagram it relates these variables. again at a relatively high level, where there is a decision that is going to be made about the act as if. That is, if you will, the design basis for siting, engineering and so on of the repository.

MR. ALLEN: Do we have this figure?

MR. CALL: I am sorry, you don't, no. This is a new slide.

DR. NORTH: Is this the influence diagram version of the tree we have in our handouts?

MR. CALL: Yes. It is just simplified one level so that actual releases have in them all the--it is essentially the release model for the repository. So that includes the saturated zone effects, the source to Calico and transport though the Calico.

The idea here is to show you how this decision and the test results, actual releases and consequences all relate together. Okay?

There is this actual flow condition that is one of the flow conditions of the Calico. That determines or conditions the test results, which in turn are observed when you make this decision. It also conditions the actual releases.

You are only acting on imperfect information. There is some risk you are facing that, in fact, the site may behave all together differently. That results in some consequences.

This also shows the performance impacts of testing over here because the testing decision both conditions this and the performance impacts of testing.

Let me put up the tree now, which is in quite a bit more detail. You can see a bit more of the flesh and bones, if you will, of the evaluation structure.

We have test strategies, test results which are defined our four possible outcomes: test results reveal to you something about the flow conditions of the Calico; a decision about your actions or the basis of your actions: the act as if releases where R is less than .01, R is between .01 and .1, and so on including R is greater than 1; the actual flow conditions; releases from the source of the Calico; Calico transport; performance impacts of testing; saturated zone transport; and, finally, releases to the accessible environment.

If you don't do the testing, you simply make this decision without the benefit of the test results and you have the actual flow. The probability distribution for actual flow are your priors or what you know currently. The rest of the events are the same except for the performance impacts of testing, which of course you don't get if you don't do testing.

DR. NORTH: Does that tree represent the actual set of branching structures you used in your analysis?

MR. CALL: Yes, it does.

DR. NORTH: I am always relatively suspicious of decision trees where the degree of disaggregation at every branch is the same.

Did you lay all that out before you had the data, or did you iterate around on it a couple of times after you saw how it was coming out and what was sensitive?

MR. CALL: There is a fair amount of iteration.

If you are just talking about 4 and 3, and 3 and 3, I am not sure.

DR. NORTH: Yes. I am wondering if we need four or five or six or seven for the uncertainties that are really driving this analysis, in particular getting down to scenarios where the probabilities are very low but the consequences might be that you don't meet performance by a wide margin, going back to Dave Dobson's slide.

MR. CALL: Right.

DR. NORTH: I want to be assured you have done that; and I would like to see the numbers that convince me you have done this disaggregation in an appropriate way.

MR. CALL: Okay. That is the next subject. We want to talk about the expert assessments we did.

DR. NORTH: No. You are missing my point.

The issue is not how you got the expert judgment here, but rather how you went from the continuous probability distributions you presumably obtained from your experts into the discretized versions you have used in this analysis.

I am not convinced you have done it to the point yet where I can sign off.

MR. CALL: Okay.

In the next section I will talk a little bit about the difference between the discrete variables and the continuous variables, and how we went about assessing those and how we went about discretizing those.

I think it would be easier for me to go through some of those slides; and I certainly welcome questions if I have not answered it by the time I get through.

I am going to change the order slightly. I think I am going to present the second slide in your packet first.

The purpose of the assessment exercise is to capture the uncertainty of the experts with whom we worked and the uncertainty on all the variables I have shown you in the influence diagram that we used to characterize this problem.

I want to reinforce the idea that the point of doing this kind of exercise is not to get a number. In fact, our whole purpose was to try to test the experts and push them into expanding the tails of their distribution as much as possible so we were getting as accurate a representation of their confidence, their uncertainty about these variables as we could.

This is just some standard stuff.

I think we have had nine or ten workshops now with the panels with which we have worked. We went through a process of, first, developing the influence diagrams or the conceptual models that we wanted everyone to use as a common basis for describing their estimates.

We defined all the variables in those conceptual models precisely with quantitative definitions. And then we went through a process of elicitation, in a few cases including use of the probability wheel just to make sure we were getting consistent estimates.

We have a formal process we use in all cases. These are the simple sample questions we used.

DR. NORTH: Could you describe the degree of documentation on your formal process. What is available?

MR. CALL: I did not do an extensive documentation. How many pages?

DR. NORTH: I don't care how many pages. I want to know what is in it. Do you have a transcript, for example, of the meeting?

MR. CALL: We don't have transcripts, no. We did not have a Court Reporter at any of our meetings. We had no taping that was done.

We have all the scoring sheets. We have the basis for all the judgments written down on the scoring sheets. We also have notes taken by Ernie and, in some cases, ourselves.

So I think we have a fairly extensive set of documentation that we would be happy to provide to you for review.

DR. NORTH: Yes. I think we would like to see it.

MR. CALL: Great. Assuming you would were very concerned about it, we wanted to make sure that all judgments we got were documented extensively.

Because we were working with a group and not a single individual, we had the classic problem of how do we aggregate judgments when we have differences. For discrete variables, of course, we had differences among the panels as we did for continuous variables.

We used a number of different aggregation techniques. The point was we wanted to make sure that--

DR. NORTH: Excuse me.

On that point, who is the "we"? Did the experts agree to be aggravated--

MR. CALL: Yes.

DR. NORTH: --aggregated in this fashion?

[Laughter]

MR. CALL: I think they agreed to be aggregated. They did not agree to be aggravated, but they were that, also.

DR. NORTH: Again, is all this documented?

MR. CALL: Yes, it is.

We were concerned that any aggregation technique we used did not mechanically suppress the variance in the uncertainty we got from the experts. In one case, for example, we used the maximum and minimum of the in-points to make sure we were getting the spread, and then we took geometric means of all the interior points.

We also developed optimistic and pessimistic sets of inputs of the expert opinions. In many cases it was simply a matter of taking arithmetic or geometric averages. But we think this meaning is clear: we had a lot of variance; we wanted to make sure that we did not suppress that.

Then we talked with the experts to make sure that both the technique was something they agreed with, and that the final result was what they agreed with. These are just some examples of what we end up with.

This is a new slide that is very similar to the other ones you have. I just want to take you through the variables that we assessed.

The priorities on Calico flow conditions: discrete variable. We went through an assessment process and came up with the probabilities associated with those four flow regimes.

Test results, or the likely functions for the tests, we assessed in the following mode: We defined a state of Calico flow condition--in this case, for example, slow matrix--

and we said if that were the true flow condition, what are the chances the test would tell you either slow matrix or would it tell you fracture matrix, concentrated fracture or distributed fracture?

It is a distribution of false negatives and false positive for the test. We conducted that assessment for all eight testing strategies.

MR. REITER: The way you phrased it was that if this is the true condition more than likely of the tests finding it, suppose you phrase it that if this is the result of the test what is the likelihood of being a true condition?

Is that equivalent or different?

MR. CALL: If you saw the test, what is the likely of that being the true condition?

MR. REITER: Yes.

MR. CALL: That is the Bayzian [ph] inverted probability, which we will be showing you. What the experts have some information about is their judgment that the site is in one of these states.

We want to know: Can the test detect those states and how good is it at detecting those states? There is a fairly standard approach for phrasing that.

MR. REITER: But he phrased it the other way. Would it be different?

MR. CALL: It would a different probability, yes. I will show you those probabilities, in fact.

We did not assess it that way. I can show you computationally what reversing the position of these two events does to the probability.

Now we are saying: If you observe the test what is the likelihood that you are in that state? This is a much more reliable way of assessing that relationship.

MR. REITER: Which is the way you are doing it?

MR. CALL: The way we did it in our analysis is we defined the flow condition and we said: If that is the true flow condition, what is the likelihood that the test tells you that.

MR. REITER: That is better than the other way around?

MR. CALL: Yes.

I will show you what the probabilities are for the other way around.

Release from the source was assessed as depending on Calico flow conditions. Our experts told us that if the need something about the Calico flow conditions they would be better able to estimate the distribution of releases from the source.

We defined source, in this case, in an unusual way. We said that "source" is the amount of radionuclide volume and distribution available to the Calico, for transport through the Calico.

Again, Ernie is going to give you a more detailed description of that. I don't want to pre-empt his description.

Releases from the Calico, or transport through the Calico, were assessed conditioned on the Calico flow condition and the source. From the previous slide you will remember we had a continuous variable for source releases.

We discretized that variable into three different states: low, medium and high. The objective of that discretization process was to ensure that we were representing the high end of the distribution; i.e., the part of the distribution that is going to result in higher releases.

These are just icons to show you a standard cumulative probability distribution.

In our discretization process we discretized this in such a way that we preserved this area of the curve, up in the 80th to the 100th percentile. We did that, in most cases, because those were the areas of the curve that were the most significant.

In transport through the Calico, we defined a flow condition; we had to define the source term, now, from our previous assessment; and we assessed the cumulative probability distribution on transport through the Calico to the saturated zone.

The saturated zone we treated as an independent variable. We said it did not depend on the flow conditions in the Calico; it did not depend on the source term; and we treated as an independent multiplier on Calico releases.

Then we went through the same process with the saturated zone: we discretized the saturated zone attempting to preserve more of this part of the distribution; we were less concerned about this part.

Finally for the probablistic assessments, we assessed the performance impacts of testing conditioned on the Calico flow conditions; and we defined it for each test strategy since each test strategy involves different amounts of drifting, different types of penetration; and, therefore, the idea being that it poses different risks for impacts on the transport properties of the Calico.

To describe for you the way we did our probablistic assessment I want to give you one slide for how we did our value assessment. Again, if you remember the simple example, we had the profits and costs of testing.

In this case, it is a little more complicated. The way we set up the value assessment was in this four by four matrix. We said that if these are the predicted releases or these are the releases you are acting as if are the site releases, these are the actual releases.

The idea here, for example, is if your predictive release is that R is two orders of magnitude below the Environmental Protection Agency standard and you build your repository acting as if that is true, and it turns out that is actually true, your value on that outcome is zero.

You would do the same thing under your current assumption as you would do if that were actually the case. For that reason, we get zeroes on this diagonal and increasing costs or increasing costs on the off-diagonals.

We went through a one-day assessment process with a management panel where we attempted to--and, in fact did--assess values for all these combinations of predicted releases and actual releases.

One of the very important points about this is that the release intervals, themselves, imply that the decisions and events are sensitive to changes from one interval to another. That means if you divided all these by a couple of orders of magnitude--let's say we tried to assess what your preference might be if you acted as if releases were 10 to the minus 8 and the corresponding actual release was 10 to the minus 7--is there a value associated with that?

We found in our discussions: Probably not; that, in fact, what we had to do was push the release intervals down into this range, as you can see, and we were able to do the assessment, and we got some fairly interesting numbers from that exercise.

The point has to be, still, that the managers who were expressing their preferences for these had to believe, and had to be able to express, those numbers based on comparisons of what they would do under this predicted versus this actual.

One of the interesting assessments we did, perhaps one of the insights from this, was that if you predicted that releases were greater than one and releases were actually two orders of magnitude below the standard, there was a very high value here.

In some of the discussion that came out in the assessment process some people felt that, in fact, it may be higher value than its corollary, which is that you predict it is very low and it turns out to be very high.

The reason for this is that in this case if you do a test and you get a result and it says releases are going to be very high, it is more likely there will be some type of precipitous decision to, perhaps, abandon the repository or site it someplace else; and, in fact, your opportunity value was very high: you gave up a very good site.

On the other hand, if you predict releases are two orders of magnitude below the standard and it turns out they are very high, it is not likely that based on that one piece of information you simply build a repository according to that predictive level along and you never collect another piece of information again.

This is some of the discussion that occurred in the course of doing this assessment.

MR. REITER: There are various kinds of costs here, right? There are financial costs and there are public health and safety costs.

MR. CALL: Right.

MR. REITER: Who determined you were going to reduce it to a common number?

MR. CALL: We started to exercise our option with the influence diagram, which for some reason is not in the slides but I would be happy to provide it to you and it is part of our documentation.

In the same way we did an influence diagram for the flow conditions, we did it for this value assessment where we tried to identify all of the variables, the uncertainties and the decisions that result in impacts, financial or otherwise, occurring.

The way we assessed it ultimately was we assessed it based on a U-tile [ph] and then we converted that U-tile scale to dollars.

MR. REITER: Who did that?

MR. CALL: We did.

We assessed the management panel's values for all the numbers in this table. Then we went through a day-long exercise and we converted those to dollar equivalents based on a couple more assessments on the n points. This is a lot of mechanics.

We discussed those results with the managers. Again, for a one-day exercise in doing this I think we had a fair amount of agreement.

We had three sets of numbers we used in sensitivity analysis. Just to give an example, the base case set of numbers had $25 billion in these to corners, I believe: something on that order. We did a sensitivity analysis running all the way up to $250 billion, $2.5 trillion.

We would like to have spent longer, frankly, in doing this exercise, but that was the amount of time we had. As a result, we did a lot of sensitivity analysis to assure ourselves that, in fact, the results were very robust with respect to these assessments.

MR. REITER: What was the assumed cost of that? You must have assumed that public health was worth a certain amount of money in this.

MR. CALL: Yes.

MR. REITER: You determined that?

MR. CALL: Right. We discussed that. There have been numbers published in the literature for $300,000 to $10 million. There is a conversion factor published in one of the Department of Energy reports that allows you to take the curies released and convert those into cancer cases over a 10,000-year period.

We used a lot of information like that directly from the published literature, most of which were Department of Energy reports.

That is the end of my presentation. Thank you.

DR. BLANCHARD: Don, do you want to take a break now? This is a logical point.

CHAIRMAN DEERE: Yes. Thank you. That would be fine.

DR. BLANCHARD: A couple of points.

Hollis, you used some viewgraphs we were not aware you were going to use. Since we want to make sure everyone here has a copy of each viewgraph that was used here, we would like to ask you to pull those out so we can get them reproduced. We will distribute them tomorrow morning.

Ernie, would you and Jack Robertson, during the break, discuss the point about fracture hydraulic conductivity that Roy raised so you can cover that very early on in your opening remarks?

MR. HARDIN: Sure.

CHAIRMAN DEERE: Thank you, Hollis. Based on the stock market's performance, you may be very lucky you are not involved in stocks.

[Laughter]

CHAIRMAN DEERE: We will take a break now.

[A brief recess was taken.]

CHAIRMAN DEERE: May we reconvene, please.

MR. DOBSON: Our next speaker is Ernie Hardin from Science Applications who is going to talk about the geotechnical input.

CHAIRMAN DEERE: Very good.

Summary of Geotechnical Inputs

MR. HARDIN: My name is Ernie Hardin. My goal here this afternoon is to give you a compact summary of the geotechnical inputs to this decision analysis.

This is the structure of the presentation. You have seen this before. The piece I am about to give you is this one. You have already been briefed on these pieces and the manner of expert assessment.

This is an outline of the presentation I am going to give you. First I am going to go through a couple of simple conceptual models: what I call the linear model for combining various inputs and estimating total system performance. Then I will describe the simple performance measure we assess directly in the study, and give you definitions for these flow regimes.

Then I will go through the six categories of the technical inputs: namely, probabilities on those flow regimes; something called test likelihood functions; the assessments on the available inventory of radionuclides from the waste form to the Calico Hills; then transport through the Calico Hills unit; saturated zone transport; and waste isolation impacts from testing.

This linear model I just referred to is really a way, as I said, to combine the assessed inputs. The boxes on this diagram, for those of you who may not have a hard copy, are: available inventory; transport through the Calico Hills; transport through the saturated zone to the accessible environment; and impacts from characterization.

The point of this figure is really two-fold. One is to point out that this available inventory item includes what we view to be the contributions to releases from waste form, waste package, other engineered barriers and the host rock. Of course, we dealt with this at a relatively high level.

The other point is to show you that the available inventory or transport through the Calico Hills--or another way of saying this would be releases from the Calico Hills unit--were assessed as cumulative distributions on the release measure R, which I will define shortly, whereas the other inputs to this part of the model were assessed as factors modifying releases from the Calico Hills unit.

Therefore, impacts from characterization was assessed as a cumulative distribution on a factor modifying releases here; and the influence of the saturated zone likewise a factor that reduces releases to the accessible environment.

This is our old friend. The performance measure affects directly by the technical panel in this study is just the sum of the release ratios, which is defined in the Appendix to 40 CFR 191.

The mixture of radionuclides of course is important when you are considering various transport phenomena. We began by taking a volume fraction of the waste, considering its components, recognizing that does not go quite far enough, and recognizing the possibility for a selective release of some of the more mobile species: for example, tecniseum [ph] 99.

So for purposes of assessments in this study we enriched the mixture of radionuclides assumed to issue from the engineered barriers by a component of the mobile species: a several-thousand-fold increase in the constitution of the tecniseum 99 inventory.

That throws a bit of emphasis on getting the hydrology right, and also forces you to consider retardation processes as they apply to those mobile species.

This slide shows some cartoons that represent the flow regimes used in this model. I will start with the concentrated fracture flow regime.

The idea here is that more than 1,000 cubic meters of year of flow is moving down through fracture pathways which penetrate at least 90 percent of the thickness of the Calico Hills unit; and that the planned area extent of these fracture pathways would be on the order of 5 percent or less of the total repository area.

The origin of the 1,000 cubic meters per year flow rate is a published performance assessment that relies on congruent leaching assumptions and shows that unless you have water in contact with the waste in amounts comparable to this number you do not develop releases that approach the threshold level used in the Environmental Protection Agency standard.

The distributed fracture flow regime definition is similar to the one I just gave you. Here we are saying 1,000 cubic meters per year or more of flow moves through distributive fracture pathways through 90 percent or more of the Calico Hills unit thickness; and that the area extent of these pathways would be on the order of 50 percent of the total planned area of the repository.

For the fast matrix flow regime the idea is that the same threshold flow rate--1,000 cubic meters per year--is now going through matrix pathways along 90 percent or more of the unit thickness; and that the average velocity of the water in that pathway is 10 centimeters per year.

As I pointed out to someone over the break, with a matrix saturated conductivity on the order of 10^{-7} centimeters per second you can get to that condition.

The definition for the slow matrix flow regime then encompasses other things. We think that slow matrix flow probably represents existing conditions. Moreover it might include flow in one of these other three modes, but in an amount that does not meet the numeric criteria in the definitions.

DR. BLANCHARD: Excuse me, Ernie.

Before you move on I think you said that the velocity in your fast matrix flow was for 10 centimeters per year, and that shows?

MR. HARDIN: 1,000.

DR. BLANCHARD: So that is a mistake.

MR. HARDIN: I apologize.

MR. ROY WILLIAMS: What is the velocity you guessed at under what you call concentrated fracture?

MR. HARDIN: There was no velocity criterion used to define that regime. The implication was that the velocity would be quite high, and that is manifested in assessments of performance of the Calico Hills unit in that retardation would be rather more expected for certain species in the concentrated fracture flow regime.

MR. ROY WILLIAMS: I don't see how you could do it without a velocity.

MR. HARDIN: That gets back to something I could say in general about the study. We approached some of these very complex issues at a high level: for example, the inventory of radionuclides, the mixture of the different species in that inventory.

MR. ROY WILLIAMS: What do you mean?

MR. HARDIN: Because to disaggregate it further would put the problem beyond our grasp given schedule and other constraints on the study.

The idea here is to develop a basis for a decision. This is not a PA; and we recognize the approximations that have been made.

This diagram represents the probablistic dependencies among the quantities that were assessed by the technical panel. I guess I could go through this again.

Hollis has shown you that the different flow regimes affect the likelihood for different test results, different releases to the Calico and through the Calico; and this stands alone.

Transport through the saturated zone was not assessed, dependent upon different flow conditions, but waste isolation impacts from testing was.

DR. DOMENICO: What did you do about the release rate from the available inventory of tecniseum? What sort of release rate did you assume for that?

MR. HARDIN: We assumed a proportion of the stream of nuclides issuing from the barriers would be tecniseum. We increased that proportion because we felt it was likely to be higher than the volume fraction would imply; and that proportion would take you to the available inventory of tecniseum in the waste.

Now, if you took all the tecniseum out of the waste--you may know this--and calculated the Environmental Protection Agency ratio from that release, your performance measure value would be on the order of 2, a little over 2.

DR. DOMENICO: You have an inventory of approximately over 900,000 curies of tecniseum, if I remember, in a typical inventory.

How long did it take to deplete that inventory in this system?

MR. HARDIN: The answer to the question is: I cannot give you a number in years, but I can say that we acknowledged that it would be depleted under theoretically high release scenarios; and we did explicitly assess the probability for releases under these high release scenarios, such as distributed fracture flow which you will see in a moment.

The implication there is that in many ways packages are involved with the ground water flow system, and that releases are high. Under the theoretically highest levels of releases associated with that flow regime you would deplete the tecniseum, so the mixture would change and it has implications for the performance of the Calico Hills unit.

So there is a coupling, but in terms of giving you a number in years we have seen some recent work for the Pace 90 exercise that showed that it could happen in 5,000 or 6,000 years. Then, of course, there are studies assuming congruent leaching where it would never happen or it would take much,

much longer than that.

I am going to go through each of these six bubbles here starting with flow conditions, then moving on to test results, then available inventory, Calico Hills transport, saturated zone, and waste isolation impacts.

There is an extra slide somewhere in the handouts I am going to skip. It is a map of the vitric and zeolitic facies.

With regard to the probabilities for flow regimes, this slide represents the approach I am going to take to describing the geotechnical inputs and the basis for them. It is rather qualitative.

I am going to try to show you representative results, and I will tell you what the some of the major influences were from the influence diagrams used during the assessment process. Moreover, I will digress a bit on the details: the things that are subordinate to these major influences, but were not represented explicitly on the influence diagrams, and what these things mean qualitatively for the assessments generated.

There are many people in this room who know that I had 50 viewgraphs a few weeks ago that got into this subject in a bit more detail, but was discouraged from using them all.

[Laughter]

MR. HARDIN: For the probabilities of the flow regimes, one of the major uncertainties here is the likelihood for return to pluvial conditions. If precipitation doubled, it is considered quite possible that underground hydrologic conditions at Yucca Mountain would be comparable to those at Rainier Mesa today.

However, the condition rests squarely on the likelihood of this right here.

With regard to the barriers that would constitute natural flux-concentrating mechanisms, the Topopaw Spring/ Calico Hills contact was identified as a likely barrier, perhaps the most likely barrier in the between the repository and the water table; but there is much that is not known about the geometry of the facies transitions within the Calico Hills.

These may act to divert flux as well.

In addition, when you are considering the likelihood for concentrated flow in the Calico Hills you have to consider what is happening above the Calico Hills to concentrate flow. So the distribution of flux produced by overlying rock units and hydrologic processes was brought into the assessments, as well.

For matrix hydraulic processes--most of the bullets on here are not as general as this--clearly safety is distribution. What we think we know now about the distribution of vitric/zeolitic facies at the site and what the properties are was taken into consideration in terms of identifying which of these flow regimes are most likely.

The fracture hydraulic properties were very important. We believe there are significant differences from the vitric to the zeolitic facies in terms of how prevalent fractures may be and the nature of those fractures.

One significant uncertainty identified by the panel was mineral coatings on fracture walls which would control, or might control, the interaction of matrix and fractures, and the likelihood of fracture flow scenarios.

A question was raised in the last presentation about fracture hydraulic properties and what we mean. I think it might be well to take a minute on the record to try to answer that question.

There are different interpretations of what fracture hydraulic properties could mean. We do not mean to imply here that the specific properties of any single fracture of minor extent would be important for characterizing flow conditions in the Calico Hills.

The idea here is to deal at a high level with the fracture geometry issues--aperture, planarity, persistence, extent and so on--as well as the constitutive issues like coatings on the fracture walls and so on. That is what we have done in this study.

CHAIRMAN DEERE: Does your fault enter into the flux concentrating mechanism? How show one picture on the graphs of the Ghost Dance.

MR. HARDIN: Right.

CHAIRMAN DEERE: Is that the way you take into account an isolated fault?

MR. HARDIN: We treated the stratographic barrier as the mechanism that concentrates the flux, and then the fault as the conduit for taking that, or a portion of it, down to the water table.

MR. ALLEN: I am not quite clear whether or not you are speaking of the fault here in its present state or what it might look like the day after a big earthquake on that particular fault.

Was this considered at all?

MR. HARDIN: We discussed fault rupture in two places in this study: for Calico Hills' performance and waste isolation impacts. In each case we were not able to come up with a really compelling argument that this would change the hydrologic regime at the site.

I think we are now down to the level of opinions of individual experts. I guess I will try to sum that up by saying that we did not deal with that explicitly or quantitatively in the study.

In my judgment in preparing these viewgraphs it was not a real big influence on the results that we report.

MR. ALLEN: I have a hard time believing that the fault zone is going to have the same flow properties right now as it would, say, after a couple thousand years just after a big earthquake and happen to have 10 centimeters of sluice on that fault.

MR. DOBSON: I would add one clarification.

I think what Ernie said is true. Each of these flow cases we were trying to characterize for the predominant condition over 10,000 years. So when we estimated a number for slow matrix or concentrated fracture flow, that number reflected our judgment about the likelihood of this changing as a result of climate change or as a result of, for example, tectonic activity.

A fundamental change in properties as a result of faulting was a part of the consideration. But I think Ernie's statement is also true.

As I recall, I don't think anybody has any large part of their rationale for estimated the probability for concentrated fracture flow that the properties of the fault might change.

MR. ALLEN: This falls into Warner's classification of a very unlikely event that still could have profound consequences.

MR. DOBSON: It depends on what the consequences are. It is something we need to address, I agree.

DR. NORTH: I think the question is: Does it lead to a much worse flow regime than the worst case you already have on there, which I guess is the distributed fracture?

It might give you a situation, for example, where part way through the 10,000-year period you went from one of the other three cases into the distributed fracture case.

MR. HARDIN: There are so many things I could say about this discussion.

We are dealing with processes that are assumed to be quasi-uniform over 10,000 years. We are worried about 10,000-year cumulative release. If an earthquake did happen at 9,500 years, it might change the process; but it also might not change the cumulative release.

DR. NORTH: But do you need that assumption, or is that just for analytical convenience? Can you think of a scenario, such as Clarence has just given you, where things get a lot worse over the 10,000-year period because something changed: you have some kind of transient phenomenon going on there?

MR. DOBSON: With regard to flow conditions, these flow regimes, there were a lot--I would say 12 or 15 different--of kinds of disturbed conditions we considered. But of those we extracted what we thought was a representative set: an independent, exhaustive kind of set, defensible; and also a set that we felt could be the basis for a reasonable decision to characterize the Calico Hills.

A basaltic dike may exist down there, but in our view it may not be a reasonable view to plan an extensive underground exploration program to go and look for it. Mineral resources is another example.

Sure, mineral resources drives part of the human intrusion set of scenarios; but again is underground excavation the reasonable way to assess the resource potential?

There were trade-offs implemented and we hope this set of four flow regimes captures the salient aspects of this problem.

The only other thing I can say here is that the relatively high likelihood of the concentrated flow regime or the distributed fracture flow regime does not imply that there is a .1 or a .2 chance that the site does not meet the Environmental Protection Agency standard.

MR. REITER: Which is worse for meeting that?

MR. HARDIN: We developed distribution functions for the releases that would be associated with each flow regime. I don't have those to show you right now.

I believe that the distributive fracture regime does result in the highest releases given the other inputs of the study.

The next category of technical inputs I would like to talk about is the test likelihood functions. Hollis has already given a pretty good accounting of what we asked the panelists when we assessed this group of numbers the idea being given that some flow regime is the correct result. Given the results from some strategy, what is the probability you will get the right result?

We asked the experts to project themselves into the future and give us a probability that they would interpret the correct or incorrect conclusion based on what they believed to the important results would be from each test strategy. So it is based on their scientific judgment.

One of the major factors that went into the test likelihood function assessments is, again, uncertainty of future changes in flux. The climate change seems to be a very important variable here.

The test strategy location was particularly important for identifying flux-concentrating mechanisms should they exist. We observed that the saturated matrix conductivity for zeolitic Calico Hills tuft is most likely too low for a fast matrix flow as we have defined it.

With regard to the fracture properties, again, the extent of underground exploration of targeted faults and features, and the extent of exposure of fracture mineralization was considered especially important for correctly identifying fracture flow conditions.

DR. BLANCHARD: Roy, can I ask you a question?

Before this presentation is finished we would like to make sure that we have covered the considerations in your earlier question about fracture hydraulic properties.

MR. ROY WILLIAMS: I concluded that what they are talking about is fracture flow treated as an equivalent course medium. They misuse the term by trying to apply it to a single aperture fault draining an overlying saturated reservoir.

That is what is going on.

DR. BLANCHARD: Okay.

MR. HARDIN: The same term being applied somewhat differently.

This graphic presents part of the results of the results from the test likelihood function assessments. We have here the probabilities for correctly identifying different flow regimes for each of the strategies.

On this axis we have plotted probability from .3 up to .9; and down here we have the different strategies. Two and Five are grouped together because of their similarity.

There are several general observations I think you can make from this result. The labels are very legible on this figure. The top curve is for a slow matrix flow.

These results say that if the actual site condition is the slow matrix flow regime that is relatively easy to correctly identify with respect to some of the other flow conditions. Fast matrix flow may be relatively hard to identify because it may be restricted in space, and it may be the consequence of a future change in net infiltration.

Another general observation is that there are some large differences in the assessed probability of correct results among the strategies.

Strategy Six at the far right is the all-surface-based testing and drilling strategy; and on the far left you have extensive exploration inside the block.

To summarize on the test likelihood function results I will say several things. The underground excavation strategies are consistently more likely to produce correct results than surface-based testing.

The Pryle Pass outcrop test facility that you heard about a little earlier does not appear to contribute much to the likelihood of correct results. Also you can say that a single small underground facility, a limited facility, in the south or the southeast has relatively low likelihood of producing correct results whether it is located inside or outside the block.

An extensive facility southeast of the block--here we are talking about outside the block--is comparable to a small facility inside the northeast part of the block. Finally, Strategies Two and Five are stated here to have significantly higher likelihood of producing correct results.

The next category of technical inputs I will talk about is the available inventory for Calico Hills transport.

Here the experts assessed the aqueous releases available at the top of the Calico Hills as a quasi-uniform process over 10,000 years for each of the flow regimes.

With this set of assessments some of the major factors considered include the total flux associated with the flow regimes; and the distribution of flux produced by overlying units and processes above the repository.

Again, we are thinking of water in contact with the waste package. How many waste packages fail, and what happens to them after they fail? How much of the waste form is broken down and mobilized?

This is reflected in a fairly high degree of uncertainty in the assessments. Your CDF gets a little bit flatter when you have uncertainty as to a process like release of nuclides from a failed waste package.

The extent of contaminated water flow through engineered materials in or near the repository was an important concern, as was the existence of perch water below the repository: for example, on top of the Calico Hills; and the impact that may have on the inventory of nuclides available for transport.

The next graphic is the result of the available inventory assessments for two of the flow regimes: concentrated fracture flow, and distributed fracture flow. The idea here is to show you the kinds of functions generated by the assessment process.

This is not the CDF exactly as assessed by the technical panel. It is processed one step. I am sure Hollis could describe this better; but it represents where the mass was placed on these functions.

DR. NORTH: What would this have been if you had not made the assumption of the enrichment on the tecniseum?

MR. HARDIN: Different?

DR. NORTH: Yes. Is that the reason you have a 2 there on the release measure?

MR. HARDIN: I would suspect it would be very much the same from about here on because tecniseum, given the assumptions we have made, is gone at about R=2.

DR. NORTH: Do I read that as we have about a 1 percent chance of a release level of 10, or does the graph go somewhere else?

MR. HARDIN: Yes, under either of these two fracture flow regimes.

DR. NORTH: Okay.

MR. HARDIN: I have another similar curve I will show you in a minute.

The next category of assessments I would like to talk about are the releases from the Calico Hills unit. Here we are asking: Given a inventory rate on nuclides transported to the unit represented by a value for R, what inventory is transmitted to the water table in 10,000 years at another value for R bounded by the available inventory?

In some of the major factors considered here in addition to the basic distribution, as we know it, of mineralogy and hydraulic properties--and, as I say, with facies distribution--we observed that fault zones may have tight intervals where matrix flow occurs, perhaps over a very limited pathway, but that this could have a major impact on the transport of radionuclides by certain mechanisms.

We also identified that flow paths would be lengthened, extended, by lateral diversion and by heterogeneous distribution of hydraulic matrix properties in the Calico Hills unit; and the variation of Calico Hills' thickness has a lot to do with the transport of radionuclides through it. So there is some dependency on where the nuclides are available on top of the Calico and the thickness of the Calico.

Finally, matrix diffusion effects were quite important in this set of assessments. This is reflected in uncertainty on the retardation of the mobile species.

We are talking about at the relatively low levels of releases where we have assumed that such species as tecniseum 99 constitute a very healthy fraction of the released inventory. By matrix diffusion here we are talking about interaction between flow in the fractures and the adjacent matrix because once tecniseum 99 atom diffuses into the matrix its progress is substantially impeded.

CHAIRMAN DEERE: Excuse me. Could we go back to that?

MR. HARDIN: You bet.

CHAIRMAN DEERE: I have a question on your fracture matrix on the right hand side where you considered fault zones may have tight zones.

What form of tight zones were considered?

MR. HARDIN: The terms that we were using were gouge and bet-you.

CHAIRMAN DEERE: How are they distributed? In other-- if you have one gouge zone parallel to one side so horizontal flow would be retarded at that point it could not get into the zone, or did you have it in a vertical direction that when it gets into the zone it goes down and finds slightly different characteristics, but probably because it goes through another bed.

MR. HARDIN: Yes, that was our interpretation: that if you had flow in the vertical direction headed for the water table and it has to detour through even a couple of meters of matrix or porous media type materials.

CHAIRMAN DEERE: I think both will happen: you are going to have the retardation in getting into the fault zone in many, many cases; and then, as it goes down through the different beds, probably as you say there will be tight zones and open zones.

MR. HARDIN: Yes.

MR. ROY WILLIAMS: What did you do with that information? I don't quite see that.

MR. HARDIN: What happens when you implement an idea like this in this process at the relatively low probability level--say 20 percent or at the 50th percentile--is you infuse into your judgments additional confidence--a factor of 2, a factor of 5--that such processes do have a significant impact on transport through the Calico.

MR. ROY WILLIAMS: How do you handle questions like: How many do you put in: how many gouge zones, tight zones?

MR. HARDIN: We are back to the level, at least in this study, of individual assessment and the basis for it. That is something we recognize as an important responsibility: a documentation job for us.

CHAIRMAN DEERE: When you are talking here about a high level, it means a low level of information.

[Laughter]

CHAIRMAN DEERE: In that right? Instead of getting into the details, you are calling them now a low level.

It seemed to me that might be high level.

[Laughter]

MR. DOBSON: We did not address at the level where we consider explicitly a different level of gouge zones and fault zones.

The point of that on the viewgraph is that in the view of the people doing these assessments it does not take very many gouge zones to add a significant amount of performance. In other words, in order to get fracture performance you need to have pretty nearly continuous fracture flow through it. If it stops in several places, you are going to get a good contribution to performance even though there are fractures.

It is qualitatively assessed such that when you say at a low probability, I actually think it still might perform okay even with fractures because it is not going to be continuous fracture flow; but at a higher probability where I am more confident of my assessment I am going to be more conservative and assume that the flow is continuous.

So it just a way, as Hollis described earlier, of characterizing your uncertainty in how good you feel it is going to perform. At some confidence level you say "I am going to be conservative: it is not going to do very well; but there is some chance it is going to do a lot better than I am saying."

That is why you do the assessments at various confidence levels.

MR. HARDIN: There are so many uncertainties in this problem we find, in general, the experts have said that at the

99th percentile the consequences are relatively dire; and at the 10th percentile there are other things that might come into play that improve performance: flatten the curve.

In some parts of the curve we are pessimistic; in some we are optimistic.

The next slide presents another category of geotechnical inputs to the study. Here we are assessing the release reduction factor associated with transport through the saturated zone from the repository to the accessible environment.

Some of these are kind of subtle, but some of the major factors that came into these assessments are, first, the implications of a water table rise. Of course, that could be associated with an increase in velocity or average velocity; but it would also be associated with an increase in aquifer transmissivity.

Another observation was that effective porosity values used in the Site Characterization Plan are rather conservative. In fact, in our view they don't permit very much fracture matrix interaction at all.

So instead of using values on the order of half a percent, for some of our calculations we looked at values on the order of 10 percent.

The experts also made clear that their experience with saturated zone transport made them feel much more comfortable about this set of assessments than the relatively novel affair of unsaturated zone transport.

Finally, with regard to the matrix diffusion process that would tend to retard nuclides along saturated zone pathways, the 5-kilometer distance to the boundary of the accessible environment was regarded as long enough to present opportunity for matrix diffusion.

Therefore, in this argument if you had a conduit for rapid flow for, say, a flat zone or a fracture of some type the persistence of that feature in the direction of the potential metric gradient would be such that we expect that somewhere along there flow will slow down enough to permit matrix diffusion.

This next figure is an interesting summary of the cumulative distributions developed from the technical inputs I have talked about so far. Here we have cumulative probability applied from .9 to 1, so this is part of the CDF, plotted against the release measure R from zero to 10.

We have plotted, here, the distributions, based on our assessments, aggregated over all flow regimes. We have combined the different distributions for the different flow regimes on, say, available inventory.

This CDF represents the release curve, if you will, for the available inventory part of the model. This one represents that plus the Calico Hills transport. And this one here represents the first two processes plus the saturated zone transport.

This shows you that the contribution of the Calico Hills--which in some way is represented by the difference between these curves--is comparable to, perhaps greater than, the contribution of the saturated zone.

However, there is some granularity in these curves that were compiled from discrete inputs. A more detailed and comprehensive study would generate smoother curves and permit you to say more about the differences between the curves.

MR. REITER: These curves represent the mean of the experts?

MR. HARDIN: Yes. If you take a room full of six or eight experts and ask them a question you will get back six or eight numbers. What you do with those depends, in this study, on which set of assessments you are working with.

We looked at arithmetic averaging, log averaging and some other schemes as well.

MR. REITER: What do you use here?

MR. HARDIN: These are based on log averages.

MR. REITER: Suppose you used arithmetic averages?

MR. HARDIN: Then chances are two or three out of your six or eight experts' numbers would dominate.

MR. REITER: Do you have an example?

MR. HARDIN: I don't have an example of that to show you. That comes under the category of sensitivity studies: things you are obligated to do in order to reinforce conclusions like this.

DR. BLANCHARD: I certainly believe so. We tried to structure In other kinds of assessments the whole ball of wax lie in whether to use arithmetic or log averaging. That is where 90 percent of the influence is.

MR. HARDIN: Yes.

DR. BLANCHARD: It overwhelms everything else.

MR. CALL: As I described in my presentation, we used a number of techniques depending on the amount of variation there was among the experts. So in cases where we had a lot of variation we used techniques to attempt to preserve some of that, particularly the tail.

MR. REITER: What you pick here may cause results to go one way or the other. You should be able to present both results and argue why you did one of the other.

MR. CALL: Right.

MR. HARDIN: I guess we agree.

MR. REITER: It is a very non-trivial thing.

DR. NORTH: With this slide you ought to reflect on the implications of the curve where you have all three terms: the source, the Calico Hills, and the saturated zone.

I think I need a microscope on my slide to see exactly how much that deviates from R=zero and the probability approaching 1. That has very strong implications if the cumulative on release is really that low.

It would be interesting to examine some of the scenarios in detail and figure out what the leading terms are in that; but I am assuming you are getting a considerable degree of independent attenuation in the saturated zone which is why the deviations I can read by eye for one term alone--the source--and then the source plus Calico Hills become smaller than I can easily discern when we put all three of them together.

If, in fact, the conclusion is this strong and we believe it, which are great big ifs, it makes me a lot less worried about arithmetic versus geometric averaging of the experts.

MR. HARDIN: As I said before, this was not the PA; and we don't take too much credit for generating this kind of result.

DR. NORTH: I think the interesting question here is: How sure are we that we really have dealt with the extreme scenarios in all three stages? And is there anything that has been missed that would give us a different result?

MR. CALL: We did spend a lot of time in our discussions and the assessments to attempt to characterize the region from the 95th and the 99th percentiles. So we are very sensitive to that.

That was not something we were trying to overlook. In fact, we pushed our experts in several cases to four 9s; and we realized we were well beyond their capability of giving us reliable numbers.

We could easily depart from the structure we developed for this analysis and do something in which we characterize some things that the events out at the 99th percentile. I would have to say, based on the analysis we have done so far, I am skeptical of how valuable that would be.

DR. NORTH: Maybe your biggest job is to communicate to others the insights you developed in this exercise; but what you have to do, I think, is persuade the rest of us that have not lived inside of this exercise for months that you really have thought of everything that could be important.

MR. HARDIN: Now we jump into an important category of technical inputs: the waste isolation impacts. Here, again, we are assessing a release impact factor for each strategy in each flow regime: a factor that modifies releases from the Calico Hills unit.

That factor may be greater than 1 indicating releases have increased, or even less than 1 indicating performance is improved.

A note here: In this process we used the level of impacts of analysis in Site Characterization Plan Section 843 as a kind of target for the level of detail for which we strived.

Some of the significant observations in this process were: If you are going to have significant transport along back-filled or sealed openings it requires water flux along those openings.

Here we are talking about: What is the impact on performance on transport of radionuclides through the unit of these openings that are constructed in the Calico Hills and back-filled with sealing measures, and so on?

If you are going to have significant transport, you need water flux. For the greatest potential flux through the openings you need a natural-concentrating mechanism to collect the water so it can somehow infuse into the openings and flow there.

A kind of corollary observation is that a lost bore hole--a bore hole of which you lose control during the drilling process or subsequently and are not able to seal--may be the largest impact you can commit at the site. This is reflected in some of the results you are about to see.

Here are some more incidental observations. The plan area and the size of openings associated with any of these strategies are, indeed, small compared to corresponding dimensions of the site and the Calico Hills unit.

Here is another slide with more factors considered in the waste isolation impact assessments. This is an interesting point.

The diversion of ground water from natural pathways into engineered materials may actually improve performance; particularly if the water would have flowed along a diversion horizon and then into a fault zone, and now it flows into a back-filled shaft.

We talked a long time about rock mass excavation damage and the water that would be used in construction. The conclusion was that these effect, while they are real effects, are probably restricted to the immediate vicinity of the openings; rock mass damage would tend to be limited in non-welded tuft; and water that was lost during the construction process would certainly diffuse in the non-welded tuft and, thereby, remain near the openings.

Ventilation of tufts will remove large amounts of water from the wall rock. Finally, flows of materials imported during construction and testing are also likely to remain near the underground openings.

So these kinds of arguments tend to restrict the spatial extent of the impact.

This next slide represents one of the kinds of quantitative arguments used in developing numbers for waste isolation impacts. Here we are looking at Strategy Two or Five, and we are concerned with what happens when you have interference between these back-filled openings and a system where flux is being naturally concentrated.

The idea here is to develop a simple measure for a multiplier on releases. So here we have a ratio of the up-dip drained area to the total repository area, which is on the order of 1 percent, the idea being that water is moving down this horizon parallel to dip. When we get to the shaft a certain quantity of flow of a proportion of that water is diverted into the shaft or even into a ramp, and it flows down the back-fill until it gets to the bottom where it drains back into the formation.

What does all this mean in terms of reduced performance?

We have a travel time ratio which is based on flow path length, which is approximated by thicknesses, and then saturated hydraulic conductivities.

The idea is if you take 1 plus the area ratio times this travel time ratio, you get a number on the order of 1.05. The technical panel felt this was certainly approximate and definitely a conservative estimate for a multiplier on releases.

It does not take into account various effects, such as where the water would have gone if it did not go down the shaft; it assumes saturated flow where unsaturated flow may actually occur; and there are other effects. You could describe a litany of effects that are not described by this particular approach; but we feel this is a conservative measure.

These curves are the cumulative distributions for aqueous releases from the total system with and without impact from Strategy Number Two, which represents the maximum impact among the strategies we considered.

Here we have a cumulative probability plotted from .99 to 1 and the release measure R applied from zero to 1. Here we are seeing that the difference between these curves, based on our inputs, is small.

CHAIRMAN DEERE: What is this? Say it again.

MR. HARDIN: This is merely a graphic representation of the kinds of arguments I have developed in words over the last ten minutes. The idea here is that the different, because of the impact, is small.

These two curves almost overlay, based on our inputs.

This is kind of a summary of the results from the waste isolation impacts assessments. Here we have a comparison of the strategies. We have taken the release curve, the distribution you just saw without impacts, and calculated an expectation. The corresponding value of R is 1.5 times 10_{-4}.

We have also calculated expected releases for each of the strategies in the presence of the impact as assessed; we have subtracted to find the delta because of the strategy:

because of the impact characterization; and we have taken the ratio of delta R to R.

So this is a relatively simplistic way to present some of the waste isolation impacts information. What it shows is that there are almost two orders of magnitude difference in terms of impacts.

I should point out at the beginning that all the impacts represent very small changes to the expected total system releases.

Down here you have the outside Strategies, Seven and Eight. These are outside plus surface-based testing. They do not include long angled dry bore holes drilled from the underground; and the level of impacts calculated here is on the order of .1. I have listed it as less than 1 percent.

When you move to limited inside strategies, you are looking at the effect--in my view: this is my interpretation--of having that shaft, which now may penetrate a natural-flux concentrating mechanism, and the increase in impact is 20- or 30-fold; then the increase in impact in extending from a limited facility to an extensive one is another 5- or 6-fold.

Now we are a talking about the effect of having all that drifting.

The Surface-Based Testing Strategy did not come out at the bottom of this list because there was some concern that some of the bore holes proposed would be difficult to drill and might be lost. That is all I will say about that.

Finally just a couple of simple points.

There appear to be some significant differences among the strategies in terms of the likelihood of producing correct results. Strategies Two or Five generate the likeliest likelihood.

MR. PRICE: That is not inferential statistics that have been applied to give a statistically significant.

MR. HARDIN: No.

MR. PRICE: That is not what you mean.

MR. HARDIN: No. This is qualitative.

Another point is that the aqueous total system releases are expected to be more than 1,000 times less than the threshold level used in the probablistic Environmental Protection Agency standard; and the change in those aqueous total system releases as a result of extensive characterization inside the block is expected to be a small fraction of the total aqueous releases.

That concludes my presentation.

MR. REITER: Where was Two demonstrated? Where is your second bullet demonstrated?

MR. HARDIN: That would be the cumulative distribution on releases, which would move back just two slides.

The threshold level for the Environmental Protection

Agency standard is here. The expectation of releases was calculated to be 1.5 times 10_4. That is on the table, which is the next slide.

MR. REITER: You did the expectations in terms of logarithmic evidence?

MR. HARDIN: Right.

MR. REITER: It is allowed: to talk about expectations in terms of logarithmic evidence?

MR. CALL: It is just the expected value.

MR. HARDIN: Under certain assumptions and conditions.

MR. CALL: Yes.

MR. HARDIN: We recognize here there are some cases in the set of numbers developed by the study where there are orders of magnitude differences among the experts.

MR. REITER: We will be able to see that when you present these results in some sort of printed media?

MR. HARDIN: Yes, sir.

MR. CORDING: So we will be able to see some of what the experts were estimating as probabilities; and in areas where we feel we have some thoughts on those we will be able to check?

MR. CALL: Oh, yes.

MR. HARDIN: That is the goal.

MR. CORDING: That will be very helpful. I think that is important for us, in working with the Board here, to be able to do that.

MR. HARDIN: We are especially concerned with the waste isolation impacts assessments.

MR. REITER: If you did that with the arithmetic average throughout, what would be the results?

MR. HARDIN: Somewhat more pessimistic release predictions.

MR. CALL: We would need to run that and present the results.

MR. REITER: But have you run the total?

MR. CALL: We have on some of them. On some of them I think it is going to distort the expert judgment group in the sense that two or three of the experts will clearly dominate.

MR. REITER: But some people because there was distortion, and other people consider representation?

[Laughter]

MR. CALL: That is true, too.

As I have mentioned, we have used several different algorithms for doing that. We can run them all with arithmetic averages.

However, I also have to say that would not be consistent with what the experts felt was the appropriate way of dealing with each variable.

MR. LANGMUIR: This is a little after the fact, but

I would like to go back to the expert list. I am concerned that, if I am correct, you only have one expert in unsaturated zone hydrology and you have a half an expert in geochemistry.

[Laughter]

MR. LANGMUIR: You have one guy who was supposed to be doing both is what I am saying.

I wonder if that represents really what it should represent in terms of a balanced view of what the problems are likely to be.

MR. HARDIN: That is a good point.

MR. LANGMUIR: Did those folks have additional input from others in the program who were expert in those fields?

MR. HARDIN: Yes.

MR. LANGMUIR: Is there any way to weight it given the distribution of experts you have? What did you do about that?

MR. CALL: They did have access to others. We did not restrict them from talking to other people. We brought in several retardation experts for days worth of discussions.

MR. PRICE: Did you consider any technique to get concordance or congruence among your experts, such as a delphi technique; or did you have each expert rate their expertise as they were giving a judgment?

MR. CALL: No, we did not.

MR. PRICE: Did you do any statistics of

concordance, like coefficients of concordance?

MR. CALL: Actually in a few cases we did some summary statistics on the amount of variation in the estimates we got; but our basic procedure was to first, of course, define the variable carefully, clearly, and have everyone have the same understanding of that variable.

Then we went through a scoring process where each person scored the variable independently, and then we put up the scores and we went through a lengthy discussion in which particularly the experts that had extreme views would exchange information.

We tried to make sure everyone had the same state of information underlying their judgment. Then we would iterate on the estimates.

The key point was getting the discussion going and making sure everyone had approximately the same understanding; and as often happens in these cases we had situations where people changed their views substantially once they shared some of that information with other experts on the panel.

MR. PRICE: I think it would be valuable to at least know when there was not concordance and the statistical treatment to know that, in fact, on some decision basis, inferential basis, there was not concordance in this case.

MR. CALL: In all cases we asked the panel if the number or the distribution we were using would be the best representation of the group's judgment. So we got that level of verification from the groups in all cases.

I think you are right: It would probably be interesting to look at those and to represent that. I do have some back-up slides if you are interested in seeing some of the representations of some of the variability models.

MR. LANGMUIR: It looks like you have 14 voting. Does the geochemist, who is also a hydrologist, get a half a vote when he has an insight in terms of the chemistry of the system, the transport of nuclides: for example releases? Is it a half a vote?

[Laughter]

MR. CALL: The person that represents the best state of information on that variable tended to have a lot of sway; or, at least, that person would have a lot of input to the discussion.

So, in a sense, the weighting took place in an informal implicit manner.

MR. PRICE: But, on the other side, there is the personality factor there.

MR. CALL: There may be.

CHAIRMAN DEERE: Would there be people voting, for instance, on the low permeability zones in a fault who were not geologists or not mining engineers? Would you have other people voting who were chemists or of some other particular expertise?

MR. CALL: We did.

MR. DOBSON: The make-up of the group was constituted partly because everybody on that group, I think, had a rather broad knowledge of the geosciences; but, in general, yes, with a few exceptions.

We did some of the performance assessments with only part of the group because with only part of it did we feel comfortable making assessments of how they felt the site was going to perform. Some of the engineers, for example, decided they did not want to participate in that.

MR. LANGMUIR: Dave, would it make sense to remind everybody where the list of experts is? It is in your presentation, CHRBA, on page 10.

MR. DOBSON: I would like to add that I agree that we could have gotten a broader range of expertise. We kept it small on purpose; and we are not presenting this as the consensus of everyone who works on the project.

Just to respond to one other asked, I think Hollis mentioned--and I think it was in every case--when we finished with an assessment we went back to the group and we said, "Does this group feel comfortable with the distribution we have put down here?" In all cases, the group was comfortable with the range of values.

Although we have not done all of them--Hollis has done a few and, like he said, he can show you a few--in some of the sensitivity studies, in general, for the performance assessments in particular arithmetic means 10 to be the most conservative.

If you had one guy say the performance was 10_3 of the standard and four others say it was 10_6, when you use an arithmetic mean the 10_3 is the bulk of the mass; but we tried it both ways, depending on the situations; and we intend to do more sensitivity work.

MR. REITER: Dave, there was literally blood on the floor in the Nuclear Regulatory Commission when the panel came up and recommended using a safety goal based on medians, which is approximately equivalent to logarithm of averages.

The ACRS literally screamed bloody murder that it had to be a mean that was equivalent with arithmetic averages.

So I think the use of a logarithmic average, if it is significantly different, you are going to really have to defend.

MR. DOBSON: I agree. As I said in the beginning, that is one of the reasons that our intent is to write this stuff up at such a level of detail that everybody can pore over it and come back and talk to us again.

CHAIRMAN DEERE: But it seems to me there can be differences in the kinds of averaging you do; but just change the make-up of that group by one person you can throw this out the window and don't let the others vote.

MR. REITER: But when they use logarithmic averaging, if you add one person or take away one person it has much less impact. That is the advantage to that.

On the other hand, the question is: If that person predicts severe impacts from the point of view of expectation and cost, you really should count that in.

The way you constitute your expert panel, the way you treat uncertainty is, in many cases, the driving element. I think you have to defend the robustness of your conclusions based on the way other people might do it with other teams and other methodologies which maybe will exempt them.

MR. CALL: That is a very good point.

Obviously there are lots of different ways we could aggregate the expert opinion. Since arithmetic averages seem to be an obvious way, we will do that.

I think we can document to you that, in fact, the model is very robust with respect to the aggregation techniques.

There is one thing I would like to add that we have not said before. That is that although we have verified our estimates with the panel in the case of each individual variable, we have not had the opportunity to go back to the panel and formally present them the results of the model along with the value assessments, and explain to them the significance of all of this.

I think that is a step you really need to be able to do. We have not been able to do it because of the schedule problems. It is conceivable that could result in some of the adjustments of some of the inputs.

MR. ROY WILLIAMS: I would like to zero in on this fault zone gouge for a minute. That is a very good one to hit.

I have walked through probably more fault zones and underground mines than anybody in this room with the possible exception of Dr. Deere.

[Laughter]

MR. ROY WILLIAMS: I would be hard put to offer an opinion on the distribution of fault zone gouge in a hypothetical fault or faults anywhere, especially at Yucca Mountain where nobody has been in an underground opening at all.

I am wondering: How did you get people on these panels to answer a question like that? How did you get them to assign a value to the frequency or the distribution?

I cannot imagine anybody taking a stand on an issue like that after having been in a bunch of them. Quantify it explicitly.

MR. HARDIN: What I presented to you were some of the more persuasive arguments I was able to write down during the assessment process.

DR. DOMENICO: What is your number, Roy?

MR. ROY WILLIAMS: I just told you. I would not have any idea what number to put on a question like that; and I have seen a lot of faults.

MR. HARDIN: On the other hand, we have some strata in the Calico Hills unit, particularly vitric units, that are relatively incompetent rock.

MR. ROY WILLIAMS: We are talking about fault zone gouge, not strata.

MR. HARDIN: So where this data are faulted one might expect those faults to be tight.

MR. CORDING: To me, this discussion is a very key part of the conclusion that extensive horizontal drifting is very important. Of course, the reason for your study is not to prove that this release is going to be 1,000 times less than threshold level, but is to evaluate what you need to do to find out what is really down there.

I think the comments on the fault gouge and the fact that they are unknown that Roy is making is a good point. I think some of us feel we are sitting on the limbs of this decision tree waiting for the decisions to get to where some of us have been: certainly in this area of faults and horizontal exploration. I am very pleased to see us approaching that.

Ernie, from your presentation here, you have not gotten to the bottom line on your summary which says, "Strategies Two or Five have significantly higher likelihood of producing correct results." Looking ahead, that is really coming more from the next presentation.

Is that not correct?

MR. HARDIN: Yes, that is correct.

MR. CORDING: You don't have anything that really confirms that in the presentation.

MR. HARDIN: Are we ready for the next set of presentations?

DR. NORTH: I would like to get another comment on the record.

I get concerned in discussions like this that we get too focused on the detail, and the detail of the numerology in which I am very comfortable, as some of you are, in geosciences. I am very comfortable in terms of the language and the arcane procedures involved.

However, I think we have to reflect that what all of this is doing is using judgment as a supplement to modeling in areas where we are uncertainty. To try to put the issue into perspective I would again like to cite from the recent National Academy/National Research Council report, which seemed to be to do a very good job of summarizing what I believe is the major set of issues.

This is from the bottom of page 24 where the Academy report is discussing natural analogues and then professional judgment as supplements to modeling, and I quote:

"A second approach is to use the professional judgment of technical experts as an input to modeling in areas where there is uncertainty as to parameters, structures or even future events. Such judgments, which may differ from those of DOE program managers and their staffs, should be incorporated early in the process. A model created by this process can redirect the DOE program substantially.

"It is important to bear in mind that all uses of technical information entail judgments of what is important and what is less so. If the technical community is to learn from the successes and failures of the DOE program, it is essential that these technical judgments be documented. Setting out the reasoning of DOE staff and of independent outside experts contributes to learning and builds credibility in the process even when the experts disagree with DOE staff and among themselves."

So let's focus on the process. Let's not get too lost in some of the details.

The very explicitness of what has been laid out here can be very valuable in terms of finding areas of agreement and disagreement; and at the level of which tests make the most sense, we may all agree on some relatively robust conclusions even though there are many areas of the input data and the decision analysis procedures on which reasonable people may disagree, and frequently do.

MR. BROCUM: We propose continuing. We have about another half-hour presentation.

DR. NORTH: Please.

CHAIRMAN DEERE: I think we should.

MR. BROCUM: Okay. The next presentation is by Hollis Call who will summarize the model results.

MR. CALL: I am going to give you the brief version of this and let you ask some questions in the remaining time.

Summary of Model Results

MR. CALL: Just to remind you where we are, I am going to talk about results of the evaluation model we presented early. You have now seen all the technical inputs into it.

I am going to talk about four types of results: what we call vent probabilities; release distributions which you have seen a lot of: I am not going to spend too much time on those; what we call optimal policy: that is, for the decisions you have to make what are the optimal decisions according to the model, and what are the consequences of those decisions; and I am also going to talk a little bit about some interpretation: interpreting some of the results.

On vent probabilities, as you remember we talked about an assessment where we said if you had true conditions of any one of these and you conducted a test how likely is it that your test tells you one state versus another.

So in the input we had prior probability distributions and we had likelihood functions, or probabilities test outcomes. I want to talk now about what we call marginal probabilities or: If you conduct any one of these tests how likely is it to say slow matrix, fast matrix, concentrated fracture, distributed fracture? Then given it has said one of those things, how likely is it that you are in one of those states?

I am sorry this slide does not give me the shadings we need. This is a comparison of Test Strategies Number Two and Six, and it is the probability of the test outcomes. You conduct either Strategy Two or Six and, for Strategy Two, there is a 60 percent chance that the result is going to be slow matrix. You can see the corresponding probabilities for the other test outcomes.

For Test Strategy Number 6 it is a little bit closer. It is more in the direction of a uniform distribution. It is less able to discriminate; and the chances of getting any one of the results are closer to even.

Now that you have conducted the test--again Strategies Two and Six--and if the test says slow matrix--that is, you have concluded, based on all of the test results, that you are in slow matrix--what is the chance you are actually in slow matrix?

For Test Strategy Two you can see it is very high: 93 percent or in that vicinity. Test Strategy Six is also very high, but a little lower. I think the graph pretty well speaks for itself.

The next one is a little more interesting. What happens if your test says distributed fracture? Distributed fracture is a relatively unlikely flow condition. What happens if you conduct the test and it says distributed fracture?

For Strategy Two, which is an extensive drifting strategy, there is a 60 percent chance you are actually in that state; and there is somewhere around a 30 percent chance with Strategy Six. Interestingly, with Strategy Six there is a higher probability that you are in slow matrix that distributed fracture: that is, that the prior probability of being in slow matrix is pretty high and Test Strategy Six is not powerful enough to change that view.

This is, I think, a fairly interesting result. It shows you simply the distribution of false negatives and false positives once tests have been conducted.

We have talked a lot about release distributions. This is a new slide, and I will get you copies. I just put this together. If you remember, this is our definition of R.

I want to do a quick comparison of the R we are estimating to the Environmental Protection Agency standard, which is a probablistic standard.

We don't know the shape of the Environmental Protection Agency distribution. All we know are two points on it, and I am showing one point here as the 90th percentile: a ratio of 1.0.

According to our model, the chance you are going to be at 1.0 or greater is around 10_5.

Alternatively you can look at it from this standpoint: At the 90th percentile our model suggests that R is 1.5 times 10_6. This is not actually our distribution: it is a graphical representation.

You have in your slide packet a number of cumulative distributions, releases to the accessible environment. I think everyone understands these are somewhat controversial, and they have almost more shock effect than anything else.

When you look at a distribution that has that kind of spike then you realize, as Ernie was showing, that you really ought to start truncating the distribution at the 95th and above to really start getting any kind of shape to the curve.

The basic message there is that given the inputs we assessed from the experts there were very confident in the performance of the site; and that is what is being reflected in these distributions.

I will skip over the rest of those and what I want to speak of now is: What does the model tell us is the optimal thing to do?

As Dave mentioned earlier, based on the analysis we have done with the kinds of limitations on the scope we have and the inputs we developed, the optimal policy according to the model is to not test.

It says that we are confidence enough in the site and the benefits that we gain--i.e.: the ability to improve our decision based on the tests--are so low that it is not worth conducting.

Again, that is all based on the analysis we have conducted so far. These are $1 million and the differences, effectively, are all just in test costs.

Obviously, that is a fairly controversial result.

DR. NORTH: What have you assumed for the costs of the tests? Is that in there?

MR. CALL: We did not assume anything. Someone involved in the project developed an engineering cost estimate for all the tests.

MR. LANGMUIR: Where would Two and Five be on that: the two preferred test types?

MR. HARDIN: We have the actual cost figures we can show you. Two and Five are not the highest cost. The highest cost is associated with the extensive outside facility.

DR. NORTH: Are these numbers net benefit minus cost?

MR. CALL: Yes.

DR. NORTH: Then I am not quite sure why 27 is better than--

MR. CALL: These are costs. So they are all negatives.

DR. NORTH: Okay.

MR. CALL: There is just a small problem with polarity.

MR. BROCUM: They are not numbered, but they are One through Eight.

MR. CALL: Sorry, these are One through Eight. I don't remember the exact figure for the test costs.

DR. NORTH: I am wondering if there is any value if I look at the costs as being separate from the value of the information. If I read those graphs correctly, you have very small chances--somewhere between 1 in 10,000 and 1 in 1,000--of exceeding the Environmental Protection Agency standard. So there ought to be some value to the information: it may be relatively low.

MR. BROCUM: Yes, that is fair.

MR. CALL: Some value in the information in the sense that it changes your decision or is there something else?

DR. NORTH: In the sense that if you have that result to which you have assigned a probability of less than 1,000 you would indeed want to change the design of the repository, or maybe not have it there.

MR. CALL: I can take you through the model again.

Based on what we have in the model currently it is saying there is no value to the information because it never changes your decision.

There may be some value in another sense. Tomorrow you are going to hear a lot more about that from the Exploratory Shaft Facility study.

Just to share a little more interpretation,--I think these are a couple of relevant points--why no testing? Expected releases are very low: orders of magnitude below the Environmental Protection Agency standard; and releases are the things we are worried about.

The test results are not likely to change that view.

There is lot being rolled up in this statement having to do with the power of the tests, as well as the confidence of the experts in the site.

The next point is: The test costs are relatively high. Cheaper incremental or phased tests might be optimal.

One of the things we always try to do in a decision analysis is figure out: Have we somehow restricted ourselves in our options to a set of choices that are, in themselves, dragging this kind of sub-optimal result?

We would like to be able to come up with alternative more flexible options--and I think this is very consistent with what the National Academy of Sciences report is saying--that it may, in fact, be a program, not a $250 million commitment to a test, that our model is assuming. It may be the program commitment in small steps that is important.

So you have to remember that is the way we looked at it in this model. We looked at eight different options, each of which represented a fairly substantial financial commitment today.

It is probably true that, in fact, there are lots of things you can do--small steps--to collect the information and improving your decisions as you go along.

MR. McFARLAND: Clarification?

MR. CALL: Yes.

MR. McFARLAND: By no testing do you mean no testing beyond that presently defined in the Site Characterization Plan?

MR. CALL: Since one of the options was, in effect, one of the Site Characterization Plan options?

MR. DOBSON: Close.

MR. CALL: Close?

MR. DOBSON: I think the answer to your question is yes. We did consider that.

MR. CALL: Finally, a preference for testing, which was very clear from the managers we assessed, implies at least one or both of the following.

One is that decision-makers, in fact, place a high value on high confidence even at extremely low levels of releases. The difference between 10_8 and 10_7 is significant according to that statement.

All I can say at this point is we did not observe that level of sensitivity in our assessment exercise. Granted we did that in a day: all those caveats; but I have to say that we did not observe that level of sensitivity.

Finally, there is a value to testing not captured very well by this model. I say "very well" because we did, in fact, in our assessment exercise try to get at the value of building confidence in the site.

I think the manner in which we did that made it very difficult to get a very good estimate of that value. As you will hear later from the Exploratory Shaft Facility study they have approached this problem in a slightly different manner.

Again, the point here is that, as Bruce was describing earlier today, this is not a, quote, technical value. The technical experts feel very confident in the site based on performance.

Granted we might revise some of those technical judgments. I think the model and these results are very robust with respect to those judgments.

But this is just saying there is something beyond merely the technical value you get from the information.

I think in another decision analysis that should also compare the investment in data gathering for confidence building with other ways of confidence building: maybe risk communication programs; maybe lots of other alternatives that fit into that that compete very well with data gathering as a confidence building exercise.

Those are just some of the options that I believe should be examined.

That is it unless there are any questions.

DR. DOMENICO: Would you give me one more feel for the inputs that were guessed at by your technical experts?

MR. CALL: Sure. Any particular inputs?

DR. DOMENICO: Just give me the list, give the smorgasbord. Summarize, again, the list of inputs that had to be guessed at by the experts: those things, those parameters, whatever.

MR. CALL: Those are the variables we assessed. I showed you some of the more detailed conceptual models we used for people to think through these problems, to think through these variables.

We went through each of these variables and did an assessment exercise. The panel has met, I think, nine times in the last three or four months. We had some fairly intensive sessions for each of those variables.

A lot of time and effort went into it. I am not saying that that, in itself, guarantees the numbers are right; but I want to emphasize, when you say guessed at I understand--

DR. DOMENICO: Well, best judgment.

When they had to give their best judgment on transport through the saturated zone, did they have to come up with velocities?

MR. CALL: They had to explain their distribution: Was there any point on that distribution in terms of things like velocity?

DR. DOMENICO: Velocities, porosities.

MR. CALL: Right: all the variables that appeared in the other influence diagram I showed earlier.

We did not just ask somebody what their guess was, tabulate it, and average them. They had to be able to justify. That is all part of the record. It is all part of the documentation.

DR. DOMENICO: Your available inventory to the Calico was just some heavy slug. Is that correct?

MR. CALL: As Ernie was describing it, it is actually a fairly complex model in itself in the sense that we had to make adjustments for different half-lives for the unretarded species; but that is basically what it is.

It is a volume and a composition of radionuclides available for transport through the Calico.

DR. DOMENICO: Okay. Thank you.

MR. CALL: Are there other questions?

[No response.]

CHAIRMAN DEERE: Conclusions, Dave.

MR. BROCUM: Dave will give a final conclusion and recommendations of the Calico Hills Task Force.

Conclusions and Recommendations

MR. DOBSON: I will take about two minutes. I have the real conclusions written down here. I wrote them down during the last discussion.

This is the same viewgraph I showed this morning that that, basically, Hollis just finished discussing. I don't want to spend a lot more time on it. I do want to make a couple of comments that are relevant to the kinds of discussions we have been having.

We tried to assemble a group to do this analysis that was broad in terms to technical expertise; that had not agendas; and that spread the range from pessimistic to optimistic with respect to how we felt the site was going to perform.

We are not claiming that group was independent. With the exception of Jack Robertson, everyone on the group was a project participant or someone who has worked with the Department of Energy in reviewing the program.

On the other hand, we do feel we did analyses which are explainable and defensible, not necessarily in all cases correct. But we welcome the chance to interact with the Nuclear Regulatory Commission and you will see that in just a minute. That is our next step down this road: to go to the Nuclear Regulatory Commission and explain to them what we did; and also with the Board and other groups who may be interested in reviewing this stuff.

To re-emphasize something Warner said a couple of minutes ago, I think communication is a very important part of this process not just in terms of what we did in great detail but what the implications are of some of these things.

If we can defend our--I will not call them performance assessments--assessments of how the site might perform based on the expert judgments we have elicited so far, we have taken a great step forward and have done something to improve the confidence in the program, I think.

Because of the anticipated performance we got out of our analyses and because of the residual uncertainty that is associated with any testing strategy, given the narrow value of information model Hollis built for us--and I built it narrowly intentionally to keep it narrowly focused on the criteria that were identified in the Nuclear Regulatory Commission objection--we identified relatively, what Bruce Judd referred to as, technical value of the testing.

However, I want to add sort of quickly that the Task Force recognized, when we started coming to some of these conclusions two things. One is the preliminary nature of the existing data. I think Pat made a good point of that this morning when he asked how many data points there were on the Calico Hills.

Also, something Bruce also discussed this morning, there is considerable value to testing even in situations were, when you use a model like this one, it is difficult to find.

Those value derive from a variety of sources, like confidence, like the demonstration of your knowledge: that you are able to convince others of what you did. In fact, tomorrow in the Exploratory Shaft Facility Task Force you will see they have a variable they call "regulatory acceptance", which sort of reflects our ability to convince others of what we believe.

I want to add one more because I think it was very important in the sort of pragmatic approach the Task Force recommended in the end in coming up with a recommended strategy.

That is that at this point in time, in the very beginning of the program, it pays to be prudent and it pays to maintain your ability to do things down the line that you are not sure you need to do now.

That planning, prudence and flexibility is a value that gets added to this study that was not explicitly in our model.

To summarize, then, we talked before that we felt our analyses had indicated, and gave us a considerable amount of confidence, that the impacts to the site from any of the strategies we evaluated were very small.

We also noted, in Ernie and Hollis' presentation, there are significant differences among the strategies in terms of their ability to correctly predict the hydrologic conditions that are likely to exist at that site, and those hydrologic conditions do have an effect on the way the site is likely to perform and how well it is able to meet the standards.

We also believe the testing will improve our confidence in performance, as I just went over.

For those reasons and several others, the recommendation of the Task Force, once again, was that as a planning basis for the Exploratory Shaft alternative the Department of Energy should plan to be prepared to go with Strategies Two or Five, which involve extensive drifting in the repository block.

Those are the only ways you are going to get that in situ ability to walk around and do the sort of exploration strategy and get the continuous exposure we feel will give us that level of confidence.

The last viewgraph I have has a few speculations on where we go from here.

We have mentioned several times that the next step in this process is for us to go back to the Nuclear Regulatory

Commission. When we have completed the final report we have committed to meeting with them prior to take further action.

Therefore, that will be in our plans. At the July 31st scheduling meeting we have with the Nuclear Regulatory Commission we will probably be discussing the scheduling of that meeting.

There are a lot of other options we could pursue with respect to the Calico Hills. We could go into a second phase of the Calico Hills in terms of the model development to try and hone in on those values associated with testing that are not in the model right now. That is one possibility.

As Max alluded to earlier today, we might elect to take the model as it now sits and do an external peer review. We might want to go to people who are totally independent and see how much their views differ in terms of the numerical assessments we did.

That, I think, is certainly a strategy that the Board has recommended and that we are going to consider very seriously. I think that over the long haul obviously you will see the Department of Energy doing a lot of things like that: trying to get the independent external peer community familiar with what the program is doing.

Hollis mentioned in one of his viewgraphs there was also the model to date that indicated that maybe a phased or a step-wise program might be optimal. I would like to point out that the recommendation of the Task Force does not preclude that.

In fact, that might be the next step we might want to take. We have recommended the Department of Energy should maintain the option to put in extensive drifts, but that would not preclude us from saying let's do some small facility first and then put in a series of drifts to the Ghost Dance Fault and then a series of drifts elsewhere. That is yet another option that is within the realm of possibility.

Finally, the broader implications of this study as well as the ones that are going to come out, I think, of the Surface-Based Prioritization Task Force and the Exploratory Shaft Facility Task Force may have a significant effect on the policy of the Department of Energy over time.

I think that all three of these Task Forces are helping us learn a lot about what it is that is driving our decisions.

For that reason, I think they have all been extremely valuable and they may result in long-term shifts of policy. Of course, we also have the National Academy report, which I think says the same thing in different words than I just used; but there are different policies we could adopt.

That is all I have to say.

CHAIRMAN DEERE: Thank you.

Before the Board brings up their questions, if they have additional ones, I would remind the members that we meet from 8:00 to 10:00 this evening in closed session in here.

[Laughter]

CHAIRMAN DEERE: Are there questions?

DR. DOMENICO: If there are, you have to join us tonight.

CHAIRMAN DEERE: Are there questions from the audience?

[No response.]

CHAIRMAN DEERE: Thank you very much. We are delighted to hear the progress of the work you have done. We questioned various elements of it, but still think it has been a very worthwhile effort and we are anxious to hear what we will get tomorrow.

MR. DOBSON: We appreciate the opportunity to be here.

CHAIRMAN DEERE: We will recess to meet here again tomorrow morning.

[At 6:00 p.m., the meeting was recessed to reconvene Wednesday, July 25, 1990.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before UNITED STATES NUCLEAR WASTE TECHNICAL REVIEW BOARD

in the matter of: BOARD MEETING

were held as herein appears and that this is the original transcript thereof for the file of the Department or Commission.

Official Reporter

DATE: JULY 24, 1990