UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

The Adolphus Hotel
Sam Rayburn Room
1321 Commerce Street
Dallas, Texas 75202

April 7, 1992

<u>BOARD MEMBERS PRESENT</u>

Dr. Don U. Deere, Chairman, NWTRB
Dr. John E. Cantlon, Co-Chair
Dr. D. Warner North
Dr. Clarence R. Allen
Dr. Patrick A. Domenico
Dr. Donald Langmuir
Dr. John J. McKetta
Dr. Dennis L. Price
Dr. Ellis D. Verink

<u>ALSO PRESENT</u>

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Dennis Condie, Deputy Executive Director, NWTRB
Dr. Leon Reiter, Senior Professional Staff
Dr. Sidney J.S. Parry, Senior Professional Staff
Dr. Sherwood C. Chu, Senior Professional Staff
Dr. Robert W. Luce, Senior Professional Staff
Dr. Carl DiBella, Senior Professional Staff
Mr. Russell K. McFarland, Senior Professional Staff
Dr. Edward J. Cording, Consultant

I N D E X

I N D E X (Continued)

P R O C E E D I N G S

DR. DEERE: Good morning, ladies and gentlemen. Welcome to the spring meeting of the U.S. Nuclear Waste Technical Review Board. I am Don Deere, Chairman of the Technical Review Board, and I will be chairing my last Board meeting as I will be retiring from the Board when my term of office expires in 11 more days.

It has been a pleasure to chair the committee and to work on this very important national program. I understand that the new Chairman will be appointed by the President very shortly. The terms of three other members also will expire in 11 days, and the affected members are Drs. Clarence Allen, John Cantlon, and Don Langmuir. The appointments or reappointments for these positions are in progress and, I am told, should also be made in a very short time.

I would like to introduce our new Board member, Dr. John McKetta. John, would you please stand? He is Professor Emeritus of Chemical Engineering at the University of Texas, Austin. Welcome to the Board.

We also have two new professional staff members that perhaps some of you have not had the opportunity to meet; Dr. Carl DiBella, a chemical engineer. Carl? Thank you. And Dr. Robert Luce, a geochemist and geohydrologist.

The legislative charge to the Board is to examine the scientific and technical work of the DOE in characterizing the site at Yucca Mountain, and includes the transportation and storage of the high-level radioactive waste. We are to report our findings and make recommendations to the Congress and to the Secretary of the Department of Energy at least two times per year.

Over the next two days we will be examining three of the present important pieces of DOE's work in this area. We are looking forward to the presentations and to discussions with the presenters and discussion from the audience on these particular topics. They are quite timely.

The three: Early site suitability evaluation, total system performance assessment, and an update on the site characterization activities.

I will introduce and turn the meeting over to Carl Gertz, Department of Energy, for his comments concerning their program.

Carl?

MR. GERTZ: Thank you very much, Dr. Deere.

We're certainly, on behalf of the Department of Energy, pleased to be here at this spring meeting. We think it's, in effect, a watershed event, notwithstanding the six inches of rain we had in Las Vegas over the last month, but we think it's an important event because there's some products to discuss. You've seen in process activities over the last year in some of these areas, site suitability and total systems performance. Now you're going to see some products today, and then we'll update you on site characterization. We are out on the site working. We have a couple construction crews digging pits; another crew doing roads and pads, and we're drilling. So we've moved in many instances from the planning and preparation stage, into the implementation stage.

So we look forward to discussing these activities with you. It's a full day. I don't really have much more to say, except we're going to start our technical presentation, and I would like to thank you for your participation individually, Dr. Deere, over the last three years, I guess, now that you've been on the Board, and I certainly--we in the Department at Yucca Mountain appreciate the time and effort and the ideas you've brought forth to the program, and I believe have helped and changed the program, and we're now ready to implement what we think is a very sound program; so thank you.

DR. DEERE: Thank you.

MR. GERTZ: With that, I believe our first presentation's going to start off with early site suitability evaluation and Dr. Steve Brocoum of my staff will make the first presentation in that series.

DR. BROCOUM: Good morning. My name is Steve Brocoum and I'll be introducing the site suitability evaluation topic. I will talk why we did it and where we think we're going on it, and then Jean, of course, will give the detailed technical presentation on this topic.

Back in 1986, as required by the Nuclear Waste Policy Act, we issued an environmental assessment which established the suitability of Yucca Mountain for characterization; in other words, we said that Yucca Mountain was suitable to be characterized, not suitable to be developed as a repository. We used the 10 CFR 960 guidelines for that.

Then in December of 1988, we issued a site characterization plan. That plan included the testing to satisfy all the data needs for comprehensive site suitability evaluation; in other words, the complete site characterization program through which would allow the Secretary of Energy to make a recommendation to the President of the United States.

Finally, in November of '89, the Secretary made a commitment. He issued a report to Congress where he committed to make an early focus on an evaluation of site suitability when it became obvious that the total period of time to do site characterization would be approaching ten years.

In order to comply with the Nuclear Waste Policy

Act, as amended, and to comply with 960 and the Secretary's commitment to early evaluation, in a sense, two kinds of evaluations are required: Early and iterative valuations that focus on conditions that would make the site unsuitable --which ESSE was the first such evaluation--and as we complete site characterization, a comprehensive evaluation that would ultimately lead to a decision on whether to recommend a site for development of a repository if it is found suitable.

When we started this process, we had some internal debate as to whether 960 was applicable. After a lot of debate and discussion, a management decision was made that 960 is applicable not only for comparison among sites--which was done prior to 1987--but also for the evaluation of a single site, and if you read 960, there are many cases in it where it talks about a single site. So that finally the question was how we were going to apply 960 for early evaluation site suitability. 960 itself never envisioned iterative evaluations for site suitability.

Following a meeting on site suitability which we had in Albuquerque in the fall of 1990, a decision was made by OCRWM to conduct an early evaluation of site suitability. Since 1987, no formal evaluation of site suitability had been conducted, and so we felt there was a need to, in some formal manner, look at the status of site suitability.

The Office of Geologic Disposal was directed to make an early assessment of the suitability or non-suitability of the Yucca Mountain site using the 960 guidelines.

The Office of Geologic Disposal directed the T&MSS contractor to conduct the early site suitability evaluation, and the results from the ESSE will be used by DOE as part of its decision-making process with regard to future and actions for evaluating the site. The ESSE is just one part of a site evaluation process. It's not the only part.

This was a schedule we put together, and I think you've seen this before, so that we issued--we started the document in January of '91. We issued the document in February or March of this year for public comment. A Federal Register notice was put out, and there will be a Director's forum in the middle of the public comment period on May 7th in Chicago.

So what are we going to do with site evaluation in the future? First of all, we hope to receive comments from the public on ESSE and the overall site evaluation process. The comment period ends on June 15th. We're holding a Director's forum in May, on May 7th, to discuss DOE's policy, strategy and plans for site evaluation, including factors that should be considered in the decision-making process. There are numerous other factors besides technical. There are institutional factors, you know, there are regulatory factors, and there are other management factors, such as costs and schedule.

We will consider and respond to all public comments in writing, and after considering the public comments, the Director will determine what actions to take with respect to future plans evaluating the Yucca Mountain site. At the forum, that determination--if it's done like past ones have been done--there will probably be some kind of a letter with an attachment that the Director of OCRWM will issue.

That is my introduction. Do you have any questions?

(No audible response.)

DR. BROCOUM: Jean?

DR. YOUNKER: Good morning. Can you hear me okay?

(Affirmative response.)

DR. YOUNKER: Great. I think I'll try this side and see how that goes.

Okay. I'm prepared to give you as much of an explanation in as much detail as you'd like of what we've done this past June in the early site suitability evaluation. You've heard a little bit--it seems to me I've spoken to you at least once, and perhaps twice as we were putting the product together, so I'm pleased to be able to report to you that we completed it on schedule and with what I think is a good product.

Let me tell you about the team that put this together. It was a multi-disciplined team because of the wide diversity of siting criteria that are encompassed in DOE's siting guidelines, so for example, we had to have experts that covered such areas as environmental quality, and in that case it was one of the SAIC/T&MSS people, Greg Fasano. We had to have people who covered diverse areas such as--let me find one of our USGS people--Bill Dudley, who's going to speak with you, covered tectonics and erosion. Bill's sitting at the front table and will tell you about the tectonics evaluation a little bit later.

To put together a team like this that covers such diverse topics, you face a lot of questions about how to get the group to work in a consensus-building fashion, and a little bit later in the presentation I'll tell you some ways that we attempted to do that. We did try to act as a body that would reach conclusions as a group, and when you're cutting across as many disciplines and as many specialties as we are, that caused some real challenges in the way we operated.

My presentation is split into four basic parts. I'll give you a general background of the task, and Steve has already covered most of that so I really won't say very much further about that. We'll then talk about the approach that we developed. As Steve told you, the guidelines were really set up to be used once at the beginning of site characterization to allow you to determine that the site or a number of sites should go forward through site characterization, and then to be used finally to evaluate whether the site appeared to be suitable for development as a repository.

So the use of the guidelines and the way that we used them in this task required us to do some site-specific adaptation and some, I would say, interpretation to use them in this particular manner at a period between the beginning and the end of site characterization, in a way that there really wasn't much guidance in the written methodology part, implementation part of the guidelines. So we really had to work that as a team.

I'll tell you a little bit about the structure of the external peer review that was conducted, tell you about the people that were on it, and I'm very pleased that you were able to invite two of the peer review panel members to speak with you today, give you their perception of the report; and I'll give you a summary of the final conclusions of the evaluation.

From our view, from the core team's view, our objectives were to develop an approach within the framework of the siting guidelines, 10 CFR Part 960, for evaluating site suitability during the site characterization process, and to use that approach, then, to provide a guideline-by-guideline status of the suitability of Yucca Mountain.

The general logic diagram, which I think we had the last time that I spoke with you, simply starts from the point of view that you have some information about the site and you have some basic design information, which you do need to have kind of a concept of the design in order to evaluate some of the siting criteria.

Here's the box that represents what we've just done, in that we've evaluated the site against the siting guidelines, the technical evaluation. That information, together, with a lot of other information, feeds into some siting decision that the DOE will eventually have to make in the final decision, but interim decisions can be made where you look at the information, determine whether you should continue characterization or whether there is information present that suggests the site should be abandoned, disqualified using the guidelines, or perhaps information is adequate that you can move ahead and recommend the site.

As I just said, this one really repeats. The kinds of information that you look at is what's your present understanding of the site characteristics, what information do we have about the design of the engineered system--and in most cases here, because we were emphasizing site feature and conditions, we made assumptions about the engineered system and really didn't use a lot of information on that--and then, of course, the present regulations give us a framework for our evaluation.

As Steve suggested, for the decision maker, when you get into this part of the logic diagram, clearly, the technical evaluation that a team like this group makes is only one part of the information that you would use to make a decision as to which direction to go coming out of this decision diamond. Status with regard to the siting guidelines is one piece of that information, but obviously, what kinds of information could be obtained by further testing, how adequate is this information to actually move forward and recommend the site, because that recommendation would be tantamount to DOE's determining that they believed they had a site that had a good chance of being licensable; and further, other management considerations, obviously, budget and cost and other information or other issues come in there.

Okay. I'll move on into the approach, then, that we developed and used in this evaluation. You've heard me present an overview, I think, more detailed than this of the siting guidelines. Just to refresh you, some of you probably are very familiar with the DOE siting guidelines and some may not be, so let me do a quick review.

There are four groups of guidelines; categories, if you will, of individual guidelines sorted into postclosure performance, preclosure performance with radiological safety being the performance measure, a category that covers environmental, socioeconomic and transportation-related impacts, and a group that's called the ease and cost of siting, construction, operation and closure. And what that really amounts to is all of the types of site features and conditions where you have to ask the question, is there reasonably available technology for me to deal with, for example, flooding hazards, seismic hazards at the site. So into this category goes all of the geotechnical and other types of information that you've gather, engineering-related information that you've gathered about the site having to do with preclosure operation, construction operation.

Each group of guidelines is divided into a system guideline and a set of technical guidelines. The system guidelines provides the general requirements; meaning it links you to performance criteria that are usually from MFC's regulations or from other regulations that are applicable to the repository program.

In the case of the technical guidelines, we get closer to actual site features and conditions that we, as a geologist design, that we can characterize ordinary environmental quality in our socioeconomic areas, parameters that we know how to measure and review to determine compliance with the criterion.

Okay. There are 24 specific siting guidelines. As I said before, each group has a system guideline and a system behavior criterion, and then in postclosure performance, for example, all of the areas that you would expect to have to gather information about the site to determine compliance of --determine if the site is a safe site, such as geohydrology, climate changes, tectonics, potential for human interference due to natural resources.

In the ease and cost area, as I said, you are getting at those features and conditions that might cause you to move into an area where you are pushing technology, so it's such things as terrain, which is under the surface characteristics one; rock characteristics, meaning rock properties, how constructible is the rock material; hydrology, meaning either potential problems with underground water conditions that would be hazardous to workers; and in the case of tectonics, the question of seismic hazards.

Okay. Getting into the details now of the evaluation, when we present the conclusions you'll see that in each case there is a qualifying condition for a guideline, and in some cases, there just disqualifying conditions. This example for human interference, the qualifying condition is generally tying you to system performance. So in this case, "The site shall be located such that natural resources, including ground water, will not be likely to give rise to interference activities that would lead to releases greater than those allowed," and if you look at this particular sub-part of the guideline, you'll see that that's the total system release standards, the EPA 10,000-year standards.

For the disqualifying conditions in general, 960 was set up so that you should be able to use the disqualifying conditions as on/off switches early in characterizing a site. They're supposed to be something with less information you could use it to screen out sites that really didn't look like safe or potentially acceptable sites. So the disqualifying conditions usually are something that you can get a handle on with less specific site information.

This one, for example, says: "Previous exploration, mining, or extraction activities for resources of commercial importance have created significant pathways." So this is one where, based on the information you have about the site, do you believe that there is evidence of significant pathways that could cause some kind of diversion and short circuit of your natural barrier system.

And the most important point about the guidelines in terms of the way they are to be implemented is here in this box. "The site shall be disqualified if evidence supports a finding that any disqualifying condition is present or any qualifying condition cannot be met." So you have to go through a one-by-one evaluation of each qualifying and disqualifying condition and reach a conclusion whether it's present in the case of a disqualifying condition, or whether it cannot be met in the case of a qualifying condition. This is exactly what the team that I've just worked with has done.

The definitions that are given for how you should think about the conclusions that you must reach in 960 are presented with double negatives, and the team had a little trouble with that so we worked a definition, a set of definitions that we liked as a group and could use, and so I've written those definitions down for you.

In the case of a disqualifying condition, if the condition is present or likely to be present, then you would make an unsuitability, draw an unsuitability conclusion. If a condition is not present, but additional information could change your conclusion, could change your conclusion about that condition, this is something that's referred to in the guidelines as lower-level suitability. It's your lower confidence position that you make until you are really confident that the condition in the case now, a disqualifying condition, is not present and it's unlikely that any future information you gather about the site will change that conclusion. That's your higher confidence; in fact, the highest confidence position that you are to take given the way 960 is to be implemented, and it tells the DOE in 960 that in order to proceed with this site, they must be in a position where they can take higher-level suitability or higher confidence conclusions on every disqualifying and qualifying condition.

The same definitions were used for the qualifying conditions, but of course, in this case, the qualifying conditions are conditions that you are asking if the site meets them, and so in this case, if the site cannot meet the condition or is not likely to meet it, you're in the unsuitability result. If the site's likely to meet the condition but you believe additional information could change your conclusion, then you're in the lower confidence, and in the site--this is now the higher confidence, the higher-level suitability conclusion: The site meets the condition and you feel confident that additional information gathered about the site will not change your conclusion about that qualifying condition being present, being met for this site, and that's your higher-level suitability conclusion.

The decision logic expressed in the diagram rather than in the words is shown on this view graph. We're in this evaluation box now where the team has done their evaluations, and as I just said, for a disqualifying condition, as an example, if you judge the condition to be unlikely to be present, then you must ask yourself the question: Could that conclusion change on the basis of new information; further information about the site? If you believe that it's unlikely that that conclusion will change, then as a team, we recommend, or we believe the information supports a higher-level suitability finding. If you believe that it's possible that additional information could change that conclusion, then we recommend that a lower-level suitability finding is appropriate at this time. So that's the logic that we tried as a group to work with on each of the siting guidelines. The same logic applies for the qualifying condition.

Now, you might ask the question: How did you really think about this as a team? And we actually did it in some cases qualitatively, using kind of a jury system, where we talked about the weight of evidence. We really didn't poll the group and work in a probabilistic sense on every criterion and on the siting criteria. In some cases we did, however, but if you're working in the qualitative sense for a lower-level suitability, you basically would conclude a statement something like: "The weight of evidence indicates behavior is acceptable."

In the quantitative sense--and we did do some probabilistic assessments, setting thresholds and figuring out where we thought performance was relative to that threshold--you would be working with a statement something like this: "The probability that behavior meets a threshold is greater than something like .9." And these are examples.

We found that as we worked as a team, when you use terms like "likely" and "unlikely" each of you has a little different thought in mind when you say what's the probability of something be likely. Likely can be 50-50 or it can be 95 per cent depending on the person's kind of predisposition about those terms.

For higher-level suitability, then, we had to move into this area of the conclusions are unlikely to change. We're confident enough about the site features and conditions relative to that criterion that we don't believe new information is going to fundamentally change our conclusion. Now, when you say "change," remember, you're changing from the site as suitable or acceptable on that criterion to it's unacceptable; meaning that conclusion is tantamount to saying we believe for this criterion, the site should be disqualified. The site does not meet that criterion.

And if you were operating in a probabilistic sense, you would lessen some probability that additional information will change your conclusion.

DR. DEERE: Question. Are those really the values that were used by some of the groups, the .1 and .9?

DR. YOUNKER: Yes, actually, they were. I used this as an example, but .1 and .9 work as probably, I would guess, maybe the average for likely and a lot of people on the team seemed to be operating around the threshold of .1 and .9.

DR. PRICE: I have a question. Dennis Price.

I noticed in the report it spoke of higher-level suitability (Level 4), and I see three levels up there and I don't quite know where 4 comes from.

DR. YOUNKER: Yeah. There's an appendix to 960 that explains what those levels are. The Level 1, 2, 3, and 4 are simply the lower level and the higher level for the disqualifying conditions and for the qualifying conditions, so when you see those parentheticals, that's simply referring people who are very familiar with the appendix, where it talks about levels of findings. It's just that 4 different states, 2 for a qualifying, 2 for a disqualifying.

This was one of our most difficult decisions, and that was how to establish what a consensus of our team would be. We had Dr. Bruce Judd, a decision analyst, working with us and I must say that probably this was the part that he found the most discomfort with in the way we decided to proceed.

For the higher-level suitability conclusion to be supported by the team, we determined as a group that we were only comfortable if that conclusions was supported by unanimity among the voting team members. So for us to take that position, which is the more aggressive, less conservative; meaning, we recommend to the DOE that in this particular criterion information is adequate at this time to support the higher confidence, higher-level suitability finding, we believe that all the people voting, all the voting members of the team, should support that conclusion.

In the case of lower-level suitability, what that meant then was that if one person of the voting team members did not support the higher-level suitability conclusion, then we would recommend that the lower-level suitability conclusion that was made on the environmental assessment should be maintained or continued to be supported.

I'm looking over at Dr. North to see whether he's going to dislike that approach. I think he already knew that we did it that way. Yeah, Leon?

DR. REITER: Jean, what was the rule decided upon unsuitability? If one member found any condition unsuitable, would that make the--

DR. YOUNKER: Correct. If we had had one member--and I didn't put that up here, I should have--if we had had one member who believed that an unsuitability conclusion should be recommended, then I think that would have been adequate. We didn't have anyone who recommended that on any of the guidelines, so we didn't face that, but we did ask the question. If you had one person who didn't believe that you could reach a lower level, or maintain the lower-level suitability conclusion--remember, those had all been at the time of the environmental assessment, so of course, in some cases, if new information appeared to question that, then we did talk about it and evaluate that as a group, and question whether at least a lower-level suitability finding still seemed to be valid.

DR. DEERE: Yes. Don Deere here again. I forgot to announce at the beginning that we should identify ourselves, and I did not identify myself in my first question so I will now do it retroactively, and also, that was Leon Reiter of our staff who asked the other question.

DR. ALLEN: Jean, this is Clarence Allen. The core team here consists of all 18 people. You mean you had to have 18 people vote the same way?

DR. YOUNKER: I'm glad you asked that question.

DR. ALLEN: I can't imagine 18 people voting the same on any issue.

(Laughter.)

DR. YOUNKER: The way we operated was because of that diversity that you are certainly aware of on that team, there are a number of guidelines where not everyone did vote. You could abstain from the vote if you didn't feel that you had the expertise to participate, and as you can well imagine, someone who may be an expert in transportation may not feel that he's really, you know, has the right expertise to, say, make a judgment of the confidence in information in geochemistry. And so we did not have every member of the team voting on every technical guideline.

DR. ALLEN: And presumably, Bruce Judd didn't vote?

DR. YOUNKER: No, he did not vote. He kept us honest, but he didn't vote.

Okay, before the document was released to the external peer review panel, we had an independent technical review, according to the quality assurance procedures that we all operate under. There were 20 technical staff from all of the DOE's participants in the Yucca Mountain project who were not involved directly in preparing the information who did review. It's a documented review. We responded to their comments and made quite a few changes in the document at that time. That was last summer.

And then DOE, of course, before we release a document, before DOE releases a document for any kind of public review, does a policy review of that document.

DR. NORTH: Dr. North. Were there any changes as a result of that review in the level of the conditions?

DR. YOUNKER: In the internal review?

DR. NORTH: Yes.

DR. YOUNKER: I don't think so.

DR. NORTH: And in terms of the lower-level suitability versus higher-level suitability, there was no change?

DR. YOUNKER: I don't--let me ask people. Steve, or someone from the audience? Steve Mattson's here, who was on the team.

DR. MATTSON: Steve Mattson with SAIC. There were no changes, as I recollect, as a result of that review.

DR. PRICE: Well, while we're interrupted, could I ask another question? Did you take one vote on each issue that you came to and then that was it for 10,000 years, or did you--

(Laughter.)

DR. YOUNKER: Let me tell you the way we actually did it. We had one of the team members from this list that I keep putting back up here. Let me take an example, say, for geochemistry again. Dick Herbst, who was our team member for geochemistry, he had the assignment to put together all of the information about geochemistry relevant to the siting guideline evaluation. He presented that to the team and we all attempted to understand and, you know, absorb as much of that as we could. And then, generally speaking, what we did was at that point, if he recommended that, let's say, the information supported maintaining the lower confidence finding of lower-level suitability, then we asked from the team if there were any people who had a problem with that, or if there were--and particularly if he had recommended a higher confidence, higher-level suitability, then we would also ask from the team: Is there anyone who can't support that; who doesn't feel comfortable with that?

Now, if we went into the probabilistic-type of voting on some of them where we really, we didn't have any kind of unanimity and we wanted to get some of the ideas out on the table just how diverse were people's opinions about that particular criterion, we would then go through two voting sessions; one where we all declared what our probabilities were--kind of what our thresholds and our probabilities were--then we would display those and talk about them, using an approach that Bruce Judd uses when he does this kind of elicitation. Then we would talk about them, especially about the extremes, and then vote again, having learned about why we had the different opinions that we did, and that final vote would be the vote that was recorded.

DR. ALLEN: Clarence Allen. Don't you run the danger here of, say, a person like the person in transportation who's the only expert in that field on the whole panel, then, indeed, that one person is going to dominate the thought of the entire panel and a unanimous vote really doesn't mean that much.

DR. YOUNKER: Yeah. That--it's a real question how you work with a team like this where you have such a broad, you know, multi-discipline area to cover.

In most cases, what we had--like in the case of, say, Bill Andrews with transportation, we tried to bring in other experts that we could ask questions, the team could ask questions, and so usually it wasn't just one person. We usually had a couple of other people there that at least had some good background in that area, but that certainly is a question, yes. It's a good question. How much does one expert, when you have this kind of spread of topics, dominate the conclusion? And the answer is probably quite a bit.

Okay. I'm ready to talk about the peer review. I think I already talked about that one. Structure of the peer review, okay. The peer review panel was also difficult to put together for the same reason that we've just talked about the team producing the evaluation being difficult to put together; 14 panel members chosen based on their technical qualifications and their pretty much complete independence from previous DOE activities, although in a couple of cases, in order to get someone with the right expertise, we did have to get someone who had some previous involvement in the program.

We tried to bring in a new team of people to get some fresh ideas, get people who really, for the most part, maybe had expertise based on geology, for example, but did not have any major or previous involvement in the program.

The peer review panel for the evaluation is on this view graph. Very broad expertise was required. For example, Dr. Stan Albrecht from Brigham Young University, a socioeconomic expert who had had some very limited previous involvement in reviewing documents produced by the Yucca Mountain Project Office. Dr. Walter Arabasz, our seismic hazard and tectonics expert, who's here to talk with you a little bit today, certainly had lots of previous expertise and experience developed on the questions that he was being asked, but no direct experience, I believe, on this program; Dr. John Bell, a radiation and health physic professor from UNLV.

Let me give you another example of the diversity. Our environmental quality expert, a private consultant, has his own company in Flagstaff, Arizona, Dr. Steve Carothers; University of Utah, Dr. Pariseau, our engineering geology rock characteristics peer review panel member. It's a very broad team and, as a result of that diversity, very difficult for them to work as a true consensus-building peer review panel.

The way it actually worked was that the geotechnical, the 10 geotechnical panel members worked more as a consensus-building panel to the extent that they did develop a consensus position, and that is in the peer review report that's published for review right now. It's in an appendix, so that the nine--nine of the ten--Dr. Pariseau did not sign the consensus statement. He didn't feel that he had the expertise to conclude what the rest, the other nine were willing to conclude or felt that they could stand behind, but the other nine geotechnical panel members did provide this consensus statement that has basically three recommendations for the Department.

Okay. The instructions that were given to the peer review panel was that they should evaluate the adequacy of information presented. Were there any major holes in the information, relevant information that we didn't know about that should be included? And then look at our overall approach and determine whether the report presents an objective, defensible, technically defensible view of the suitability of the site with regard to 10 CFR Part 960.

They had about three months. They received the report at the end of August--two months, I guess, wasn't it--and the comments were due in early November. So it was a really pretty limited time to come up to speed on the information and draw some conclusions. Let me give you a summary now of the results.

First, by telling you that what you see when you read the report, for each guideline is summarized on this view graph, and both Bill Dudley and Dwight Hoxie will walk you through this information for the two specific guidelines that they're going to describe for you today. You'll find the section where we review the basic findings in the environmental assessment and the information that supported that finding in a very kind of broad way.

Then we look at new information and analysis. We certainly don't present all of that information, but we try to reference the key critical and information that leads us to the conclusion that we reach. So this is a summary, then, of information that is available about the site from the time that we last--we didn't restate the information in the environmental assessment or in the site characterization plan. We referenced those, and any other information that's relevant to this evaluation.

You then see a section where we talk about whether the disqualifying condition or qualifying conditions are present or cannot be met. We then have a final section that talks about what information, if you don't find a recommendation for support of a higher confidence of higher-level suitability finding, you find a section that talks about additional information that we believe is necessary to support that higher confidence finding. And this just says we provide the peer review results to DOE.

There are different ways to count up the results of this evaluation. The disqualifying conditions are fairly straightforward, although even there, the disqualifying conditions in several cases have sub-parts, and the way they're worded you have to meet each sub-part. So if you do the count--what you'll find is my counts on this view graph and the next view graph--take every sub-part as a specific criterion that I must, or that I must evaluate at least.

So if you count the way I've counted--and this is consistent with what you'll find on the next four pages that are the detailed summaries--13 of 17 disqualifying conditions are not present, in our judgment, and new information is unlikely this conclusion. So the core team then has recommended to the DOE that 13 of 17 of the disqualifying conditions can, on the basis of present information, be supported in the higher confidence, the higher-level suitability finding.

Four of 17 disqualifying conditions are not likely to be present, but additional information could change that conclusion and, therefore, we support only a lower-level suitability finding at this time. These conclusions were in the package that was reviewed by the peer review panel, and we have not changed the conclusions as a result of the peer review.

For the qualifying conditions, the total if you break it out into each of the sub-parts is 32, and at the time that we went to peer review, this number would have been 15 because three conclusions that we made in our draft report that went to peer review were challenged by the peer review panel members and we did change them as a result of those challenges. We agreed with the comments of the peer reviewers and went from the higher confidence for the qualifying condition present and new information unlikely to change the conclusion to just the likely to be present, which is the lower confidence for the qualifying conditions.

Those three were postclosure rock characteristics; and 2, preclosure guidelines. The radiological safety, which is the system guideline, it's the compliance with the preclosure worker safety and public safety radiological criteria, and one that is actually really just a restatement of the radiological safety, but having to do with any kind of releases from off-site facilities combined with releases from a repository facility, the question being could those summed releases lead to public or worker safety hazards.

DR. ALLEN: Jean, Clarence Allen. The 13 out of 32 was before the change, or after?

DR. YOUNKER: This is after the change. These are the results after the change. Before the peer review, this would have been 15 of 32--or 16. Sorry; excuse me. It's early in Las Vegas. Yes. Three were changed; I'm sorry.

Okay. The next four view graphs go through in detail every one of the guidelines and what our conclusions were on those guidelines, and we did some shortcuts to try to make it easy for you to see. We did put asterisks by the findings where a higher-level suitability conclusion was not supported by the team, and the two that are highlighted in green here, the geohydrology guideline and the postclosure tectonics guideline, are the two that will be presented in detail by Bill Dudley and Dwight Hoxie.

The statements that you see over in this column in the conclusions, if it's the short statement, "Condition is likely to be present," that's the lower confidence recommendation. That's the one that additional information could change that conclusion, but we didn't carry all that information here. If it says, "Condition present," or "Condition not present," in the case of a disqualifying condition, but says: "New information unlikely to change conclusion," then that's that higher confidence, higher-level suitability conclusion.

Now, the one that I said was changed, for example, the rock characteristics postclosure, has no disqualifying condition. It's only a QC, or qualifying condition, and this one, pre the peer review, would have had that second statement: "New information unlikely to change the conclusion."

That one, the basis for the change--I think the most succinct way of describing the basis for the change is that the peer reviewer, Dr. Pariseau, felt that without an underground excavation, you know, knowing that we were going to have a large underground excavation, that even though he really, when we asked him in our discussions with him, "Do you think that there's a chance the information that we'll find when we do extensive underground characterization would lead you to think that the rock materials and rock properties are such that you can't accommodate thermal, chemical, mechanical stresses that would be induced by the repository?", his answer was, "No, I really don't think you'll have that problem, but I don't think you're credible making that conclusion without having the underground excavations." So it wasn't really the question--in his view, it wasn't that he thought we were going to find the information to be--to cause you to draw the conclusion that the site wasn't suitable, as much as it was a question of, "Is the team credible drawing that conclusion without that information?"

And I won't go through these one-by-one, but if you have questions, I'd be happy to answer on any of them. I was going to mention the ones that did change as a result of the peer review. The other two that changed as a result of the peer review panel on this second page, the radiological safety standards for preclosure operations for both worker and public, this was a higher confidence, had the statement: "New information is unlikely to change," before the peer review. So did this qualifying condition here; off-site facilities will not lead to unacceptable releases." The rest of that statement is: "--when combined with our operational releases." This one also had the higher confidence, higher-level suitability finding.

The peer review in that case, the question who questioned this most dramatically was Dr. Bell from UNLV, and his comment on this one was that although he also, in answering our questions and helping us understand his position, didn't believe that the site conditions would lead to a facility that had unacceptable risks from the standpoint of public health and safety, he also didn't feel that we had detailed enough design information to prove that to him.

So it was a question of the maturity of the design. We didn't have operational releases, for example, that we could show him, and he's the type of person who didn't really feel comfortable saying, "Well," he said, "I don't think your conclusions are very credible until you can give us that detailed design information." We had accidental release calculations that were fairly old from the SCP days, and no operational release, except for similar facilities, and we, of course, did try that in the discussion, but we really needed specific release calculations in order for him to feel comfortable supporting our conclusions on that one.

So we decided that based on his comments, which were quite strong--and they're in the written record--that we should change our conclusions on the two related to that, which was this qualifying condition and this system guideline.

The next two are simply the rest of the guidelines. Remember, there are four categories. The next page summarizes the conclusions for the environmental socioeconomic impacts and transportation guidelines. In this case, because the kind of information that you must have in order to make these evaluations for the most part is the kind of information that you gather during a NEPA process, when you look at compliance with the Environmental Policy Act.

The conclusions, for the most part, are all the lower confidence or lower-level suitability conclusions. We have one specific one where a disqualifying condition asks whether the facilities would be located in federally-protected areas, and we believed that we had adequate information at this time to recommend to the DOE that that one could be supported at the higher-level, higher confidence finding, but there weren't any of the others where we really have the information we need at this point to recommend to the Department that they can support higher-level findings.

The fourth category, the one that I told you has to do with availability of technology to handle site conditions, is summarized on this view graph. In this case, for the most part we have recommended higher confidence, higher-level suitability findings can be supported. The one specific one I'll mention--I think Dr. Arabasz will probably comment on this in his statement later--the qualifying and disqualifying conditions for preclosure tectonics get at the question of what expected conditions are related to seismic hazards, and this one was a very difficult one for us because the qualifying and disqualifying conditions are written very, very similarly so that you almost can't reach a higher confidence finding on one without reaching the same conclusion on the other.

If you read the text, what you'll see we did was to say that in the case of the disqualifying condition, we took the position that the guidelines allowed us to, which was you can evaluate a disqualifying condition on the basis of less detailed site-specific information. And so you'll notice in the text we describe that although the information base is not adequate to support the higher confidence finding on the qualifying condition for seismic hazard preclosure design, the disqualifying condition, we did recommend you could support the higher confidence finding based on our group's conclusion that we really do believe that technology is available to accommodate the kind of seismic conditions that exist at the site.

Well, if you do kind of a bottom line summary of what's in the report all in one view graph, the areas where we do not reach higher confidence findings or recommend that the information supports higher confidence findings are summarized on this view graph. There are a few other ones that I've left out, if you surveyed the last four pages, that we don't think--that really aren't as important if you look at the way the things are prioritized, but the ones that--this is not in order, by the way. It's just kind of the list of items where we have sections that say: "Here's the additional information we believe is essential in order to determine if a higher confidence finding can be supported."

Climate changes, tectonic disturbances--and in this case, it's kind of the coupled process, the tectonic effects on other conditions over 10,000 years--source term for gaseous release. In our total system section we do talk about the question of gaseous releases and the Carbon-14 problem being an area where we need additional information.

The groundwater travel time, which Dr. Kreamer will comment on; potential for fast flow paths--the consequences of the existence of fast flow paths is a critical area--potential for natural resources to attract human interference. We don't recommend at this time that we have enough information in that case to support the higher confidence and the qualifying condition for the human interference guideline.

Potential for unacceptable environmental quality, socioeconomic, and transportation impacts, I mentioned in that area we really just don't have the information to make the evaluations at this point in time. The preclosure rock characteristics guideline is kind of an unusual one. We did not reach a higher confidence or recommend a higher confidence finding on that one.

This one, once again, had to do with the question of not having enough information until we get underground to be certain that the vertical and lateral extent of the candidate potential host rock is adequate, and that, of course, had all kinds of design assumptions in it; meaning how much area do we really need. And we, in this case, assumed the reference design back in the SCP days, so that's quite a bit more than if you went toward one of the hotter repository concepts that are being considered. And then seismic risks, which I've already mentioned as the preclosure tectonics qualifying condition.

Okay. That wraps up what I intend to say. I think, Dr. Deere, Steve Mattson wanted to make a comment. Would that be acceptable?

DR. DEERE: Yes.

DR. MATTSON: Steve Mattson. I just wanted to make one correction to my earlier statement. We did change one of the findings during the internal review process, and that was on preclosure tectonics on the qualifying condition. I'm sorry, I had too many review processes to keep them all straight, but we did change one of those from a higher-level suitability finding down to a lower-level suitability finding on preclosure tectonics.

DR. YOUNKER: That's right. Thank you very much, Steve. I do remember now, too. He's jogged my memory.

The question was we had both the qualifying condition and the disqualifying condition for preclosure tectonics recommended at a higher-level finding before that technical review, and that was when we went through that whole debate that I mentioned to you about whether you could separate the qualifying and the disqualifying condition for preclosure seismic, or preclosure tectonics or not, and we decided at that point that as a team we could separate them and recommend supporting the higher confidence for the disqualifying, but not for the qualifying, and that was based on fairly intense comments from some of our reviewers.

DR. DEERE: Thank you.

Board members have questions?

DR. ALLEN: Clarence Allen. Two questions, Jean.

Tectonic disturbance includes volcanism; right?

DR. YOUNKER: Bill Dudley will explain this in just a little bit, but the way that tectonics guideline is written, the disqualifying condition excludes the postclosure tectonics, excludes volcanic activity. The qualifying condition includes it. That's right, though, and he will explain that when he presents the detail.

DR. ALLEN: And the next, what's the--what do you anticipate the effect of this will be, assuming that the investigation of the site proceeds and it's not disqualified at this point, is the hope that these items and others similar will then receive greater emphasis in the site characterization program? Is that the whole idea of doing this exercise?

DR. YOUNKER: That would certainly be my recommendation.

DR. DEERE: Yes, Bill.

DR. BARNARD: Bill Barnard, Board staff.

Jean, this whole process took on the order of, what, a year and a half to complete? If you were going to perform a similar type of evaluation, say, five years after we go underground, would you use the same process; and if so, could it be streamlined in any way so that an evaluation could be made in a shorter period of time?

DR. YOUNKER: I guess I would probably use about the same process if I was asked to do it, partly because I really believe in the team approach to this, given that it's such a diverse set of criteria that you have to evaluate. I do think unless you made a smaller team--in which case you wouldn't cover all of the criteria very well--then I think it will take about that long. I'm not sure there's any way you can streamline it that much more.

I suppose if you don't go to an external peer review panel, which I also do believe is essential--that was about a three-month process, but I think that's really an important part of it. The actual evaluation, we didn't officially meet as a team--although there was a lot of scoping back in the end of '91--we didn't officially meet as a team until it would be--I'm sorry, '90--January of '91, and the report was ready for peer review in August. So we really wrote it between January and put the whole idea together and put it on paper between January and August.

The technical, internal technical review was in July, so I guess I'd say January and July, with one review cycle before we went out for peer review. So about six months is probably as short as you can do it, and then the additional three to four months is the peer review process and responding to peer review comments, and finally, production.

DR. DOMENICO: Jean, everybody seems to--oh, Domenico.

Everybody seems to agree that Carbon-14 is a problem, or this is what we've heard. What was the panel's finding on the release of contaminants to the environment, keeping that in mind?

DR. YOUNKER: Yeah. What we say in the report is that we believe Carbon-14 is definitely an issue for the site, but we don't believe it's a site-specific problem as much as it is a problem potentially either with unrealistic regulations, or perhaps something that you have to take a look from a design perspective. We don't specifically say in the report that we believe a lot of site information should be collected relative to the hazards since we don't think that that's really a safety hazard.

At least the evidence that the team had at that time was that the amount of Carbon-14 that you would release just doesn't constitute a public safety problem, so I think our statement in the report is it's not really a site-specific problem.

DR. DOMENICO: But that means you gave your interpretation to the statute in that case?

DR. YOUNKER: Sure.

DR. DOMENICO: Yeah.

DR. DEERE: Don Deere. I would comment on the question that Bill Barnard raised, and your answer, which was that you would go through the same process again.

I still feel very uncomfortable having only one expert in a given field, because if he is a persuasive individual, he may well get votes that he wouldn't otherwise get if he weren't able to be convincing. It would seem to me that at the least you should have two there; one that can agree or can raise another question, but wouldn't it be better to look at it from this point of view. And if you don't have that kind of expert, the question will not be raised, and maybe he won't be considering something.

DR. YOUNKER: Yeah, I think in most cases, Dr. Deere, we did have at least one or two people who had pretty specific expertise in each criterion so that--I mean, for example, I guess, I would suggest like Bill Dudley in tectonics. Well, Steve Mattson, who's been commenting a little bit also, besides having natural resource background, has a lot of expertise in the tectonics area. I have some background in that, so that, you know, usually we had at least a couple of people who could exchange and bounce ideas off from each other.

We also had like Jerry Boak, as the technical monitor from DOE, did participate in the discussions and he has a good background in that, so we had more than one person except in a couple of areas. I think in the preclosure, say, environmental quality and transportation, socio-ec, those areas we clearly didn't have real depth on the team because we tended to focus more toward the postclosure geotechnical panel member expertise.

DR. DEERE: But how about in the peer review group? Because the same thing applies there.

DR. YOUNKER: Same thing applies, yeah. As I said there, I think that the situation was probably about the same. The expertise in the non-geotechnical certainly wasn't nearly as broad. I think, say for example, in the environmental quality, that, in fact, all three of our--four of our non-geotechnical peer review panel members really didn't even work as a consensus team. They were really just independent specialists who gave us their comments in their area of expertise, whereas in our geotechnical group, because there were ten of them, they did talk. We had several meetings where we got as many of them together as we could, and they were able to work a little bit more like a consensus panel. But once again, the problem exists that you're talking about.

I don't know how it's--in thinking about Bill Barnard's question again, I guess the only way you could do it, which would be a lot--take a lot more time--would be if you had, say, a four-man or five-man panel for each guideline, but managing that and making that operate, I feel pretty confident would take quite a bit longer than what this took, rather than streamlining, but maybe would give you a much more credible result.

DR. DEERE: Yes. I'd look, for instance, at the rock characterization from the engineering geology and the rock mechanics in tunnel wall behavior.

DR. YOUNKER: Right.

DR. DEERE: I don't see that the other persons have expertise, whether they're in the geotechnical panel or not. They're not simply involved in designing construction.

DR. YOUNKER: Yes.

DR. DEERE: So that would be almost a lone voice. I'm not saying anything against what he has said or against him, but at least two in the area would give it a lot broader--I'm faced with this all the time because I serve on review boards for hydroelectric projects in a number of countries, and we always have three or four or five, and it's surprising what an experts or two experts might be able to look at that one probably would not because of his particular background. And there's not necessary agreement, but it brings a point up that is discussed, and then eventually, usually they're able to come to an agreement, with perhaps some change one way or another. So I think this is always a point on peer review panels and on any other kind of panel.

DR. ALLEN: Or our own Nuclear Waste Technical Review Board. I mean, we can have the same problems.

DR. DEERE: That's right. But we often take care of that by bringing in consultants to aid on points, and also have technical staff in the same area. So there often are three people looking at a given problem, plus those who are in borderline fields that have an interest in things that should be considered, but it has no easy solution, but more qualified people is really the answer, I think.

DR. LANGMUIR: Jean; Langmuir.

Looking at your bullets here, you've got eight, four of which, on the summary sheet, I'm going to get the additional information without underground testing or underground excavation. Is that going to be a strong recommendation of this group to the DOE in terms of prioritizing their funding and their activities?

DR. YOUNKER: Well, we really didn't--we didn't in writing, I believe, make that recommendation, but if someone asked me personally what I think the core team position would be, I think it would be a strong recommendation that the underground excavations are going to be very key to understanding evaluating suitability of the site.

DR. DEERE: Don Deere again. Another comment on that same subject.

I think a number of your statements in the report say this: "Until we get underground, we'll not be able to find information to raise it."

DR. CANTLON: Yeah, Cantlon.

In looking at the siting guidelines in the four groups that you start with in this process, and you look at postclosure performance, it's surprising to me that the great public unease with this whole process really relates to public health and safety, and yet those words and those criteria really aren't in--

DR. YOUNKER: Yeah. They're in what I call system behavior, because that is the NRC and the EPA safety requirements for 10,000 years, or 1,000 and 10,000; yeah.

DR. CANTLON: But system behavior clearly is the way that we've been arguing it should be that rigorously, but it isn't set off.

DR. YOUNKER: It doesn't jump out at you.

DR. CANTLON: Yeah, it isn't set off, and yet that is the crunch point and the interaction point that we have with the regulatory agencies, so it just seems strange to me that it didn't get identified as a separate category.

DR. YOUNKER: Well, each one of the qualifying conditions for every one of the guidelines does refer you back to that total system performance, and really asks you the question: Is there anything about the geohydrology, the geochemistry, the rock characteristics, the climate of the site that leads you to believe that it will not allow you to meet the 10,000 year requirements or the NRC requirements. So it's there, but it isn't as direct or, I think, as frontal as what you're suggesting it maybe should be.

DR. CANTLON: And I think the way in which you treated CO_2, in which you now went to the public health question as opposed to the regulatory guideline sort of signals that that should have been maybe a way that was put together.

DR. REITER: Leon Reiter.

Jean, a couple points. I notice that on your peer review panel there were four people listed whose specialty is tectonics. Now, I'm a seismologist. I love tectonics, but only one who listed his specialty as hydrology. Isn't that a little bit skewed? I mean, we all recognize that hydrology is really a key issue. You have one economic geologist, one petroleum geologist. How did you decide only on one hydrologist and four people in tectonics?

DR. YOUNKER: I think what we were trying to do was to make sure that each of the areas that we needed to cover, we really had a specialist to cover it. And some of these guys --I think like Tom Vogel, for example--you know, his real expertise is really in the volcanology part of tectonics, and so we had him for that. In the case of, well, Dr. Arabasz here, I think you know his expertise is really in the engineering side, in the seismic hazard. So I think those are sort of, in a sense at least, a little misleading because there's such diverse parts of tectonics.

If I had had my way, by the way, I would have been very happy--if I could have afforded it time-wise and money-wise--to have several hydrologists on the panel since hydrology is such a key issue for the site. I think having a saturated zone person and an unsaturated zone person, and maybe even a third, you know, people with different perspectives on hydrology would have really been very useful, but, you know, it's just--you have to decide how you're going to run these and go with it if you're going to keep it within the time and budget that you have set up.

DR. DOMENICO: Domenico.

I'll help you. It's hard to find another hydrologist on this planet who hasn't been associated with this project in some way.

(Laughter.)

DR. YOUNKER: Thank you, Dr. Domenico.

MR. GERTZ: This is Carl Gertz, and I'll just add, we did have a fairly comprehensive hydrology peer review about a year ago that did involve some of the nation's foremost hydrologists.

DR. YOUNKER: That's right. Yeah, we had one led by Alan Freeze.

DR. REITER: Again, I just wanted to follow through. Jean, I'm not quite sure about the Carbon-14, the ES--the early site suitability cites this as perhaps the most significant technical problem. There are words in there saying that there could be a 10 per cent or even greater chance of exceeding accumulative releases.

DR. YOUNKER: Right.

DR. REITER: And I'm not quite sure if your answer to this is, well, there is no health effect associated with that. Are you trying to preempt the regulations? I'm not quite--

DR. YOUNKER: I think what we recommend in the report is that the current discussions that are going on between the EPA and the DOE should continue on that question of whether that regulation is set at the appropriate level from the standpoint of health effects, and then we also suggest that it's one where I think we as a team talked about this probably more than almost any other issue; the question of whether you would be--whether it would be in DOE's best interest and be a prudent decision to spend a lot of money characterizing the site specifically to determine how it will retard C_{14} and other gaseous materials, but rather--and I think our recommendation is to take a balanced approach and look at potential engineering fixes for gaseous release as well, rather than recommend that you look at the site as your barrier for gaseous release.

So I think you'll find that we kind of tried to take a balanced approach and say, we don't know the answer, but we believe--continue to look at the regulation to make sure it's set at the right limit, and then look at potential engineering fixes for gaseous release, especially Carbon-14, and there is some effort, I think, recommended to look at the site's potential for retarding gaseous materials. And we do think there's quite a good chance. I mean, some of the people on the team were quite optimistic.

DR. REITER: But in the context of the exercise, and assuming the regulation remains--and we know there's an effort upon EPA to keep the regulation there--

DR. YOUNKER: Yes.

DR. REITER: --did anybody on the team feel that even a low-level suitability condition cannot be supported at this time?

DR. YOUNKER: Yes. As a matter of fact, we really did talk about that. If you took the letter of 960 and looked at C_{14} and said, you have a 10 to maybe higher per cent chance of exceeding the C_{14} release limits for 10,000 years, then by the letter of that criterion for total system, you could make the judgment that the site was unsuitable.

DR. REITER: Is there anybody who made that judgment on the panel?

DR. YOUNKER: We didn't make the judgment on the panel, but we certainly talked about the question of whether that finding could be supported.

DR. DEERE: Dr. Cantlon?

DR. CANTLON: Yes; Cantlon.

You're aware that we've suggested a sort of iterative approach to this. Do you have in mind now how you would proceed to set this in motion as an iterative process?

DR. YOUNKER: Well, we certainly recommend to the Department that, in a variety of different ways, that something like this should be done either at major decision points or, you know, on some kind of periodic basis, but I think Steve can answer that, and then, also, in Russ Dyer's comments tomorrow, he'll make some comments about that when he does the wrap-up.

DR. BROCOUM: One of the policy issues being asked at the Director's forum is: should this be done in a periodic fashion, or should it be done at the end of some major completion of major tasks. That will be discussed, I assume, in surprising detail at the Director's forum.

MR. SHAW: Bob Shaw from EPRI. First, a comment. I just wanted to echo the difficulty in determining experts for particular areas. We just conducted through EPRI an expert judgment workshop on the seismic arena in which we selected--it was either six or seven--I guess it was seven experts, and some people came forward after that to question those particular experts and how we went through their selection.

I would suggest that it may be even an area that the Technical Review Board might like to look into because I think the use of expert judgment is going to continue to be a very important feature of what we do, and some objectives with regard to how you defend the selection of a set of experts would be very useful and valuable, I think, for DOE in the continuing process.

Secondly, I had a question with regard to your summary; actually two questions. You have a list of items there that say additional information is most critical, and two of them I don't understand. The first one, the effects of climate change, what additional information could you be talking about here, and I have the same question with regard to the potential for natural resources to attract human interference. I don't understand what additional information you might be looking for.

DR. YOUNKER: All right. In the question related to climate change, the conclusion we reach on the qualifying condition for climate is that there is additional information that would give us more confidence in what the climatic conditions, the range of climatic conditions might be over the next 10,000 years, and I could ask Dwight Hoxie, who is the expert who wrote that section for us to comment specifically, but it has to do with additional field studies, I believe.

DR. HOXIE: Dwight Hoxie with USGS. Actually, I think what I'm going to do is talk about that when I talk about the geohydrology guidelines, so if we can wait for that, I will address that issue.

DR. YOUNKER: And with regard to the mineral resources or natural resources, the question that comes up here--and this was--the peer review panel member, Dr. Einaudi from Stanford really pushed us in this one quite a bit, although he didn't challenge the findings on our disqualifying conditions.

He very strongly suggested that there's a number of different types of site studies, most of which are included in general within the site characterization plans, although he did specifically suggest a couple that are in addition to what we had in the site characterization plan, to get a better handle on the mineral resource potential of the site. Because what you're asking there, Bob, is what's the potential that that area would draw human intrusion due to the fact that it looks like a good place for precious metals or for hydrocarbons.

The other person on the team was a petroleum geologist, who is a basin and range expert in petroleum, or in hydrocarbon potential, and he also, too, felt that there was additional specific information that we need in order to get a better handle on what kind of resource potential there is in the area.

DR. DEERE: Are there other questions from the audience?

Carl?

MR. GERTZ: Yeah. Don, I just wanted to answer both your question and Don Langmuir's question that certainly in 1993 we are going to be focusing on getting underground. We testified to that effect to a Congressional committee the other day, and in our current budget projections, we have quite a bit of emphasis on getting underground and there is speculation as attested to by four utilities that with an additional 70 million, we can increase or accelerate the schedule by a year to get into the main test level. So that is utmost on our agenda and in the forefront of our planning and thinking, is to get underground now as soon as possible.

DR. DEERE: Thank you. Let us take the coffee break. Excuse me.

DR. YOUNKER: I was just going to say I wanted to introduce the person who was going next, but if he's not going next I'll wait and introduce him after your coffee break.

DR. DEERE: Fine. Coffee break; ten-fifteen.

(Whereupon, a brief recess was taken.)

DR. DEERE: We have an additional question from the audience, or a statement on Dr. Younker's presentation, and this is from Senator Tom Hickey of the State of Nevada.

SENATOR HICKEY: Thank you, Dr. Deere.

In the methodology and the conclusions of early site evaluation, I was looking at the transportation issue and one of my concerns had to deal with the originality was we had three sites and then we made judgments on them on how well transportation was delivered to those sites. Up until now, and one of the problems that was presented at least in Nevada, was the building of 100 miles of railroad. The no conflicts due to location and access routes, I think it was fairly well-determined that one of the issues was going to be the movement of the waste by rail.

This would present a problem, and I think one of the issues lies in how the transportation is addressed, and that is almost on an isolated basis as concerns Yucca Mountain versus maybe a national level, and I think that has to be addressed. That's my only criticism, and there is at the present time no consideration of a national plan dealing with this site specific, and so somehow that has to be brought into your thinking and it may raise questions about these conclusions.

DR. YOUNKER: Well, I think the only comment I would have, Senator, is that the way we addressed this one as a team was that for all four qualifying conditions that are shown here, we did reach the conclusion that we didn't have adequate information to recommend anything but a maintenance of the lower-level suitability conclusion from the environmental assessment, and that was specifically on the basis of no information, no additional information really since the--or very limited information since the time of the environmental assessment. So I think the team would, you know, the team would say that's an area that needs some additional focus in the future.

SENATOR HICKEY: Thank you.

DR. YOUNKER: Well, since I'm up here again, I will then introduce Dr. Dwight Hoxie, who was our core team member who helped us out with geohydrology. There was actually a Sandia person, Dr. Les Sheppard, who was responsible for this evaluation but he has since left Sandia, and so Dwight had worked closely with him. We asked him to make the presentation to cover this one for you, so he'll talk about the geohydrology technical guideline evaluation performed by the ESSE core team.

Dwight.

DR. HOXIE: Well, I would like to talk about the geohydrology technical guideline. This is one of the postclosure guidelines, and Jean sort of preempted my introduction, because I did want to give credit to Les Sheppard. I just would like to remind everybody that the way the core team operated is that each of the technical guidelines was assigned to a core team member in the sense that these were the people that were to draft up information that had been gained since the EA, this kind of thing, and recommend some preliminary kinds of conclusions, and then the write-up, the draft write-ups were circulated among all of the core team members for review and comment.

This is in preparation of the original document before it went out for technical review, and as Jean indicated, I worked very closely with Les Sheppard on the geohydrology guideline. My technical guideline was the climate, climatic changes technical guideline, but since there is a strong interface between climate as input to the geohydrologic system, Les and I were, by necessity, had to work together very closely.

So I would like to try to accomplish two things. First of all, I would like to give you a summary of the results that we obtained for the geohydrology technical guideline, and I would also like to illustrate the process that we used in going through trying to evaluate these guidelines.

I think that I don't have to say a lot about the significance of a geohydrology technical guideline because it's our general idea that for any mined geological disposal system, radionuclide dissolution and transport in moving groundwater is going to be the primary mechanism by which we are going to release radionuclides from the repository to the accessible environment.

The thing is, is that the geohydrology technical guideline does not, in 10 CFR 960, does not take explicit cognizance of the possibility of gas-phased transport. We've already discussed that earlier this morning, and the point is, is that at Yucca Mountain anyway, we are examining a potential unsaturated zone site where we have this possibility of gas-phased transport from the repository to land surface through the unsaturated zone. We don't discuss that with the geohydrology guideline specifically. As Jean indicated, we discuss that in terms of the overall system guideline, which has to do with performance, postclosure performance of the system as a whole.

And if we are going to evaluate the geohydrology technical guideline in particular, we need to examine it in the context of the overall geologic setting. As I've indicated, at Yucca Mountain we are concerned with the unsaturated-zone system where the repository would be located if the site is found to be suitable and the license is granted to construct such a repository, and in conjunction with the unsaturated zone system, we have the site saturated-zone system, the local system in the saturated zone, and that, of course is imbedded within the overall regional groundwater flow system. So we have to examine the geohydrology technical guideline in the context of these essentially interconnected systems.

And so what I would like to start off with is just a very, very brief review of our present understanding and concepts of the geohydrologic systems, and this is going to be like watching a tennis match, because I'm going to bounce back and forth just a little bit, if I may. And I'm also going to cheat. I'm going to use a colored slide, which you don't have.

But first of all, I would like to talk about the site unsaturated zone system, just remind you of a few things and a few of our concepts regarding what we think that system entails, and I might point out first of all, in talking about conceptual models, that we are taking standard essentially unsaturated zone geohydrology and applying it to the thick, 500 to 750 meter thick unsaturated zone in indurated rock at Yucca Mountain. So there's a great deal of uncertainty here, and we're cognizant of that, and a great deal of our testing program at Yucca Mountain is directed towards trying to reduce our uncertainty regarding processes and conditions at the site.

But just to talk about the conceptual model a little bit, the boundaries of the system are going to be land surface, where presumably we have water entering the system as land surface infiltration that can then percolate down through the unsaturated zone. At the base of the unsaturated zone, the lower boundary is the water table, which of course is determined by the saturated zone system. Intervening between land surface and the water table, we have a sequence of geohydrologic units. These are units that in some sense or another have consistent hydraulic properties, porosities, hydraulic conductivities, store activities, this kind of thing, that we can define that act statistically, anyway, as distinct hydrogeologic units.

And in the case of Yucca Mountain, just to remind you, between land surface and the water table, we are looking at unsaturated welded and non-welded tuff, and the welded tuffs are a little complicated because they tend to be fractured. So when we're looking at the water moving through the system, we have to be very careful that we are able to characterize the interaction between water moving in the rock matrix, water moving in fractures, and interaction between the two, and the possibility that we can have transient flow, episodic flow moving down through the fractures essentially out of equilibrium with the surrounding rock matrix. So we're looking at a complex unsaturated zone hydrologic system.

And another important thing is that the non-welded tuffs tend to be more conductive intrinsically; that is, the matrix, the rock matrix itself is more conductive for water transport than the welded tuffs. So this adds to our degree of complication.

We have two very important non-welded units that are located near land surface, and another one that's located beneath the Topopah Spring welded tuff, which is the host rock for the proposed potential repository which on this very schematic diagram, would be located here someplace.

In order to characterize the unsaturated zone hydrologic system, we have to identify those processes that are taking place. As I indicated, our presumption right now is that we have water entering the upper land surface boundary as infiltration in response to precipitation events. In this, of course, we have a connection right there with climate because it's climate that's going to determine the amount of precipitation that's going to be available at land surface, and I will try to talk a little bit more about that when I talk about the technical issues associated with the guideline itself.

So once we have water that enters at land surface and is moving down through the unsaturated zone, we have to define where that movement is taking place, whether it's taking place in the rock matrix, in the fractures, a combination of the two. And presumably, another thing I should point out is that the non-welded tuff units tend to be relatively unfractured, so we may not have to worry about fracture flow there. So one possibility is that we have water moving down from land surface in the fractured Tiva Canyon welded unit that is exposed at land surface. It encounters the underlying non-welded unit shown here in green, which we just referred as the Paintbrush non-welded unit, and therefore, can move laterally, down-dip towards the east. So that we have vertical flow of water moving down and we have the potential for lateral flow of water moving in the non-welded units.

In addition, since we have an unsaturated zone site, we can have the potential for water. The overall moisture balance can be determined by water that's moving upward as water vapor, advected by gas-phased transport from the water table or from the capillary fringe above the water table, perhaps most effectively through the fractured welded units so that in looking at the overall moisture balance, moisture distribution within the mountain, we have to be cognizant of both the possibility of liquid water movement and gas-phased movement.

So our modeling efforts have to take this into account with all of the consequent uncertainties that are involved in defining the processes, in defining the boundaries for this system, and in defining the actual properties, hydrologic properties of the various units that we need to characterize.

And once we get below the unsaturated zone, we are able to enter the saturated zone system, and there are some very interesting things there as well, if we just look at a site scale. And so we're looking down, essentially, on a topographic map of Yucca Mountain itself, showing the perimeter drift of the potential repository here, and we are looking at various wells that tap the local water table at the site. And from the water levels measured in the wells, we can draw contours of the water table or the approximate potentiometric surface at the site, which are shown here for the contour intervals, where the contour is actually shown in meters above sea level, and there are some very interesting things to note.

First of all, if you look out here, we have no contours, and if you look at the individual data points, you'll notice that we have a very, very flat water table out here with an elevation of about 730 meters above sea level. However, as we go to the north of the site, in particular, we notice that the water table tends to steepen significantly, and we refer to this area up here where the contours are getting very close together, and you will notice we don't have an equal contour interval shown on here as the so-called large hydraulic gradient zone.

This is a feature that is present at the Yucca Mountain site apparently. It's based on not very much data. We have a well here; G-1, H-1. We go up here to G-2, WT-6--WT stands for water table--so we have essentially a 300 meter rise in the water table over a span of about two kilometers to the north, and analyses that have been done, modeling that has been done indicates that this particular large hydraulic gradient zone probably has little potential impact on waste isolation and containment characteristics of a repository in the unsaturated zone at Yucca Mountain. Nevertheless, it is a feature that we need to not only characterize, but understand.

Similarly, it would seem that we would need to understand why we have this flat gradient zone down here to the southeast. The regional water level, I mean, the direction of water flow is towards the south through here, essentially perpendicular to the potentiometric surfaces, but once we get done in here it's kind of difficult to determine exactly which way water is flowing, and one possibility is that this represents a region of very high transmissivity so it doesn't take much hydraulic gradient to move however much water is moving through the system. So that would explain the flat gradient.

Another possibility is that this is a stagnant body of water down here that isn't going anywhere at all, so we have at least a couple of conceptual models there that we need to deal with.

In order to come to better grips with the system, we would need to look at what kind of aquifers that we have that's conducting the water in the saturated zone, and all of the wells shown on here tap volcanic rock aquifer system, where the permeability seems to be largely due to fractures within the rocks themselves. That's what has been indicated by hydraulic testing in various wells.

However, we have one well sitting right over here just east of the repository site--known affectionately as 25-

P No. 1--which was a well that was drilled down to and drilled through the volcanic aquifer and penetrated the upper part of carbonate rocks, which are also considered to be a potential and effective aquifer beneath the Yucca Mountain site. But this is the only well that we have that taps that particular aquifer, and it encountered the carbonate rocks at a depth, as I recall, of about 1200 meters below land surface. So we have at least two possible aquifers.

And one thing is that the head that was measured in P No. 1 is about 20 meters greater in the carbonate aquifer than it is in the overlying volcanic rock aquifer, which indicates that there is a potential for water to move vertically upward at that particular point. The heads are higher down below, but that's the only information that we really have. We have some indications in other wells that we may have increasing hydraulic head with depth, also, but it is not good information.

So one of the things that will have to come out of our surface-based drilling program is to better define the relationship between these two aquifer systems and what is the vertical head distribution, the depth, and what impact will that potentially have on waste isolation containment within a repository at Yucca Mountain.

DR. DEERE: Question before you leave that slide.

DR. HOXIE: Yes?

DR. DEERE: Don Deere here. Did they have a measurement of the water level in the hole before it penetrated into the --into limestone on this case that you mentioned?

DR. HOXIE: Yes. They measured the water level before they went into the Paleozoic, so actually, they did it by packing it off.

DR. DEERE: Right.

DR. HOXIE: So the water level in the volcanic rock aquifer is about 730 meters, and then in the Paleozoic carbonate rocks, it's about 20 meters higher.

DR. DEERE: Thank you.

DR. HOXIE: I might mention something about the large hydraulic gradient zone, actually. If you read the ESSE document--what I'm talking about is this gradient shown here in the contours--if you read the ESSE document on the geohydrology technical guideline, you will notice that the large hydraulic gradient zone is mentioned only in passing, I think in the last or next to the last sentence on the very last page of that section or chapter.

However, as I recall, the tectonics peer review team personnel actually were very interested in the large hydraulic gradient in the sense that it possibly could be caused by some kind of tectonic process, and they pointed out and we recognized that even though the large hydraulic gradient zone may not have any adverse impact on waste isolation and containment at the repository in the unsaturated zone, it is a feature at the site that we need to understand, and there are essentially two conceptual models right now that have been put forward to explain.

And briefly, one of them is the idea that we have a dam. We have some kind of permeability contrast, essentially subsurface permeability contrast in this region such that water is essentially backed up behind it, and this could be due to something discrete, like a fault that we don't know is there, a buried fault. It could be an intrusive body. It could be a change in the rock fabric that would lead to hydraulic conductivity changes, or it could be a massive rock that has been altered hydrothermally or something like that which is at depth and we can't see it from land surface.

Another possibility that recently has been suggested is that we do, indeed, have a buried fault zone there, but instead of acting as a barrier, it's actually a conduit from the overlying volcanic rock aquifer downward into the Paleozoic carbonates. And so what we have here is kind of a case of aquifer piracy, like stream piracy, so that water is being diverted downward into the Paleozoic carbonate rock beneath Yucca Mountain and then out to the south.

And this is kind of a very nice model because if the water is doing that, moving down into the Paleozoic carbonates at depth where the heads are higher--at least out in this region--than they are over in the overlying volcanics, this region out here of flat hydraulic gradient may, indeed, be a stagnant water body and that would have--

DR. LANGMUIR: Langmuir. That second idea would certainly be testable in terms of the geochemistry's going to be very characteristic. If it's dropping, the isotopy and everything else should be very logically related. Has that been pursued?

DR. HOXIE: I think that is the pursuit. The problem is we don't have any wells up here that go deep enough, but the plan is right now G-5, I believe, is planning to be drilled up here and would penetrate the Paleozoics. It's going to be a very deep well.

DR. DOMENICO: Domenico. Do any of those wells penetrate the Eleana shale?

DR. HOXIE: It's not known. Well, no, they don't as a matter of fact, but I'm not even sure that the Eleana is recognized to be--it's not known to be here. There is a magnetic anomaly that some people have associated with the Eleana argillite, but...

DR. DOMENICO: I was under the impression that there's a paper coming out--well, Bill Dudley's a co-author--that that barrier coincides with a pinch out of the--not the barrier, the large gradient coincides with a pinch out of the Eleana; is that right?

DR. HOXIE: I think that my colleague is actually part of this team, but I will allow Bill to speak for himself, if I may.

DR. DUDLEY: The paper that you refer, Chris Fridrich is the senior author on that, and that is mentioned as one possibility is the Eleana shale, which is thought possibly to be present because of the magnetic anomaly which continues westward from the Calico Hills, could have been providing a cap over the Paleozoic rocks and that the southern end of the feather edge, then access to the Paleozoics could be possible. However, that is at relatively great depth and would be overlain, still, by a large thickness of tuffs that are probably poorly permeable.

Therefore, the preferred interpretation in that paper is basically of aquifer piracy, or the drain model based on a fault that is predicated both on stratigraphic information and, more particularly, on a gravity lineation that coincides roughly in position and orientation with the large hydraulic gradient.

DR. HOXIE: Thank you.

DR. DEERE: That was Bill Dudley speaking; USGS.

DR. HOXIE: Well, briefly, to look at the regional groundwater flow system, of course, this is very important because it determines the site, the configuration of the site water table ultimately, anyway, so that is of importance if we want to look at the possibility for water table rise, at least in natural conditions such as increased recharge. That could affect the repository.

So what we want to look at, just briefly, is putting it into perspective. What I'm showing you here is a map of the approximate potentiometric surface in the region, so we're looking down here. Here's Las Vegas. Here is Yucca Mountain shown right here; Death Valley located over here, just to give you some idea. Generally, the Nevada Test Site is located right in here.

And the potentiometric surface contours are here plotted in meters above sea level, and we've got some arrows in here which are showing essentially the general directions of groundwater flow, and effectively, we're looking at a not topographically closed, but hydraulically closed groundwater flow basin. So what we have are high lands up here to the north which receive recharge, and the recharge areas are essentially defined as any upland area that receives more than, or at least 200 millimeters of precipitation per year. That defines a recharge. The water enters the system up here, and then it tends to move south, perpendicular to the contour lines, and then to discharge at the southern end.

The particular discharge areas that we are concerned with are located down here to the south. We have a spring line that's controlled by a fault at Ash Meadows, which I believe discharges about 20 million cubic meters per year, but this is not an area--and this is also the area where Devil's Hole is located, the site of the famous Devil's Hole pupfish, which is of concern to Death Valley National Monument and also to us at Yucca Mountain.

But our concept right now is that the regional system that's discharging at Ash Meadows and Devil's Hole is probably deriving most of its water off the Spring Mountains and moving through the carbonate aquifer towards Ash Meadows, and discharging there. The water that's moving beneath Yucca Mountain is presumed to be moving further to the west and discharging down here at Franklin Lake Playa in California, or possibly being diverted beneath the mountains located here and discharging over in Death Valley.

And one thing I would like to point out on this regional map is here you see the steep hydraulic gradient zone north of the Yucca Mountain site, but you'll notice that in terms of the regional hydrology we have lots of large hydraulic gradient zones. We have one here that comes off the Spring Mountains, which is topographically controlled. We have a steep gradient zone over here descending into Death Valley, which again is topographically controlled. We have a steep gradient zone up here just to the northwest of Yucca Flat, and that presumably is controlled by the low permeability Eleana argillite, which is acting as a barrier, so that I just want to make the point that steep hydraulic gradients are very common in nature.

Nevertheless, we don't have a ready explanation for the occurrence of a large hydraulic gradient just north of Yucca Mountain, and this is something we need to pursue.

Well, with that introduction, let me get on to the business at hand of actually how we proceed with the evaluation of the geohydrology technical guideline. There's one qualifying condition and there's one disqualifying condition for this guideline. I will simply state part of the qualifying condition in all of its regulatory eloquence, and it simply says that: "The present and expected geohydrologic setting of the site shall be compatible with waste containment and isolation," and then it goes on to add a few specifics, specifically, that we will comply with the EPA release limits to the accessible environment; that is, those limits, whatever those limits might be that EPA decides is acceptable.

And then we also have to satisfy specific requirements that are promulgated in 10 CFR Part 60 that relate to allowable releases of radionuclides from the engineered barrier system. So our groundwater system has to be, or our geohydrologic setting has to be compatible with these requirements.

The disqualifying condition is an issue that has been talked about before this body before, and that is essentially groundwater travel time, and we are required that the "pre-waste emplacement groundwater travel time from the disturbed zone to the accessible environment must be less than 1,000 years," and the statement here, which is the 10 CFR 960 statement differs from the 10 CFR 60 statement of the same requirement by adding the term, "significant radionuclide travel." It has to be a pathway that can carry radionuclides. So these are the things that we needed to evaluate.

I just want to remind you again that in looking at the site suitability, unsuitability issues, a site will be unsuitable if we find that we cannot satisfy a qualifying condition, but it would also be deemed unsuitable if we find that a disqualifying condition is present. So if the geohydrologic setting is not compatible with waste containment isolation, we would not satisfy the qualifying condition, and if the groundwater travel time can be shown to be less than 1,000 years, then we would, unfortunately, satisfy the disqualifying condition.

The thing is, is that the way these regulation-derived requirements are stated, they're very hard to address, to get one's hands on. So what the ESSE did for the geohydrology guideline--and similarly for other guidelines--was to try to define specific--site-specific, actually--

addressable, technical issues that we could look at, examine, and evaluate. So we redefined or transformed the legalistic wording into something that we could comprehend.

And then I tried to identify the information actions that would be needed to address these technical issues, and as part of that, we went back to the environmental assessment analysis and looked at that to try to summarize the information that was available at that time and what findings were made from the EA, and I remind everybody that in order even to begin characterizing Yucca Mountain site, the EA had to find at least lower-level suitability findings on all of the technical guidelines.

Then we reviewed all the information obtained since the environmental assessment. We assessed the present status of the issues in light of this new information, and then we developed a set of conclusions and recommendations, and I would just like to run through those very quickly.

ESSE identified two technical issues for the geohydrology guideline, and the first one was conditions for sustained flow, as we call it in abbreviated form, and this is simply the occurrence of preferential pathways that would be capable of sustaining groundwater flow sufficient to affect waste containment and isolation. Our second technical issue was simply a restatement of the groundwater travel time, and I will get back to that one in just a moment.

What do we mean by conditions for sustained flow? Well, what we're really talking about here are the presence of some kind of preferential pathways through the unsaturated zone that could bring water in from land surface to the repository where it may encounter the waste packages and, therefore, affect waste containment; or we're talking about, similarly, preferential pathways from the repository to the water table, essentially, or to the accessible environment that could convey radionuclides from the repository to the water table, and subsequently, out to the accessible environment, again, looking at only groundwater flow and transport kinds of mechanisms.

So when we're talking about preferential flow and transport pathways--or some people would call these fast pathways for short--we're looking at things like faults or fractures, permeability contrasts within the hydrogeologic units, perhaps saturation anomalies, like perched water bodies and this kind of thing that could provide particular special pathways. But just identifying potential pathways is not enough.

We have to look at the spatial distribution to see if they would affect the repository in any way whatsoever, and we also have to look at the capacity of these pathways to convey water and radionuclides, and that is not enough because if the pathways are dry, they're not a problem. So we have to look at a way in which we could activate those pathways, and this is where the potential for climate change in the next 10,000 years really comes into play, because it would be presumably, by the occurrence of future wet periods --pluvials, if you will, in the next 10,000 years--that could provide the water to the land surface that could flow into these preferential pathways, activate them, and cause us a problem. So I think this is where we really need to be examining the climate issue to make sure that we have some kind of understanding of what the climatic regime might be like over the next 10,000 years. So that's my response to Bob Shaw's comment and question a little earlier.

DR. DOMENICO: Dwight?

DR. HOXIE: Yes.

DR. DOMENICO: This is Domenico. The first three bullets, you--of course, those are unfavorable conditions; is that not true?

DR. HOXIE: Well, not quite. I mean, we have the pathways, we know where they are and we know that they have sufficient capacity to pose as a potential problem. We look ahead and we say, "Well, we might be able to activate them," but then we've got to make another assessment, and what are the consequences, if any, for waste containment and isolation.

DR. DOMENICO: In other words, you go back to your first slide that said you must meet the EPA standard?

DR. HOXIE: Essentially, but this is where we tie right now to performance assessment. This is where the performance assessment calculations come into play.

DR. DOMENICO: Then my question is simply: What condition or conditions if you find, turning to hydrogeology, would in your estimation qualify as a disqualifying condition? What must you see down there? Obviously, not these. It seems that the disqualifying conditions will come from a model calculation or some other calculation on travel time and waste release solely, not on the basis of what we see once we get down there. Is that true?

DR. HOXIE: Well, I think the answer is, is that in--what we're looking at is the performance of the system. So we have to look at the radionuclide releases and transport to the accessible environment. So it goes beyond just looking for specific site conditions and features, but I mean, I can imagine something like--we have the Ghost Dance Fault that transects the whole unsaturated zone, and I can imagine dumping water down the beast, if you will. But of course, it may not have any consequences for waste containment and isolation because we may wisely not put any waste beneath the Ghost Dance Fault.

And I think the other thing is, in terms of identifying these pathways and characterizing them, once we get underground both in the host rock at the main test level and in the Calico Hills, we can walk down the drifts and if we see water pouring in, we might suspect we have a problem, especially under present arid climatic conditions.

DR. DOMENICO: Yeah, I understand. I'm just trying to get in my mind something straight that with regard to geohydrologic issues, the disqualifier will come on the basis of calculations.

DR. HOXIE: That's correct. I believe that is right.

DR. DOMENICO: Not measurements, not observations; calculations.

DR. HOXIE: Unless we really could see something like water pouring in that we couldn't explain, or if we had reason to believe that we had very extensive perched water zones that could ultimately cause us problems.

DR. DOMENICO: You would consider those disqualifying conditions?

DR. HOXIE: I think I would be a little leery if I had a large perched water zone above the repository. That's a personal opinion.

Is there a question, Bob?

DR. LUCE: Luce, staff for the Board.

Does engineering design come into that last bullet item that you have down there?

DR. HOXIE: I think that our directive was that we were supposed to be looking at the site and the site conditions and site features, so we did not really take engineering design into account except that we had the site characterization plan conceptual design, if you will, for a repository to use to guide us and, you know, we're trying to think about releases. So we had a waste package, conceptual waste package and the conceptual design for the repository, other than this idea that if we find we wouldn't place waste under the Ghost Dance Fault kind of thing.

DR. LUCE: What I was thinking about--

DR. HOXIE: So we did not look at design remedies to correct site deficiencies. We did not take that into consideration.

DR. LUCE: Okay. That was the question.

DR. HOXIE: We were looking at site intrinsicability to act as a barrier.

The second technical issue is what we called for ESSE the expected travel time, but it's really just the disqualifying condition for groundwater travel time. I've actually had the opportunity to discuss the groundwater travel time issue with you at an earlier occasion, so I don't really want to go back into that again. I just want to emphasize we have some problems with the groundwater travel time issue.

Conceptually, it may be very simple. We have a source point, A; we have some kind of compliance point, B; and a path in between of length, L, and we have some groundwater velocity, V. What we need to do to get the travel time is divide L by V, and voila, we have the groundwater travel time. But it's not that simple.

The repository would be a distributed source in the unsaturated zone. We're looking at complicated flow paths to the accessible environment, which is actually a compliance surface, it's not just a compliance point, and so it's very difficult to get a handle on what are the appropriate path lengths, velocities, and so forth. So there's a lot of ambiguities with trying to analyze groundwater travel time.

And one very important thing here that you will notice on this particular slide is that we have to define something around the repository called the disturbed zone, and you'll notice on this slide that disturbed is, indeed, disturbed, so in this case it's pretty simple to make that identification. But when we get out around the site it's going to be more difficult, because this is the zone that's been damaged, presumably, or altered by the construction process, and perhaps by the heat introduced by the repository itself.

There's another problem if you look at the disqualifying condition as it's stated, is it talks about the expected groundwater travel time. What do we mean by "expected"? Can we interpret that in the terms of probabilistic expectation and therefore, calculate CCDF's or this kind of thing and evaluate it in a statistical or probabilistic manner, which kind of makes sense. But then we have to define something which, what is an acceptable limit on our probability distribution for groundwater travel time.

And then there is the problem, we're talking about pathways for radionuclide travel. They have to be likely and they have to be significant. Well, I think it was the intent of our Technical Issue No. 1 for the geohydrology guideline that the pathways we're talking about there, the preferential pathways are just these pathways, the ones that are likely and the ones that could transport significant quantities of radionuclides. And so even though groundwater travel time sounds like something that you can simply calculate maybe on the back of an envelope by a simple equation, it really does not lend itself to a truly deterministic kind of approach, and we have to adopt some kind of stochastic analysis of the groundwater travel time.

And the important point here is, is that groundwater travel time cannot be measured in the field. We have to measure parameters and properties and perform calculations based on conceptual models for our site and for the processes that are prevailing at the site. There's a great deal of uncertainty in here, and so this leads me to the conclusion that--and, in part, because of all of the uncertainties that we recognize for the geohydrologic system at the site--that at the present we can continue to support the lower-level suitability finding for the geohydrology condition, but we could not recommend a higher-level finding at this time. We need more data, which will come from the surface-based testing program, as well as from the exploratory studies facility.

So our recommendations, in conclusion, are that first of all we need to get out into the field and identify and characterize potential pathways in the unsaturated zone. In the exploratory studies facility--and I might mention that many of the boreholes that are planned as part of the surface-based testing program are going to be penetrating specific features, like the Ghost Dance Fault, Solitario Canyon Fault, and so forth, in an attempt to characterize these particular features and their hydrologic properties.

We need to look at the non-welded tuffs as possible attenuators and mediators for flow of infiltration entering at land surface and moving downward through the unsaturated zone. This might be very important. The non-welded units above the repository horizon may act as an umbrella, and we have some evidence from our neutron, our shallow neutron hole program that these kinds of processes may, in fact, be working at the site.

We need to quantify the ambient hydrologic conditions from the standpoint that they provide the initial conditions for future calculations, and we need to look at the hydrochemistry, as has already been pointed out, in order to try to infer what the history, what's been going on hydrologically at the site and hydrochemically at the site.

And if we're going to take credit for the saturated zone, we need to determine what its hydrologic properties are, and so we certainly need to conduct pump tests, tracer tests, these kinds of things such as we have planned at the C Wells complex.

And finally--and not least by any means--is that we need to develop and refine our modeling capability; that is, not only our conceptual models, but our computational modeling capability both for flow and transport in the unsaturated zone and in the saturated zones, and we need to test these models, validate them either at the Yucca Mountain site or at some site where we have analogous kinds of conditions. So there are many challenges, and I thank you.

DR. DEERE: Thank you very much. Before you take off the slide you have there on your left, is the accessible environment also one of the unknowns here a little bit? You say "expected," what did they really mean by that, and "likely" and "significant," but how accessible environment?

DR. HOXIE: Okay.

DR. DEERE: Hasn't the thinking changed a little over the years on that?

DR. HOXIE: Well, I'm not sure. All I can--it is defined by the regulations, and the way it is defined right now is that it's that boundary in space that is five kilometers distant from the repository, the perimeter drift, essentially.

DR. DEERE: And how would you access that in your groundwater flow model? Because this is the bathtub full of water down there you have.

DR. HOXIE: Well, I think that--I think what we--we have the potentiometric surface for the saturated zone, so if we can develop our models to look at transport through the unsaturated zone to the saturated zone, then I think we can probably, with some degree of reliability anyway, use standard saturated zone modeling techniques to examine--

DR. DEERE: Move it laterally.

DR. HOXIE: --the movement out to the accessible environment. Of course, the accessible environment is also the air mass above the repository for gas-phased transport, and that's a lot closer than--

DR. DEERE: Because could you have the situation where the water table is with the very low--the deep water table with the very low hydraulic gradient, and it's either moving very rapidly through some very permeable material or, as you say, it's pretty much stagnant. Couldn't you, in some cases, have the water go right through from the surface right down to that zone and still maybe not go very far horizontally?

DR. HOXIE: I think that's right, but that means we need to understand what's going on out there in that flat gradient.

DR. DEERE: Absolutely.

DR. HOXIE: And that's an order of business.

DR. DEERE: Thank you.

DR. HOXIE: Yes. You're welcome.

DR. DOMENICO: This is Domenico. I don't think static is the right word here. That implies no movement. Whether it's a barrier there or a drain, what you've done is decreased the flow that's downgradient from that--from whatever that--well, from that steep gradient. You've decreased the flow in the system such that the hydraulic has got to flatten out. It doesn't--but static means something else.

DR. HOXIE: Well, said stagnant, actually, but I was thinking of a pond of water and I was taking a little liberty with the metaphor.

DR. CANTLON: Dr. Cantlon. Is there--there were--some of the nuclear device fallout isotopes were found in some of the perched water there on Yucca Mountain.

DR. HOXIE: It was not found in perched water. It was found in the surrounding rock in, as I recall, the UZ-1, and it's Chlorine-36 at some depth, a couple hundred meters or so.

DR. CANTLON: Right.

DR. HOXIE: We don't know where that came from, unfortunately, and there is--I don't want to--there is some possibility that it was contamination occurring during the boring of the hole itself. There is that possibility, but on the other hand, we have ample evidence from tritium, and also from Carbon-14 in the shallow zone, that we have the potential for fast pathways, at least above the non-welded units.

DR. CANTLON: Right. Now, do any of those materials show up where the water is coming out in the spring?

DR. HOXIE: I don't think so. We've got Carbon-14 dates, but we don't--

DR. CANTLON: None of these more recent isotopes?

DR. HOXIE: Not the more recent ones, not the bomb pulse-type isotopes.

DR. DEERE: Are there questions from the audience, or comments?

(No audible response.)

DR. DEERE: Thank you very much, Dwight.

DR. HOXIE: Perhaps I should introduce Dave Kreamer, who was one of the peer review team members for geohydrology guideline.

DR. DEERE: I might mention, Dr. Kreamer is Associate Professor and he's Director of the Water Resources Management Program at the University of Nevada at Las Vegas. We're very pleased that you could come and speak to us on this subject.

DR. KREAMER: Thank you very much. I very much appreciate the opportunity to come and address you. In spite of Dr. Domenico's comment on the paucity of hydrologists associated with Yucca Mountain, I want to vehemently deny any association with extraterrestrial beings or bodies or anything of that sort.

I feel privileged to have been associated with some of the scientists involved in the project, and what I would like to do this morning--and I guess I have two minutes remaining in my time--very briefly tell you a little bit about the process.

I was invited to speak here by Leon Reiter, to speak candidly about the processes and the findings of the peer review group and, in particular, the hydrology and hydrogeology that I looked at. I want to say, first of all, that's it's very difficult in a two and a half month period to review the hydrogeology of such a complex system, but I want to also acknowledge right off the bat that there were some terrific people involved in the project who helped. They were very giving of their time. Very often they were able to go for several hours on the telephone and answer questions that I might have, and I know they have a very busy schedule.

The project personnel, I think, should be commended. I also think that from my own personal point of view, they very carefully tried to give an honest assessment and not be swayed by political issues, but more they were involved in the technical issues of the hydrogeology of the site.

In the review of the documents, I also reviewed a group of hydrogeologists who looked at the unsaturated zone in the previous year. That's the group of Alan Freeze, Dr. Alan Freeze, et al., with some very eminent hydrologists and hydrogeologists and their study of the system.

The short time frame did allow for some problems for the peer review. Obviously, in ten weeks you can't very well find out everything there is to know about a site when you haven't been associated with it before, and one of the things that Freeze, et al. suggested that go on with regards to hydrogeology was more integration of the technical reviewers, and this was echoed by some of the peer reviewers; Dr. Vogel and Dr. Webb both suggested that more integration of hydrology of the climate and hydrology with the tectonic group were appropriate, and I know that the review team has made a real effort to integrate that.

I want to point out, though, that in spite of a consensus document that some of the geotechnical group put together, I don't feel as though we really interacted on any strong basis as far as a peer review group, and more or less, our findings were fairly independent. We did come up with a consensus document and I made one or two personal addendums to that consensus document that is in the appendix of our report, but one of the disadvantages of the short time frame was an inability of the peer review team to really interact as fully as we would have liked.

I must say as far as the process goes, too, there was something that was somewhat distasteful to me, and that was the fact that we were given three choices: highly suitable, low-level suitability, and unsuitable. Usually, in scientific decisions, you have a whole range of possibilities, and particularly the hydrogeology, which I feel there is a high degree of uncertainty in where we're at right now with the hydrogeology. I felt rather uncomfortable with any sort of statement of likelihood of site suitability. I felt much more comfortable with a recommendation that a site characterization was suitable, but as far as the overall likelihood of the site being suitable from a hydrogeologic standpoint, I think it's really too early to tell, and that's probably one of my major bottom line conclusions on the hydrogeology.

This uncertainty in the hydrogeology, the high degree of uncertainty has a couple of spinoffs. Because we haven't gotten underground as much as we ought to, myself and several of the other reviewers recognized that we really ought to look at site-specific data and relate it directly to the models we're generating. There's been a fair amount of modeling effort that's gone on already at the site. I am somewhat skeptical over the total utility of these models without proper grounding in site-specific data, and so, therefore, I think we're at a point now with the site that all of the peer reviewers that I spoke to feel that site assessment should go on and we should actually do much more site-specific testing and get underground.

The ESSE, early site suitability evaluation document itself points this out. There are several comments that the confidence in the models is limited by lack of site-specific data; that the models are based on many simplifying assumptions that should be verified using site-specific information; that analyses have been conducted, however, with a limited amount of hydrogeologic data set using models that may not correctly approximate dominant conditions. I would like to echo that, and the comments of the peer reviewers--not just myself--but several peer reviewers felt like they needed to make comments on the hydrogeology, and I chose one of my own and one from another external peer reviewer, Dr.

Hodges.

"Without adequate site-specific field data that could establish realistic bounds on in situ permeabilities in saturated and unsaturated zones at the scale of the facility," Dr. Hodges would be skeptical about any hydrogeologic models of Yucca Mountain. I stated that: "Predictive approximations have to be grounded and must be grounded in appropriate, defendable assumptions," and therefore, feel that testing of those assumptions is imperative at the site.

There is a possibility that the hydrogeologic system eventually will not adequately be able to be characterized from some hydrogeologists' point of view, or at least with a reasonable certainty. I think that there are tools certainly to maybe not allow us to get in that position. As Dr. Langmuir pointed out, I think geochemistry is one of the keys.

I'm personally a big proponent of geochemical techniques. They've already told us a lot about this site and, in fact, in a meeting in Tucson on the saturated zone hydrology, which I attended--and Dr. Domenico was there, also--I suggested that we should be reluctant to do hydrogeologic testing or hydraulic testing for the saturated zone if it in any way might have the possibility of upsetting the geochemistry of the site. I think that we can proceed--

we don't have to proceed so quickly in getting the hydraulic information as to disturb other things. I think we have to be cautious in that regard.

I also want to mention that the hydraulic considerations and the large hydraulic gradient, to establish the credibility of the program, those have to be further looked at. We have to develop much greater site-specific information on those. As far as the high--the large hydraulic gradient, I am actually more concerned with a small constrained drain off of that that might go through the unsaturated zone. Some connection between that gradient and the unsaturated zone, I think, would be probably the worst case scenario.

Some preliminary models that have been done on the dam scenario, if there was a dam break, it probably still wouldn't reach the site from some of the models I've seen, but if there were a small constrained drain that would go through the unsaturated zone, I think there is a possibility that that might bring water into the site in a sustainable way.

The site is a varied site. Hydraulic considerations are very important. Test of the hydrology two months ago show that with several days of pumping, no appreciable draw-down in one well in Fortymile Wash. I think this indicates perhaps high hydraulic conductivities and what has been referred to as a zone of stagnation. The high hydraulic conductivities have up sides and down sides. Probably if there is high hydraulic conductivity, perhaps tectonic events might not force an up-welling of water as easily, but by the same token, it might create a situation under a hydraulic gradient, you might be able to get faster flow in that flat water table area where there is perhaps high hydraulic conductivities.

So dealing with the possibility that it is possible that we may not get enough information to easily characterize the site with any significant uncertainty--without significant uncertainty--I think that we again have to turn to site-specific data and get underground, and I would urge that the models that we develop and the things that we look at according to the site not only be based on models; that the models, by necessity, be verified at the site by site-specific data.

My own conclusions for the hydrology is currently there is not enough defensible site-specific information to accept or reject the site; that the site is acceptable for continued characterization; that it's premature to state the likelihood of suitability; and I found the three categories of high-level, low-level suitability or unsuitability a little bit difficult when, in fact, I think we're at a fairly early stage as far as saying the site is likely to be good or unlikely to be good. I believe that site characterization should continue.

I have further recommendations as far as postclosure goes. Because of the potential uncertainty, I would like to suggest that the waste packages be easily removable. An idea of retrievability, I guess, is built in right now. I would also like to suggest that the waste packages and engineered barrier be inspectable, and the waste packages and engineered barrier be able to be modified or corrected with time, particularly when you consider in the last hundred years the scientific progress we've made and what might occur in the future. There are even such ideas as eventually being able to transmute the waste, but certainly, we may--we very likely will have improvements in engineered barrier systems. I believe that a system for emplacement that cannot be modified has some distinct disadvantages, so I would like to advise a correctability or an ability for that to be modified.

That's what I had to say.

DR. DEERE: Well, thank you very much.

Questions from the Board?

DR. NORTH: Warner North. I'd like to ask the extent to which you reviewed the performance assessment calculations. As Dwight Hoxie pointed out, there is really a conceptual leap as you go from the detailed information to the calculations that is required to address the suitability conditions.

DR. KREAMER: I agree with you. There is a conceptual leap, and there are many, particularly with the pathways and the determination of pathways, there are many, many simplifying assumptions that go in. I view the models used right now to be in a testing way you can test different scenarios and see what would happen under different scenarios, and therefore, that's the utility of the models as far as I'm concerned at this point. I think that it's too early in the game to place much credence in any probabilities that have been developed. I'm not sure I answered your question adequately, Dr. North.

DR. NORTH: I'd like to ask you also about the small constrained drain scenario which you described.

DR. KREAMER: There has been a little bit of debate, and as I talked to several members of the committee, on saturated zone hydrology, a couple of the members of the peer review committee expressed concern over trying to--particularly the vertical fractures, to try and get a good handle on what's happening. Usually, if you drill boreholes downward, you may get a good idea of perched layers or horizontal layers, but the vertical fractures you might not.

If there is water flow that might come off a large hydraulic gradient to the north and it were to be able to come in a constrained pathway, then it might be sustainable in significant quantities, but yet, not affect the overall hydraulic gradient long term. If it were a large and not a constrained drain, or if the drain were merely down a fault, as one scenario is given, perhaps there's less of a problem because then what you see now is what will occur. But if there are small constrained fractures that would move out from the large gradient into the unsaturated zone in the repository area, then you might have a problem in that regard, and I actually consider that to be one of the things that has to be looked at as far as evaluating the site for its credibility as a hydraulically safe site.

DR. NORTH: Let me see if I can restate that a little bit. My concern is are there situations where basically we will never be able to resolve the uncertainty adequately by the underground exploration that we're prepared to do? And if we have a potential constrained drain scenario, in your judgment, are we likely to be able to learn enough in underground exploration so that we can put some bounds on that?

DR. KREAMER: I think that the possibility exists that we can. I've made some very specific suggestions in my review. I think we should go back and we should re-video all the boreholes that haven't been done in several years to see if seeps have opened up under different hydrologic conditions in some of the fractures. They have been TV-logged once, but everything from specific suggestions like that, I think, as Dwight Hoxie mentioned, Dr. Hoxie suggested that once we get into the drifts and walk through them and assess the site in a very particular way, we will get information that will be helpful. So, yes, I think that there is a possibility we will able to glean more information that will get us in a more acceptable mode than we presently are.

DR. DEERE: Dr. Cantlon.

DR. CANTLON: Yeah; Cantlon.

Since most of the other countries that are looking at this problem don't have the luxury of being in an arid region, they've all coped with hydrology in a much more wet situation. I gather from your last slide that you're, in a sense, visualizing an engineering solution as opposed to expecting the hydrology to be perfect. Am I reading you correctly?

DR. KREAMER: I think that under the--by dint of the fact that we may have significant uncertainty at the end of the process, I think that in looking toward engineering systems might not be a bad way to go.

DR. DOMENICO: Domenico. In view of all the uncertainty that's been waved here, what was the finding in geohydrology? Because you were apparently the hydrogeologist and you seem a little--

DR. KREAMER: Well, it was a low-level suitability.

DR. DOMENICO: Low-level suitability?

DR. KREAMER: Low-level suitability.

DR. DOMENICO: And if you had other options?

DR. KREAMER: I would say it's too early to tell. If there was something that straddled the line a little bit more, I would be more comfortable with that.

DR. DEERE: Don Deere here. I have a little more confidence, I think, that they will be able to get a pretty good handle on the hydrogeologic model, but it takes the exploration to do it. We have--any time you build a reservoir in a limestone or karstic area, there is always concern that the water will not accumulate behind the dam, and this has happened on a few dams, where the water simply flows out faster than it comes in, which shows a terrific permeability.

But on many other projects, they have been successful, and what comes out is that your groundwater level in the countryside around the reservoir rises rather rapidly to the reservoir level, and we have a very, very flat hydraulic gradient. I'm thinking now of a project in Mexico, another one in Greece, where they extend literally 20, 30, 40 miles in the limestone, but the whole thing is now maybe 50 feet higher than it was before the dam was built, and this has made some very interesting springs, because where underlying shore units intersect the surface and that contact comes up, you suddenly have a perfect new spring develop, and although the people who built the dam were sorry about losing 20 cubic meters a second, the farmers who lived along that shale outcrop had the best water they've ever had in their life and their irrigation is very cheap.

But it takes a lot of drilling and it takes a lot of piezometers, and I particularly would like to recommend once more the use of the multiple point or multi-pore type piezometers that can pick up water levels every five meters if you want, because they have a built in piezometric head, where this has gone in both in saturated and unsaturated zones and picked up some perched water, picked up deep water, and what we have found is there can be some very complex hydraulic pressures and hydraulic gradients in just a few feet as you get into a different fracture system.

And usually, we find also the geochemistry is different, the temperature is different, and so it's not a simple integrator, but where we have the karstic conditions where there is a lot of solutioning of the limestone, then we tend to have a fairly uniform condition.

I can't help but think, having seen this several times, that when you get more piezometers down into the limestones and then multiple piezometers so you're also in the volcanics above them, that things will simplify and the picture will become clearer, but it does require a lot of exploration.

DR. KREAMER: There is some indication that there is up-welling into the site, as I'm sure the presentations to the Board have indicated, both geochemically, and some piezometric data, but it is limited at this time. I'm not sure that the farmers in the Amargosa Valley really want springs to develop down below Yucca Mountain, but still, the idea that there might be some up-welling downgradient of the site, and there is some vertical component of the hydrogeology is certainly a consideration that has to be considered with travel times.

DR. DEERE: In the--Don Deere again, if I may continue for a moment.

The idea of a pinching out of a shale unit with higher perched water levels in the shale and then a very deep groundwater in the limestone is a condition that has been met at several dam sites. I'm thinking of one now near Delphi, Greece, where the reservoir came up on the shale very nice and tight and the water level was fairly close to the little creeks and small rivers that were in the area, rising gradually. Then all of a sudden you came to a limestone outcrop and the first boring there, going 10-20 meters, didn't encounter the water, although it was just a few meters away. They had a water table in the shale, and there we found the water level was 300 feet deep. So there was a much greater hydraulic gradient than you have here by a factor of probably 500 times greater.

And this was such a karstic limestone running out, that it actually runs right out to the sea and out in front is where they have the fresh water boiling up, and as has been known by the shipping people for centuries. But it sort of affected the reservoir that was going to inundate a piece of that limestone, so the solution there was an engineering solution. It was to cut off and put a new dam just up against the slope so that the reservoir did not come in contact with this permeable limestone that had a very very deep groundwater level.

But again, it takes exploration and borings to do it. I recall when this problem first came up about three years ago, when we had a presentation on one hole, hit the deep limestone, and the water came up--and I don't remember the distance, but I believe they said 50 feet today, something like that. Well, we here know much more about that, and in looking at the program that was projected, I made the comment: "It looks to me like you're going to have to get more information on that deep groundwater level of the regional system."

Bill, you may remember, have they added to that original program? Because it appeared to me they were not going to go much down into that deep water, into the deep aquifer.

DR. DUDLEY: This is Bill Dudley. As far as having added to the system formally in terms of changes to the baseline, that has not yet been done, but there are several sets of recommendations, including those in the ESSE document, and in recommendations, I believe, that are evolving in a follow-up task, called the Integrated Task Evaluation Effort, that do all lean, for several reasons, towards some exploration of the deeper materials; Paleozoic rocks specifically.

DR. KREAMER: I might add that there were specific recommendations on the hydrogeology, and one of the-- conversely, I also believe that the water table should be carefully monitored for water quality as an end member to the unsaturated zone for both the aqueous part of the unsaturated zone, but also the gaseous phase in the unsaturated zone.

DR. DEERE: Thank you.

Questions from the audience or staff?

(No audible response.)

DR. DEERE: All right. Thank you very much.

We'll move on to the next speaker, Dr. William Dudley of the USGS. He's speaking on the disqualifying condition of 10 CFR 960 technical guideline for postclosure tectonics.

DR. DUDLEY: I'd like to note that in this presentation, in contrast to Dwight's, we will be discussing only the disqualifying condition of the postclosure tectonics technical guideline, whereas Dwight discussed both qualifying and disqualifying condition.

I'd like to begin the presentation by showing that we're all on a common basis, that we all have a consistent understanding of the wording of the guideline. Then, as was noted by Jean and again by Dwight, I'd like to go back and revisit what the environmental assessment expressed with respect to the site's status for this guideline; then to develop some of the very general considerations that have to be taken into account in the specific approach; then to describe that approach; and finally, to summarize or to reach the conclusions.

Turning first to the statement of the disqualifying condition, many of you have read this many times. However, it states: "The site shall be disqualified if--", and very specifically, "--based on the record during the Quaternary Period, the nature and rates of fault movement and other ground motion are expected to be such that a loss of waste isolation is likely to occur."

We get two of our words here, "likely" and "expected" that the ESSE team had to define for themselves, what were their levels of comfort or discomfort. I think, in general, the term "likely" was understood by the team members to be--or "expected," I'm sorry--to be more likely than unlikely and--

(Laughter.)

DR. DUDLEY: --that "likely," unless it was used to define expected, generally could be as small as a 10 per cent or even less probability.

Moving then from the statement of the disqualifying condition, let's review the environmental assessment findings. Now, this map, I apologize, is not in your handout. We added it in at a late date, and somehow it did not make the package.

First of all, the environmental assessment which the DOE released in May of 1968 (sic) reached a lower-level finding; that is, that the site was not believed to be disqualified by the tectonic condition, but that it was not--there was not a significant degree of confidence that future information would not prove otherwise, so that the higher-level finding could be reached.

Now, this expectation was based on a peak ground motion of about .4g. You'll find the site located by X here, and rather than one of the several faults that are quite close by the site, that ground motion was expected to result from about a 17 to 18 kilometer long movement on the Bare Mountain Fault, which is about 14 kilometers west of the site.

It was recognized that, in general, subsurface ground motion is somewhat less severe than that at the surface, but that was not brought out prominently in the EA description. In the EA, it was decided that one could not be confident that some containers would not rupture; therefore, they stated some containers could rupture from movements on faults that would intersect the repository. However, the basis for maintaining a low-level finding here was based on the hydrology, that that would be a small consequence. A smaller number of containers would be ruptured by a linear feature randomly oriented with respect to the grid work of waste canisters; that the groundwater flux is and is believed in the future to be relatively small. Again, that is one of the questions that comes up with respect to the climate guideline; and that there was a long groundwater travel time with lots of opportunity for retardation by geochemical and diffusive processes, things of that sort.

Some of the general considerations, then, that the early site suitability evaluation team considered were, first of all, what could we do and this, of course, was generic to all the guideline evaluations, and Dr. Kreamer wishes that there another--oops, Dr. Kreamer is not here--that there were another choice between here. Certainly, we could support the lower-level finding that was expressed in the environmental assessment. We could reverse that lower-level finding, disqualifying the site, or possibly raise the finding to a higher-level finding based on an expectation that future investigations would not reverse the expectation that the site is not disqualified.

Now, some of the disqualifying conditions read like the converse of the qualifying condition. Others, such as the groundwater travel time with its 1,000-year pre-waste emplacement travel time are quite a bit different. In this case, they sound a little bit the same. The site will be located in a geologic setting where future tectonic processes or events are not likely to lead to releases greater than those allowable under the NRC and EPA regulations.

However, the disqualifying condition has a narrower focus than just the converse of the qualifying condition. Nonetheless, it is completely imbedded in the qualifying condition. Therefore, the considerations that are given to the disqualifying condition will come back to be revisited automatically because they are fully imbedded, but they'll be revisited with a requirement for a greater level of confidence and more information upon which to base a future decision.

The key provisions of the disqualifying condition really, that are somewhat different from the disqualifying are that rather than just future processes or events, it's those that are documented in the Quaternary geologic record, not hypothetical; processes and events that one might associate with the overall geologic setting.

The disqualifying condition calls for an evaluation of expected conditions, not those that are probabilistically somewhat credible, and expected conditions, then, that are consistent with the Quaternary record. Further, it's restricted to fault motion, or fault movement and ground motion, not to all tectonic processes that might be associated with this geologic setting; for instance, hydrothermal activity that might well up into a repository if it were constructed at Yucca Mountain. Volcanism was mentioned, of course, and that figures prominently in the evaluation of the qualifying condition, but does not figure into the evaluation of the disqualifying condition, which is something that we can evaluate with perhaps a limited data set and early in time, get some sort of a feeling as to whether the site should be walked away from or whether investigation should proceed.

The disqualifying condition does eventually come down to a consideration similar to that of the qualifying condition in, that is, the allowable releases to the accessible environment.

This brings us down, then, to the actual approach that the ESSE committee took. We decided to express our approach to the--to this condition with two questions that would be considered sequentially if the process was completed.

First: Is there a likely tectonic cause associated with either faulting or ground motion that would lead to a loss of containment of the waste within the engineered barrier system? And that should be--I should have said an expected tectonic cause; again, based on the Quaternary record. If no, then tectonism is not expected to be the reason why the site would be disqualified if it were disqualified, and therefore, we would have answered that question negatively. We would have answered the disqualifying condition in the negative sense that the site is not disqualified. If the answer to this question is yes, then we have to look at this as providing some sort of a source term, and go on to a second question:

If there is a loss of containment, is that likely to result in a loss of waste isolation? In other words, releases to the accessible environment that exceed the regulatory allowances. This, of course, may require a system performance assessment, and if the answer from that turned out to be no, the site would not be disqualified; if yes, then the site is disqualified.

Now, if it were to go to the stage where a performance assessment calculation were required, then whether that could be done at this early time would depend on the degree of confidence that was developed in the performance assessment models. As was discussed earlier with respect to the hydrogeology guideline, there would be considerable question.

DR. ALLEN: Wait a second, Bill. Clarence Allen.

DR. DUDLEY: Yes.

DR. ALLEN: Again, the definition of the word "expected" in the first line there--

DR. DUDLEY: Yes, sir.

DR. ALLEN: --that was a 50 per cent probability?

DR. DUDLEY: That's the way the ESSE team felt about it, yes, sir; as opposed to likely in the probabilistic performance assessment mode. I think that we were well, in terms of a comfort level, on the positive side of expected, so we could have defined it a little lower and still reached the same conclusion, I believe.

Continuing with the considerations, then, and those applying to the first of the ESSE questions, there are three basic modes in which there could occur damage to the engineered barrier system and then, thus, releases of waste that would result from fault motion--fault movement or ground motion.

First of all, fault displacement itself intersecting the waste product, producing a rupture; or even stress, bending such that corrosion would be concentrated and release at greater than expected rates. This, then, would require some understanding of the dimensions of faults and their orientations relative to the waste emplacement pattern, and secondly, of course, estimates of the amount of fault displacements that would actually occur. Would the entire emplacement hole be sheared off and dislocated, or would a waste package merely be crimped a bit within such a hole?

Secondly, ground motion could, indeed, depending on the design of the EBS, be expected if it were severe enough to produce ruptures or, again, just a stress concentration within the waste packages. Some of the considerations for that, of course, would be the general ground motion spectra relative to the EBS dimensions.

Third, then, hydrology does come into this, that if either faulting within the repository or ground motion could lead to hydrologic changes that might accelerate the rate of waste package degradation, then we would have to say that tectonics has a credible possibility of eventually leading to a loss of isolation.

Now, with respect to fault movement, let me keep this on just for a moment. Since the environmental assessment, there was a recognition that the Bare Mountain Fault probably is not the controlling seismic source, seismogenic source; rather, there are faults in the vicinity of the site.

The longest of those that has a history of Quaternary movement is the Paintbrush Canyon Fault, extending at least 20 kilometers, perhaps a few kilometers more, and being much closer to the site, so that since the environmental assessment--and this is true for the site characterization plan--the Paintbrush Canyon Fault has been considered to be the principle seismogenic source. That fault, again, is shown as the eastern limit of this area here.

I'll point out the site, which has not been put onto this--the map is cluttered enough as it is--the site for the potential repository basically lies just to the south of this fault, known as the Drillhole Wash Fault, stays to the west of the Bow Ridge Fault, which marks an area of relatively closely-spaced faulting, stays to the south of an area where faulting, again, increases southward--this stays to the north of that--and then it is bounded on the west pretty much by the Solitario Canyon Fault. This field, of course, as this Board is aware, does include a fault known as the Ghost Dance Fault.

Now, the faults that are shown here or by virtue of dotting are concealed, inferred, or some might say a figment of the imagination in some cases, at least in terms of the directions that they go, but identified from a number of investigations. There's been very thorough geologic mapping, hard looks at the surface, walking over that surface. There have been geophysical surveys that have been run across here in a stack of rocks with varying magnetic properties, such as these volcanics; the detailed magnetic investigations as well as the low level aeromag investigations are very likely to pick up any faults of any significant displacement at all.

There have been investigations by remote sensing of various types, and detailed geomorphic studies, although there is more yet to be done in all of these areas. They generally have not identified any evidence of faults that have Quaternary movement within the potential repository boundaries.

DR. ALLEN: But Bill, can I ask--Clarence Allen here. Can I ask a question?

DR. DUDLEY: Yes.

DR. ALLEN: This is the first time I've ever seen a statement in writing that I can recall that says we can document no Holocene displacement in the Ghost Dance Fault.

DR. DUDLEY: The one problem is the Ghost Dance Fault, and what we can document is that where it has been covered by alluvium and where it has been trenched--now, this is three locations out of a dozen or so that may have an alluvial cover over the Ghost Dance Fault--that there is no movement within those. Now, those deposits are estimated by John

Whitney to be not more than about 30,000 years old, so that we would say there's no Holocene movement. We are left with a lot of the Quaternary that we could not base that statement on.

However, the geomorphic indication is that we do not have a young scarp, a Quaternary scarp, certainly not a Holocene scarp along the trace of that fault.

DR. ALLEN: That isn't quite the impression I got from talking with John. I think he feels, yes, there's probably no Holocene displacement, but to be able to document that is a different story.

DR. DUDLEY: That's one reason I got John to review this. We certainly would--I think in the ESSE report it's stated very much at about that same level of strength. This may be somewhat stronger. It does indicate that the evidence is somewhat limited in that we have only a thin veneer, and we don't have even that thin veneer over the entire thing. However, there is, again, the geomorphic evidence that we do not see what is typically--the type of geomorphic expression that is typical of Quaternary faults in the basin or range.

All the faults for which we do have--demonstrated or based on geomorphic considerations--likely Quaternary movement do lie outside the potential repository boundaries and they accumulated most of their displacement during the Miocene; that is, between about 13 million years and perhaps

7 million years, six to seven was the period of greatest displacement, and actually, the paleoseismic interpretations are that actually between 13 million years and about 11½, that most of--or the highest rates of displacement occurred; from about 11½ down to the beginning of the Quaternary at about 1 3/4 million; that these rates were less by about an order of magnitude, and then during the Quaternary relative to that previous 10-million year period, they were less by again factors of three to ten, something of that sort.

Therefore, the faults that we know have occurred in the Quaternary, and which are likely then to have recurrent movement in the future, do not intersect the potential repository area.

The in situ stress is measured by hydrofrac measurements in boreholes as--at least in terms of directions, as inferred from borehole breakouts, and is consistent generally with a west/northwest direction of extension, and that direction then is somewhat consistent with continued movement on faults that are sub-perpendicular to that.

The extension axis for a much larger area of ground from earthquake focal mechanisms is more durable than that we've seen in drill holes right at Yucca Mountain, but it is, in general consistent with a west/northwest direction of extension again.

These do suggest that as long as there is a reasonable coincidence between the extensional axis in terms of crustal stress and that which would be required for these faults to move, suggests that these faults are liable have any movement that takes place in the near future, such as 10,000 years, rather than force the crusts to initiate new faults.

We're getting some additional lines of evidence that suggests that these north/south faulting directions, particularly the area of between the Bow Ridge and the Paintbrush Canyon Fault, and the area of the Solitario Canyon Fault are, indeed, those that are refreshed by tectonic activity, and that this is reflected in such things as groundwater temperatures in turn reflecting groundwater flow paths.

These contours are the temperatures of groundwater at the water table, so they'd be at different depths, but along a surface that is more or less flat except for rising to the west and to the north, as was discussed by Dwight, and we are then showing that within this general area of the repository, the temperatures are relatively low, about 30°C, but that to the west and to the east, that we have somewhat higher temperatures; more so on the west, where these temperatures get up almost to 39°C at the water table or, say approximately 8, 8½° higher than just off a little to the east.

In the vicinity of the Bow Ridge to Paintbrush Canyon Fault, not quite so warm there. This does suggest that there is a strong tectonic control on movement within the saturated zone. In turn, it suggests that the--that control is dependent on the continued refreshing of permeability; therefore, that these faults are, indeed, the ones that have had not only Quaternary movement, but are likely to have continued movement in the future at whatever rate the current tectonic setting requires.

Continuing, then, the ESSE team judged that damaging fault motion within the repository is not expected; again, our Paintbrush Canyon Fault being considered the principal seismogenic source. Paleoseismic studies do indicate, as I mentioned earlier, a decreasing slip rate during about the last 12 million years, and although this--the average rate over a long period of time must be combined with the frequency of sudden movement along a fault in order to determine the rate of energy release or the instantaneous energy release, that also appears to be a decreasing factor in this area.

The displacements, or let me say the average slip rate has ranged from something on the order of tenths of a millimeter per year 10 million years ago, down now to thousandths of a millimeter to a few thousandths of a millimeter per year of the projected rates in the Quaternary, depending on the fault that is being considered; the highest rates being somewhat to the south along the Stagecoach Road segment and the Busted Butte segment of the Paintbrush Fault, and then along the southern extensions of the Windy Wash Fault down in this area.

Probably if an earthquake were to occur, we judge that it would be probably greater than six, because there is evidence of breakage at the land surface, and other work indicates throughout the Great Basin that where--when surface rupture occurs, the earthquake magnitude is generally six or greater. Generally, just based on the overall set in the ESSE, we judge less than seven. Recently, as Bob Shaw indicated, there was an EPRI workshop in which a number of experts were drawn together on this question and I believe that their consensus was that it was about magnitude 6.5 that could be expected at the site, and I don't know whether that refers to the preclosure or the postclosure. Perhaps Dr. Arabasz can comment on that when he gets up to speak.

We have some indirect evidence. It is completely uncalibrated in terms of evidence, but the steep slopes at Yucca Mountain do have a mantle of boulders. These boulders we know from the heavy coating of desert varnish on them that still has remained on the upper surfaces, have remained unrotated during the development of that varnish. Now, there's some question as to the degree of precision that can be obtained in looking at the mineral evolution with depth in the varnish to estimate its age, but the method has been calibrated reasonably well, we think, for the Mojave Desert area, and the estimates are from on the order of 100,000 years to as old as even 700,000 that these colluvial boulders have sat there essentially unrotated.

Again, as I mentioned, this is an uncalibrated method, but it does suggest at least that accelerations on the order of 1g, and certainly not much exceeding that, have not occurred for up to hundreds of thousands of years.

Finally, then, the expected wavelengths for earthquakes of this magnitude within this area we would expect to be rather large with respect to the EBS dimensions. It would be possible, perhaps, to design the waste package emplacement like a chime so it would go ahead and ring every time ground motion came through, but I think we have confidence that that would not happen within the context of this project; that that design would be carefully reviewed with respect to ground motion standpoint. Therefore, we believe that designs are feasible to assure that a waste package within a vertical borehole or, if necessary, within a horizontal borehole, would not be stressed differentially by the ground motion; that basically wave lengths would be such that the entire waste package, the EBS, and the surrounding materials would move essentially as a unit, and with proper bracing, there would not be a severe impact on the waste package.

Finally, then, turning to the hydrologic considerations, again if we do not expect the Quaternary faults to cut this area, then we do not expect that water from the westward draining slopes of the mountain or the westward dipping subsurface units would become impounded either at the surface or in perched water bodies from the faults that will not occur.

Similarly, we would not expect, then, that these faults which would not occur would develop new or strongly-enhanced zones of permeability through any existing impoundments if they were to occur. We certainly know that there are not impoundments at the surface, and we believe with some relatively strong degree of confidence that there are not impoundments in the subsurface in terms of perched water at this time; although, as you're aware, many committees have suggested that that is something we must continue to look at and, of course, we fully intend to.

Finally, then, this will not surprise you as to the summary or the conclusions reached at this point. In summary, we believe that neither faulting nor ground motion--which were the two conditions specified in the disqualifying condition--is expected to cause either a physical loss of containment or hydrologic changes leading to accelerated EBS degradation, or an increased flux through the repository that might increase rates of release.

Therefore, the ESSE team finds that the first question is answered in the negative, that the tectonics is not expected to lead to a loss of containment, and therefore, we did not need, for purposes of this, to proceed to a performance assessment evaluation with respect to the disqualifying condition. However, recall that this condition is imbedded in the qualifying condition and that we are quite aware that the confidence in the information will be required to be much greater in order to reach a similar conclusion, or to recommend a higher level finding for a qualifying condition.

At this point, the ESSE core team recommends to the DOE a higher-level finding that the site is not disqualified, but with respect to the site qualifying, it reached only a lower-level finding.

Thank you.

DR. DEERE: Thank you very much, Bill. I'm going to ask that we all hold our questions until after we come back from lunch, but I would like to ahead, if we could, into Dr. Arabasz's presentation. He is Research Professor of Geology and Geophysics at the University of Utah, and I'm very pleased to welcome him to speak to the Board; and he was a member of the peer review team.

So Bill, you will be back after lunch?

DR. DUDLEY: I will.

DR. DEERE: Fine.

DR. ARABASZ: Yes. I just know there's a Bible for speakers somewhere that says never, never keep them from their lunch.

I was invited by Leon to give my candid views on the tectonics portion of the ESSE. My background, briefly, you can read, the words are in front of you. I just emphasize one thing relating to the single expert problem with review. The last bullet, subsequent to my review of the ESSE document, I did become involved in the GEOMATRIX/EPRI project to assess earthquake and tectonic issues, and that process had started with EPRI's risk-based assessment look at the Yucca Mountain problems and, plainly, earthquake-related faulting related issues have been under the microscope for a long time.

There's just a wealth of literature and scrutiny, circumstances in the western U.S. with siting of high-hazard dams, nuclear power plants. Earth scientists are used to being center stage with their issue of earthquakes and faulting, so plainly, lots of scrutiny, lots of other independent judgment available.

The formal charge for the review, I'm sure you've seen this litany many times; the documented, in-depth critique, the adequacy of information, review of the methodology, determine ultimately whether an objective and technically defensible review of suitability available.

Now, my own perspective--and I'll repeat this theme a few times because I'm an earth scientist and very unique in the earth science arena to be dealing with multiple working hypotheses, and to no one's surprise, lots of working hypotheses about the tectonic models apply to the Yucca Mountain area. So as an individual, just mentally dealing with all of these perspectives, I had to come to the conclusion that it wasn't my job to find the truth amid these competing hypotheses, nor to provide a solution, ultimately, for the complex problems involved. But it was my point to question whether the weight of evidence so far indicates that the Yucca Mountain site is suitable.

And what I found, through this review--a little bit different from reviewing the technical information in other reviews--and that related clearly to the compliance with the guidelines initially set forth in 10 CFR 960, such that the review, to a significant extent, was a legalistic one, starting first with the technical wording, all of the double negatives in the guidelines, and then looking at all of the tectonic judgments and conclusions arrived at, and ultimately, I think, I'll go specifically through the guidelines and I have been able to agree, but ultimately, for me as a reviewer, I think this was an easy stage to review.

The disqualifying conditions tended to be early screening conditions for tectonics, lots of mental room for agreeing with the higher-level finding for the disqualifying condition, and finding enough residual uncertainty with the qualifying conditions to say, yes, you still have to be at that lower level finding now, and so then I think the tougher part is going to be going from these lower level findings for the tectonic-related issues at the site, to those higher-level suitability problems, and I'll speak more to that in a second.

Indeed, from the viewpoint of seismic hazards--and I'll speak both to postclosure and preclosure issues. Bill was focusing on the postclosure issues, but yes, I was able to agree that the available evidence supports the conclusion that the site is suitable, although additional information is needed in specific areas to strengthen this conclusion, and that certainly is a recurring theme with most of the tectonic pieces of information.

With regard to the postclosure guidelines, the qualifying condition, the lower-level suitability finding, as I--again, part of my purpose in being here this morning I perceive to be to vouchsafe for both the rigor and the integrity of the ESSE core team process and the review. I was surprised to find a remarkable degree of devil's advocacy explicit right in the report.

When I started out sequentially reading the documents through the EA, the SCP, by the time I was finished with the EA, I was loaded for bear and said, "I've got lots and lots of tough questions for these people." Fewer questions by the time I was done with the SCP, and when I came to the ESSE report, I was just amazed at, again, the internal devil's advocacy. I think plainly these people were in the fishbowl, lots of public scrutiny. The issues had been defined, many issues, as important, and no one was going to--on that core team, as I perceived, was going to come out of that report just being embarrassed by neglecting key issues that were already in the public arena.

I found very well-reasoned logic. The geoscience information, at least by the time we were done iterating with the review, was presented in a thorough and objective way, and the evaluations were ultimately conservative, and I found that they, again, by the time they were done iterating with the review process, they certainly stayed within defensible bounds.

Now, I have to say that I will vouchsafe for the words that I saw. I think I perceive a little bit of a problem in this conservative wording at some stages of the documentation, and then when perhaps people come to the stage of trying to make this language more familiar to the general public, that a little bit less conservatism starts to creep in there, and again, I can only vouchsafe for the words that I saw along the way, and I think it's something that people involved in the process just need to be careful with.

As an example, I was flying here yesterday afternoon and picked up the latest version of the executive summary, which I hadn't gone back to to read after the review process was done, and found a wording saying: "No credible scenarios were identified in which fault movement and ground motion in the underground facility could directly cause loss of waste isolation," and we're certainly through the subsequent EPRI process worrying about secondary faulting and some minor fracturing, that I would be a little bit uncomfortable making that statement with full bravado today.

The postclosure guidelines, the disqualifying condition, a mental roadblock initially; a higher-level suitability finding for the disqualifying condition while having a lower-level suitability finding for the qualifying condition, and as described to you, I resolved the dilemma by finding that there had to be a separation of the faulting ground motion part of the disqualifying condition from the hydrologic part, at least for the screening of the disqualifying condition as a screening guideline.

If you allowed tectonic/hydrologic coupling, then I think you would have to go back to the tectonic disqualifying condition and worry a little bit more. Also, the team led me through their attention to the guideline wording, and that the disqualifying condition related to the geologic record rather than the setting to fault movement and other ground motion against setting aside hydrology coupling, and then the word "expected," and ultimately, from what I saw of the geologic record, the record of faulting in the region, the likely levels of ground motion, I was comfortable with that higher-level finding for the disqualifying condition.

Preclosure, lower level for the qualifying condition; disqualifying condition, the higher-level suitability finding. Basically, we can look at the available geology, what's captured in the landscape by way of the information from prehistoric earthquakes. Enough analysis had been done on expectable ground motions at the site to lead one to a level of comfort, particularly when the engineers intervened and said, "Hey, you're going to throw a .6g at me, no problem. We can handle it." The engineering intervention with the Subramanian Report, I think, to me was an example of how the engineering perspective comes in and brings to closure the earth science open-endedness in deliberation.

But here a simple example of the disqualifying condition in a tectonic-related issue, yes, I think what's apparent either by way of known geology and/or modeling and analysis, significant comfort in looking at the disqualifying condition and saying, okay. Going to the qualifying condition, residual uncertainty there that says, "Yes, we know a lot, but we don't know everything yet. And so we're going to be conservative and stay with the lower-level finding, or recommend the lower-level finding."

Okay. To conclude, let me see if I can wrap up my thoughts here. I don't mean to insult your intelligence by going through the difference between earth scientists, engineers, and regulators, but for me, this was a very important part of my perception of this process, particularly as I bumped into the core team and its individual members ranging from worriers--W-O-R-R-I-E-R-S--in the earth science group that perennially deal with multiple working hypotheses, almost sometimes to the point of paralysis, and then on the other side, maybe the engineer, in my perception, with a little bit too ready bravado to say, "We can handle anything," and so somewhere in the middle, making sure that as a reviewer I was able to critique their positions, and ultimately within the framework of the legalistic words in the guidelines.

The earth scientists I say, okay, facing abundant complexity, indeed, much of the available information still tends equivocally to support multiple interpretations, and notably in the form of the tectonic models. We just don't know that subsurface structure, and certainly in the ground motion arena that becomes more important. For the surface faulting, I think what you see in the landscape is what you get, but if you're talking about ground motions, what's down there, the size of the faults, their potential for producing earthquakes of different sizes, that's another hooker.

The engineers, then, have solutions. They are problem solvers. They are able to intervene and to bring to closure, or at least define the relevance of the earth science considerations that are being dealt with, wrestled with, and this was something, I guess, that I had never quite paid attention to before, at least not in dealing with dams and other critical facilities. In the nuclear arena, the intervention of the regulator to define the acceptable risk, to put that into law is an incredible deus ex machina. To those unfamiliar with the term, Greek drama, it's so complicated a god gets lowered in the basket, jumps out of the basket and resolves everything.

(Laughter.)

DR. ARABASZ: Here we go--okay, I envision--happily, I step off the stage and I envision this process continuing. People left with this problem are going from the lower level to the higher-level suitability findings with these tectonic issues. That's going to be an incredible process, knowing what I know about the residual uncertainties, the multiple working hypotheses for these tectonic models, and so on, and my belief that the regulator, indeed, has this power to intervene at some point and perhaps provide the resolution needed.

Thank you.

DR. DEERE: Thank you very much.

We will convene at, let's say, 1:35, give us a little more time to get lunch, and I've asked Dr. Cantlon to chair the meeting this afternoon.

(Whereupon, a lunch recess was taken.)

A F T E R N O O N   S E S S I O N

1:35 p.m.

DR. CANTLON: Well, the appointed time has arrived. Let's reconvene the Board.

Dr. Arabasz has got a plane to catch, and so I promised to put him on the grill first, and we'll open this up first to the Board members for questions, and then we'll move to the audience; Board members and staff.

DR. ALLEN: Clarence Allen.

Just in summary, basically, you do not take issue with the--am I correct--with the recommendations?

DR. ARABASZ: That's correct.

DR. ALLEN: In any one of the categories of yes, no, not yet, you know.

DR. ARABASZ: After wrestling with some initial skepticism, and after going back to the wording in the Appendix 3 of the guidelines and then coming back to the positions, the conclusions that the core team reached, ultimately, I was in agreement, and that's where I say this review, at least in my case, was really a legalistic review.

DR. ALLEN: I can't help but comment on the use of the word "expectable." The U.S. Geological Survey loves to use that. You've heard the term, "maximum expectable earthquake," but the last thing they mean is they expect it during the life of the structure. It's really what most people would call a maximum credible earthquake and they try to avoid that term, but in that case they're using "expectable" in a very different way than here where it's apparently likely versus unlikely and sort of a 50 per cent probability base.

DR. ARABASZ: I should amplify my point about the intervention of a regulator, because some would immediately say, "Well, the regulator has already defined in law acceptable risk in the form of the guidelines," but within tectonic issues there aren't clear guidelines, for example, as to the admission of a probabilistic approach to dealing with the likelihood of faulting or some ground motion, and I believe both in ground motion and in the faulting events we will be dealing with very small probabilities, very unlikely events, but yet, to reach resolution, it may be that a probabilistic approach need be admitted.

DR. CANTLON: Other Board members, questions; staff, yes, Leon?

DR. REITER: Walter, Leon Reiter from the technical staff.

Walter, you have the unique position of being both a speaker now, and also a member of the EPRI expert panel. If you had any recommendations to the early site suitability, is there any experience you have that--based on working with EPRI--that you think that they could take advantage of and make the process better, if possible, next time?

DR. ARABASZ: Excuse me, the EPRI?

DR. REITER: No, the next time they do an early site suitability.

DR. ARABASZ: It would appear to me that it depends on the rules that are given at the outset, and in this instance, it appears that the rules that they were given was the legalistic framework of the guidelines, and so I watched them wrestle with interpretation of words and reach, or try to reach resolution within the context of what a disqualifying or a qualifying condition was meant to be, and that kind of divides them in the middle.

They spend half of their psyche dealing with this legalistic framework, and the other half of the psyche trying to come to terms with the technical issues involved. And I must say, too, that as an earth scientist, I was taught in grad school and I come to appreciate it more and more, if you want to solve earth science problems, you get yourself back out into the field and rub your nose in the rocks rather than spending the inordinate part of your life in an office dealing with legalistic words or whatever context in planning.

Okay. Let me see if I can sharpen my answer for you. I think it depends on the rules that they're given at the outset, what the name of the game is that they have to play. If you were to tell them, we will admit--and I don't know if this is possible somewhere along the way--we will admit a probabilistic approach to dealing with the likelihood of faulting, then certainly going back to the earth science arena and allowing the type of approach that EPRI has promoted, it becomes clear in that analysis what the sensitivities of the probability are to specific pieces of information, and then home in on those pieces of information for investigation.

DR. REITER: Let me just ask two specific things, and both exercises in some way are legalistic, are really solicitations of expert judgment in one form or another, and I was wondering whether the EPRI exercise, which tends to try to use outside experts and tends to use multiple experts, whether those kinds of--your experience is using those parameters might add anything to the process that we've seen in the early ESSE?

DR. ARABASZ: If I perceive the expert solicitation process correctly, there are those who argue that one should never do science by a democratic vote. I believe that the intent of that exercise is to capture uncertainty, and so if the uncertainty bounds are important to define, then that process becomes very important. Where one goes to get guidance, whether--well, I guess in some form it's an expert panel, whether it's duly constituted as a probabilistic seismic hazard analysis panel. I mean, certainly, just looking at the group of seven EPRI consultants who are sitting around the table I would recognize immediately that, yeah, there's a bigger body of knowledge available.

For example, when the issue of new faulting arose, it appeared to me that someone in rock mechanics would be able to contribute importantly about what happens when a new fault forms. I mentioned at that meeting that it takes work to create a new fault. You just don't do it in one instant. You have to create a fracture, and you've got to move that fracture a number of times until it becomes--until it has some coherency and the ability to sustain some finite amount of significant slip.

And then from my experience in other projects where we worry about what's underneath the ground that we don't know--in one case it was a high hazard dam--and the resolution ultimately came when the decision was made to strip the alluvial cover down to bedrock. In this case, you'll have direct access into the tunnels to move along the process in a hurry.

DR. CANTLON: Other questions? Questions from the audience?

(No audible response.)

DR. CANTLON: Thank you.

Well, let's see, our other speaker before lunch, Dr. Dudley. Questions from the Board?

DR. ALLEN: Clarence Allen.

Bill, in reading your higher-level finding here, let me make sure I interpret this right. You say based on the Quaternary record, fault movement and ground motion are not expected to be such that loss of waste isolation is likely to occur. "Expected" means greater than 50 per cent; is that right?

DR. DUDLEY: Right.

DR. ALLEN: Okay, and I guess I'd--from what I know of the situation, I would agree with that. Then you say it is unlikely that new site characterization information will change these expectations. Now, when you say unlikely there, do you mean the chances are, say, less than 10 per cent that any kind of new site characterization information will alter that?

DR. DUDLEY: Well, I'll have to admit that unlikely in that specific context, I don't recall that we went through a quantification exercise on the ESSE as opposed to in the probabilistic performance assessment context. I think it was, again, something that we individually might rank at certainly not more than a 30-40 per cent probably, something of that sort.

DR. ALLEN: Oh, I see. So you'd go that high, but--

DR. DUDLEY: Yeah.

DR. ALLEN: I guess I would have to agree with that. I think there's certainly a very finite chance we're going to get underground and find that the fault system down there is much more complicated, or there are many more faults than we had thought in relationship to the Ghost Dance and the others, or at least so unclear that we'll be in a state of confusion. But I certainly think that's a finite chance, not--I mean, really finite, not something less than 5 per cent, but I guess I would agree with you and the way you explained the term "likely."

DR. DUDLEY: Well, there was a certain feeling of at least partial permissiveness where the disqualifiers are concerned if they are fully imbedded in the qualifying conditions; in other words, we're saying we know we're going to have to have a lot more information to meet the qualifying condition, or to evaluate whether we meet it and, therefore, we can perhaps, if you will, even cut back just on the bookkeeping exercise if the disqualifier is fully imbedded in it. If the disqualifier is something else, such as the thousand-year groundwater travel time, then you cannot apply the permissiveness in terms of the availability of site information to this.

DR. CANTLON: Cantlon. Let me ask the same question that Leon asked Dr. Arabasz, and that is that this process of site suitability hopefully will become an iterative process, and as the research on the area on the site proceeds, the information is not going to come in uniformly across that broad front.

Do you have any kind of feeling about the nature of the way the process moved, about how one could address these pulses of large amounts of information that will come, for instance, when you get underground? Do you have any thoughts about how that could be managed in an iterative way to update in a fairly rapid way?

DR. DUDLEY: Well, I certainly agree with you that we're going to have blocks of information in one area of concern, whereas other areas are going to be slow-moving. That really relates to the question of would we have to convene the entire group with the entire scope each time, or could a more focused group--also, perhaps, with a greater variety of experts in that particular topic--be brought in so that one group would not have to try to represent such a broad spectrum of professional judgment and expertise.

So that I would anticipate--this is, of course, trying to second guess the project manager, I guess--I would anticipate that we would have groups that were more focused on individual issues and, to a certain extent, that is underway right now, that things such as erosion, the calcite silica issue, and so forth, have been singled out to begin to develop some sort of a closure position on those.

DR. CANTLON: Other questions from the Board, staff, audience?

(No audible response.)

DR. CANTLON: All right. Thank you. Appreciate that.

Let's move, then, to the afternoon agenda, total system performance assessment, and we'll start with the first speaker, Jeremy Boak.

DR. BOAK: Thank you.

Some of the discussion this morning led me to a thought I had about a year ago. We talked a lot about what was the actual mix between the human activity of good engineering and the sort of technological faith we might have, or scientific faith we might have in our understanding of the natural barriers that was going to get us to some kind of comfortable feeling about the degree of compliance of this site. And it dawned on me about a year ago that that was nothing more than the age old debate over whether it was going to be faith or good works that got us in the gates of heaven.

As a geologist, my tendency is to believe in those natural systems. I look at 3.8 billion-year-old rocks which I happen to have in my possession and say I'm much more comfortable about these materials having lasted for 10,000 than I am about engineered materials, but it seems to me that it is performance assessment that ends up having to deal with that faith-oriented aspect of things, and I guess maybe that makes my performance assessment team the faith healers of the repository program.

What I'm going to talk about is, what was the purpose of this exercise in performance assessment that we conducted during the course, actually, of the past several years, but which was mainly rolled up beginning about in June of last year into a total system performance assessment, which is now in to us as two reports; one from the Pacific Northwest Laboratories and one from Sandia National Laboratories.

I'll talk a little bit about the scope of that performance assessment, but I'll leave elaboration of that to later speakers; list the participants; talk about what the steps were that we went through to get through this and how those relate a little bit back to a process that was laid out first in the site characterization program; give a few caveats; and then show roughly our schedule for the course of this exercise.

We had a number of purposes in developing this total system performance assessment. The major one was to really get going on the process that we call now abstraction, that necessary process that pulls bits and pieces of detailed models of the various subsystems that are involved in this complicated system, distilling essential features from very computationally complex models and merging them into models which then link various processes. Those higher-level codes are not necessarily more simple, but they are, in fact, the only way we see to get to an assessment of the compliance of this site, to simple, straightforward performance measures.

A second aspect was to compare two different approaches we had between the Sandia and the Pacific Northwest Laboratories' approaches, and then finally, to demonstrate the production of a meaningful estimate of the system performance. In this case, we used the 1985 EPA standard for high-level waste, and produced cumulative--complementary cumulative distribution functions.

I'd like to say a little more about this process of abstraction. I think this, a slide very much like this has been shown several times before. The base of this pyramid shown here are the detailed process models that interact with site data and produce some kind of analysis of a piece of the problem, and they tend to be limited in their scope, but comprehensive in their treatment of the details of those processes that they are modeling.

Above these perhaps is a tier in which we attempt to integrate several subsystems into something that is a little broader in scope, looks a little more widely; elements like the waste package performance assessment models, and are used in order to evaluate the effect of certain processes or parameters.

And finally, the most comprehensive models up at the top, which attempt to roll everything together into one or several major performance measures. These are the total system performance assessment codes. They are comprehensive in scope, generally have stochastic inputs of many, many variables, and they are most abstracted; that is to say, we have attempted to distill out only those processes that are really relevant to the performance of a site. And they are used to evaluate system sensitivities and ultimately, also, to show regulatory compliance.

One of the lessons we had learned before we got going on this was--and that drove this process--we learned from one of the other sites that's being evaluated under the 40 CFR 191 criteria at the WIPP site, they did a compliance evaluation in 1989, and as you'll see, they have transgressed fairly heavily into the forbidden region over here, and the major reason for that, even though they had gone through fairly extensive levels of site characterization, was that there was a critical piece of data they did not have any really good experimental values for, which was retardation in the Culebra Dolomite. And so as a consequence of that, they were forced--by agreement with EEG--to do without that particular influence on their system, and that's why that showed up that way.

Undaunted by that slightly negative-appearing result, they then proceeded to gather data and their 1990 CCDF does, in fact, show compliance. With respect to that, I would say that we are somewhere even further back than this, because we do not have much in the way of site characterization data, and the results that we present should be looked at in the light of this kind of presentation.

I'm afraid those two last slides that I just got from Rick Anderson are not in your package. I'll try to get copies of them.

We looked at a collection of different scenarios, of different phenomena that we were going to model, as shown here, aqueous flow, gaseous flow, which we evaluate the question of Carbon-14 release; human intrusion, because it has been a very highly visible issue; basaltic igneous activity; and tectonism. These were then rolled into conditional CCDF's for each scenario and then merged into a total system CCDF.

This was an effort that used the work that we have from virtually every one of our contractors to some extent, that the major participants were Sandia National Laboratories, which started off as the major coordinator for this effort and performed calculations using fairly moderately abstracted models which were run stochastically to evaluate the performance of the site.

PNL had a somewhat different approach it uses that is slightly less abstracted, and they performed--also performed dose calculations on the results that they got, the releases that they got from their models as well as they performed dose calculations from the Sandia releases.

Los Alamos National Laboratory provided us information about volcanism and we enlisted in one of their experts to get data on retardation; and Lawrence Livermore National Laboratory provided the source term that Sandia used. A similar source term was used by PNL, but it actually was generated by their source term code. The fundamental underlying details will be discussed a little bit later.

Shown in sequence on this slide and in a more graphic display over here are the steps that we went through in producing these results. The first step is to review the scenarios which we have in a preliminary stage right now. George Barr had produced a series of reports that summarize a number of scenario trees for the site, and we reviewed those, selected specific ones, assigned them some probabilities. The models that we had were pretty much developed, although there was a certain amount of development effort went on in arriving at the several different aqueous flow models we used.

Next, then, having chosen, developed and chosen those conceptual models, to estimate some of the parameter uncertainties. We'll discuss to some extent the elicitation of those values for variables for which we have relatively limited data sets; then performing the calculations and ultimately rolling them into a CCDF, which is, to some extent, an interpretive tool, and we did not rely strictly on that CCDF as the only interpretive tool there is. In the report, quite a good deal of data and discussion of the implications of the things that were done in this performance assessment.

We'll try to present the highlights of that process, but I heartily recommend that those of you on the Board who now have copies of this report read through it in detail. I was impressed. I was pleased. It's a really good report. I haven't read all 300 pages of it yet, but I've read about half of one and I've still got another to go, but I'm pleased. I'm really impressed with the level of thought that's gone into this process.

A few caveats, however. This exercise does reflect our current understanding of the site, and it is expected ultimately to contribute to determinations of the ability of a potential repository system at Yucca Mountain to meet regulations, but it is not comprehensive in terms of the modeled components. The data and models that we have used here--or the models are not validated. The data, some of it has not been qualified, and the ranges of values we assumed in our distributions go well beyond what is available in the actual database and, in many cases, go very much to the limits of credibility.

We've used very wide distributions. We made this total system performance assessment an ardent attempt to drive the system to failure so that we could see where the relevant issues were.

The process built on exercises that we have completed earlier; validation exercises like HYDROCOIN, and the calculations done under the PACE 90 exercise, which did not actually arrive at a CCDF, but which gave us important insights into the relevant issues that we needed to model in a total system performance assessment. In addition, there were calculations that were done for the site suitability effort which fed almost directly into the total system performance assessment we did here.

And in essence, this effort began in the early part of 1991, immediately after the cutoff for some of the input to the site suitability report, and has progressed through construction of the data sets, calculations, and consequence calculations then, or the releases led to a stage of beginning the dose calculations. We did a presentation which some members of the Board's staff were present at in November.

Since then, we have been working on assembling the report. We've had a few upsets and alarms and excursions in the course of preparing the report. We found some parts of the original analyses that needed to be rerun, but we do, in fact, have a draft copy of each of these reports in hand. It is undergoing under the policy review procedure at Yucca Mountain, and we hope to have it out into the publication cycle sometime in the next week or two.

Then, of course, we have been told that we can push the publication through in a month and a half, but we'll see how quickly that can be done. So we hope to see that report out in the fairly near future.

With that, I'll turn the podium over to Holly Dockery who will introduce the problem setup, problem definition for the total system performance assessment.

DR. CANTLON: Questions? Any questions from the Board? Staff?

(No audible response.)

DR. DOCKERY: All right, fine. Thank you.

The part of the talk that I'm going to give right now is called problem definition, but it really is tying together a lot of loose ends so the other presenters can go ahead with their parts of the discussion.

The outline of my talk starts with scope, and Jerry had a portion called scope. Mine should actually be perhaps called components of the total system performance assessment.

Then I'll follow with the PNL/SNL common data set, a little bit of information on the retardation factors that were used, and the boundary conditions. So you can see, it's kind of a potpourri of things other people don't want to have to talk about.

(Laughter.)

DR. DOCKERY: On the scope of the total system performance assessment or, as I said, the components, there were a number of new scenarios that were modeled that have not been modeled in the past. Groundwater flow has been modeled in the past, but we could use different conceptual models for the groundwater flow. You can see that I very carefully cut out this section here because somehow that missed the editing of the slide, so you can take the gas flow in fractures away from the tectonism.

But for groundwater flow, and we used both a composite porosity flow model and a Weeps model, which both of those will be described in more detail by Mike Wilson. From gas flow, we simply did surface release.

DR. DOMENICO: Excuse me; Domenico. No model on gas flow?

DR. DOCKERY: No model on gas flow?

DR. DOMENICO: No model to simulate gas flow?

DR. DOCKERY: Yes, there was a model on gas flow release to the surface.

DR. DOMENICO: Okay.

DR. DOCKERY: Each one of these components that's shown up there had a model to simulate release to either the surface or to the accessible environment.

Human intrusion was modeled for surface release and direct release at the saturated zone; basaltic volcanism just to the surface; and tectonism, which was simply modeled as a water table rise, this one was done by PNL Laboratories and not by Sandia, and then we'll talk about that. Then the various release models that came out--or really, simulations that came out of these particular components were put into a conditional CCDF. Those were combined in a fashion that Rally and Paul Eslinger will talk about during their talks, and each of them came up with the total system CCDF.

Now, this is a considerable expansion on what we talked about last year for PACE. For instance, you saw that we had a number of new phenomena that were modeled. Prior to this, we had only done a nominal case with aqueous flow. This time we had gaseous flow and transport. We also had the human intrusion, the volcanism and the tectonism. The releases were calculated to the accessible environment along two paths. In PACE we went to the water table, and that was the end of the calculation. In this case, it was taken to the accessible environment at 5 kilometers from the boundary of the repository and to the surface.

We had more sophisticated source term use. This was thanks to the work that Bill O'Connel at Lawrence Livermore Laboratory had done. The primary differences from the PACE model were a better understanding of the water contact modes; that it was a slightly computationally simpler model; and that there was a larger suite of nuclides encompassed. You may recall there were four radionuclides used with PACE. This expanded to ten radionuclides, including Carbon-14 for the gaseous releases. The plutonium, uranium, and Americium were included because of their large percentage in the inventory. The Carbon-14 for gaseous release, and then Celenium and tin were both added for the dose calculation purposes.

We did perform stochastic simulations. That required probability distribution functions to be formulated for a number of different parameters. They were then sampled randomly, and the realizations were used in the simulations, and some sensitivities were performed. Primarily, this was for human intrusion and for volcanism, where we simply varied a few parameters, and human intrusion, as an example, was varying the drilling density. In volcanism, it was varying the amount of material that was carried up.

The dose calculations, as Jerry said, were performed by Pacific Northwest Laboratories in their DITTY

Code, and both the results from Sandia and from PNL were used in those, and Paul Eslinger will be presenting that particular information. What I wanted to show with this slide here was that basically, the disturbed conditions, like basaltic volcanism, human intrusion, and climate change were simply perturbations on what we considered the nominal conditions where we have gas flow or vapor transport, and then water flow. Climate change was treated simply as a distribution of fluxes in the nominal case.

The next thing I want to talk about was the PNL/SNL common data set. The reason we thought it was important to have a common data set is that we did plan on looking at our results and comparing the results once the simulations were complete, and we assumed that if we started with a common data set, that it might be a little bit easier to do.

The common factors in this data set included the stratigraphic cross-section, which I'll talk about, the geohydrologic parameters and their distributions, the suite of radionuclides, and the boundary conditions.

The aqueous flow domain in the horizontal was an east/west transect that went from H-5, essentially at the crest of Yucca Mountain, across to G-4, and then UE-25a and 500 meters on past UE-25a into Drillhole Wash. Now, the reason we chose this particular transect was that in the northern part of the repository there was more data available, especially in the geohydrologic parameters, that we could use for sampling distributions. We wanted to be able to cross the Ghost Dance Fault, which at that particular area is modeled with 14 meters offset interpreted from downhole data.

Although it is simply a transect to the north, it was used to represent the entire repository. The saturated zone in the horizontal domain extends from the repository out to the accessible environment five kilometers from the repository boundary, and since the groundwater flow is basically northwest to southeast, that boundary was, in essence, over here.

In the case of the vertical domain, the different release pathways included to the surface for volcanism, gas flow, and human intrusion. For aqueous flow and tectonism, down into the saturated zone directly below the water table; and then for the second human intrusion problem, we actually went down into a lower aquifer.

The stratigraphic cross-section that was developed has up to five layers. In this part of the repository, we transected off five layers, but over in this area it only went down through four before you got to the water table. PNL used the entire transect because they did a 2-D simulation. Sandia used six representative cross-sections that were picked along that transect, and they were randomly sampled, as Mike Wilson will talk about during his discussion of the aqueous and gaseous flow.

The number of layers that we used was decreased quite a bit from PACE. The layering that was used in PACE, at least for the 1-D simulations, seemed to be much too detailed for the type of information we were getting back out of the simulations. It was determined that the five layers would be significantly representative, and would not be numerically as time-intensive an exercise.

There was one layer that was in PACE, you may recall, that caused a fair amount of lateral diversion, and that was the only high contrast layer within the PACE stratigraphy. However, that layer, that particular layer was placed in the stratigraphy simply to cause numerical problems, and it did, and we decided since there was no analog for this information, that we went back to using the data for the particular layers and simply stuck with the information available.

The data was sampled from three wells, the USGS information on the three wells, and we took multiple units in the stratigraphy and lumped them together and then designated them on the basis of the gross characteristics, and the layers we used were the top layer, which essentially went from the top of the repository down to this level. We didn't model any of the rock up here because problem domain or problem simulations began at this boundary.

There was a moderately welded unit, a vitrophyric unit, a vitric unit that was described as non- to partially-welded, zeolitic, non- to partially-welded, and then modified partially-welded unit that went essentially down to the Paleozoic boundary.

The saturated zone was used as two layers, and I didn't show you where these sit on the stratigraphy because it really didn't make a difference. In the simulation where the carbonate aquifer was used in the human intrusion, it was simply the material was dropped to the bottom of the hole, and so there was no transport time involved. And the top aquifer indicates the material between the water table and the carbonate, it's not truly an aquifer, it's not an economic producer of water. It's simply saturated tuffs, and we're using tuff aquifer as a shorthand. The carbonate aquifer is the analog to the lower carbonate aquifer that the USGS has identified in the drillholes to the southeast of the site.

The geohydrologic data set, the parameters that were used in this particular stratigraphy are going to be detailed by Paul Kaplan, which those are, but the information itself was taken from what site data were available, and also analog data; primarily, the Apache Leap. The matrix values came from Peters, et al. Every time we trace back some information, we find it comes from Peters, et al. We also went back to the PACE document and used some of the information from PACE, and used the Apache Leap to constrain some of the distributions a little bit better.

The fracture properties came from Spengler, et al. for fracture density and fracture orientation, and then the flow properties were the sand properties from the USDA report, Carsel and Parrish and Zimmerman. I'm sure that Pat and other people recognize where that would come from.

Distributions were then developed for each one of these parameters, and Paul Kaplan will go into detail about which distributions were chosen and the ranges for those parameters. This particular data set we felt like was providing a long-needed tool, and has already been used for other simulations other than the TSPA. This information will be coming out in a separate report that details all of the information and how it was developed; where it came from.

The data set applications included, for the flow and transport calculations, Sandia used them for the unsaturated aqueous scenarios and for the saturated aqueous scenarios. The other models were abstracted models, did not require the same amount of simulation in the aqueous transport phase; however, PNL, since they were using the basic nominal case for all of their simulations, used this geologic data set for all their scenarios.

The next thing I will talk about very briefly is the retardation factors and where they were derived. For the tuffaceous rocks, the information, all the information I'm going to give you--both for the tuffaceous rocks and for the carbonate rocks--were elicited from Meijer from Los Alamos National Laboratory. He had divided the rocks into three types; the vitric, devitrified, and zeolitic tuffs. The nuclides that had a retardation equal to zero were Tc, I, and C. Where there was complete retardation--Am, Pu, and Sn, and then there were different distributions developed for the other four nuclides.

The range of retardation values was established for the range of pH values in J-13 water. Oxidizing conditions were assumed. It is assumed in reducing conditions, that the sorption would be less effective.

DR. DOMENICO: Excuse me. Domenico.

DR. DOCKERY: Yes.

DR. DOMENICO: Does that 100 mean 100 milligrams, millimeters--

DR. DOCKERY: No, that means per cent.

MR. WILSON: It's milliliters per gram.

DR. DOCKERY: I'm sorry.

In the carbonates, we don't have any retardation information for the carbonates underlying Yucca Mountain at this time, so we went to the WIPP data base and came up with information for the Culebra Dolomite. In this case, we used the matrix values only because there are clays present in the fractures at WIPP, and it was assumed that that would not be a reasonable assumption for the carbonates underneath Yucca Mountain.

In the water chemistry, the oxidation conditions again were assumed, a conservative assumption, and the chlorides were also assumed to have no effect on the K_d. Information and data that's been collected indicates that the chlorides in the water don't really have any measurable effect, and therefore, we assumed that even though there were brines at WIPP and not at Yucca Mountain, that would not be a major factor. And so PDFs were developed for all the nuclides and carbonates, except for the same ones that there were none in the tuffs, the Tc, I, and the carbon.

DR. LANGMUIR: Holly, Langmuir. I think that assumption that the chlorides have no effect on the K_d is probably very, very wrong. Work we've done on brines related to a study some years ago showed that the high cations with the chlorides prevent cations from--radionuclide cations from sorbing at all. So they have a drastic effect on the K_ds of the metal irons. The higher the chloride, the less sorption you get. It's very profound.

DR. DOCKERY: Well, that's certainly something that Meijer and his group said that they would very much like to study. However, they did cite a report--and I don't remember who the author of that report was--that had information to the contrary. So I'm sure that that's something that's in their study plan report.

DR. LANGMUIR: This would relate to the levels of chloride, of course. If it's a brine, that's one thing. If it's just a couple hundred parts per million, that maybe doesn't matter then.

DR. DOCKERY: But, of course, what you're saying is that the WIPP brines would be very non-sorbing; whereas, the material at the Yucca Mountain site would not have that problem.

The last thing I wanted to talk about were the boundary conditions. For the PNL calculations, the lateral boundaries were assumed to be no flow for 2D, and one of the reasons that the cross-section was extended 500 meters east of the last drillhole was to ensure that there were no ponding conditions caused by the numerical simulation. Each one of them was run from initial saturation and flux to a steady-state for the specified percolation--which I'll talk about the percolations in just a second--and the range for flux at the repository ranged from zero to 39 millimeters per year, and only the Sandia calculations went up to the extremely high values.

The reason that they were pushed to the high values were because this range of values does allow for climate change. As I said, that's how we incorporated climate change into our calculations, was simply in the distribution of flux. More importantly, we found with the PACE calculations, when we didn't have a high enough flux, we couldn't force the transition to fracture flow, and so we wanted to have some values pushed over into the area that we would get fracture-dominated flow.

And the shape of the distribution, as you can see, is much weighted toward the low end values. The mean of this distribution is approximately one, and you can see how quickly this distribution tapers off as it gets to the higher values. The basis for this assumption was the inverse calculations that you've seen Jack Guardia, show given the 1D simulations, with the given initial conditions and the given stratigraphy that we've been using, that the actual ranges of percolation flux at that level may be closer to .01 to zero, and so it's at this point the information we have seems to indicate that we should be looking at the lower ends for the nominal conditions at this time.

DR. DOMENICO: Domenico. Can you give me the rationale for the no flow boundaries--for the lateral boundaries again, please?

DR. DOCKERY: I would like the modelers--Paul, would you like to--

DR. DOMENICO: Well, I can wait until their presentation, then.

DR. DOCKERY: Okay. That's essentially the end of my presentation, and the next person that's on the docket to talk is Paul Kaplan, who would be discussing the parameter distribution development, as well as the expert elicitation that was used for those parameters, unless you have any questions.

DR. CANTLON: Any questions from Board members?

DR. NORTH: Warner North. I'd like to ask a question that maybe falls on the interface between you two, and that's where this distribution came from of the percolation fluxes?

DR. DOCKERY: That's Paul's bailiwick.

MR. KAPLAN: It's actually fairly simple. They assumed that their mean precipitation or infiltration rate was one millimeter per year, and based on the methods I use, I argued that, again, given that that's the only information you have an exponential distribution's the maximum solution to that.

The finite tail of 39 comes from approximating that, the beta distribution for the simulation.

DR. NORTH: I think it's an interesting question, how high this percolation flux could reasonably go. If we assume that there is a rain shadow from the Sierra Nevada that's going to continue over 10,000 years and look at what we can find looking around the world's meteorology, maybe plus using a general circulation model, you know, could we get 600 inches a year of rainfall there? That's the figure I remember. I think it's probably something that one could rule out. Can you get 39 millimeters a year net infiltration? Maybe it's easy. Maybe you could go higher than that, or maybe you could cut off at a boundary that's somewhat lower.

I suspect the points of very high flux would be very, very interesting realizations of the Monte Carlo to look at, so I would urge that we do not skip through this portion too quickly and that we not simply assume that exponential is the right way to do it.

MR. KAPLAN: If we were to run a series of simulations asking specifically what are the consequences of time flux, we would have to do it with something other than the one-dimensional models. You put in much less than the 39 millimeters per year and you saturate the thing, and now you have a one-dimensional saturated flow problem that's going through layers of varying conductivity, so it's an inappropriate--the models are inappropriate to ask that question.

So that's--I see us asking the question and coming up with a different distribution, but again, as part of another simulation.

DR. NORTH: So might I summarize your answer that you haven't really thought through global--or climate change scenarios that would result in very large increases in infiltration?

MR. KAPLAN: I think that's fair, yeah.

DR. DOCKERY: Bob Shaw.

MR. SHAW: Bob Shaw of EPRI. In response to some that you were discussing there with respect to climate changes and global warming, in our most recent work on performance assessment we have included such considerations, but we also included what happens at the soil, particularly as you get plant growth--as you normally will do--as you get increases in precipitation. And we find that the ranges that are actually shown on this slide are probably pretty reasonable, even though you might have an increase in precipitation of a factor of four. Then that infiltration is still fairly modest because of the other processes that become involved.

DR. NORTH: I was going to invite Dwight Hoxie to make a response to that.

DR. HOXIE: Dwight Hoxie, USGS. I would just like to make a comment on the basis of the ESSE evaluation for climatic changes for which I was responsible, and one of the recommendations that we made was to adopt the approach that we examine what kind of percolation fluxes would give us a problem at the repository site, and then try to go backwards and say, but what is the probability that we could have climatic changes in the next 10,000 years that would produce those kinds of problems for us. So we were kind of doing it in the inverse sort of approach.

DR. DOMENICO: Domenico, my last question. I notice that you didn't stipulate boundary conditions for the transport problem. Are you considering only advection and retardation? Is that why?

DR. DOCKERY: That's fair.

DR. DOMENICO: Or maybe that's a modeler's question, too. Okay, fair enough.

DR. DOCKERY: I think that all the details of both the transport and the modeling that you all want to hear for both, simulations are somewhat difference, and you'll hear from Mike, from Rally, and from Paul how the different models were handled.

DR. DOMENICO: Okay.

DR. CANTLON: Paul?

MR. KAPLAN: We're going to briefly go through what are actually two separate problems we tried to solve in preparing the data for this analysis.

The first thing I got involved with was, again, how do you prepare a stochastic data set for a problem like this? You want to do the sampling, but you have very sparse data, and we tackled at first the unsaturated zone hydrologic data. The other thing that happened a little later on was we ran into a number of parameters for which we wanted to, again, do a sample from a distribution that we had absolutely no data on and, again, no time to go through some of the other methods you'll see here, and we went through an elicitation of expert opinion using what admittedly were patchwork techniques, but it seemed to work fairly well and this is why we're presenting it.

The consensus hydrologic data set for the unsaturated zone that was agreed on by the participants included six parameters for the matrix of the problem, six parameters for the fractures; and again, our problem was to do the simulation we needed a probability distribution for each of the parameters and each of the hydrostratigraphic units. So we had upwards to five units in the 1D model. That's 50 distributions.

Many of you are aware, I'm not sure that we've actually gotten 50 samples yet from the site, from the entire site. To solve this problem, we used methods that are not uncontroversial, but I can't think of any methods that aren't, and this is something you've seen from me in the past. We discussed a little bit of this last year with respect to groundwater travel time in performance assessment.

We followed a formalism that actually I've sort of taken from Milt Harr's work and from Shannon and James. We start out in a world that's not deterministic. That's why the first bullet is up here. We are looking at things we are not going to know with certainty, and from the analyst's point of view, that density function, that probability distribution that goes into the model now is a model of his uncertainty as an analyst as to what the appropriate parameter is to put into that model.

In the framework we work in, uncertainty has a quantitative basis. The quantitative basis that we went through last year is a concept of Shannon's informational entropy. Within the framework, if we can get information, we should be able to reduce the uncertainty. In fact, in the framework we're in, by definition, information will reduce the uncertainty. You can take all the data in the world and you can pay for it. If you can't change your opinion, you haven't paid for any information.

Information now we define as the elements of a set of quantitative constraints, and again, borrowing from Milt Harr's work, we define four quantitative constraints; the minimum value of the parameter, the maximum value and expected value, and a coefficient of variation. You may not have all this information, but you should be able to obtain some of it.

As an example of how the process works, we'll work our way through one parameter, and then realize we did this, again, for all 50 units and for all 12 parameters you saw up there. Porosity in the lower hydrostratigraphic model unit of an unsaturated zone model. We had a sample from Peters and Klavetter that we felt was representative of that type of material, the bedded and non-welded units in the Calico Hills. We had one or two measurements from the one sample. We had an expected value of 21 per cent. We had no reason to believe that this is not at least a plausible hypothesis.

Coefficient of variation, a dimensionless measure of how dispersive the process is, the process meaning the sampling of porosity in this case. From the literature and from analogs, from Apache Leap tuff, from the USDA soils base, from measurements on man-made properties, 20 per cent is a high value. It's a conservative value of how dispersive the process is. From the definition of porosity, minimum value of zero, maximum value of one. This is the information available to me as the analyst.

I put this into that algorithm that says I want to maximize my uncertainty with respect to this information. By the formalism we use, we get a beta distribution. The distribution is actually continuous from zero to one. We see that the actual probability density looks fairly normal, so we get an intuitively comfortable model for porosity. It's intuitively comfortable, too, because although we use one as the maximum, we would be very disconcerted if we were actually putting values of 80-90 per cent into the model.

So what this is saying is even with these assumptions, we've constrained the problem to the point where the model looks reasonable. We had a number of criticisms in the review of this approach. In fact, using porosity as an example, a number of numerical people put out, "You can't do this. I have great numerical difficulty with some of these problems." My reply back as a geologist is, "I can't alter my description of the world because you have numerical difficulties."

(Laughter.)

MR. KAPLAN: Another comment was, from a reviewer, is porosity can never be greater than 40 per cent. Now, I did not point out to him that within the document he was reviewing, the expected value for one of the units was 41. What I did point out to him that at the time he just called me, I had just been going through Flint & Flint, one of the new USGS open file reports on the bedded and non-welded tuffs, and I said, "Are you absolutely convinced that any value of porosity greater than 40 per cent is ridiculous?" And he said, "Yeah." He said, "It can't be larger." I said, "Well, I'm looking at a data report," and I said, "Many of the values in this one unit are well in excess of 50 per cent." I said, "That's the sort of bias I'm trying to take out of the process that assigns at least numbers to our simulation."

And we followed that process through, that train of though through for the other 49 distributions.

DR. NORTH: If I can interrupt with a question, is it assumed that all these distributions are independent?

MR. KAPLAN: Yes. We've done a lot of looking at the data. For theoretical reasons, we should see correlations between certain parameters. Now, we don't have enough data from the site to ask that question, so we've asked it with respect to soils data, with respect to data from the Apache Leap tuff. We have found you can prove any hypothesis you want with respect to correlation if you go to the right data set, if it's a real data set.

One of our surprises is that we're finding second moments in real data appear to be strongly correlated, even when first moments aren't. The next time we go around, we will probably start using that information and that will change some of the distributions of these things going in.

We've done a number of sensitivity studies to ask, again, what if certain correlation structures exist that we think are reasonable? And in these layered models, with this many parameters, output doesn't seem to be strongly sensitive to at least cross-correlation. That's still a very open question. There's a lot of CPU time that's being spent right now on "what if" sort of questions on the correlation. I shouldn't dismiss that completely. Autocorrelation,

correlation--the structural distribution of that property in space we think is going to be very important.

The next problem we had is later one we needed distributions for some of the following parameters. We didn't have time to go through the process, and we didn't want to do this arbitrarily. If we go back to the formalism that we followed, keep the first part and redefine the probability density function as a model of expert's uncertainty, and realize that the expert perceives uncertainty on a qualitative basis, and go through and try and extract from the expert again quantitative pieces of information that we can take and put into our algorithm.

We can generate hopefully what are reasonably unbiased distributions of these properties here, and this is what we did. An example of some of the results from the expert elicitation, dike trend, this is orientation degrees from north looking down on the map surface. Distribution, again, gives very high weight to, again, the orientations that you see there right now, feels that certain orientations less than 10 degrees from north to the west would be highly unlikely along with, again, trends towards the east.

Fraction of wall rock entrained, this you'll see come up in the basaltic volcanism problem. The K_ds for the tuffs, you'll see these used. One of the things that I got a kick out of was they give a wide variety of distributions we can capture using these techniques. In fact, we sort of made a separate results column for the expert elicitation. It proved to be fairly easy to apply.

Now, right now there is only one trained interrogator in this particular technique. I've been putting myself through this process for several years, so it was easy for me to put somebody else through it to question them. The real reason it works is it's graphic and interactive. We asked the expert, again, for four pieces of information: Can you give us some estimation of expectation, some estimate of dispersion, the ranges of this process.

We take that information and we put it into the simulation and out comes a density function. We don't ask the expert how he thinks his information is distributed. We get the density function and we explain to him what that density function is saying. If it's porosity or if it's Kd, we say, "What you're telling me, then, is it's unlikely we're going to see this; that it's more likely to see that, that within one or two standard deviations, we're going to see the following." And we go through this, again, interrogation to find out if that's what he actually believes.

What surprised us was how quickly this worked. Some of the distributions took only several minutes to generate from the time we sat down cold with the expert to the time when he said, "That's great; I'm done. That's what

I meant."

One of our surprises is the product appears to satisfy both the expert and the analyst who's getting it. This actually is not funny. We had a number of very hostile witnesses when we first told them we were going to invite them down to Sandia for an elicitation, because they'd been asked this information over and over, and they came in with a chip on their shoulder, and they've proven to be some of our best defenders. Again, because of some of this, it was very cost effective. We had what we wanted within a matter of hours. It wasn't a long process.

The summary is fairly simple. We did what we started out to do, generated a probabilistic data base that could be used for this analysis--and you'll see, again, the results of using this data over the course of the afternoon--and a data set that we think is a reasonable data set for asking certain types of questions in performance assessment on other problems, and has been used for that.

DR. CANTLON: Questions from the Board?

DR. DOMENICO: Domenico. Paul, where's like velocity and dispersion and things of that sort? Aren't those parameters that are required in this model?

MR. KAPLAN: Velocity will be calculated. This is basically--we're up front on the problem where we're preparing the data as the coefficients of the model. We're going to run the model and then we'll get distributions for those--for the derived parameters.

DR. DOMENICO: Does the transport model incorporate dispersion?

MR. KAPLAN: Yes.

DR. CANTLON: Other questions? Staff? Yes, Leon?

DR. REITER: Paul, I gather that most of the solicitations were one expert for each parameter?

MR. KAPLAN: Given our schedule, yes.

DR. REITER: What would be the sensitivity if you had more than one expert, or several experts?

MR. KAPLAN: I don't know. This we did--this was something that was not planned, and it was something that when it came up, we said, "Okay. We've got a short period of time. We want to do this at least using assumptions that we can document, would be repeatable if we ran the person through the same sort of thing." It worked so well that we have thought about doing this in the future.

I'm the one who last year got up in front of you and said I was developing the methods and the formalism because of a quote I gave you from Ian Hacking, "Chicken guts and experts are prone to flights of fancy and corruption." I've been arguing I don't want to ask the expert. What I'm worrying is on these large integrated programs, given the schedule, given the complexity of them, you are going to be reliant on expert opinion at least for a long period of time to come.

I would probably still take the analyst's point of view. I would elicit each expert independently summarize some of the information and put that in my model. I'm not sure that I would try and work this by trying to get consensus out of a large group. It would be an interesting experiment to try, though, but I haven't thought, really, past the point of where I presented it here so far.

DR. NORTH: I seems to me it would be very valuable to find out of your list of 50 distributions, which ones are really sensitive.

MR. KAPLAN: I can tell you which ones for groundwater travel time, because that report's in review right now and uses basically the same parameters. Almost none of them. With respect to, again, consequences of exceeding the GWTT criteria as defined in the report, the one sensitivity I keep turning up is to a property that's derived from this fracture porosity.

The other sensitivity is to porosity, hydraulic conductivities, there seems to be no correlation between output and input with respect to, again, increasing the odds of failure, and I think that you'll see the results of some of the sensitivity studies today and, again, my opinion is the problem is remarkably robust. As Jerry said, we have tried hard to stress the system, to impose loads on it to generate the failures to understand them. One of the hard parts of performance assessment on this problem is stressing that system until it fails.

DR. CANTLON: Other questions? Questions from the audience?

DR. LUCE: Luce, staff of the Board. Was the distribution coefficient for carbon that we saw that Holly presented derived by this method?

MR. KAPLAN: No. It was assumed to be zero, so, again, extremely conservative.

DR. LUCE: What was the basis for that? It seems kind of low.

MR. KAPLAN: Already today I think the project's been accused of almost excessive conservatism. Maybe we bent over backwards since the days of the EA, but because carbon is such a concern, we assumed there was no retardation. There are studies, and I think particularly, I think it's some of Ben Ross's work that suggests that we should be taking, again, more advantage of a retardation coefficient for the carbon.

DR. CANTLON: Other questions?

(No audible response.)

DR. CANTLON: If not, then let's take our break and we'll come back in 15 minutes.

(Whereupon, a brief recess was taken.)

DR. CANTLON: All right, let's reconvene.

Our first presenter after the break is Michael Wilson. Michael, you've got the floor.

MR. WILSON: Okay. Well, I'm going to talk about the calculations that we did at Sandia on aqueous and gaseous releases basically for the nominal conditions, undisturbed; and first I will talk about the source term, that is, the releases of radionuclides from the engineered barrier system; then groundwater flow and transport; gas flow and transport; and then go over some results at the end.

Starting with the source term, this diagram shows some of the factors that go into our calculation of releases from the waste containers, and as has already been said before, the source model that we used in our calculations was defined for us by Lawrence Livermore National Lab, and some of the important things that go into the calculation are container environment. Some containers are in wet conditions, some were in dry conditions, some have rubble filling their air gap so that there is diffusive connection to the outside rock, and some of them don't. There is releases by advection and by diffusion. Container failure is included as a parameter, just representing container failure time that is sampled from.

The thermal effects are not really included in our calculations. The only place they enter is by way of an early thermal period in which we assume that the containers are hot and dry, so there's no releases for some amount of time at the beginning of the problem, and then there's different kinds of mobilization of the waste, and the basic model that is being used in this Livermore source model is a concept of alteration of the spent fuel uranium dioxide matrix by an oxidation alteration, and the picture is that as the fuel oxidizes, then the constituent radionuclides are freed up and available to be dissolved in the water.

Now, for the more soluble nuclides, they will be able to dissolve and get away as the alteration proceeds. For the less soluble ones, then you have to worry about the solubility limit because the solubility of the individual element will limit the releases.

And then, in addition, for the more volatile elements there is some fraction of the inventory that migrates to the pellet cladding gap and the grain boundaries in the fuel matrix during reactor operations, and it's available for quicker release.

In our calculations, these are the nuclides that we included. We included five of the highly-soluble, alteration-limited species, and five of the low-solubility, solubility-limited nuclides. All of the actinides are solubility limited. The alteration limited ones are mostly efficient products, and Carbon-14 was taken to be transported as a gas. The others were all transported in aqueous form. This notation down here is indicating that in all our calculations, we only assumed spent fuel as the source. We did not include a term for the glass waste.

And I wanted to point out at this point that you'll see later on that essentially all of our contribution to the releases come from Carbon-14, Tc-99 and I-129. The solubilities of these nuclides are all so low that--and they're all so highly-retarded, so that it's very difficult for them to be released in a short amount of time. The selenium and cesium are freed up from the fuel pretty quickly, but then they are still highly-retarded, so they don't go anywhere very fast.

Now, this is kind of a repeat of some of the terms that were on the bubble diagram. These are some of the different terms that go into the calculation of the release. There's advective releases, there's diffusive releases, and sometimes there's both. And here's an example release curve that shows some of those things. I had to kind of fiddle with the numbers awhile to get one that showed some of these different modes reasonably clearly.

You can see, if you look hard, three different modes here. There's one with short times that on this log scale doesn't show up very much, and then here is one that is a little bit longer times, and then here is another mode at even longer times, and you notice this scale goes up to 10 million years, so these here are representing a very long time.

Now, basically all we have here is this part of the curve is from the wet containers that have advective and diffusive releases, and then this part here--and probably the prompt releases from the gap and grain boundaries that are contributing to this first little peak, also. And then this part here is releases from the containers that are in wet kind of conditions and have advective releases, but no diffusive releases. And then this, at the long times, is the containers that are in fairly dry conditions and have only diffusive releases with real long time scales. And there's a fourth component that you don't see here, and that's the ones that have no releases at all.

This one here is a little more typical of the release curves that we actually used in the calculations. You can't see all these different modes in it because it turns out that the time scales for the different modes are pretty similar to each other. This curve is basically determined, as it turns out, by three parameters. There's the container failure time, and then there's the time representing the early thermal period, and then there's a matrix alteration time, and in the calculations that we made, those three times were assumed to be the same for all of the different containers, and this is something that we'd like to improve on in the future.

For example, I think that it would be reasonable for the drier containers to have longer container lifetimes and longer matrix alteration times, but we didn't take that into account in these calculations, and so all three of the modes kind of merge into one.

This is an example of what a solubility release curve looks like. For these, after an early ramp-up period, the releases are just constant until such time as the inventory runs out and we set the release rate to zero. The other thing to notice about this is that the release rates are quite a bit lower than in the previous one. In the previous one, the scale went up to 10^0 or 10^1, I forget which, and because of the low solubility, the releases are quite a bit lower for the uranium here.

Now, these are some of the important simplifications that we've made for this source model, and that's going to be a recurrent theme throughout these talks, is the fact that we are taking complex processes and making some simplifications to make them more tractable, and hopefully, as time goes on, we will zero in on the most important things and I'm not sure we can say that at this point. We don't have enough of a backing in the detailed modeling to know for sure that we have all the important things in these simple models.

In fact, this first one is something that is very important that I know we need to include the next time we do this, and that is that in this model, after container failure is supposed to have occurred, the container and cladding are basically assumed to vanish, and in fact, I think that the container and the fuel rod claddings are going to be mostly intact, with only small holes and cracks for at least 10,000 years. So this is an extremely conservative assumption and one that we definitely want to improve on in the future, and I think it has the effect of making our releases considerably higher than they should be.

As I've already mentioned, the releases were represented as several different modes. The simplification comes in in the fact that for each one of those modes, we represented it in a very simple functional form, only using a few of the important time scales to represent the different processes.

And then this last bullet is for the source model, there were a number of parameters--I think about 20 parameters--and we ended up for the purposes of these calculations only developing probability distributions for some of those parameters, and not necessarily the ones that--the most important ones. You know, it's--if you don't vary a particular parameter, it's hard to make conclusions on how important it was to your calculations. So we know that in the future we need to define some additional distributions.

All right. Now we come to the next part, groundwater flow and transport. Here's another bubble diagram showing some of the important things that go into the calculations. The most important thing that I want to point out here is that in order to more fully represent the uncertainty we have about how flow occurs in Yucca Mountain, and also as a demonstration of how to handle alternative conceptual models, we carried through two different conceptual models of flow in the unsaturated zone at Yucca Mountain.

The first is the composite porosity model, which is kind of the old standard model that has been used for most calculations in the past and is still the model that is used in most of our big computer codes, and I'm not going to say a great deal about it because it's very standard and Paul Eslinger, in his talk, is going to be presenting a lot of results that they made based on this model of flow.

Anyway, the basic assumption in this model is that you have a strong coupling between the flow in the matrix and in the fractures. Basically, you assume a pressure equilibrium between the matrix and fractures, so as an alternative to that and to kind of see what effect it has on things, we took the opposite assumption. We assumed--we developed a rather simple fracture flow model in which we assumed that all of the flow is through fractures, with no matrix fracture interaction at all. The water flows down the fractures without being imbibed into the matrix, and as I say, we carry that through as an alternative.

Now, this one, as is indicated down here, is a much more complicated calculation. You have to use all the things that Paul talked about. You have to set up stratigraphic layers, and for each layer you have to have matrix and fracture parameters of a number of different kinds. This one requires a lot less information because it's a very abstract model.

Oh, and lastly, there's a saturated zone calculation after the unsaturated zone calculation. We used the same saturated zone calculation for both of these models. After the nuclides get down to the saturated zone, the calculation from there on was done the same way in each case, and it was done using the composite and porosity model in the saturated zone.

This picture is a conceptualization of how flow would be under the composite porosity model, and it shows not necessarily a uniform flow field, but a large scale, basically continuous flow field, with a regular progression going downwards; younger water at the top, going down to older water as you go down.

On the other hand, in the "weeps" model, the conceptual picture is of a fracture network with discrete flow paths down different parts of this fracture network, and in this model you wouldn't have that regular progression of younger water down to older water. The young and old would be mixed up throughout the mountain, and I mention that just because that's a possible test to distinguish between these two different models, by measuring the ages of water geochemically.

Now, I'm going to give a brief description of the "weeps" model, and it's based on a very simple concept, and that is, conservation of water. The main part of the calculation consists of figuring out for a given amount of water, how many fractures does it take to be able to carry that amount of water. And for example, if you take an infiltration rate of one millimeter per year at the top of the mountain, spread over the area of the repository, that's 5600 cubic meters of water per year. How many fractures does it take to be able to handle 5600 cubic meters of water?

Now, I did a simple calculation which, with 10 micron fractures, it took five million of them; with 100 micron fractures, it took only 5,000; with some big thousand micron fractures, it only took 55. These numbers depend on some other parameters that I'm not displaying here. There's three or four parameters that go into this calculation, but the most important ones are that we--I just used half a meter long fractures and then the three different widths.

Now, as you can well imagine, if you had five million fractures flowing with water, there's a good chance that you're going to be getting most of the waste containers wet, but if you only had 55 fractures flowing with water, chances are most of the waste containers are going to be dry and you're not going to have a lot of releases, and that's the next step in the "weeps" model calculation.

Using a simple geometrical argument, we determined the probability for a given fracture in a given waste container, the probability that they're going to intersect, basically, and then extend that to multiple fractures and multiple containers using the binomial probability model, and the result of an example calculation looks something like this.

For a given set of input parameters, we found that if the fracture apertures are less than about 8 micron, then you contact all the waste containers with flowing fracture, but it goes down very steeply with the increasing fracture aperture.

I think the most important simplifications that go into the "weeps" model are, number one, the major flowing fractures were all taken to be the same size for a given realization. We do have sampling, probabilistic sampling on the fracture apertures from one realization to the next, but within a given realization, all of the major flowing fractures were taken to be the same size. That's something we're on and extending right now, and it's not that hard to put in a distribution of fractures for each realization, but I'm not going to talk about that here.

As I already said, one of the main assumptions behind the model is that there's no matrix interaction. The flow is just down the fractures, and because fracture flow times are so fast, we simply neglected the travel time through the unsaturated zone. If the travel time is a couple or three years, then you might as well just say it's zero.

And lastly, another major assumption is that we assume that only the waste containers that are contacted by a flowing fractures fail and release the radionuclides, and all the others are sitting out there basically in relatively dry conditions, and they do not have releases.

This is a reminder of what the composite porosity model looks like and we're going to talk about it a little bit next. As I said, we need a lot more information for it. The main thing we did to simplify this calculation is to go to one spatial dimension in our transport and flow calculations, and what we did is to divide the repository up into six strips of equal area, and each one of those strips was represented by a single 1D column at the points shown here along the transect that Holly talked about earlier.

This is what the six columns looked like. There were five layers, as has already been said. The two easternmost columns only had four layers because the fifth layer is below the water table.

Now, this is just kind of a place for me to talk a little bit about retardation. This table is a little bit different from the one in your handouts, but all I've done is changed the order so that it's a little easier to talk to. The top two, iodine and Technetium are known to have small retardation, and so they were just assumed to have zero retardation.

The top three here--tin, plutonium, and Americium--are known to have a lot of retardation, and what we did is we followed Los Alamos National Lab's minimum K_d approach for these. They felt that for all of the kinds of minerals that the nuclides were likely to flow through and for all the different water compositions that there might be, that the K_d would be at least 100, and so we just used 100 for those, which should be conservative.

Then for these middle ones, we thought it was important to actually define distributions, because for them the retardations are kind of in an intermediate range, where it's not negligible, but it's not huge, either. Actually, cesium, the retardation's big enough that we probably could have used the minimum K_d, maybe with a K_d of 20 or something like that.

The important simplifications that go into the composite porosity are, number one, the basis of the model is that you have strong matrix fracture coupling. We did not do any thermal modeling, so all of the flow and transport calculations were done under an isothermal assumption. The water flow is just taken to be steady-state. We did not try to model the climate change dynamically, but instead, chose the infiltration rate corresponding to some future climate and applied it from the beginning of the time. And as I already indicated, one-dimensional vertical flow and transport. And it's always something to keep in mind that representing retardation by a K_d is a big assumption and something that we do have people trying to determine the validity of.

All right. Now going on to the saturated zone part of the calculation, we decided to take a rather simple approach and just use the old standard USGS model which was set up by Czarnecki and Waddell several years ago, around 1984-1985, and you've already seen pictures of the important parts of this. This is the high-gradient area to the north of the repository, and down in this area, which is the direction of flow, basically to the south or southeast is the expected flow direction from the repository. Down here you have a relatively flat hydraulic gradient, so the water velocities are lower.

One thing about this Czarnecki and Waddell model, though, is that it's kind of an amalgam. The material properties are kind of an amalgam of the tuff properties and the lower carbonate properties, so that we think that the water velocities in this model don't represent the water velocities in the tuff part of the saturated zone particularly well, and the tuff velocities are probably lower, as Dwight Hoxie said this morning. Some people think that the water in the tuff saturated zone may be essentially stagnant and you may have very low water velocity. So we think that in doing this, we're being somewhat conservative, but still, it's--this is definitely something that needs to be improved the next time we do this.

This is the distribution of saturated zone water velocities that we used based on that model, and it's a pretty narrow distribution, around four meters per year velocity, which means a travel time to five kilometers of about 1200 years. So it's a travel time that's well below the 10,000-year time limit, which ended up meaning that the saturated zone did not reduce our releases very much.

The important simplifications here are this business of not really representing the tuff aquifer properly, and right now we have some people working on trying to put together a 3D representation of interaction between the different aquifers so we can get a better handle on this.

We assumed strong matrix/fracture coupling in the saturated zone, which may not be conservative at all. There could be fast paths in the saturated zone, just as there are in the unsaturated zone. That's something that we did not look at in these calculations.

And lastly, a fairly important shortcoming of this is that it's basically based on a single realization of the saturated zone, so we are not representing the full range of uncertainty very well.

Okay. Moving on now to gas flow and transport, here's another bubble diagram, and the factors under ambient conditions in Yucca Mountain, the factors that are important to a gas flow or travel time calculation are that the distribution of temperatures and pressures--and also humidities--at the surface, and the geothermal gradient, and the distribution of permeabilities and porosities within the mountain. Those are the things that go into a calculation under ambient condition.

Now, you add a repository, the heating from the repository is an important additional driving force for the gas, and if you're going to talk about transport of Carbon-14, which is what we're really interested in here, then you need information about any retardation of the Carbon-14.

The model that we used as the basis for our calculation was developed by Ross and his co-workers at Disposal Safety under contract to Sandia Lab, and it's a two-dimensional, steady-state gas flow model, and these are some examples of the kind of results that they have.

This top one shows a full pattern at ambient conditions, and it shows basically air being sucked--well, not sucked in--but air entering, being drawn in at lower elevations and then expelled at higher elevations. There's a very strong chimney effect here. This black line here indicates the location of the repository, though in this one there is no repository. There's no heating there.

The lower one, there is a heating along this line, and that heating does a lot of things. It makes the flow pattern more complicated. You can have convection cells form. The concentration of the outflow at the higher elevations is lessened because you have so much stronger driving force forcing the air vertically upward.

For the Carbon-14 transport calculations, we did use a retardation factor. These are retardation factors imposed on the gas flow velocity now, not on the water flow velocity, and the retardation factors used are temperature-dependent, and what these are based on is an equilibrium calculation of a partition of carbon between carbon dioxide in the air and by carbonate in the water, and there are important things that are left out of that.

There is certainly some possibility that the carbon may sorb onto minerals in the rocks, or that there could be calcite precipitation, giving additional retardation to the carbon. Those things were not included in this calculation.

DR. LANGMUIR: Langmuir. So that makes this a conservative assumption, because you could have Carbon-14 exchange with calcite.

MR. WILSON: That's right. The fact that we've left those things out is conservative.

Now, we had available calculations like this along four cross-sections. These right here were done on this particular cross-section here. We also have results from three other cross-sections, and what Ross and his co-workers did for us is to generate travel time distributions of Carbon-14 by releasing particles at various locations along these cross-sections within the repository area, and then using a particle tracker to determine how long it took the particles to reach the surface.

And these are the Carbon-14 travel time distributions that they calculated, and number one, don't be confused by the fact that there's four of these and there were four cross-sections in the previous figure. These are not figures for those four cross-sections. Each one of these lines has all of those four cross-sections built into it. These are travel time distributions for four different temperatures, and this one here represents nominal condition, and as you can see, there's a strong effect when you had some repository heating. These are the temperatures at these higher curves. This one is 15° higher than nominal, basically; and this one's 30 and this one's 60.

And the interesting thing you see about this is that these curves are all bimodal, and what that's coming from is the faster, or the shorter travel times with the small probability first hump are coming from the Carbon-14 that manages to escape directly out the Solitario Canyon wall, and the majority of the Carbon-14 in the second hump is the part that has to go through this lower permeability layer and all the way up to the surface.

Now, these curves of travel time were done deterministically at one particular configuration of gas permeability and retardation. For our probabilistic calculations of Carbon-14 releases, we did some sampling on those and what we did is we just moved these curves over, depending on what was assumed for the permeability. The permeability for the welded tuff that was used here, for example, is 10^{-11} square meters, if that means anything to anyone, and we assumed a range going down to lower numbers, down to 10^{-12}, which is a factor of ten lower in permeability, and what that would do is move the curve up a factor of ten in the travel time, and as I say, that was sampled from a distribution.

DR. LANGMUIR: Michael, before you take that off, to what extent is the reduced solubility of CO_2, and therefore, it's equilibration favoring your long travel times? If your right-hand curve was 27°, that's when the CO_2 is most soluble and where you might expect the most C-14 exchange; and therefore, from that reaction, the most retardation and the longest travel times. You've got that effect, but you've also got temperature enhancing diffusion at the other side of it, too. Presumably, they're working together, but how important are those effects relative to each other?

MR. WILSON: I'm not quite sure I'm understanding the question. Our transport was assumed to be entirely advection-dominated; there's no diffusion.

DR. LANGMUIR: I'm just wondering what components of the model are driving this across there?

MR. WILSON: You mean the different temperatures?

DR. LANGMUIR: What the effects are we're looking at here that do this.

MR. WILSON: Well, the most important one is just the fact that the flow is a lot faster with the higher temperature. There is an assumed temperature dependence of the retardation factors, but it's not a strong dependence, so that's less important.

DR. DOMENICO: Michael, Domenico. That retardation factor, again, of 30 or 40. In the unsaturated zone, that means retarded with respect to what; vapor?

MR. WILSON: This is retarded with respect to the air flow.

DR. DOMENICO: The air flow?

MR. WILSON: Right.

DR. DOMENICO: Okay. So a retardation factor of 40 is not a big retardation. The air is moving pretty fast?

MR. WILSON: Yeah, that's right. What you saw here is that you have Carbon-14 travel times on the order of thousands of years, maybe down to hundreds of years, and that means that the air travel times are down as low as tens of years.

Now, in order to use those--maybe I should emphasize again that these travel time distributions were calculated for steady-state conditions, and that's an unfortunate drawback. In order to use them, we had to be able to associate those repository temperatures with time in some way, and so what I did is to use some results from a few years ago by Tsang and Pruess in which they did some site scale gas flow modeling with their program, TOUGH, and this is the repository temperature curve that they came up with.

Now, these results are not entirely comparable to the Ross, et al. results, the most important reason being that in these calculations, Tsang and Pruess used a much lower value for the air permeability. Now, how close this would be to the correct Ross, et al. temperature curve depends on whether the temperature--the cooling is conduction-dominated, or whether it be the high gas flow that provides an important additional cooling, and that's uncertain at this point.

DR. DOMENICO: Michael, Domenico again.

The last meeting we were at, the thermal loading meeting, we saw suggestions of temperature on the order of 200° C for long periods of time.

MR. WILSON: Of how much?

DR. DOMENICO: 200°C. Can you comment on the effect of that on the Carbon-14 problem?

MR. WILSON: Yeah. Well, number one, those very--temperatures that high are temperatures near the waste containers; whereas, this is intended to be an average over the repository. Now, it is a fact that with a higher thermal loading you may end up with the temperatures staying higher for a longer time, and I don't have any real way of knowing how that would affect our results.

One thing that needs to be included if that is studied, though, is getting all the correlations between gas permeability and everything done properly, because something that Ross is seeing is that at the higher permeabilities that we think are correct, the gas flow is strong enough that it provides an important cooling to the repository and it prevents the temperatures from getting up as high as they might otherwise, but there's a lot of work that needs to be done on that.

These are some of the important simplifications that go into the gas flow calculation. Number one is we're assuming pretty high gas permeabilities, and as a result, we leave out diffusion in our transport calculation. The transport is taken to be advection-dominated. That's a fine assumption as long as the permeability is high. If someone can convince us that the gas permeability is lower, like the Tsang and Pruess value of permeability, then diffusion would become very important.

Secondly, in our calculations, the gas flow is decoupled from the water flow, and once again, with the parameter values we're using, we think that's a good approximation. However, if you change the parameters to get into a different regime, then that could become a problem.

The travel time distributions are calculated for steady-state conditions. That's something that we have to work around, basically, and the temperatures, we always try to take conservative values of. That's something I didn't point out on this curve, is that these are the different steady-state temperatures that we have, and we chose them in such a way as to remain above this temperature curve. And then, lastly, the carbon geochemistry was simplified by leaving out interactions with the solid phase.

Okay. Now let's go on to some results--

DR. LANGMUIR: Michael, before you go on, just one last --Langmuir.

Your overhead in which you show the temperature effects on arrival times, aren't you really looking at a composite curve in the real case, where you start with the higher temperatures, and as you move away from the heat source, of course, you're crossing these lines and you rather quickly end up on the, maybe the 27 Celsius line; in fact, for most of the flow path.

MR. WILSON: Yes, that's right. This curve here is used for any releases that occur, I believe, in the first 2400 years, and then releases that occur between Year 2400 and 4800, we use this curve; and then between 4800 and 10,000 years, we use this curve.

DR. LANGMUIR: But in the actual repository, you're starting at the higher temperatures, and as you move out away, you're going to the lower temperatures; and so, in fact, within a few meters, or tens of meters of the repository you're going to be at 27 Celsius for the rest of the flow path?

MR. WILSON: Yes. That's right.

DR. LANGMUIR: Which means you're going to be--

MR. WILSON: So this is another way that this is conservative.

DR. LANGMUIR: So you're fundamentally looking at that longer times of arrival in general, if this model's correct?

MR. WILSON: Yeah. There are several ways in which this is conservative, and that's another one, yes. Obviously, when we do this again we would prefer to have a coupled gas flow and thermal model and Ross and his co-workers are working on that right now.

Results. In order to get the probability distributions of releases that we want for comparison with the EPA standard, we used the Monte Carlo simulation, in which for your model parameters you define probability distributions. Then you sample some set of realizations from them, and for each realization, you do your calculation of flow and transport and source releases. Then you put that into the form that you need to compare it with the EPA regulation--we call it the EPA sum--and so then at the end of the calculation you have an EPA sum for some number of realizations, and I wanted at this point to say something about how many realizations we're using.

For the composite porosity calculations, we did 300 calculations for each one of the one-dimensional flow column, so that means a total of 1800 flow and transport calculations. For the other calculations, the "weeps" calculations and the gaseous release calculations, they're much simpler and we did a thousand each of those, and the reason for wanting to do somewhere in that neighborhood of calculations has to do with the EPA mentioning a probability of one part in a thousand in their standard.

This is what the aqueous release results looked like for the composite porosity model and for the "weeps" model, and you can see that the releases in the "weeps" model are somewhat higher because of the fact that the travel times in the unsaturated zone are very fast, basically, though it's actually surprising that they're as close as they are, and I don't think there's any meaning behind that. It's basically a fluke, the fact that these two curves are as close as they are.

This shows the contribution of different nuclides to that, and as I mentioned before, the non-retarded elements are the ones that dominate the releases. Technetium accounts for most of the releases in both models, with Iodine contributing about 20 per cent, and that just has to do with their relative inventories. With the "weeps" model you have much faster travel times and so you get a small amount of some of the intermediate retardation nuclides showing up.

This is the curves that we got for gaseous releases in the two models. This is for the composite porosity and for the "weeps", and I should say explicitly, in case people are wondering why there are these two different curves for the gas flow, when composite porosity and "weeps" are different models of water flow, and that's just because of the source releases are determined by the water flow.

So for this one you have a gas flow calculation with the source releases of Carbon-14 determined by the composite porosity flow; and for this one, the releases of Carbon-14 are determined by the "weeps" flow model of water. And you can see that these are kind of high. Personally, I'm not concerned about that because of what I said at the very outset, that I think that the source release model is very conservative. It's unrealistically making our releases quite high, and so I think if we put in a realistic accounting for the slow-down by the cladding and container barriers, and the fact that probably most of the fuel rods won't even fail in 10,000 years, I think that'll move these over some. And it's also something that's possible to address by engineering.

If people do decide, after looking at things more realistically, that Carbon-14 is a problem, it's something that can be taken care of by building the engineered barrier system to contain Carbon-14 better, also.

This shows the--oh, and I wanted to say at this point that the gaseous release curves were enough higher than the aqueous release curves that if you make CCDF's for the combination of aqueous plus gaseous, it basically looks the same as that. And this is the contribution of the different nuclides to that combination curve. For the composite porosity model, the Carbon-14 releases are so high that they are accounting for almost all of it. In the "weeps" calculation, it's not quite as dominant, but it's still the major contributor.

And in conclusion, let me just say that I think that the way we're going about this, using relatively simple models, but in many cases the simple models are directly taken from more complicated models, that I think that works reasonably well.

These preliminary calculations show that Carbon-14 is the greatest contributor to the releases, and that, given no change in the regulations, I think that will continue to be true. I think that a more realistic calculation will reduce the numbers, but it seems likely to me that Carbon-14 will still be the one that has the greatest releases.

And then, also, the preliminary modeling shows that localized fracture flow is actually preferable to the more large-scale flow field because you affect fewer containers. Fewer containers release their nuclides.

DR. CANTLON: Questions from the Board?

DR. DOMENICO: I have one. I have an observation. Domenico.

You mentioned that the travel time to the accessible environment was on the order of 1200 years--

MR. WILSON: For the saturated zone part, yes.

DR. DOMENICO: --for the saturated zone. Assuming you've been in the barrier for 500 years, that means that anything that has a distribution coefficient equal to 4.5--a retardation factor equal to 4.5 or smaller will break through.

MR. WILSON: Right.

DR. DOMENICO: It seems to me that with the large inventory of technetium--and that is a large inventory. Iodine's a pretty small inventory. It seems like you would appear to me to be in violation with technetium unless there's something about your source code that prevents its release from the engineered barrier. Is technetium released slowly from the barrier for some reason?

MR. WILSON: Well, the inventory of technetium is high enough that if all of it were released right away, it would have an EPA ratio of about 1.2, I believe. So it would exceed the limit, and the fact that it was below the limits indicates two things. The source releases do prevent its releases sometimes. In some realizations, all of the technetium would be released pretty quickly, and then that would contribute to these ones here that go up to about one. These are realizations where essentially all of the technetium is getting out.

But it just turns out that in the calculations, that doesn't happen that many times because there is a distribution of container failure times. Some realizations have a container failure time as long as 10,000 years, for example, and the flow factors in the saturated zone and the unsaturated zone release it in some of the realizations.

DR. DOMENICO: Yeah, it also shows that a distribution--a retardation factor greater than ten won't get out, and those that have 100--which represents the majority of the inventory--are totally immobile.

MR. WILSON: That's right. Yeah, in fact, if--I've done some other calculations in which I put in some retardation for technetium. Instead of having its K_d equal to zero, put in some distribution that is just a few tenths, and even a K_d of a few tenths is enough to move it over an order of magnitude or two.

DR. CANTLON: Other questions from the Board?

(No audible response.)

DR. CANTLON: Staff? Yes, Leon.

DR. REITER: Leon Reiter, staff.

Mike, I didn't quite understand. Did you allow a colloidal transport, also?

MR. WILSON: No. That's something else we're leaving out. I would very much like to include that, but we have not had the time yet.

DR. REITER: So in listening to all the conservatisms, wouldn't that be an unconservative?

MR. WILSON: Yes. I didn't think to include that in any of those lists, but it is included in the report.

DR. NORTH: Similarly, organic complexing of some of the actinides?

MR. WILSON: That's something we haven't looked at at all. I think people--well, some of us probably have some, but that hasn't even entered my consciousness yet. But I have heard of it. I know that it is something to worry about.

DR. DOMENICO: Domenico again. The thing to point is that you are definitely depending on a sizable retardation to meet the regulations.

MR. WILSON: Yes, for things like plutonium and Americium, I would say so.

DR. DOMENICO: And everything else, really, because if we include complexing and we include colloid transport, to me, retardation goes to zero just about and you lose it.

MR. WILSON: Yes. And we want to do some calculations on that, but the thing to remember is that chances are, if you have things like that going on, it's only going to be some small fraction of the nuclides that are being affected that way, and so we need models for a number of different things; not only the colloidal transport, but the formation and various things like that. So that's to be done.

DR. CANTLON: The engineered barrier that you used is the one in the base plan?

MR. WILSON: Right. Basically, it's modeled around the repository layout and container shown in the SCP.

DR. CANTLON: Questions from the staff? Yes.

DR. LUCE: Yeah, Luce, staff for the Board here.

Is it in the offing to sort of update this particular model when fracture distributions and matrix flow proportions in various parts of the repository area become available?

MR. WILSON: Well, certainly, as time goes on and additional information are available, we will try to incorporate them as best we can.

DR. LUCE: Because right now you have sort of like a fixed amount of porosity, and you're divvying it up in one case between the two.

MR. WILSON: You mean in the way we're handling the two different flow models?

DR. LUCE: No, within the composite porosity modeling. I mean, you're assuming a total amount of porosity and--isn't that correct?

MR. WILSON: I'm not sure what you're getting at.

DR. LUCE: Well, I'll talk to you about it later, maybe.

MR. WILSON: Okay. I'm sorry I don't understand the question.

DR. CANTLON: Other questions? Staff? Audience?

(No audible response.)

DR. CANTLON: Okay. Thank you.

The next speaker, then, is Ralston Barnard.

DR. BARNARD: It's nice to have another Barnard on the staff, because it assures people how to pronounce my name.

I'm actually going to give three talks, one after another, and so watch closely so you can tell where one stops and the other begins.

First, I'll talk about human intrusion, and what we did in this case was to investigate two scenarios from the human intrusion of entry, which I show here. The two scenarios are a surface release and a saturated-zone release; other words, one straight up to the surface and another one down into the saturated zone and, thence, out to the five kilometer accessible environment boundary.

The reason we chose those is because those cases seem to be those with the greatest potential consequence for release, and they relied on essentially a direct; that is, mechanical transport of the waste. The consideration was that aqueous or gas transport processes in the unsaturated zone would certainly be slower than mechanical processes.

Now the way we did this all was to abstract what we felt was occurring in the event tree. This is a slightly different process of an abstraction that has been described to you before, because, we took into consideration every aspect, every one of the features, events and processes (the box is shown here in purple) to some degree. But generally speaking, the way we considered them was to less detail what we knew would be occurring.

We looked at two drilling incident scenarios. One is where the drillhole intercepts the waste and directly removes the contaminates, and either takes the contaminates to the surface or deposits them in the saturated zone. We looked at both a base case analysis and also sensitivity studies where some of the input parameters were varied.

Some of the conceptual model assumptions that we made are that drilling occurs by 20th Century practices, i.e., rotary drilling with diamond bits and big long drill stems and the whole bit; nothing exotic. As you heard this morning there is a great imponderable, a complete open question about what is the probability that anyone would have any reason whatsoever to go out and explore the Yucca Mountain site at all? Well, we chose to finesse that by just saying that we would say the probability that somebody was out there at sometime in 10,000 years was 1.0. We are going to give that one away.

The number of boreholes that were drilled was taken to be the guidance provided by the EPA in 40 CFR 191. That specification, that guidance is three boreholes per square of kilometer, per 10,000 years for drilling in non-sedimentary areas. The importance of that I will show you later in one of the sensitivity studies.

As an illustration of the simplification, what we did is assume that the probability of a hit is based strictly on the geometry. In other words, the intersection of a circular drill bit with a circular waste package. And as I said, the transport is mechanical, and as a result of that, the source term is one of the most important determinants of the release because we factored out almost every other consideration that would be involved.

So, here is an illustration of what is going on for the surface release drilling scenario. You have a drill bit which has passed next to a waste package here. As the result of passing by it, the package is ruptured and the drilling fluid, which is circulating for the process of maintaining the bit and removing the cuttings, goes down the middle of the pipe and comes up past this breached waste package, entrains the waste and up it goes and dumps it in the mud pit.

The other scenario for the release down into the saturated zone considers that the driller has now drilled his hole and left the site, and there is an empty borehole here. In the process of drilling, the drilling skimmed passed the package and broke it open. So there is an empty drill hole here with all these fuel rods teetering around them and they start falling down the hole. They manage to fall 265 meters down, or at least, down below the water table where they are sitting at the bottom of the hole in the saturated zone. The flow of water in the saturated zone comes along, rapidly dissolves the waste sitting here and off goes red water instead of blue water.

We considered not only direct hits, as I illustrated, but also what I call near misses. That is, the bringing up to the surface of contaminated rock, which I will illustrate next.

Some of the assumptions we had to make in order to model this, is that the waste is uniformly distributed in the repository and up to an entire waste package can be released.

Here is an illustration of the waste being uniformly distributed in drifts and around it is the near miss, the contaminated rock. As we have made a recurrent theme through this talk about how conservative we have been in our assumptions, and I just wanted to reiterate some of the conservatisms that were done for this analysis. If you talk to a driller about what happens if he has a couple of hundred feet of drill stem down a hole and he is drilling away and he hits something hard, like a big chunk of steel, most of them will tell you that the drill bit will be deflected away and move off and not go through this chunk of steel down there.

Despite that, we bored ahead resolutely and said that the package would be damaged and up to the entire waste package could be entrained in the drilling fluid and be brought to the surface, although we considered not necessarily all of the waste package would, but this was a factor that we included in the analysis.

Now the contaminated rock arises, because if you have a waste package here which due to natural causes gets breached and then you have a transport of some kind into the adjoining rock, you would expect to get a halo of contaminated rock surrounding each waste package. The simplified model that we used for that was based on the work that we did in PACE-90, which found that for the infiltration percolation rate we used in PACE-90, a very low value, the transport processes was essentially diffusion dominated. Based on that, I assumed that diffusion was the method by which you would generate these halos and calculated results which are not to scale up there.

These are, as I have illustrated, those are the mechanical transport methods used. Now in contrast to the 10 radionuclides that were used for the aqueous transport and the gaseous transport problems, for this source term, since the radionuclide inventory was essentially it for the variable that we had to consider for releases we used a 43 nuclide source term. That consists of all the radionuclides for which there is an EPA limit, i.e., those with half life greater than 20 years and for which there is a sufficiently large inventory that you should bother to worry about them. So 43 radionuclides were used to be carried to the surface. Ten were used in the case where it was carried down to the saturated zone. We did consider both decay and chain ingrowth that would occur from decayed chains as well as for fission products.

Here is an example of what the surface release distribution looks like. What this represents is 20,000 simulations of repository histories. Each repository history is for 10,000 years. Each repository history assumes that there are 17 boreholes drilled over those 10,000 years. The 17 number rises because you take three boreholes per square kilometer times the size of the repository.

So, you punch 17 boreholes into this over 10,000 years and you get a certain number of hits. Well, with an extremely infrequent occurrence as this, it turns out that it is very nicely described by a Poisson distribution. At the rate of 17, it turns out that the most likely number it hits is zero for this level. You do get in many cases more than zero. Sometimes you can get up to three or four occurring in 10,000 years.

What you do is you see how many hits you have, how many near misses you have and over 10,000 years you sum up all the releases that you get whether they are from zero, one, two or three hits, and produce an EPA sum, which is the sum of the EPA ratios of all the 43 constituents that you are looking at.

The releases fall into three categories. One is where you have a direct hit and the distribution that you see here arises from two causes; one of which is that since the drilling time was randomly specified for the 17 drilling incidents over 10,000 years you have some decay in some cases. So, if you have a hit late in the game, you might have a release down here. Up here for the very highest releases you might have one or two hits and if one of them occurred early in the repository life, you could get a fairly large release. The other aspect of it is, as I said, not the entire waste package was brought up if there was a direct hit. A range from zero to 100 percent of the waste package was allowed to be brought up.

For near misses the same decay was applied. Only the mobil elements were considered to be able to diffuse out those being iodine and technetium. There was range allowed for the amount that was brought up resulting in a peak, considerably bigger peak for that case. Last but not least, what this shows is that there are very few cases where you came home absolutely scott free in these 20,000 analyses--

DR. CANTLON: Before you take that off, did you assume that all of the containers had the halo leak around them or some percent?

DR. BARNARD: All of them did.

DR. CANTLON: All of them were leaking?

DR. BARNARD: Yeah.

DR. CANTLON: Another conservative element.

DR. BARNARD: Yes.

Okay. So that is what the distribution looks like showing the releases measured against the logarithm of the EPA sum. In other words, direct releases.

Expressing as a CCDF, we see that for the base case, our releases can be as big as about six or so, six times the EPA sum. But, this is down at the level of two in 20,000; one in 10,000 down here. So, these are the direct hits and as you saw it was quite bimodal and you get over here and here is where the near misses are occurring at the order of about 10^{-5} of the EPA sum. The red zone here is the EPA limit.

Now I mentioned we did some sensitivity studies. And, there are a lot of nice parameters for which we don't have a lot of confidence of our values, so I decided to vary just about everything I could think of. Some of the variations were in the magnitude of the diffusion coefficient; the magnitude of the amount of waste which was considered to be in the halo; the nature of the source term; and biasing the time to assume that you might have institutional controls so there would be a relatively less drilling occurring at the beginning, but relatively more towards the end of the 10,000 year period. And, finally, the only one that made any difference at all was to assume that the number of drill holes that was drilled in there could be varied.

Now here the base case is based on the EPA guidance for drilling in non-sedimentary--what is the phraseology for rocks that are not underlain by sedimentary things, structures or something. So, anyway, that is what this is. This supposedly describes Yucca Mountain. That is the blue curve here. If instead you decide to say that you will use the values of 30 holes per square kilometer which is the value which is used for sedimentary structures, you get the orange curve here. And we are getting close. But, what it takes is for you to double that again to say, well let's suppose you punch 340 holes into this repository area over 10,000 years and you come up with the green curve here.

I suppose I could have gone on, but I ran out of patience on my VAX, so I didn't. That was the most significant sensitivity study that I did which showed anything that was worth reporting on.

For releases through the saturated zone, these are the CCDF's looking at the results for the tuff aquifer. There are separate results for the carbonate aquifer, but I am not going to show them here. What I am going to show is the value for the sum, and then the different components for the most important radionuclides. The message here is that with 21,000 rather than 20,000 trials were getting in the order of 10^{-3} of the EPA sum in contrast to values up about here for the direct surface release. So, this is telling us as logic would certainly lead you to believe that a direct release at the surface would be the most direct way of getting radionuclides to the accessible environment, which is not much of a surprise.

We included Carbon 14 in this because we assumed that the Carbon 14 some of it would be contained in the fuel rods and could therefore be carried down directly to the saturated zone and be dissolved.

What happens if you come up with an overall conditional CCDF looking at all three drilling scenarios. AS I will discuss in part three of the talk, this is one of the ways in which CCDFs for separate events are combined. And what these are is for mutually exclusive events. What we considered, which was definitely a modeling simplification is that if you had a drilling event, either the stuff was brought to the surface or it fell down the hole, and if it fell down the hole it either stopped at the tuff aquifer or went on down to the carbonate. But no mixing. We kept them separate. The result is that we have the surface release direct hits here. Here is the surface release near misses. And, this other little slope in here is the contribution from both the tuff aquifer and the carbonate aquifer releases. So there isn't too much of a modification to this CCDF as a result of including the saturated zone releases.

Well, to conclude this part of the talk the releases from human intrusion are below the EPA limit. It looks as if based on this model you need to increase the drilling density considerably before the releases approach the EPA limit. The near misses are way away from the EPA limit and it looks as if it is not necessary to consider the impact of drillers hitting contaminated rock and bringing that to the surface.

It also appears that all these results are independent of site characteristics. It is pretty hard to conceive of any particular property of the potential Yucca Mountain site that impacted the analysis in any regard. Possibly the only way in which there would be an impact would be to include the probability of drilling.

This is a conditional CCDF, meaning that we have assumed that drilling is going to occur. Now, if you include and factor in the probability of drilling, if we have already started with the probability of one, assuming that it is 1/10th or something like that, you would move this curve down throughout all the way along, and it would be even farther removed from the EPA limit as a result of however much you assigned the probability of drilling.

Now, the aqueous releases as Mike has talked about and I have conveniently glossed over, are quite dependent on the estimates of ground-water velocity and retardation. The velocity that we used was taken from the work that Mike described and I am certain that if we had used another velocity we would have gotten a considerably different answer.

Lastly, it isn't clear that if we were to use more detailed models that we would come up with an estimate of human intrusion releases that would be of any more use to us than what we have right now. But, I am not sure of that.

Moving along to basaltic igneous activity, we look at one scenario from the basaltic igneous activity of entry. That is illustrated here. There is a number of possibilities, but the one we looked at is where the intrusion acts directly on the repository and we have a dike forming. The dike reaches the surface, forms a basaltic cone and the flowing magma fragments the waste, entrains it and hauls it up to the surface and dumps it out on the surface where there is direct exposure.

It is probably true that there are other scenarios which actually may have a greater impact than a direct one like this, but this is certainly scary and is pretty high in the public perception of a massive catastrophic failure of a site like this, so we decided we would do this one first.

Again, we use abstractive models. But, this time we abstracted them in a different fashion. What we did is we borrowed a lot of other people's work and applied it to this analysis.

Bruce Crowe has done considerable work on both the model and the parameters and we developed two simple models for the process. I'll cover them in a minute. But, what we did in this case was to rely on the years of work that Bruce Crowe has done to identify exactly what the processes are and to discuss the probabilities of all of these events happening and to use that as a model. In the--we did both a base case and several sensitivity analyses as we did with the human intrusion case.

Well, what were our conceptual model assumptions? Well, we said that a basaltic dike is going to act directly on the waste packages. What we looked at is a dike passing through the repository and by means of thermal-mechanical processes, grinding up the waste package, entraining the waste, and off it goes for its merry ride to the surface.

We assume that the fragments are erupted as part of either a cinder cone or a lava sheet, something like that. And that somehow miraculously, the waste is not encapsulated in lava when it is at the surface, but it is lying there as glowing chunks of uranium oxide and stuff, so the people can get the maximum dose from this. In other words we take no credit for any kind of encapsulation or weathering or anything else like that.

Well, how did we model this? The main assumption was that the amount of waste entrained is linearly related to the volume of intersection of the dike and the repository. To illustrate that, here is a bunch of dikes modeled as going through the repository. The process we considered to be happening is, that as a dike moves up from depth, it does not knock a plug of waste or a plug of anything up to the surface, like when you squeeze a tube of toothpaste or something like that. What happens is, all this material is pushed aside and the dike shoots up a crack or something like that. After that occurs there is erosion of the wall rock due to vesiculation and other processes which after the dike has gone up and made a conduit, you can start to scrub the stuff off the wall from any depth on up to surface and that is what is expelled at the surface.

Well, of all that stuff which is scrubbed off the wall and taken to the surface, some portion of it is going to come from the repository horizon. Some portion of that which comes from the repository horizon is going to be the waste. Not all of it waste, it isn't poured like concrete in there, a certain amount is. So, a certain fraction of a certain fraction of a certain fraction is what reaches the surface.

We looked at two different models as a way to do that. One was geometric. We said, all right, what do we know? We know that the interaction has to be at the periphery of the dike with the repository. So, what exactly is the periphery? It is given by the perimeter by the dike times its thickness and times the extent to which it sticks into the repository. If you make that calculation that is what we have called Method 1, the geometric method for calculating the amount of waste reaching the surface.

The other method was to say, Bruce Crowe has tromped around that area for ten years and has observed a number of volcanic cones and stuff like that and he has looked at all the xenoliths in there, the country rock which he finds at the surface and he has identified how much of that comes from the depth at which the potential repository would be located. So, let's use that information and we will modify that by some fraction to reflect the amount of waste which would be characteristics of those rocks from the repository horizon and we will make an estimate of how much that would represent bringing to the surface.

So, we have two methods. One of which calls for an explicit determination of what the dike width is, what its orientation is, how much wall rock fraction and so forth. Using the method described by Paul Kaplan, we elicited by means of a rubber hose and a computer from Greg Valentine in Los Alamos, a lot of good information and got a lot of distributions. Then we went to Bruce Crowe's work and got the probability of occurrence for this event happening.

The first thing that we found that wasn't a big surprise is that it was an extremely low probability of occurrence for a volcano, igneous activity to occur. The prediction is that it is a low probability of occurrence for it to occur at the site. If we had relied on the same method that was used for the human intrusion, it would have been necessary to run hundreds of thousands of analyses, simulations, in order to get one or two volcanos popping up in this length of time.

The assumption was instead, what you do is you calculate the consequence of a igneous eruption and assume it is going to happen and then when you are all done you multiply by the probability of occurrence to save yourself a lot of computer time.

The distribution of results that we got then, is dependent on, not on the probability of occurrence, but it is dependent on the variations in the parameters that we used. As you can see an itty bitty dike here like this should logically produce a smaller release at the surface than should a humongous dike like that. So that gives us the variations in values.

For sensitivity studies, we looked at reasonable parameter variations. Certainly if dike width is of the order of meetings, and you get a certain result, if you were to say that the dike width was one kilometer wide, you would get a very large result, but we didn't consider that reasonable, based on observations.

Here is what the two models for surface release look like where we are including the probability of occurrence of igneous activity. That number happens to be about 3 X 10^{-4} over 10,000 years. So that is why the peak value here is 3 X 10^{-4}. But the method one, the geometric method where you look at the periphery at the dike and make that calculation, comes out here and it is up about 8 or 9 times the EPA sum at the 1 in a 1,000 level. For the surface observation method, it is about an order of magnitude lower.

What do we conclude about basaltic igneous activity? The direct releases are below the EPA limit and even though we made a whole bunch of conservative assumptions, we still don't have to apologize much because the results are below the EPA limit. We were unable to find any sensitivity studies that we could do which resulted in a major increase in the releases. Because of the small probability of occurrence, it doesn't appear that releases from the basaltic igneous activity are going to contribute. It is just a fact. They do not contribute significantly to this estimate of total system releases.

But, most importantly, we have now taken a look at the direct scary possibilities that would occur from volcanism. But I think much more importantly are to look at some of the indirect effects,such as, what happens if you have a volcanic igneous activity which changes the regional water flow? And instead of having a water table at a certain level, it might rise. Or you might have an increase in the head or something like that which would change significantly in an indirect fashion what goes on.

Okay. Third talk. I am going to talk about combining CCDFs.

So far you have heard from two of us guys about the four different components Sandia Total System Performance Assessment. The aqueous, the gaseous, the human intrusion and the igneous. Now we have to roll it all up to present you one measure of performance which this product intends to be able to produce.

There are two methods, stepping back and looking at this in general, there's two methods for generating an overall CCDF. For the first one, what you do is you go out and get the world's biggest computer. Then, you write a single Monte Carlo simulation with all important aspects of the problem included in there. And you crank away and come up with an answer that directly gives you an outcome based on everything that is important in the problem.

Well, we couldn't get a hold of the world's biggest computer on short notice, so looking at number 2, this method is what is discussed in the SCP as a method that should be--is described in the SCP as one way to do the job.

Another method is to look at all the scenarios, all the scenarios and decide which ones of those are significant and arrange them into mutually exclusive and exhaustive scenario classes so that everything is covered and everything is covered exactly once.

For each one of these scenarios you calculate a conditional CCDF, then when you are done, you weight them by the proper weighting of each scenario class for its contribution to the overall system performance. Well, we didn't do that either. What we did was to use a modification of method 2 and to pick four scenarios which I have just identified. For those we calculated conditional CCDFs.

There is no representation whatsoever that these scenarios are exhaustive, clearly not. And they are pretty much mutually exclusive because of the ones that we picked. But, what we did is it was necessary to combine these by various techniques which reflect the lack of knowledge of all the possibilities going on and the fact that it wasn't mutually exclusive and so forth. And we want to emphasize that the result that we have is still conditional because it isn't complete.

So, how do you go about combining CCDFs? There are three methods, one of which is to use a weighted sum. That would be appropriated for mutually exclusive scenarios. An example of that would be the human intrusion, as I said, it either falls down the hole or is carried to the surface, but is not both. So you can say that there is a 50 percent probability that it is going to surface and 25 is going to one aquifer and 25 is going to the other. That adds up to 100 and that would be a weighted sum for doing this.

The second method, a horizontal condition is really an expedient for not doing the problem in a more complete fashion. If this particular problem could be solved in which the correlations were actually included, then you would automatically include--you would automatically associate high releases from one aspect of the problem, with high releases from another.

For example, talking about the aqueous and the gaseous problems, if the same factor, namely the source term is driving both problems, then they are highly correlated, because a large aqueous release implies and is implied by a large gaseous release. So, they are correlated. And the way these are handled is to say well, if we know that high releases are associated with high releases, what we will do in a CCDF is just add horizontally. I know I have a CCDF around here. So if you had two curves on here you add the releases across for every single probability value and come up with a single one which combines them.

Lastly, if your results are completely independent, there is no relationship whatsoever between one scenario and another, what you do is you calculate each of them individually, and you take those sums and then you draw sums from each of the components and get a distribution of values when they are combined in that fashion. This was done for the six unsaturated zone columns used in the total system analyzer.

So, to illustrate those, we had altogether 14 CCDFs to combine. There is only 13 shown here because the Volcanic 1 and Volcanic 2 are kind of different case and it is hard to say whether they are independent, mutually exclusive or what they are, so we considered only Volcanism 1, since it was the higher of the two.

But, for type three where we want to treat independent cases and combine them, here is what happens when you look at the six columns from the unsaturated zone and for the combination. So, what was done is values were randomly drawn from each of those resulting in an outcome here like this. The law of small numbers is grabbing us nicely here, because you see that one of the components is larger than the total, but we will ignore that.

Okay. How about method 2, which combines the composite porosity, the gaseous and the aqueous for the composite porosity model. That is the composite gas and the composite aqueous being combined here to give us a composite result for the nominal case. Here we see the aqueous. This is kind of an eye test. If you look down here you can just see the orange for the gaseous, but since it dominates so much, the green curve which is the sum overlies it almost completely.

I mentioned already how the Type 1, which is the mutually exclusive are combined, so I am not going to show that to you again. But, what I am going to show you is the overall conditional CCDF which includes the composite porosity and the human intrusion. And volcanism isn't even on here because it is way down here at 10^{-5} or 10^{-6} and we just didn't plot that. So this is this combination here giving us this CCDF.

Here is the human intrusion with the shape that you remember from before. The nominal conditions again dominates and so the only place that you can distinguish it from the combination is right about there. That is the overall results for the total assuming the composite porosity model.

Lastly, it is possible to combine the results and give you a total-total and the reason these have been split up in this fashion rather than reporting a total-total in the first place, is because there is some question raised by the NRC about whether alternative conceptual models should be combined in any fashion whatsoever. They believe it shouldn't and we are using that logic here.

So, despite the fact that we shouldn't do it, we are anyway. This is the total for composite and the total for weeps resulting in both. But since we have no idea about what weighting one should use, this is 100 percent weeps, this is 100 percent composite porosity and this is what a 50/50 would look like. But you see all we can say about this is that hopefully it is bounding the total system performance based on the two alternative conceptual models that we have.

Since I am the last Sandia speaker, I am going to give you a two bullet wrap-up of what our Total System Performance Assessment achieved. We used abstracted models and data and we think the results have been quite successful. I guess this is our definition of success so, what we feel is that the results we presented are consistent with our understanding of the processes that we have gleaned previously from the more detailed models. In other words, we have not produced inconsistent off the wall results based on the prior work that we have done.

We have produced conditional CCDFs for the four scenarios and rolled them all up into one. We recognize that there is a very long list of work that this has engendered a number of questions for and were excited about doing more of this and seeing if we can start to resolve some of those.

So, I guess that is it and I'll entertain questions.

DR. CANTLON: Questions from the Board.

DR. ALLEN: Can you explain just very simply why your results of the volcanic scenario seems to differ so greatly in their consequences with those of John Trapp?

DR. BARNARD: No. I can't.

We have used results which we feel are consistent with Crowe's work, but what I was unable to show was that the volume of material which we calculated coming to the surface that would be waste is consistent with the amount that he predicts, namely of the order of less than 100 cubic meters worth of waste being brought up. In fact, it is like 20 to 50 cubic meters. It is consistent with that. It is consistent with some work by Link, et al, who look at the fraction of the repository they would expect to be released. So, I really cannot try to resolve the difference with

Trapp's work. I am sorry.

DR. DOCKERY: Holly Dockery, of Sandia. I might be able to add a little bit.

I think most of the difference in John's models is that he is talking about a different probability of occurrence in the first place. And the distributions of frequency of occurrence within the area and within the repository block, took a distribution of values. The values were based on information from Bruce Crowe's work on probability of occurrence as well as the UNLV structural model which has the Stagecoach fault being the most likely extension.

So, there is a fairly broad range of frequency that is incorporated into this. However, Jerry, might be able to talk better to the specifics of John Trapp's numbers. I don't know them right off hand.

DR. BOAK: Jerry Boak, DOE. I have to admit it has been awhile since I have looked at John Trapp's analyses.

DR. CANTLON: Other questions from the Board? Staff?

(No audible response.)

DR. CANTLON: Audience?

(No audible response.)

DR. CANTLON: Thank you.

The last speaker, Paul Eslinger.

DR. ESLINGER: This going last, before supper is just as bad as going last before lunch time.

I'm going to talk about some analysis that Pacific Northwest Laboratories did starting from the common data set, in most cases a common data set with the Sandia work. I might emphasize here that I am a speaker for a team of nine people who worked on this, so hopefully I can relay all the assumptions and results, but I certainly didn't do this by myself.

As talked about by several other people, we looked at the same set of models for our--the same set of scenarios that the Sandia work did with the one addition that we looked at water table change from tectonic activities. So the other scenarios are defined similar to what you have already heard this afternoon.

We did a little bit difference analysis with the source term than the other work. The model we used for spent fuel considered inventory of the spent fuel, the crud, the gap between the cladding and the spent fuel, the grain boundaries and the fuel matrix. We also looked at the glass dissolution model using SRL-202 glass, so that was one difference in terms of what we did.

Both of the models, the spent fuel and the glass model looked at an alteration rate of the fuel matrix and the limited released based either on the alteration rate or on the individual rate of radionuclide solubility. And we looked at a 1-D mass transport model from the waste container into the host rock.

One other difference in what we did from what Sandia did is we looked at lower infiltration rates than what they did and didn't do anything that would be considered a climate scenario; our climate change scenario.

As such, most of the releases or all the releases we looked at were diffusion dominated in terms of the transport from the waste container into the host rock. Because we don't have a really nailed down waste container, the material we looked at failure times from a statistical distributions rather than calculating the failure of a container.

To go on with the source term model, because of the human intrusion, we looked both at the unsaturated zone and the saturated zone in terms of what you had to model. Inventories of spent fuel mix that was the same as Sandia's runs from origin. Then we used some reference inventory based on radionuclides we are looking at for the glass waste form.

In terms of looking at these CCDFs, some of the things that we varied in the source term model was whatever flow rate, in fact it is the only "random" parameter that we varied for the source term model. There is dependence on chemistry, there is a dependence on temperature, but we used one reference temperature profile. We used one ground-water chemistry history and those types of things.

Then in the unsaturated zone we looked from 0.0 to 0.5 mm a year. In the saturated zone we looked at pore velocities. We had a movement of about four. There is a magnitude based on different assumptions of conductivities in hydraulic gradients. And for all this stuff, I am talking about where we are looking only at 10 radionuclides. And those 10 radionuclides are listed on this next plot.

This gives a release profile for a single container, a container is assumed to hold two metric tons of waste for spent fuel again you see the kind of measure we have talked about. In this particular, and this is just an example of some of the source term calculations. In this particular one, this container started failing at 2,000 years and had all failed by 4,000 years, so you get this ramp up based on number of containers contributing to release.

Then you get radionuclides. If the fuel alteration rate is high enough that they soon reach their individual solubility limit and then you get others at the top of the graph here that release at a much higher rate.

This is an average waste container release for spent fuel. If you convert that to a release from an engineered barrier system, technetium which is the top line and goes up to about ten curies a year.

DR. LANGMUIR: Paul, did you take into account the temperatures at the fuel in the calculations of the affected solubility on release?

DR. ESLINGER: Yes. There is a functional dependence of the solubility on temperature.

If you look at similar sort of analysis for one of the proposed glass forms, the SRL-202 glass, have plotted on the same scale releases from a glass waste container that was assumed again to hold around two metric tons of equivalent of waste. One thing I noticed is that three of the nuclides, iodine, carbon and cesium aren't contained in that reference waste form. They are assumed to have been removed either through the processing or within the processing.

Again, this is a single container average release. It is unclear at this time how much total volume of glass waste will be in the repository. So, these can be compared directly this way.

DR. LANGMUIR: How do you release a radionuclide to a aqueous phase via solubility when its temperature is 200 degrees at the waste and there is no water.

DR. ESLINGER: This particular example starts at 2000 years. The temperature profile has dropped dramatically.

In no case did aqueous phase releases start before the temperature dropped to 100 or down to the boiling point. We sometimes assumed that the waste container failed before that, gas could get out, but the aqueous phase release had to wait to start until temperature dropped.

You have seen the Yucca Mountain conceptual model several times already today. We modeled the same stratigraphy as Sandia did so we will just set that one aside.

Now if you look at gas phase transport, what we did is we ran some transient thermal modeling using the mountain, the cross-section that I just showed there. The transient model used two-phase flow, and again this is a composite porosity sort of model, and it is two-phase flow and the heat transfer could be by convection and conduction both. species transport and here we looked at only Carbon 14 for the gas phase, could take place in the liquid or the vapor phases. We allowed boring to go on and as you probably know, most of the computational effort takes place in the first couple three hundred years in the mountain because of the highly transient effect.

We ignored capillary hysteresis, rates in thermal equilibrium assumptions and assumed no conductive heat transfer through the gas-phase. I think this assumption is different than some of the current assumption that Ben Ross is doing, if I understood Mike Wilson.

We used a decay heat source that is the reference that came out of the reference information base. But just one thing to note on this on thermal histories, is that by the time you get out to around 2,000 years, the power output from the waste has dropped off to a very small fraction of what happens in the very early years.

Some of the boundary conditions in the gas-phase transport. We assumed no flow on both ends of the domain. This one was moved out away from the end of the repository to try to get away from boundary effects. This one, the end of the model domain does coincide about with the end of the repository, so we are missing off the left end the crest of Yucca Mountain, this is the crest here, and then going down to the Solitario Canyon. So these results are a little bit different than what Ben Ross's results were.

Carbon 14 releases, this one is an example of release profile not scaled to release from the engineered barrier system in curies per year. One thing to look at is that this peaks at about a curie per year in terms of Carbon 14 being released into the host rock. Now, this is Carbon 14 that comes off some, a small fraction that comes off as a gas that starts with and then the rest of it is released from the spent fuel matrix over time.

If you look at temperature and liquid saturation, the top half of this plot is temperature. This is at 100 years past waste emplacement. Down here is a liquid saturation field. What you see, for this particular analysis the repository horizon is peaking out at about 120 degrees at 100 years. Down at the bottom you can see the affect of the different layers in the saturation field. The water table here--nearing saturation at the water table. The non-welded unit down here. The different hydrologic properties giving you a different saturation. The top blue line across here is the repository horizon and this is our gradation on the grid on the particular model. Then you see a drying out down to the very low saturations. In fact it gets down to the residual saturation in the repository horizon.

This was a case we ran to start out our analysis trying to come up with decent starting conditions for the rest of the runs at 0.00 mm/yr. recharge rate. Trying to get our domain to a steady state. I am going to go ahead and show results of 0.0 mm/yr., because as we get later on, the transport results depend heavily on the infiltration rate. But it does have an example here. Now at 1,000 years, the thermal pulse is propagated roughly half-way to the surface in terms of being elevated. The saturation field at a 1,000 years, there is still a significant dry-out. In all these runs a little bit of water has migrated laterally around the end of the repository and gone down. So, at least in this 2-D slice you get some higher saturations, both above and below the repository horizon and some lateral movement in the water in that case.

If you go on to 6,000 years, the thermal pulse, at least relative to this scale of quad has virtually disappeared. The hydrology has come back close to what it was before you put the waste in and started the thermal pulse. And again, these are the units below with a different saturation.

Now look at an example plotted 6,000 years of Carbon 14 contours in the mountain. We ran into an interesting problem trying to figure out what gas tortuosity factor to use as a function of saturation and porosity. There is a couple of them out in the literature and we have a couple of here; one by Millington & Quirk on the top-half of the plot. It shows a gas tortuosity that leads you to much higher concentrations around the repository and much less spread. The Penman down at the bottom, now this is just a plot over three orders of magnitude. There is material reaching both the water table and the ground surface. The next plot will show some of that cumulative.

But, what we found is because of this high dependence of tortuosity factor on the saturation, that when we ran higher recharge rates, the saturation in the mountain went higher and effectively eliminated the transport to the surface. I should note here when you get the reports, you can read all the fine details, we ended up using the gas permeability value for the Topopah Springs horizon and used it for all the way to the surface. We don't have the contrasting layer at the top. We used a value that is on the order of 10^{-14} to the meter squared which is three hours of magnitude lower than what Ben Ross was using. It has a big impact, of course on the results.

This one is diffusion dominated. If I understand correctly, they are drilling some holes and are going to be running some gas permeability tests. So, when we get some real data from the mountain, we can stick it in there and see what kind of results we get at that stage.

Cumulative release of Carbon 14 to the ground surface, two curves here, the top one is cumulative release in curies and the bottom one is the flux rate as of function of time. This particular example cumulative release to the surface, used waste containers that started failing at 300 years. Cumulative at 10^{-6} curies on the model starts showing up on the linear scale at about 1200 years. By the time 10,000 years has gone by, 2.4 curies has been released to the ground surface. Now this is much, much smaller than the results you would get if the gas flow is invection dominated as the other analysis shown today. We used a zero retardation factor for Carbon 14 in these analyses.

To summarize a couple of things I already said, there is in this particular model is a very strong coupling between the level of saturation of the rock in the gas tortuosity factors, so strong that when you get our model up to about .01 millimeters a year, there aren't any gas phase releases to the surface. There is movement in the mountain but it is very slow because of the relatively high saturation.

There were some interesting saturation profiles that Al Flint, actually, I guess at the January meeting of the Nuclear Waste Technical Review Board they showed a couple of plots of saturation versus depth. Right above the Topopah Springs, there is a unit where saturation is essentially one, not quite saturated but real close to it. If that sort of unit exists over the entire repository, then there is a possibility with this coupling between tortuosity and saturation you will have very low releases of Carbon. If that unit isn't contiguous, then you can get some much higher releases. So, it would be some interesting analysis with the data coming up.

Switching now to the unsaturated zone, for the rest of the analysis we went back to isothermal, single phase flow, steady- state hydrology and constant infiltration rate. The infiltration rate between 0.0 and 0.5 mm/yr. in this composite porosity model in a 2-D domain.

To look at hydraulic head distribution for one of these runs, just for illustrative purposes, one thing to note, this thing is exaggerated about 8:1 on the scale, so if you plot it the scale looks a lot different. But, Ghost Dance fault, the dotted lines are material properties here. The Ghost Dance fault is in as an offset and again this is UE 25A, G-4 and H-5. The two different holes.

If you look now at a darcy velocity plot that goes along with this particular head distribution, you can see some affect of the different hydrologic layers here in some horizontal movement. The fault here leads you to slightly or higher flow rates as the stuff moves down here and it goes down beside the fault and then the same, you get some movement off and running against the boundary on the far side.

As you increase infiltration rate each of the features here become more pronounced. There is more lateral flow. There is more effect to the fault.

DR. DEERE: Question. Ghost Dance brought me to the service here.

Are you treating it with the same permeability and its only effect in your analysis is that it offsets units?

DR. ESLINGER: There was a ten-fold increase in the conductivity in a one meter wide region. So it is an offset plus a conductivity increase. Actually the fractured density was assumed to go up by an order of magnitude in the narrow region of the fault. So it was much more highly fractured and then properties were computed off of the fractured density for the conductivity. So, it wasn't offset in some other parameters.

DR. DEERE: So you will be anxiously awaiting some of the underground data results?

DR. ESLINGER: That's right. I can't wait until they drive the tunnel boring machine past the fault and we all take a look.

DR. DEERE: You got an "A".

DR. ESLINGER: I've been primed a couple of times here.

If you look now at the unsaturated ground-water travel times again an example picking off of the same recharge rate, used a particle tracking approach. Looked at nine particles placed evenly along the repository horizon; looked at their travel paths and their travel times to reach the water table. The first 150 meters until they reach one of the next hydrologic layers goes relatively fast. This is from one to six million years down here and then they go slower after that.

This again is assuming composite porosity model and you have flow rates where you don't start stressing the fracture flow. So, this is basically through the rock matrix. When you get above one millimeter a year, then you start getting much more interesting things going on.

But given those extremely long ground-water travel times, you shouldn't be surprised to see that there aren't any releases to the water table for the gaseous phase. And again to point out here 10,000 years, a range of infiltration rates, because of the range in infiltration rates and the hydrologic properties, you don't get water moving through the fractures. Now if we get down in there and we find some weep somewhere, then the model has to be revised to take into account those kinds of things.

If you go onto the saturated zone, set the stage of the hydrology for the human intrusion case, there isn't any release from the saturated zone for our base case because we don't have any source term that went into the saturated zone. But to set it up for the human intrusion case, used again isothermal single phase flow on a 2-D domain, but this time we ran several parameters of statistical distributions to get the CCDF. We did the distribution of hydraulic gradients. We put on spatially correlated hydraulic conductivities fields. We used the distributions on radionuclide sorption values discussed earlier. And the time of drilling is a random event as well.

2-D domain started, for the repository this is basically conceptualized as a north-south cut, four kilometers wide, seven kilometers long. We started out with a four kilometer domain because we were going to look at the effect of the base case into the 2-D and we didn't get any results out. But, we didn't bother to change the domain either.

Here is just an example of hydraulic heads in the carbonate for one of our stochastic runs as a function of a spatially correlated conductivity field. The data from the conductivity field here came from some of the analysis we did for the early site suitability work. But, as you go on to some of the path lines in the saturate zone hydrology, the conductivity fields that we used left things essentially one dimensional in terms of the way the travel paths looked. As you would expect travel times are dependent very strongly on hydraulic gradient in the conductivities.

Looking at travel time ranges, the carbonate, we got some stuff that is a very wide range here. Partially welded tuff though is much slower moving, starting out at about 5,000 years out to two to three million years. Then, because if you look at the saturated zone, the north-south cut, it has both the partially welded and the zeolitized enter the saturated zone. We looked at some analysis that had both properties and it had much slower travel times.

One thing to note here is somebody this morning mentioned, I think, the JF-13 hole where they had done some pump tests. My understanding of the data, if that is the right hole designation, is they pumped for 36 hours and they got 500,000 gallons of water, 2 X 10^6 liters. That will show up in the dose calculations a little bit too. But what it does, that was a Topopah Springs unit they were pumping from, which is part of the tuff here. And it says, the assumptions we made back last summer on conductivities may not be in the ballpark for what you would get when you analyze that particular data. In fact, they would be much higher than the values that we used in this analysis. This was based on data available at that time. It certainly will change in the next go round.

As you go onto human intrusion, the analysis that we did was similar to Sandia's analysis. I think the only one thing we did different is they used a 15 centimeter diameter hole and we used a 30 centimeter diameter hole that was our common data base that had a couple of glitches left out that we didn't figure out until we got through, that we had done something different.

Rally talked about near misses. We did near misses by looking at transport to pick up on just the hydraulic head distribution. When we did a drilling event, we randomly picked a location somewhere along this transport domain and then drilled a vertical hole through the domain. So, when you talk about a near miss to waste container we took the concentration field corresponding to that time from our transport runs, drilled through it, looked at the concentration down that column and then counted them out of material. And the drilling events did occur at random times.

However, for our near misses, the material going from the engineered barrier system into the far-field transport model, was dumped into modeling domains which were five meters high and five meters long. So, you lowered the effective concentration right next to the waste container into the effective concentration in our smallest modeling grid block.

What happens then if you miss but you drill holes through the waste domain, is you get very small effective releases because of the amount of material in the domain is very small in any one 30 centimeter hole if you missed the waste container.

If you make the assumption that when you are drilling you are unlucky enough to actually hit one of these waste containers and bring it up, then you get releases which are much higher. In our case, topping out at about .1 EPA limit, .1 in terms of the 10 radionuclides happens to be the entire inventory of a waste container at relatively short times, times between 100 and 400 or 500 years.

So, again the same sort of assumptions on drilling rates and the number of holes going in the mountain, expected number of holes is three per square kilometer. So, you get releases that look very similar to what Rally looked at.

Now, if you look at the scenario where you are injecting, or you are drilling into the carbonate aquifer which is 500 meters or so below the saturated zone and somehow when the driller pulls out you get the waste in there, you get a release now which maxes out for a single waste container at below 10^{-2} times the EPA limit. What happens is that the things with the fairly high retardation value, fairly high being anything over about two or three in this case, get retarded enough based on our travel times of several thousand years that they don't show up even if the drilling occurs fairly shortly after the repository is closed. The retardation makes a big difference. There is no plot for the tuff aquifer because all the travel times were too long in the tuff aquifer that even if you had it drilling at year zero, it wouldn't get there.

Moving on to basaltic intrusion. We took a little different approach than Sandia did. We went to the literature and chose a rather recent analytic model for basaltic dike formation. These are some of the assumptions, it's isothermal within the magma, low Reynolds number. You get turbulences as it comes up; undersaturated. And again, when the dike happens to intrude through the repository, whatever waste takes with it is homogenized in the magma and then you get a partition function of the amount of rock that you pick up. It goes from depth to the land surface.

I put in just a little picture of the model. You get both width and breadth out of this model as a function of several of the variables. I guess one thing to note, that this thing turns out to be a 1/10th power. So, you can start out with some fairly large numbers and by the time you take your 10th root you are back down to a fairly small number.

We did do a random analysis on this. We chose ranges of these parameters that go everything from the smallest basaltic analysis we have seen to the flood basalts of the Columbia Plateau, which are some of the largest that have been known to occur and got some ranges on these particular parameters. After sampling some distributions, what we came up were a random set of dike widths and lengths which we could use to couple with our transport analysis to find the amount of material that is entrained when the dike comes through.

Now these scales are different. They are meters on the horizontal axis and kilometers--

DR. ALLEN: Excuse me. What is breadth?

DR. ESLINGER: In this case it is the length.

DR. ALLEN: Length. Okay.

DR. ESLINGER: Yes. The literature used the term breadth, so I put it on there to be consistent.

We see, based on this analysis, most of the dikes have a width of around half a meter or slightly less and a length on the order of 1500 meters.

These dikes were assumed to form and go through one of the concentration fields based on random occurrence times, again similar to the human intrusion analysis. The dike intrudes through the concentration field, which is time dependent and entrains some of the rock and goes onto the surface. It also, if the time of occurrence is beyond 2,000 years, can start to entrain some of the waste form that is within a waste container. We assume that in early time frames the container has enough structural integrity that the magma will go by it and not physically lift it. But at later times, when the structural integrity has been compromised it can come up through a drift, hit a waste container and entrain some of the waste. In fact we assume that you can entrain up to some maximum number of waste containers which is a random function of the orientation of the dike relative to the orientation of the waste emplacement drift.

Now here conditionally, we are assuming volcanism has occurred. So, given that volcanism has occurred what kind of releases do you get? Well, we get stuff that goes out to about 2.2 times the EPA limit which happens to be an occurring dike that intersects a couple or three waste containers and entrains some of that waste. The left tail I didn't plot, it goes on down for another four or five orders of magnitude on this plot, in that you have a volcanism that goes through. It is a narrow dike and entrains a little bit of rock. We use the maximum of 5.8×10^{-4} for action. I think if the earlier PDF on that, that was down within the range of the PDF that Paul Kaplan showed earlier in terms of the amount of rock that was entrained as it went through the repository. So, we get consequences which can be above the EPA limit, but not huge.

Now there was a fair amount of talk this morning about tectonism to the early site suitability. We took a look at tectonism, starting to look at that after we started some of the other analysis. We wanted to include some of the effects of tectonism, so we had to find a model for initiating event. To compute a CCDF, you also have to have at least some estimates of occurrence probabilities and then we can calculate the consequence.

What it is, we looked at a quick review of some of the literature. We did not do a new tectonism analysis in terms of the field studies or anything like that. And the literature seems to say that there are at least three processes that could have potential impacts. Early failure of waste containers due to faulting; changes in rock permeability due to faulting, which could affect both gas phase and liquid phase; transport and then a rise in the water table due to earthquake.

We happened to pick on the event just to pick one of them instead of all three of them to work on, we picked the water table rise event and we looked at some stuff done by EPRI in their analysis. I guess this stuff preceded some of the stuff talked about this morning. But, basically they were talking about earthquake, looking at a range of parameters you need, compressive strength area, full compressibility, porosity. They had a table in their results, which I have copied here. It is based on some real simple 1-D models, probability of exceedance in 10,000 years versus the amount of water table rise in meters you could expect from one of these tectonic events. And based on some expert opinion in this simple model, they have got some water table rises, a range as a function of this compressability.

We used this to kick off our tectonism analysis. Now this result here is much different than the December paper by Carrigan-Barr and a couple of other folks, who talked about a similar sort of analysis, but they used a 2-D model rather than a simple 1-D model and they are talking about water table rises on the one to five meter sort of range. So, it is much smaller on their estimate of the maximum water table rise than this analysis.

But, if we take this as a coupling into a transport model, we said, well let's just do a water table rise. We started the upper end of 100 meters, changed our base case stratigraphy to move a water table up 100 meters, and guess what? We didn't get any releases to the water table. You have got a difference in saturation in those lower profiles, but again, because of the infiltration rates of 0.5

millimeter or less a year you were still in a diffusion dominated transport process. So that is the big caveat to stick below this. Based with that, when we got through the analysis we didn't have anything different than what we had in the base case, which was zero releases.

If you jump on now to these conditional CCDFs and combine them together, we took a little bit different approach than Sandia did. But, what we did is we ran several base case analyses with transport as a function of time. On those base case analyses we superimposed human intrusion events drilling with the mass of all waste containers, drilling and bringing a container to the surface and then drilling and injecting waste into the two aquifers.

Based on the same base case analyses, we also had an occurrence of volcanism or not. So we ran multiple simulations down each of these particular paths and we came up with, these aren't the conditional CCDFs I have show you so far. Those have been turning on individual events. But anyhow, if you go through that, you get multiple samples which statistically you are able to combine in the weighted sum using probabilities to get an overall CCDF.

In this one, it is the CCDF for overall performance. Again, with all the caveats on it that we have been talking about all day. In the upper right-hand corner are the EPA limits. If you look at this particular CCDF, you can pick out the different scenarios going on. Up here on the left is human intrusion, variable release rates, but where you miss all the waste containers, but you bring up that little bit of contaminated rock.

The flat portion in the middle here was our zero recharge rate where you get Carbon 14 released to the surface. Only one of several base case analyses that we did, did you get any Carbon 14 to the surface.

The next hump down here is human intrusion where you start hitting a waste container and the down below the 10^{-4} level is the volcanism consequence now weighted with a crunch probability around 10^{-4} of the occurrence over 10,000 years. This doesn't have any high recharge rates in it to incorporate the affect of the climate change scenario.

If you look at sort of a summary of what we did, we took some of these scenarios and they have showed some scenario trees earlier, and we found ways to incorporate the transport analysis in our case using transport models which were 2-D, some transient thermal analysis and to incorporate effects into our total systems analysis. We did it with a few preliminary data and models which haven't been validated yet. But still based on the data in hand, when we started and did the analysis, I don't think there is any result that we get that says not to continue on with looking at the site.

I am going to switch gears now and talk about some dose estimates based on the releases that we got out of these transport models and also some releases that Sandia provided from some of their runs. We did dose estimates just on a few runs.

Let's go back and take a look at the regulations. If you look at a 1985 version of 40 CFR 191, it talks about individual protection from ground-water for the first 1000 years for an individual. And, it applies all the significant sources of ground-water.

If you look now at Working Draft 4, which came out in February, they look relative to doses, individual protection for 10,000 years, move this out to 10,000 years instead of 1,000 years for ground-water. Then they also have one of the options they are considering for doing population protection for all scenarios. But, when you are doing population protection there is not an individual protection limit for the disturbed scenarios. So, there is an option here of doing a much more extensive dose model.

Now, taking that regulatory framework which I'll point back to in a little bit, we looked at exposure pathways for calculating doses. One was gas-phase Carbon 14 to the surface and the other under undisturbed performances, we had a well 5 kilometers away where you are extracting water or using it, either for drinking, having a garden or doing a farm.

We also did some doses based on the human intrusion analysis where the exposure to the driller and exposure to the post-drilling dweller. We also have analysis in the saturated zone from human intrusion drilling.

To point out the model we used, we used the model that started out in ICRP 26, which is modified on through 30 and 40 where dose equivalent is the linear combination of organ doses. Working Draft 4 of 40 CFR 191 points to ICRP 60 instead of ICRP 40, which is their basic dose modeling, but there are very few changes between the one we used and the ICRP 60.

When we look at exposure times for doses, we have the driller who gets a 40 hour exposure, assuming he is already an adult by the time he goes and drills, he has a 50 year commitment after that. All the other scenarios, you assume a 70 year half life where you get an exposure over the entire lifetime. I am going to report individual doses, not population doses, but they are not necessarily a maximally exposed individual.

The next three slides are here just for your information on assumptions. One of the assumptions we used was a farming 20,000 square meter farm. You irrigate it six months a year from a well. One thing to point out here is you need on the order of 10^7 per year to irrigate that farm. The farm then provides all your edible plants, beef, eggs, poultry and milk intake. And because you are farming, you spend a lot of times outdoors. The exposure pathways include ingestion, external exposure, re-suspended dust.

If you look at a garden scenario, scaling back in order of magnitude in terms of the area of 2500 meter square garden, which is still a big garden. Again, you irrigate it. Here now something is that you provide 25 percent of your fruits and vegetables, but you don't raise livestock on this particular garden. So that changes your ingestion pathways somewhat. You spend less time outdoors.

Food consumption rates, we went to the Hanford Defense Waste Environmental Impact Statement and pulled out the sort of the latest generic assumptions on food consumption and used those as well. Drinking water is 2 liters per day which is what the EPA suggested to being a reference value. And then other consumption rates.

One thing to note as well before we get into the values, we took models that we had designed to look at cumulative release of radionuclides to accessible environment. We took those releases and now we are translating those into concentrations to do dose estimates. We made an attempt to make those concentrations make sense. If you set out to calculate doses, you may well set up a conceptual model aiming at concentrations of cumulative release. I don't think we misapplied things, but you would do it different if you were heading out to start with to calculate doses.

We looked at dose from Carbon 14 to the surface. We have three particular runs here. We calculated doses for the maximum rate that material was released to the surface and we calculated it for one lifetime. So we are assuming that it is coming out as a constant rate at the surface. This is Sandia's composite model, not component model here; the Sandia weeps model.

But basically source and curies per year released to the ground surface of 10^{-2} a couple of orders of magnitude larger and a couple orders of magnitude smaller so that the three runs here spend quite a range.

What we did is we calculated an air concentration by assuming a 10 meter mixing depth, 3.3 meters per second average wind speed across the top of the mountain and then looked at the repository area, assuming material comes out of the entire repository footprint.

The doses quoted here come from a garden scenario in terms of millirem per year. The highest one here is .1 millirem per year and they go down into the microrem and even smaller stage.

If you look at what you get, if I am not growing a garden, if the guy is just living there, then those go down by a little more than an order magnitude smaller. What happens is you are not getting very much release to the surface per year and the wind reduces that concentration dramatically.

If you now look at a spent fuel container, we chose four drilling scenarios out of the set of all drilling scenarios that we did at four different times, remember there are several random things going on. The amount of waste entrained, the drilling time, recharge rate and some other things. Now, if you look at the dose the driller gets, now he's only there 40 hours, but he is dumping it right around his boots and he is chewing on his fingernails and gets a little bit of dirt ingestion that goes along with it.

In fact it is interesting, based on the set of 10 radionuclides that we looked at, chewing on your fingernails is the worst thing to do in terms of the pathway for where you get dose.

Now this is millirem he gets for the 40 hour exposure that he was out there drilling the hole. Americium 243 is the maximum contributed to that dose. Now if you assume the waste that came up is spread out over a garden and you start gardening, living there and eating those products, you can get a fairly hefty dose rate in millirem per year, mostly from neptunium out of the 10 radionuclides that we looked through an ingestion pathway.

The thing to note here is that if you look at the regulation individual protection limit doesn't apply for an intrusion scenario. So, this would fall under the category of part of the population exposure that you would have to compute for their 2.5 million person rem exposure over 10,000 years at a 90 percent confidence.

If you look at just the external dose that you get, assume you didn't grow a garden, here in this particular case they drop about an order of magnitude, not quite, but pretty close to an order of magnitude in terms of the dose. But you are getting on the order of rem tens of rems per year.

Now this was based on one lifetime. The rate drops off fairly quickly because irrigation leaches stuff down deep enough into the soil you get below the root zone and also get enough ground shielding that the rates drop off fairly quickly. But this is the lifetime that starts when the stuff gets down out and plowed in the first time.

We were looking at one waste container there. If we look at one waste container now, looking at the injection into the carbonate aquifer, the material dissolves and the aquifer moves 5 kilometers and comes up an extraction well, here looking at five of our particular runs at different drilling times. This is the year when the maximum dose was received at the five kilometer. In some cases it occurs 1500 years later, in this case it is significantly later.

The maximum dose and millirem per year now are ranged over three orders of magnitude depending upon the particular drilling run we made. And also, it depends a lot on the aquifer dilution values. Again, if you look at the recent pump tests in the tuff, in 36 hours, they got more than enough water to irrigate this garden for a year. So, the Topopah Springs may supply, in some reasons at least, plenty of water to do a whole lot of water extraction. But, what happens, you have a limited amount of material going in each year in terms of contaminate, the further you dilute it the lower the dose rate is.

Again, for this particular case neptunium was the radionuclide that provided like 96 or 97 percent of the doses. Iodine does a little bit and technetium does a little bit.

If you look back at some of the cases that Sandia ran, the base case composite model, the base case weeps model and then two drilling scenarios, one going into the tuff aquifer and one in the carbonate aquifer. If you look at the time the maximum dose occurred in a couple of cases it is well beyond 10,000 years.

I went ahead and did them here, because if you ran these particular ones only to 10,000 years you would get virtually nil out. If you look at the release profiles at the extraction well, they are down to negligible and they start climbing after 10,000 years.

Exposure scenario for three of these looking at aquifer properties, we decided we only wanted a drinking water scenario. Based on the analysis that we did, it didn't seem reasonable that you could get enough water to irrigate a garden or a farm out of those particular wells. It is something we have to reevaluate as the drilling program goes on.

The carbonate aquifer was assumed to supply enough water to have a farm. The drinking water based on the base case runs neptunium was sorbed getting to the unsaturated zone and some in the saturated zones. Technetium and iodine were the dominant contributors.

Now in the drilling cases where you bypass the unsaturated zone, neptunium dominated by quite a bit of the 10 radionuclides that we looked at. And dose and millirems per year, in this case, the sort of maximum we looked at is in the order of 2 or so millirems per year. These are very broad ranges but it was really uncertain as to the aquifer dilution to change the amount of material we had released into a concentration value.

Sort of a summary here, I said regulatory requirements for dose estimates are uncertain. What I mean is they haven't promulgated 40 CFR 191 yet, so we are not sure exactly if you are calculating doses, what we are going to have calculated. We have, I think a good idea of the range of things we may have to do. We used some preliminary model and data, got some results out.

As I pointed out a couple of times the aquifer dilution properties are very strongly contributed to the dose rate, for individual dose limits. If the aquifer is very low conductivity, you can get in trouble relative to the drinking water standard because you withdraw most of the water in the aquifer, if you manage to get enough up then you get most of the waste.

If the aquifer can supply a whole lot more water, then the relative concentration goes down tremendously. And again, even if you look at the suggested dose modeling in 40 CFR 191 there is nothing here that says that you shouldn't continue on. There is no show stoppers.

Questions.

DR. DOMENICO: Maybe you can help me out here.

Curies per year is not a concentration; curies per milliliter might be. Curies per year would be your mass release rates. Somehow I have seen you go from curies per year--I thought you have to go to a concentration and then you have to assume you drink two liters per day and get a dose.

DR. ESLINGER: That's correct.

DR. DOMENICO: I didn't see any of that. I don't know if you can do that without a dispersion model.

DR. ESLINGER: That is one of the reasons for the wide ranges here. If you look at the gas-phase, I explicitly showed this gas, made an explicit mention of the mixing model and the atmosphere we assumed to go from release to a concentration.

We also, when you look at the ground-water base results, our modeling gave us curies per year into the aquifer or transported through the aquifer. Then we looked at the aquifer parameters to see if I start drilling there what do I think dilution I am going to get? How much water is that curies per year diluted into. Then you go into the exposure pathway.

DR. DOMENICO: So that is a guess.

DR. ESLINGER: That's a guess. And that is why I have got wide ranges here.

DR. DOMENICO: Well, you see that is a point that I think a lot of people forget, that a lot of the people want the EPA to go on a dose standard instead of a mass release, but keep in mind that dose standards requires a very accurate transport model and that requires information on dispersion and things that we don't even know anything about.

DR. ESLINGER: That's right.

DR. DOMENICO: So, I was curious as to how you made that connection from a mass release to a dose without that intermediate step.

DR. ESLINGER: Well we did the intermediate step but it is one that is based on a lot of conjecture at this point. Some of the pumping tests and some of the other things will help us tremendously in terms of getting closer ranges on those things.

DR. CANTLON: Other questions from the Board?

(No audible response.)

DR. CANTLON: Staff? Yes, Leon.

DR. REITER: I couldn't help but notice that all your releases started at 2,000 years after the waste was put in. When I looked at the Sandia, all their releases started about 300 years. There must be some built in assumptions about EBS or thermal loading. I wonder if we could understand what those are?

DR. ESLINGER: I showed a few examples of releases that start at 2,000. We ran stuff that started at 300 years; we ran stuff that started as soon as the thermal profile reached back down to boiling, when it went above boiling and came back down.

So in the analysis in the report we looked at a range. I picked a few out here to show today. So we did look at a range.

DR. REITER: But, is that based upon the thermal profile, is your assumption of release based upon when the thermal gets below boiling?

DR. ESLINGER: Yes. That is one of the assumptions in there.

DR. REITER: Is that the same thing, Mike in your assumption?

DR. WILSON: Yes.

DR. REITER: So, essentially we are looking at here a sensitivity release to thermal loading.

DR. ESLINGER: We didn't use different thermal loadings, but that is a case. You can--if you have a higher thermal loading and a higher temperature for a long period of time, liquid phase releases would not start in our models until the thermal came back down enough that you could start to re-wet. Gas-phase releases could still occur if a waste container failed.

DR. CANTLON: Other questions of staff?

(No audible response.)

DR. CANTLON: Questions from the audience?

(No audible response.)

DR. CANTLON: If not then, thank you. I'll turn it back over to the chairman.

DR. DEERE: Thank you very much and I certainly wish to thank all of the speakers today, even though we got behind here and there we ended up in pretty good shape I think.

I hope you will be back tomorrow for the conclusion of the performance assessment. Dr. Warner North will be the moderator tomorrow. So, we will see you at 8:30 a.m. Thank you again.

(Whereupon, the proceeding was concluded at 5:45 p.m., April 7, 1992, to resume at 8:30 a.m., April 8, 1992.)