UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

1996 SUMMER BOARD MEETING

EXPLORATION AND TESTING ACTIVITIES
PAST AND FUTURE CLIMATES AND HYDROLOGY AT YUCCA MOUNTAIN

July 9, 1996

Red Lion Hotel
Grand Ballrooms I & II
3203 Quebec Street
Denver, Colorado 80207

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Edward J. Cording, Morning Session Chair
Mr. John W. Arendt
Dr. Garry D. Brewer
Dr. Jared L. Cohon
Dr. Donald Langmuir
Dr. John J. McKetta
Dr. Jeffrey J. Wong

CONSULTANTS

Dr. Patrick A. Domenico, Afternoon Session Chair
Dr. Ellis D. Verink
Dr. Richard Grundy
Dr. Richard Parizek

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Mr. Russell K. McFarland
Dr. Daniel Metlay
Dr. Victor Palciauskas
Dr. Leon Reiter

NWTRB STAFF

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Michael Carrol, Director of Administration
Ms. Nancy Derr, Director of Publications
Ms. Paula Alford, Director of External Affairs
Mr. Frank Randall, Assistant, External Affairs
Ms. Karyn Severson, Congressional Liaison
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

PAGE NO.

P R O C E E D I N G S

(8:30 a.m.)

DR. CANTLON: My name is John Cantlon. I'm Chairman of the Nuclear Waste Technical Review Board. It's my pleasure to welcome you to our summer meeting. I'm pleased that all of you could join us. We have an interesting two days ahead of us, I think.

As most of you know, the Nuclear Waste Technical Review Board was created by Congress in 1987 in the amendments to the Nuclear Waste Policy Act. The Board is charged to independently assess the technical and scientific validity of DOE's efforts in designing and developing the nation's spent-fuel and high-level radioactive waste management system, including characterization of Yucca Mountain.

My field of expertise is environmental biology and I'm the former Vice-President for Research and Graduate Studies and Dean of the Graduate School at Michigan State. Now, let me introduce my colleagues on the Board. Clarence Allen, a Professor Emeritus of Geology and Geophysics at the California Institute of Technology; John Arendt, a specialist on nuclear fuel cycle and transportation of radioactive materials; Garry Brewer, Professor of Resource Policy and

Management at the University of Michigan; Jared Cohon, Dean of the School of Forestry and Environmental Studies at Yale University; Ed Cording, Professor of Civil Engineering and a specialist in underground construction, University of Illinois; Don Langmuir, Professor Emeritus of Geology at Colorado School of Mines; John McKetta, Joe C. Walter Professor Emeritus of Chemical Engineering, University of Texas; Jeffrey Wong, Chief of Human and Ecological Risk for the Department of Toxic Substances Control in the California Environmental Protection Agency.

Past Board members who are serving as consultants pending their reappointment or replacement are Ellis Verink, Distinguished Service Professor Emeritus of Metallurgy at the University of Florida, and Pat Domenico, David B. Harris Professor of Geology at Texas A&M. Pat is a hydrogeologist.

Richard Parizek, Professor of Geology at Penn State is here also here as a consultant to the Board. In addition, I would like to introduce Bill Barnard, the Board's Executive Director, and sitting along with Bill are members of our Senior Professional Staff.

We are all aware that this is a time of great political, regulatory, and funding uncertainty for the U.S. High-Level Waste Management Program. However, this is also a time where there has been a large increase in the kinds of scientific information that could greatly enhance our understanding of the Yucca Mountain site and those processes that are critical to assessing the performance of the repository. Today and tomorrow, we will be concentrating on the scientific side of the program, particularly the new information and what it means for the program.

This morning and in the early afternoon, we will be hear updates on key exploration and testing issues on the waste isolation strategy. Ed Cording will chair this session and he will provide some introductory perspectives at the close of my remarks. Later on this afternoon and tomorrow, we will devote our time to the critical topics of past and possible future climates and their possible hydrological implications for the Yucca Mountain repository. Pat Domenico will chair that session and provide some opening remarks for those for those talks. After the presentations, Garry Brewer will moderate a round table discussion on climate and hydrology.

We have asked each speaker to leave adequate time for questions after each talk. We will ask for questions and comments first from the Board members, then from our staff, and if time permits, we'll be able to ask for brief questions from the floor. I do want to point out, however, that as with all of our meetings, we have set time aside on the agenda at the end of each day for public questions and comments. Thus, if you are unable to get your question asked when the paper is presented, make your comment immediately during this summary session.

When you raise your question or make your comment, please go to one of the microphones in the aisles and identify yourself and state your affiliation. Those wishing to make comments are urged to sign the public comment register at the back of the room at the sign-up table staffed by Helen Einersen and Linda Hiatt.

Ed, would you please introduce the first session?

DR. CORDING: Thank you very much, John.

Good morning. The purpose of our session this morning is to hear about the progress that has been made in the construction and site characterization in the exploratory studies facility, what we have learned or are learning from the information gathered, and what work remains.

Years of underground testing have shown around the world the conditions underground do differ from those at the surface, and that new understandings are gained or even in many cases surprises encountered as the underground is opened. The particular advantage of underground access at Yucca Mountain is the opportunity to gain access to the major geologic structures, fractures, joints, lithologic units, faults, systems in the proposed repository area. Particularly of crucial importance is the evaluation of groundwater flow conditions, both ambient and the paleo conditions that are present in and adjacent to such features.

The underground at Yucca Mountain is being opened now, as we'll see in the presentations this morning, to a variety of crucial testing and exploration activities. New information is being gained at a very rapid rate and has reaffirmed, or in some cases changed, our understanding of the characteristics of the mountain. Finding bomb pulse chlorine-36 at several locations in the ESF is a case in point where this new data has affected our understanding or our conceptual model of flow in the unsaturated zone. Although much has been achieved, much more exploration and testing remains. We're pleased to see, for example, increasing emphasis on obtaining information on the ambient paleo flow features and conditions and taking advantage of the underground access to do this.

We're going to begin the meeting this morning with a presentation by Rick Craun of the DOE. I'm going to go through a little bit some of the presentations that we will have, and then we'll get back to specifically introducing Rick. He's going to discuss the progress of the tunnel boring machine and other construction, update on the engineering and construction aspects in the facility. The following presentation will be by Russ Patterson, and he's going to present an update on key scientific studies that are taking place or will shortly begin in the exploratory facility. Dennis Williams of DOE will then provide us with a synthesis of what we've learned in the ESF and will present an assessment of what further work remains and what further exploration is being considered for carrying out and completing the testing and exploration work.

All of the testing and site characterization activities certainly must be viewed within the context of a concept or strategy, a waste isolation strategy. In the next few years, a much better understanding of various elements of the waste isolation strategy should be gained. The Board believes that a robust strategy is crucial for focusing and prioritizing exploration and testing activities and eventually for providing a defensible license application. Jean Younker of the M&O TRW will present an update about the progress towards such a strategy.

And, finally, this will be after our lunch break. Bill Boyle of the DOE will present information on the thermal testing program; both, as I understand, in the exploratory facility and also surface facility, the large block tests. He's going to be discussing various thermal tests and how they will be intending to provide information on key thermal management issues.

All these topics certainly are vital to an eventual decision about the suitability of Yucca Mountain as a site for waste isolation. There are many issues to discuss and questions to answer. We've given, we feel, adequate time or certainly more time than we've had in some of our past meetings for these discussions.

And so, we'll begin with a presentation by Rick Craun. Rick is Assistant Manager for Engineering and Field Operations at Yucca Mountain for the DOE. His presentation is an update on ESF activities. Rick?

MR. CRAUN: As Dr. Cording indicated, I'm Richard Craun, the Assistant Manager of Engineering and Field Operations for Yucca Mountain. Today, I'm going to have a fairly short presentation. I'll talk briefly about the tunnel boring machine operations, some operational issues, some changes we've had to do in the operations as a result of some funding constraints that we have in '96, alcove construction, and then our board of consultants that we have for the ESF.

At the time the slides were made, we were at Station 58+98, but as of this morning, we were at 59+61. And, as several of you will know, the corner starts about 59+54. So, we've actually started into the corner within the last couple of days. In addition, we're going to be down for the next three days and the purpose of that outage is to add some conveyor systems, booster stations so that we can go ahead and extend the conveyor on up the south ramp and to go ahead and put some power transformers in.

Currently, we're forecasting a "hole out" date or a completion of the main ESF, which will be the south ramp, around March. Now, the date that you'll see and I'll have some more information later on is actually a range. The range is from December to March. There will be some more information on why the variability in that range or the extent of the range, but the latest "hole out" projection we have is March of '97.

We did start the excavation on Alcove 6 which is the North Ghost Dance Fault, and we started that around May 10. We have completed the main drift of the heater test. That would be the first east-west drift. We have not started the north-south portion. I've got some slides later on that will point that out a little bit better. And, that we will be passing the three mile mark or have passed the three mile mark. With respect to the tunnel layout itself, we are right down in this section right here right now. We just started the actual turn itself.

I wanted to give you a little more information on the "hole out". Now, if you look at these numbers and you are quick and you look at my later part of my presentation, you'll see a different set of numbers. These are basically--we're forecasting meters per day and we don't care if it's a mining day or a day that's down. We just take the number of days between now and an end point, March, and we say, all right, here's what we think we can do as far as meters per day which is a very simple division process. When we get into other forecasts, material forecasts, et cetera, we look at geology, we look at meters per day in the different types of ground conditions, whether we're setting steel or whether we're doing rock bolt installation; those sorts of issues. But, at 17 meters a day, we'd "hole out" at about December of '96, and at about 12 meters a day, we would "hole out" in the March time frame. On the outer portion of the slide or the graphic is actual meterage that we have actually performed. So, those are actually real numbers. 16 meters a day would be what we accomplished along the main drift.

As a result of the FY96 funding profile, we started out at the beginning of the year, as most of you will remember, with a restriction of funds. We were initially going to operate the TBM to Station 39+40. We've obviously been able to go beyond that and stretch that to the point where we'll be able to operate all year. As a result, however, of the funding constraints that we do have, we've actually had to reduce our worker time. Prior to this latest revision, we were running about 30 hours of miner availability in the tunnel itself. As a result of the elimination of the overtime, that's gone about to about 25 hours of miner availability in the tunnel itself. That also affects the TBM availability or the amount of time that the TBM is able to actually push on the rock and actually do the excavation. We went from approximately 20 hours a day availability down to about 15 hours a day availability.

Now, in the next slide, I'll get into a little bit more information. The additional overtime reductions that we've had to recently implement have also had a little bit more of an impact within the last 30 days, and I'll go through those right now.

I broke down the information for you on both Category I and Category IV. This is the average post-installation of the conveyor. If you recall, prior to the installation of the conveyor, we were very much muck limited or the ability to get the muck out of the tunnel was controlling most of our mining operations or tunneling operations. Post the installation of a conveyor, in Category I, we've been averaging since that time approximately 29 meters a day; and, in Category IV, approximately 15 meters a day. Over the last 30 to 90 day period or that 60 day window in there, we've been averaging actually 30.1. We've been continuing to improve our average on Category I and on Category IV ground conditions. The overtime was restricted approximately 30 days ago, and since that time, this number has dropped to about 25 meters a day and this number has dropped to about 13 meters a day. That's again as a result of the reduction of overtime.

I should add a little note as we go through that and I'll go back to actually the previous slide. We chose to reduce the overtime simply because it allowed us to keep the crew there. So, we were able to retain all the expertise of the crew so that as our '97 funding profiles come in, if we have sufficient funds to go ahead and restore overtime, we can recover from the situation in the shortest possible period of time.

As I indicated earlier, Phase I of the thermal test alcove is complete. That main drift is complete, and it was completed in the June time period. Now, for your information, at the bottom of your copies that you have, you see the Phase II--for example, this is Phase II starting in this area going down to here--and then for Alcove 6 or the North Ghost Dance Fault, you see that we should be complete in the September time period. We are on schedule for that and that would be complete to this point here, and then Phase II will be this portion here. And, also, for Alcove 7 which would be the South Ghost Dance Fault, there are the construction times that you have on your charts there.

I'm going to go out of sequence just a second. I want to show you a couple of slides or photographs and then I'll come back to the concluding overhead.

Prior to the mapping operations, we actually wash down the walls of the tunnel itself. It's a high-pressure blast of water that you can see taking place here. So, this is actually the evolution that takes place which is prior to the mapping. So, it's actually on the mapping gantry area where we perform this task and that's what this photograph is showing.

This is a photograph of your standing at the main drift looking approximately 120 some meters into the thermal test alcove area. No personnel there; simply it was just prior to a blast sequence that was taking place. So, that would be the thermal test alcove. Typically, the alcove construction, as I pointed out before, will have a conveyor system which will allow us to, if possible, depending on how we're doing the excavation, put the muck on the main conveyor and haul the muck out via the main conveyor versus bringing a muck cart down to the alcove itself.

I have just a couple of more photographs for you. Actually, the main drift is right here on this slide. This is the conveyor coming up to it and the scrubber system for the thermal test alcove. You're down in the thermal test alcove just a few meters looking back toward the main TBM. Off to the right is the test area.

This is a core drilling activity down approximately 128 meters down from the main center line of the main drift. It's a coring operation, core drilling operation, to check our positioning of the tunnel to make sure that we're at the proper standoff distance from the formations above us. At the single heater element test area, we're starting to install all cable trade system and all the monitoring systems for the test itself. The block itself is right over there. So, this will be supporting all of the instrumentation, data acquisition systems, et cetera, that will be being installed actually now.

And, this is on the single element block. This is a grout swabbing operation of the perimeter holes. These are the holes that will actually be used in some of the assessment of moisture travel time, et cetera.

And then, I've got two of the same area. This is the North Ghost Dance Fault alcove. This is where we're actually turning under. The North Ghost Dance Fault for right now is--I cheated and got some information this morning. It's about 22 meters in, and we're actually doing some back cutting. As you turn under, the conveyor system for the main TBM is right up here. We have to back cut all of this out. That will then allow us to bring a conveyor system up and tie it in. So, that's what we're doing right now.

I just have another photograph of that same shot, different angle, of the Alpine Miner turning under on the North Ghost Dance Fault.

Now, with that, I wanted to go back to the final slide on the Tunneling Consulting Board. We established a board of consultants--oh, heavens, we've had three meetings. They've completed their third report. They've gone final with their third report. It's been a very successful operation from my standpoint. We've gotten a lot of very good feedback from them, some good recommendations and ideas on ways for us to improve the operations of the ESF. We have addressed the majority of those. We still have some open issues that we need to address. Their final report for the ESF was just issued. In that report, they concluded that, in fact, since the ESF is so close to being complete that there's probably not a lot left to be done in the area that they can help us with.

So, they were actually recommended that they look at the repository. We shifted them over to the repository. Their first meeting was, I believe, last week. In the repository area, we've got them looking at underground--I wanted to have them look at ground control for the emplacement drifts. We thought that that would be a significant issue for us and thought their expertise would be of benefit to us in that area; drift stability, constructability, and retrieval. And, as they showed up for the meeting, I added one last barrier or topic to their review. That was we wanted them to look at how we're testing our ground control systems or ground support systems in the thermal test itself. So, we got them actually looking at that to make sure that the test really will adequately test the ground control systems that we're considering for the emplacement drifts for the repository.

And, with that, that's my presentation.

DR. CORDING: Thank you, Rick. Opportunity for questions now?

DR. CANTLON: Rick, your slide indicated that placement of the ground support seemed to be the primary controller of your rate of progress. You didn't mention it, but it was on the slide. Does that give you any thoughts about a shift in the type of ground support you're thinking about? Has the panel talked about that?

MR. CRAUN: Well, we've got several different ground control systems that we can install. Predominately, we use Category 1 and Category IV. Category II(a) which is a variability of the ground--different combinations of rock bolts and wire mesh and those sorts of things. We have noticed that on Category I typically the machine is not limited by the ground control installation; whereas, on Category IV installation of ground control, the machine is limited by the installation of the ground control. We've taken that as a lesson learned as we are looking at the types of machine that we'll consider for the emplacement drifts. Since there are approximately 130 some miles of emplacement drift that we would have to construct, we're now looking at how we can select a ground control system that will not influence the machine's production capability. The board of consultants was here, as I mentioned, last week. They really strongly recommended the use of a pre-cast concrete liner. So, we are taking the lessons learned.

Actually, as you look at our production over the last 90 days, it really is a direct relationship between how much Category IV ground control we're installing. We may be actually from a safety standpoint installing more ground control than what would be commensurate with the design itself. But, the more Category IV ground control we install, you see our production rates going down with a direct relationship there. We've seen that, we've observed it, and we're also trying to use it as a lesson learned in the repository.

DR. CORDING: I know there was some changes you made in the Category IV support to optimize it a little more in terms of mesh placement and things like that. Do you see any other opportunities to improve the installation times on that support by adjusting the support system any further?

MR. CRAUN: Actually, in the Category IV area, most of the time we use this in Category IV at W-8. It's a much heavier I-beam. We've now got the W-6 coming in so that we can--its' a W-6/20 which will allow us to install that much faster. It's much lighter. We'll provide adequate ground control for the ground conditions that we see. We are retaining some W-8s in case on the way out on the south ramp in case we need some heavier ground control. But, that cold be another way in which we could improve our production time. The miners would be able to--it just will be lighter. It will be easier to rig around. So, it should allow us to go faster.

DR. CORDING: The more you can utilize the mesh as opposed to steel lagging, too, that's a--

MR. CRAUN: We have an interlocking mesh and that has turned out to be quite successful. Portland Project started using that, I believe, and we brought that down from there. So, we've been looking around at the different locations, the different excavation projects to try to pick up whatever we can and bring that technology down. That is a technology that we bought down from another site.

DR. CANTLON: Rick, you indicated you had--or it looked like from your photograph, you had two different miner type hardware at work there in the side drifts.

MR. CRAUN: We have just one Alpine Miner.

DR. CANTLON: Oh, okay.

MR. CRAUN: It was just two different shots.

DR. CANTLON: All right.

MR. CRAUN: It was little further in, but the angle was a little different.

DR. CANTLON: Okay. How are they performing?

MR. CRAUN: Well, the Alpine Miner, actually we've got that back in the ground. The more fractured the block or the ground, the better it actually works. The rock comes apart a little bit easier for the machine. It's a W-75 or it's a 75 ton device. If the fracture pattern is fairly light, then the machine is right at its limit. So, we also turned, I believe, on the turnaround on the thermal test area. The direction of excavation makes a lot of difference also. As we turned and went to the south, oh, we went from 2 to 4 meters a day and we did seven in that one day. So, it makes a tremendous difference as to the orientation, the direction that the machine actually is going.

DR. CANTLON: And, you've shifted to drill and blast in a couple of areas. Is that right?

MR. CRAUN: The thermal test area, the main east-west run for the thermal tests was done drill and blast. And, we now have the Alpine Miner back in Alcove 6. So, we'll be doing some excavation. It looks like it's a little more fractured. We were getting quite a bit of overbreak on Alcove 6 from the drill and blast. So, this should allow us to get a much better tunneling job that will likely be faster. It's easier to get the muck out. We'll be able to put a conveyor system in.

DR. CORDING: So, you're going to be bringing the Alpine

Miner--another one in or the same one?

MR. CRAUN: I'd like to bring a second one in, but right--

DR. CORDING: But, you're going to bring the other Alpine Miner back up to finish the alcove, is that right?

MR. CRAUN: The thermal test cycle, yes. We're looking at the sequencing of having the Alpine Miner in either Alcove 6, the thermal test alcove, and Alcove 7. So, we're looking at a machine sequencing there as to where that machine can be and when. But, yes, we are looking at that right now as to where the machine will be and what portion will be drill and shoot and what portion will be mechanically excavated.

DR. CORDING: But, your plan would be to mechanically excavate the actual--

MR. CRAUN: Thermal test area, yes.

DR. CORDING: Yeah, the actual full size drift experiment?

MR. CRAUN: Uh-huh.

DR. CORDING: That section?

MR. CRAUN: That's correct.

DR. CORDING: And then, are you planning on the Ghost Dance extension to continue that with drill and blast or after you get it further away from the fan line and that sort of thing?

MR. CRAUN: Right now, we're going to go ahead and use the machine because of the ground conditions. It's fractured enough. This machine will do quite well in this ground category. So, we will go ahead and use the Alpine Miner. If, in fact, the fracture density diminishes and it becomes a little tighter and the machine has a harder time, then we'll probably go back to a drill and shoot operation and save the machine. It's an older machine that we were able to get at a good Government rate. So, we want to save it for the main thermal test area.

DR. CORDING: The consulting board you have, are they looking at any other aspects of potential ESF work; for example, something like an east-west crossing? Have they provided information on the approaches to construction for those sorts of things?

MR. CRAUN: As I mentioned earlier, we've got the consulting board now working on the repository area. So, yes, they are looking at the east-west emplacement drifts. We're actually looking at a potential waste orient, maybe some performance confirmation drifts, maybe below the emplacement drifts. Those discussions are very active now. They're looking at the different types of machines that we could use, different types of TBMs. So, they are looking at that. They're looking at the production rates that we should be using as a goal for the emplacement drifts; smaller diameter, what diameter to use, and those sorts of issues.

They're not specifically looking at an east-west drift from a site characterization perspective at this time. But, as that is a planned activity and currently still in our planning documents, as we get closer to that, I would get them involved in the machine size selection, a review of that. I think the board has a background and experience there that would help us select a machine that would give us the best tunneling rates.

DR. CORDING: I think that could be helpful in terms of how you--with other parts of the ESF construction, as well, and if that is to be done even at a later time, getting set for that and preparing for it would seem to me to be fairly a current sort of activity that would need to be done and accomplished.

DR. CANTLON: With the considerable experience that your board has in terms of underground construction and so on, are you having them look at the Advanced Conceptual Design from the perspective of operating a repository?

MR. CRAUN: Yes. In fact, we sent them the Advanced Conceptual Design about a month and a half ago. They're very efficient; they read it and they had a lot of questions. The design between now and VA and then from VA actually to the license application will continue to grow and evolve. So, this first series of meetings was to really bring them current from the Advanced Conceptual Design to where we are now in our thought processes which not only include the layout of the tunnels, the construction technique; in their exit briefing, they were making a recommendation that we bring in some real strong underground operation experience so that since we're at the very--I'd say formative; it's not really formative. We've got the ACD out. Since we're at the stage of the design where we can change it fairly easily and accommodate operational issues, they're recommending that we do that. We will go forward with that concept, that recommendation, and that way we can get some lessons learned brought into the design from an operations perspective.

DR. CANTLON: Okay. Thank you.

MR. MCFARLAND: Rick, the 75 ton road header, is that a leased machine or is that Government property?

MR. CRAUN: It was Government excess that we refurbished. So, it is Government property. I believe, we got that for the price of the overhaul.

MR. MCFARLAND: From Rainier Mesa?

MR. CRAUN: I'm not sure. It's from the NTS from the test site.

MR. MCFARLAND: No significant--

MR. CRAUN: It was one of two. That one kind of worked. So, we took it.

MR. MCFARLAND: Have you considered leasing a heavier machine that would be more productive perhaps; one that could be brought in under a lease arrangement?

MR. CRAUN: We looked at leasing, a 75 ton and a 105 ton machine. The 105 ton machine for our alcoves was a little too big. So, the constructor, Keiwit, was recommending that we not go that big. It would have done much better in this ground condition or this formation.

MR. MCFARLAND: What if a small diameter, full faced machine?

MR. CRAUN: Actually, we've located a couple. Looking at the timing pattern for an east-west drift, I was curious as to the availability of a small 14 or 15 foot diameter. There are many of those as compared to 25 foot diameter machines. So, they're much more readily available. The Robbins machines, you can change the face size fairly easily. So, there's more of those available in the United States than the larger ones. So, if we do an extended run construction in an east-west orientation or any other orientation, a small diameter machine would be probably the best along with a conveyor system to get the muck out in very short order. It would allow us to get tunneling rates where--as I showed you earlier, we're 20 or 25 meters a day on Category I. You know, we might be upwards of 100 meters a day on a smaller diameter machine that's really set up for production.

DR. LANGMUIR: Rick, just some clarification for me.

Your, I think, ninth overhead has a diagram of thermomechanical testing alcove. I should know this, but don't; maybe, you can help me out. It looks as if the thermomechanical testing, it's started now or it's in progress within the initial alcove?

MR. CRAUN: A single element. Actually, I was out at LBL and Dennis and Russ will be able to address that probably in their presentations, but they have started getting some data from the--they haven't turned the heater on, but they've been doing some pressure tests, some pneumatic tests, in the single element block area of the thermal test area.

DR. LANGMUIR: But, the intent is to strictly characterize the mechanical behavior of the system under heat? Mechanical effects?

MR. CRAUN: On the single element?

DR. LANGMUIR: Well, the alcove that's described as the thermomechanical alcove?

MR. CRAUN: Uh-huh. Uh-huh.

DR. LANGMUIR: You'd strictly be looking at thermomechanical effects?

MR. CRAUN: I'm going to hold off on that question until we get to, I think, Dennis' presentation.

DR. LANGMUIR: Are you going to talk about it, Dennis?

MR. CRAUN: Bill Boyle's presentation.

DR. LANGMUIR: Okay.

MR. CRAUN: Bill Boyle--in fact, I saw that presentation before I came down here--will address the large block, the single element, and the cool drift scale as to the information obtained--

DR. LANGMUIR: And, the schedule for it and all that?

MR. CRAUN: That's right.

DR. LANGMUIR: That's fine, thanks.

MR. CRAUN: And, I think that's his entire presentation.

DR. DI BELLA: Rick, I think at the last Board meeting in April in Austin, I believe there was discussion of augmenting the board of consultants to look also at waste package design or perhaps even having a separate board of consultants. Is that still under consideration and, if so, could you give us status or update?

MR. CRAUN: Yes, it is. Right now, we've assembled a list of candidates. Our next step would be to take that to the selection of a chairman for that and then start contacting the candidates. But, we have assembled a list of names. We've pulled their vitae together so that we have their background information. We are looking at that now. So, that is still moving forward.

DR. DI BELLA: As a separate board or as an augmentation of your--

MR. CRAUN: I would plan on it being separate. Its focus and the background of the personnel involved in that board would be so much different than the members of the existing board we have now. Their current background is much more underground construction, design, and waste package would be much more materials.

DR. CORDING: One question in regard to the drifts to the Ghost Dance Fault. We don't know exactly where it is. We don't know exactly what it is in terms of, you know, which sort of features will be most likely to conduct flow of water, for example. At the surface, we see expression of a major offset, but we also see expressions of other features adjacent to it, other fracture systems or faults along with the main Ghost Dance Fault. So, it's really a zone. How do you look at being able to get through that? For example, with the planning you have, you've got a certain drift length. As you get in there and you see features, how are you going to determine whether you've gone far enough or will you have the flexibility that allows you to actually go and continue to excavate or go far enough to get through to features you want to look at?

MR. CRAUN: Let me turn the overhead projector back on for just a moment to address that. I think this slide captures the answer fairly well if I could borrow the pointer back. Our intent on the Phase II portion would be to bore ahead or probe ahead and then mechanically excavate so that we can proceed kind of cautiously--not cautiously, but in a very planned method to go ahead and identify that fault. Over the process of constructing the ESF, we have altered our design control process to allow, I believe, the TCO, test coordinating official or officer, and the construction management and engineering to develop what's called a rapid field change to allow us to alter our specific construction techniques in an alcove. I believe, Alcove 4 was the first time we actually used that process. So that in that alcove I believe we were coming into the formation a little bit lower than what the designers and the scientists wanted. So, it was a very minimal amount of paperwork. About 24 hours, we were able to change the orientation of the alcove. That same process will be used here so that as we go forward, probing forward and exploring that area, we will be able to alter construction techniques, orientation geometry, and those sorts of things fairly much in a rapid response mode and measured in less than a day type time period. So, we should have the flexibility to respond to the conditions that we see.

DR. CORDING: I know there's some thoughts that some of the flow may take place along faults and going through upper layers like the nonwelded Paintbrush, and then as it comes down, steps off onto other fracture systems. Very often, the flow may not be where the greatest displacement is, but adjacent. And, even some ideas that perhaps it's towards the east; steps down along bedding and fractures to the east. So, I'm just wondering if, for example, there was a recognition that it would be desirable to go another 100 or 200 feet, could something like that be done, 30 or 60 more meters? If you wanted to extend further to the east, for example, in those faults, is that something that with the program it would be very difficult to do that you could not consider or would you be able to entertain something like that as a possibility?

MR. CRAUN: We would be able to entertain those sorts of modifications. There's a fine line between having field flexibility to respond to construction and losing design control. So, there's a balancing act there. But, the whole purpose of that process that I've described to you that would allow us to change the design as we go is design that's intended to be responsive to the needs of the scientists. For example, on the thermal test alcove, we needed to go an extra 2 or 3 meters. It wasn't 60 meters, but it was 2 or 3 meters and we were able to accommodate that.

DR. CORDING: I mean, I see that as--this is not an adjustment to fit some minor variations, but it's actually a decision to go accomplish or do things that you really don't have as part of the present program. So, I'd see it as a--it's kind of higher level type of decision because it involves so much more time in getting access and things like that.

MR. CRAUN: Well, for example, an east-west drift would not be done under a field modification.

DR. CORDING: Sure.

MR. CRAUN: It would require a design, a revision that would go out with that.

DR. CORDING: Thank you.

Any comments or questions from audience; questions particularly related to the topic?

(No response.)

DR. CORDING: Okay. Thank you very much, Rick. Appreciate your presentation. We look forward to seeing the progress as it continues in the next months.

Our next presentation is by Russ Patterson. He's going to be talking on an update on key scientific activities. He's with the Project Office at Yucca Mountain, the scientific program.

MR. PATTERSON: Thank you.

I brought a few slides. I'll be using both projectors every once in a while. So, I'll maybe just stand right here in the middle. As said, my name is Russ Patterson. I'm the hydrology, geochemistry, climate team leader for Susan Jones' AM of scientific programs.

First, I wanted to clarify a couple of things. One, that I'm not just going to be talking about what's going on in the ESF, especially since that's what Dennis is going to be talking about next. I'm going to be talking about some of the scientific activities that's going on in the ESF, some of the activities that are going on from surface boreholes, and some activities that are going on completely on the surface. Also, some of the key scientific activities does not include everything that we're doing in geochemistry and the transport modeling and other key activities in near-field environment studies and waste package materials testing which I think is also key.

With that said, we'll get into it. What I am going to talk about is the latest stuff in unsaturated zone flow including the discrete fracture model of the Tiva Canyon, the flow modeling; the pneumatic testing, some of the monitoring, some of the results and interpretations. Ed was sort of getting ahead because I'm going to go into the Ghost Dance Fault investigations and a little bit about the testing that's going to be done in the Ghost Dance Fault alcove. Then, I'm going to sneak in some saturated zone investigations, even though they're not in the tunnel, fortunately; but, something that's key. I'm going to talk about G-2 and tracer testing at the C-Holes and the flow modeling.

First, to start off with the unsaturated zone flow. And, I'm sorry, I go a little bit quicker than Rick Craun.

So, I'll try and slow down, but I don't know if I can. We recently received the Tiva Canyon flow model and the model will simulate the interrelationships between the fractured geometry and the flow system. We got this from the USGS, and the data has been provided to Lawrence Berkley National Labs for use in their UZ flow model. This Tiva Canyon model was based on mapping in the Tiva Canyon Tuff and the ESF starter tunnel. And, it's basically a 3-D fracture, network simulation using FracMan.

And, this model has simulated the fracture intensities. This is where I get to use multiple slide projectors. And, perhaps, you can see some of the simulated results versus the mapped results and see that the model has simulated the fracture intensities to a large degree. The modeling has also indicated that there's a large number of fractures, but most of them are not interconnected. Therefore, the actual flow paths through the rock, there's very few flow paths. Application of this will also be used in modeling of other stratigraphic units. The next one will be the Topopah Spring unit.

Flow modeling. In the unsaturated zone, the flow modeling being done at Berkley, the model has calibrations of gas flow, thermal, moisture tension, saturation, and perched water. The modeling has been trying to have an assessment and evaluation of the chlorine-36 and the other work that's being done by the USGS on tritium and et cetera. The model is doing an evaluation of the percolation fluxes, and basically we've recently got some infiltration maps from Flint and Hudson and the flow model has been using those maps and putting in some data from those into the--to calibrate the model.

This is just a small schematic and I want to thank Bo for providing these. They're a little busy, but we'll let Bo get by with that. But, anyhow, what we're talking about here is you've got the--this is how the model is calibrated. They have the points--these are the liquid saturations and the water potentials and you can see that the model fairly well matches the actual data. That just shows how well we've calibrate the model. This is for SD-7. This schematic basically runs through the flow diagram for calibrating the model and I think it's provided in your chart. So, I'm not going to go through that. I mean, in your packages.

So, we'll jump into pneumatic testing program and pneumatic testing/monitoring. DOE currently has eight boreholes that we're monitoring. Nye County has one which is ONC-1. We're looking at responses at depth to the barometric pressure and the fluctuations recorded. We're looking at the pneumatic response to the ESF penetrating the PTn and all the other units in the faults and the fractures. And, of course, we're looking at the pressure response calculations which are made with the UZ gas flow model.

And, I have a few slides here I wanted to show you just to kind of give you an idea of what it looks like with the model. We have the model here. This is at Day 235 and the ESF length. As you can see for UZ-5 and NRG-6, basically what you have is the fluctuations that are--your daily barometric responses, and the model simulations which pretty much correlate with that. When you see the response to the ESF, everything starts jumping all over the place. That's one way to tell that things are going on and you, once again, see the model simulations.

Then, I have it in a different way which I put up here. I skipped a couple of those other ones are in there. This is sort interesting. I like this one real well. You have the map of the tunnel and then we have the boreholes listed and when we saw the response and how far we were away. If you walk back and forth, say, NRG-5 tunnel was at 16+56, and on this one, tunnel at 16+56 in this area. And, you can see, you've got a response at NRG-5, approximately 197 feet away. And, there's some interesting items. That one is in your package. I don't want to belabor this because I'm sure you guys all heard about everything along the north ramp before. So, basically, I'm trying to get through the north ramp as quickly as I can and go on to the south ramp--I mean, yeah, the south ramp--north-south main.

So, north ramp boreholes, what we've learned; Air K measurements of greater than or equal to--sorry. 10 Darcies have been recorded in the Tiva Canyon. The Topopah Spring welded stations showed no amplitude reductions due to the presence of the PTn. Air K measurements based on air-injection data indicate horizontal to vertical anisotropy; 10:1 and 1:10. What does that all mean? You have to put it in simple terms for me. The dramatic difference means that due to sub-horizontal unloading fractures are not present. But, most important for us water people is the implications for water movement, that water will probably most likely have a stronger tendency to move vertically downward than to spread out horizontally. These conclusions are also being incorporated into the UZ site-scale flow model.

Now, we'll get on to the good stuff. Pneumatic testing response to the TBM in the main drift. SD-12 records suggests that the pneumatic response effects were seen on February 26 which was shortly three days after the TBM entered the highly fractured zone at 42+96. I was smart enough to leave this map over here. If you look, that's around in this area someplace.

For those of you who might not have been out to the site and seen these fractures, I happened to bring along some pictures of those also. I'm sure a lot of you have been out there, but there's that fracture pattern in there and here's a closeup.

DR. CORDING: That's in the Topopah Springs in the main drift, isn't it?

MR. PATTERSON: Yes, it is.

DR. CORDING: On the right?

MR. PATTERSON: It's at 47+65.

DR. CORDING: Okay. Looks like a lot of almost kilometer cooling type features at that location.

MR. PATTERSON: Okay. The SD-7 record showed effects on June 5, 1996 when the TBM was about at 56+11. That's right about in here. And, of course, that makes sort of sense because SD-7 is down there.

And then, UZ-7a; UZ-7a which is right here, you might think we would have seen something, but in actuality, UZ-7a is a totally different animal, if you will. We've been monitoring it since November of '95, and unlike all the other holes, you see very minor attenuation across the PTn. In addition, the atmospheric pressure changes in the TSw are slightly before and this suggests short circuited by the Ghost Dance Fault. We don't really expect to see the TBM effects, at all.

DR. CORDING: Is 7a on the other side of the Ghost Dance at repository level?

MR. PATTERSON: That may be mislocated. It may be--actually, it's probably right--well, it crosses the fault.

The fault crosses the hole. Actually, I think it is spudded on the west side just slightly when you stand out there. Am I wrong? I can't remember. Help me out. Okay.

In the ESF in Alcove 3 for pneumatic testing, Alcove 3 which is up along the north ramp, as you know, we have two boreholes, radial borehole 1 and radial borehole 4. Those have both been tested. We have minimal pressure decreases and time lags observed. This suggests that the upper nonwelded unit has a large gas permeability and, in fact, I have some permeabilities for the Tiva crystal poor lower nonlithophysal unit which is this one. It's approximately 2 Darcies. And, for the crystal poor vitric, it's 1 to 24 Darcies. Those are preliminary numbers. I didn't write them on this slide. I'm giving them to you though if you want to write them on. And, basically, this testing generally supports the results that we've obtained from the cross-hole testing or the ESF to borehole testing.

Now, we'll go into Ghost Dance Fault investigations. The geothermal borehole actually in the North Ghost Dance Fault--let me put on that schematic because this gets a little different here. Okay. This is what Rick was talking about with Phase I, if you will. Phase I is to come in and to push in towards the Ghost Dance Fault to a certain degree and then stop and do what we call a geothermal probe borehole and do testing across this borehole before we disrupt any of our hydrologic parameters that would be found in the fault.

Now, you had a very good question which is there's so many splays and breaks and everything else; how are you going to know when you're here? And, that's going to be a difficult task to do. I think the only way we can do it is step fashion, if you will. We may have to go forward, drill a hole, see if we're there, maybe do a little more excavation, and then keep punching that hole until we find where we're going across the fault and then do our testing. This is supposed to be a 30 meter horizontal exploratory borehole. The core samples would be taken from the borehole and do our general hydrologic and hydrochemical analysis on those cores.

And, in the borehole itself, the plans are to do several different things including temperature logging; geophysical logging, of course; pressure monitoring, packed-off intervals just like we do in the radial boreholes and the other alcoves; Air K testing just like we've been doing; gas sampling. Once we're done with that--and that will take a while and that's one of the things that has been a concern and that is a concern for Rick, I'm sure, is planning the machine. It's hard to plan the machine and how quickly we're going to need it back there, you might say, to go on with Phase II. It's sort of difficult for us in the testing world to say, well, we're going to quit testing after six weeks or six months because you need to have the time to test and then go on with Phase II which would be to, of course, go on and make the dogleg, drill these holes on each side of the fault, and drill these holes across the fault coming in from the east, and do additional testing. A lot of the same tests that I just talked about for the first borehole would be done in those holes.

Okay. We're going to jump out of the ESF. We have our own exit route. We don't have to take the south ramp. So, we'll just go right directly to the C-Hole Complex and talk about saturated zone. The purpose, of course, is to conduct hydraulic tracer tests in the saturated zone to provide flow and transport parameters and try and get some sort of handle, of course, on dilution. We have completed two hydrologic tests and two conservative tracer tests during this past year, in fact, and a lot of those have been done over the past six months. The third conservative tracer test continues and we have plans to do future activities there including reactive tracer testing and microspheres and additional conservative tracer testing. Those should all be done through FY97.

We've also done testing at G-2. The purpose is to investigate the large hydraulic gradient north of the site. We've done two pump tests. The first pump test was completed and then we had a second pump test and that test is--we're still waiting for complete recovery. We're monitoring. We're awaiting results of lab analysis and interpretations of field data. We're still in the middle of interpreting this one.

We've been doing site-scale saturated zone flow modeling, model domain and potentiometric surface contours with well control which means not good control, but well, which is the wells, the control has been established. The first iteration of the model has been developed. This is he area of the site-scale model; this being Armagosa Valley, Jackass Flats, Yucca Mountain here, down to Franklin Lake, Death Valley Junction. For those of you who like those things, here's the potentiometric surfaces on there.

And, finally, the regional saturated zone flow model, the first iteration of the model has been completed. The second iteration, the final iteration, is under development and will be completed early next year. And, that area actually is much larger than this area and goes down into Death Valley. I don't have a map with me of the regional area.

DR. DOMENICO: Russ, excuse me, is that measured or is that a model right there?

MR. PATTERSON: This would be model output.

DR. DOMENICO: That's a model output?

MR. PATTERSON: Yes. I believe that's correct. Actually, maybe, Dick could help me. Dick, is that--I just asked him this morning.

MR. LUCKEY: Measured.

MR. PATTERSON: It's measured, okay. Sorry. And, I believe you have those--these are in your handout.

UNIDENTIFIED SPEAKERS: No.

MR. PATTERSON: No? Oh, okay. Sorry. I thought they would be.

DR. CORDING: On that map, can you show the location of that large hydraulic gradient there just on the--up there at the north end, is it?

MR. PATTERSON: Let me clear that up some. Is that better for everybody? Okay. Now, what was your question? The large gradient, the high gradient?

DR. CORDING: The large gradient is really in the area of those concentrated--

MR. PATTERSON: Right. It would be right in here. Right about--yeah, it would be right in this area, what we call the large hydraulic gradient. As you can see though, that's really--actually, if we had the regional map here, that swings on over. It's not a local thing. It goes all the way across the test site. It appears to actually be a mappable feature, if you will, across--

DR. LANGMUIR: Is it better confirmed than it was in the past? There were just a few points indicating its characteristics as of a year or two ago.

MR. PATTERSON: Not on the site.

DR. LANGMUIR: Is more known now than was a year or so ago about it?

MR. PATTERSON: Well, I believe in almost all the regional maps that you see that have been put out by everybody from Inyo County to us at least have some indication of a large hydraulic gradient in this area.

DR. LANGMUIR: Where is G-2 and where is the C-Hole test on this map?

MR. PATTERSON: Okay. G-2 is right about in here, I believe. And, C-wells would be--I'm trying to find--it must be J-13 and what--it's got to be right on the edge here someplace. Right about in here. I can't see them on there, but I believe that's the proper location.

MR. LANGMUIR: Are we in the question period or are we still--

DR. CORDING: We're not quite at that point.

MR. PATTERSON: Do you want me to do my summary slide or not? I don't need to do the summary slide.

DR. CORDING: Let's see your summary slide and come back to this.

MR. PATTERSON: Okay. It don't matter to me.

DR. CORDING: It's an interesting issue.

MR. PATTERSON: Conclusions. Pneumatic data continue to constrain the role of the PTn and the Ghost Dance Fault and in the UZ. The geothermal borehole in the Ghost Dance Fault alcove will provide important data. Discrete fracture model of the Tiva Canyon simulates somewhat what we actually see. The calibration with observed conditions is--okay, the calibration of the model with the observed conditions is in progress of the UZ site flow model and, of course, of the saturated zone model. The G-2 and C-Hole tests continue to provide constraints on saturated zone flow. The first iteration of the saturated zone flow site-scale model is in progress, this model. The final iteration of the regional-scale is in progress. As I say, we're a little bit ahead on the regional and closing in on the site-scale.

And, now, those are my conclusions. Now, do we want to take questions?

DR. CORDING: All right. We'll go back to Don Langmuir.

MR. PATTERSON: All right.

DR. LANGMUIR: You got us intrigued by saying you had some results, but you didn't give us any with regard to the C-Hole test work and the G test. So, I'm wondering what you've learned, so far, if it's any different than what we thought we knew about groundwater flow in the saturated zone?

MR. PATTERSON: Well, I'll tell you what little bit I know about G-2 testing to start with and then maybe if Dick wants to jump up and help me, he can fill me in because he's the one that actually--Dick Luckey of the USGS--doing the analysis.

But, the second test was successful. We pumped for several days and I can't remember the exact number. We were watching the recovery. And, it's still coming up slowly and I think it's recovered in about seven feet of where it started, somewhere in that area. It's coming up very slowly. I'm going to let Dick give you any preliminary interpretations that he may want to or results or--I don't know exactly.

MR. LUCKEY: I didn't bring any results because I didn't anticipate this question. But, I can tell you kind of our level of confusion with the results.

The test was originally planned for approximately 10 days. We went on further than that. It seems to me we went into about 15 or 16 days of pumping at about 60 gallons a minute. We were pumping from the Calico Hills formation. The well was plugged below that. It was somewhat surprising that we were able to produce the much water from the Calico Hills. It was somewhat surprising to me. We had draw-downs at the end of the test in 120 or 130 foot range. I didn't bring the numbers with me. We got a typical early, relatively flat draw-down curve after about three days. The rate of draw-down on the semi--increased somewhat and then remained constant until about the end of the test. So, I think we have an interpretable test in terms of porous media equivalent.

We had fairly rapid initial recovery the first few days after the test, and then we went into kind of a long-term, very slow recovery over the last several months. And, you can kind of project it that we believe that over the next six months it will probably come back to pre-test conditions. That's a phenomena we've seen in several tests we've conducted out there; rapid initial recovery and then very slow, long-term recovery. We're not really sure how to interpret that. We use terms like compartmentalized flow or dual permeability and dual porosity. It seems like we have a rapid--an interconnected network of large fractures, but very sparse. And then, connected to that, we either have matrix flow or very tiny fractures contributing to it.

So, I guess, that's kind of a thumbnail sketch of what little we know about those test results. They were very interesting, but they certainly do not tell us everything. There's still a difference of opinion whether we're dealing with the regional saturated system there or perched water. You can argue both sides of that issue based on the current test results.

DR. CORDING: The piezometric service shown there, is that in the Calico Hills or--

MR. LUCKEY: Yes, it's very high in the Calico Hills, almost near the top of the Calico Hills at G-2.

MR. DOMENICO: Is this a single well test? I presume you have no observation wells that you're monitoring, is that correct?

MR. PATTERSON: That's correct. Yeah, that's correct.

MR. LUCKEY: Single well tests. There's no nearby wells that could have served as an observation well.

DR. LANGMUIR: You've clearly shown us on a number of occasions in your overheads of the significant interconnections in gas flow that can go on in the mountain. As a geochemist, I don't understand how this is tied into the hydrology and I guess I'd be interested to have someone comment on how this information extrapolates to providing us information on the flow of fluids or potential flow of fluids. If you know the gas flow is there, obviously they don't have to be vertically; they could be up or down or sideways. But, how is the information on pneumatic flow being integrated to improve our understanding of the hydrologic flow in the unsat zone?

MR. PATTERSON: Right. Actually, I think the best way to answer that is through the modeling. I'm going to ask Bo since he's hiding in the back to talk about how he is incorporating the gas flow into the modeling of the unsaturated zone flow.

MR. BODVARSSON: The pneumatic signal cannot directly be related to moisture flow. That's absolutely correct, Don. It can give us many clues about conditions within the mountain. For example, one area that we're very interested in is the extension of the perched water body and some tests that Gary Patterson has been doing for the USGS measurements of gas pressures can tell us below and above a perched water body how continuous that body is over what distances which is a key issue at Yucca Mountain as you'll see from Ed Kwicklis' presentation later on.

It can also tell us about the continuity of fractures in the mountain, and also if you have some layers shallow in the mountain, that kind of limited moisture flow, for example, has full saturation and some layers would indicate that perhaps the moisture flow would be very gradual through those because they might prohibit gas flow and then subsequently moisture flow.

And, thirdly, the air permeability/gas permeability is assumed to be very large and they give us an indication of how much strain that we can expect to get with different climate changes at Yucca Mountain. So, if there were tremendous increases in infiltration, can the fracture system throughout that watered down preferential air flow pass? So, there are many direct implications of the gas on the moisture flow, but not a direct correlation.

DR. LANGMUIR: Can I ask a followup related? Have you at this point in time, given the test work that's been done, identified any connections between your pneumatic test information and the indication that you have fast pathways from the chlorine-36 data? Are there any ties yet that you've seen there?

MR. BODVARSSON: Do you want to answer that, Russ? Russ, I think you--do you want to answer that one?

MR. PATTERSON: Oh, I thought you were answering.

MR. BODVARSSON: I cannot say that there are direct ties, but there certainly are complimentary data. For example, when we explain the flow of Chlorine-36 in water getting to the repository horizon, it requires fracturing within the PTn to some extent; faulting within the PTn or fracturing within the PTn. The pneumatic data tells us that basically the signal-at-depth in the Topopah is directly related to the thickness of the PTn and that's the most important factor that attenuates the signal and locks the signal. And that, in turn, gives us implications of how much fracturing to really expect in the PTn and at which locations we use to estimate if the chlorine-36 will get to different locations in the mountain.

So, to answer your--I know this is along answer, but Dennis Williams has--and to try to predict if we will see chloride-36 when the ESF goes through the Ghost Dance Fault in the southern part of the mountain. And, generally, the pneumatic data is one piece of data that we are making sure that our predictions are consistent with the pneumatic signal. We can't put so many fractures through the PTn that the new pneumatic signal will go to easily. I know it's a long answer and not a very good one, but that's the best I can do.

DR. CORDING: Thank you.

DR. COHON: Your overhead #6, could you put that back up?

MR. PATTERSON: Okay.

DR. COHON: It's the one showing the simulated fractures comparing to the map. In what sense does the simulated match the mapped?

MR. PATTERSON: Okay. In which sense--

DR. COHON: How do those match? Is this based on visual inspection saying, well, that map looks pretty close to that simulated or is there some statistical analysis?

MR. PATTERSON: There is a statistical analysis which is done, I believe, and I would--it does match somewhat if you look--I mean, if you just look in general, if you want to do a visual, you could say you've got a higher density of fractures in your simulated in some of these areas and the map seems to correlate somewhat with that. But, you have to do the statistical analysis to actually tell.

DR. COHON: Right. I mean, your overhead 5 says the model simulated fracture intensities match well somewhat with mapped fracture intensities. So, that's based on a statistical analysis?

MR. PATTERSON: Right.

DR. COHON: While I'm thumbing through here, let me ask you a more general question.

MR. PATTERSON: Okay.

DR. COHON: I'm assuming that all of this work that you're reporting is in support of TSPA. Is that its primary purpose in life?

MR. PATTERSON: Actually, it's important--yes, TSPA and general regulatory requirements and also the general site characterization requirements.

DR. COHON: What general regulatory requirements?

MR. PATTERSON: Well, for groundwater travel time or environmental. You know, environmental NEPA, et cetera.

DR. COHON: Okay. So, because you have to do an EIS, you expect this to be one of the issues in the EIS that's in support of that? Is that what you mean?

MR. PATTERSON: This work?

DR. COHON: Yeah?

MR. PATTERSON: No, not this work. This work was to help refine the saturated zone flow model. The fracture mapping was to help refine the fracture flow model that

Berkley is doing.

DR. COHON: I understand all that. What I'm trying to connect it to is DOE's program plan and the goals that you're looking at for 1998 and beyond.

MR. PATTERSON: Right.

DR. COHON: TSPA is certainly a component of that. You said also general regulatory requirements and then--

MR. PATTERSON: I wasn't talking about this. I thought you were talking in general--

DR. COHON: No, I'm talking about the work that you presented today. I'm trying to get the connection between this work and the program.

MR. PATTERSON: Okay. Are you talking strictly the fracture modeling or all the work?

DR. COHON: No, all the work.

MR. PATTERSON: Okay, yeah. So, I mean, the work that we're doing in the saturated zone looking at dilution is, of course, tied back to the hypothesis of seepage and the waste isolation strategy, and also all the unsaturated zone work flows into the TSPA modeling because the UZ flow model is one of the main components of the TSPA.

DR. COHON: Okay. In your discussion of your overhead #7, you also put up a plot showing water potential data. We didn't get a copy of that, but if you could put that back up. This is in the context of UZ flow modeling.

MR. PATTERSON: This one?

DR. COHON: Yeah, that one.

MR. PATTERSON: Okay.

DR. COHON: I'm looking at the water potential plot and again you said here that the model matched the data well. Again, what's your basis for saying that?

MR. PATTERSON: Geez, I'm going to ask Bo to answer that since this is his slide that he gave to me.

MR. BODVARSSON: Excuse me, what was the question, I'm sorry?

DR. COHON: I believe that Mr. Patterson said that looking at the plot on the right, the model matched the data well. And, I don't doubt that, but based on looking at that, it doesn't look especially good to me. What does that mean?

MR. BODVARSSON: These data are measured data on cores that the USGS has measured; saturation and moisture tensions on cores. The saturation values which is on the left hand side are much easier and more reliable than the moisture tension values on the right hand side. You see a lot of scatter on the right hand side in the moisture tensions. Whereas, you see saturations are much less scattered because the saturation relate very much to the formation. If the formation is very welded, then you have very high saturations like in the Topopah and in the Tiva. When it's nonwelded or vitric like in the Paintbrush in the vitric Calico Hills or

Prow Pass, you have low saturations. The data as it is therefore on the saturations we tried to match very accurately, as you see on the left hand side. The moisture tensions are more difficult to match, and we use correlations that Lorrie Flint at the USGS uses or has measured to try to match the data. I agree with you there's a lot of scatter on the right hand side, but moisture tension measurements are rather difficult to make and rather inaccurate in some cases.

DR. COHON: Thanks.

DR. CORDING: Isn't one of the questions here also in terms of the modeling and the fitting of the model as to how much--this is a process of fitting to the data and what your model is showing is coming from other independent information and this is actually showing that overall your model is actually comparable to what you get in the borehole? In other words, some of the information you're using in the model to develop the model, is it coming from other sources than that that information you see right there on that slide?

MR. BODVARSSON: Yeah. That's correct, too. We have moisture tension and relationships to saturations that we use and then we use the first estimate. You see the first gas on there. Russ didn't have time to go into details, but you have two curves there. One is the solid curve and the other one is kind of the bluish or greenish curve that you also see. Yeah, if you can point them out there, Russ. And so, we start with the green curve which is our best estimate and guess in the beginning from all the measurements and parameters that we have. And then, our computer code, ITOUGH, automatically adjusts all the parameters. That means the rock properties, permeabilities, porosities, and Van Gnuchten parameters to get the best fit to both the moisture tension and saturations. Like Ed said also, this fit what you see here, we tried to match all of the wells simultaneously, all of the boreholes simultaneously, to get the best estimates for all of the layers. Therefore, all of the wells will not match as well as some of the other wells.

DR. CORDING: Good, thank you.

DR. COHON: Just a point of clarification for me. Your first conclusion was pneumatic data continue to constrain role of PTn and GDF in UZ flow. What does that mean?

MR. PATTERSON: Okay. What I was referring to there was the attenuation that we see across the PTn at the boreholes unlike UZ-7a where we did not see attenuation because it's been faulted out at the Ghost Dance Fault--or not faulted out, but the fault has messed it up so much that you don't see the attenuation. So, that's basically what I was trying to say with that statement.

DR. COHON: But, when you say the role of PTn's constraint, what do you mean by that?

MR. PATTERSON: Maybe, I should have said defined instead of constraint.

MR. COHON: Well, I'm not challenging your language here. I'm just trying to understand it. You mean, the PTn will serve to constrain UZ flow or that the role of the PTn in UZ flow is contained, removed, reduced?

MR. PATTERSON: What I was trying to say was that actually the pneumatic data continues to define the role of PTn in the Ghost Dance Fault. So, I guess--

DR. COHON: Oh, okay.

MR. PATTERSON: Constrain was probably the wrong word.

DR. COHON: So, without indicating any kind of direction or--

MR. PATTERSON: Yeah. It was the wrong word to use.

DR. COHON: Okay, thanks.

DR. DOMENICO: The corehole crossing the Ghost Dance, it wasn't clear to me whether or not this has been completed or is planned or is in some state of progress. What is the statement there? Has that been completed?

MR. PATTERSON: Which one?

DR. DOMENICO: The bored corehole that's presumably is going to cross the Ghost Dance?

MR. PATTERSON: Oh, no, that--as you heard from Rick, the Ghost Dance Fault alcove has just begun.

DR. CORDING: Is he talking about the vertical hole?

DR. DOMENICO: I'm talking about you had one slide on it and you said you're going to use all that information to get some idea on--

UNIDENTIFIED SPEAKER: That was radial. Wasn't that the radial?

DR. DOMENICO: It was the radial. My question is is this something you're planning for the future or is this done?

MR. PATTERSON: Yes, I'm sorry. If we're talking about the geothermal probe borehole which is in the future in the Ghost Dance Fault alcove, then, yes, that's in the future because they've just started excavation of the Ghost Dance Fault alcove.

DR. DOMENICO: What kind of future are we talking about there?

MR. PATTERSON: Hopefully, I think we heard from Rick that the alcove would be completed to Phase I by September which would mean that we would be drilling that hole probably the first part of next fiscal year and doing the testing.

DR. DOMENICO: Yeah, it's reported on your Slide 19, I believe. That's where the information is. But, that's for the future, okay. Thank you. Thanks, Russ.

MR. PATTERSON: But, this is what we were talking about here. Do I need to go to Slide 19?

DR. DOMENICO: No.

MR. PATTERSON: Oh, okay. Just wanted to know.

DR. LANGMUIR: Just a generic question. We were made aware that the DOE would place more concern and more emphasis on the attenuation of potential releases of radionuclides in the saturated zone. This was a change in direction of the program about a year ago. I'm wondering where are we in the program of learning what the saturated zone can do to attenuate radionuclides? I gather part of what we've been hearing, so far today, is further characterization of the groundwater system itself and the flow, transmissivities of the saturated zone. But, how far along are we in this program of determining what the saturated zone is going to accomplish to attenuate releases and where are we headed on that? What does the schedule look like for that?

MR. PATTERSON: Okay. Well, basically, we've done, like I said, two C-Well tests, conservative tracer tests, and we have one that we're doing right now. And, we'll continue to do testing at the C-Wells at least through fiscal year '97. As far as other saturated zone type testing, we have plans for a southern tracer complex or whatever you want to call it, and we also have plans in the out-years for a transport test in the tunnel either in the ESF or else over at P Tunnels. Basically, I believe, WT-24 which is sort of to get at the large hydraulic gradient and also some saturated zone work would be happening in that out-years. So, that's about it right now.

DR. LANGMUIR: We can go on forever learning more and more about the system, but I assume before you began any of this--I know you did. You had some sense of the bounds on what the flow rates were, travel times, quotes--bad word. What are doing to reduce the uncertainties and how important is it to reduce them beyond a certain point? Do we have any sense of where we're headed with this thing or is this just an open-ended activity? What are the time goals and where are we headed, I guess, is what I'm asking. Scientists can go on forever. When have we had enough? Are all the tests you propose to us necessary?

MR. PATTERSON: You don't want my advice because I don't think you'd be happy.

DR. LANGMUIR: When will Dick Luckey be happy?

MR. PATTERSON: But, yes, I believe it's necessary. We need to do at least one more tracer complex in my mind. And, I would like to do several cross-hole tests or single well pump tests similar to what we did at G-2, WT-12, and WT-10 and other WT holes throughout the site and try and get geochemistry from those holes, also. Try to get some EH measurements at some of those holes like WT-17. That's going to be difficult at the best.

DR. LANGMUIR: Why bother? Why bother to do EH, at all?

MR. PATTERSON: EH to get rid of the technetium problem. If you have the right of reducing or non-reducing--

DR. LANGMUIR: It will be oxidized.

MR. PATTERSON: You're just going to make that assumption?

DR. LANGMUIR: Yeah.

MR. PATTERSON: Okay.

DR. CANTLON: What distances are involved in the tracer tests in the C-Well? What are the distances?

MR. PATTERSON: I don't have the exact distances. I mean, I don't have the schematic with me. I could have brought it with me, but I don't remember the exact numbers. But, I'm going to say it's somewhere between 50 to 80 meters at the largest distance.

DR. CANTLON: Okay.

MR. PATTERSON: So, fairly small distances.

DR. CORDING: Okay. Thank you very much. We're going to take a short break here and reconvene at 10:22.

(Whereupon, a brief recess was taken.)

DR. CORDING: Before we introduce the speaker, I'd like to inform you of something that's very important for your evening. There was going to be a reception tonight. Again, there's going to be a reception tonight from 6:00 to 8:00 p.m. in the Oak Room. It's a cash bar, but it is for everyone attending the meeting and snacks will be provided by the M&O. So, it's a cash bar at 6:00 to 8:00 p.m. in the Oak Room for everyone in the meeting, and we look forward to that.

All right. The next presentation is by Dennis Williams, Deputy Assistant Manager, Scientific Programs for the Yucca Mountain Project; his topic, Synthesis of Results of ESF Exploration. Dennis?

MR. WILLIAMS: Good morning. The title of the discussion, the Synthesis of Results of ESF Exploration, of course it's going to be kind of a thumbnail sketch because the whole presentation for the next two days talks about the synthesis. So, when I get to that part of it, I'll just be hitting some high points of what we're finding.

In the beginning, for the benefit of some of the new Board members, I wanted to just make a couple of comments and show a couple of slides on basically how did we get here in the ESF? I'll go back to the dual shaft concept, the ESF Alternative Study, those planning exercises just to give us a little bit of a feel for where we started and now where we're at with regard to our ESF excavation. Then, we'll talk a little bit about what we've learned from the north ramp and the north-south main. Again, this is just going to be thumbnail hits on a lot of the other things that will be discussed today. Then, of course, of interest to the Board, where do we go from here with regard to some of that underground excavation?

How did we get here? We got back to the SCP days in the original plan. We basically talked about two vertical shafts at the north end of the proposed repository block, some lateral drifting at key horizons, and, of course, that gave us a proposed excavation method of drill and blast. One of the things you could see in that north end of the proposed repository block with those vertical shafts, location was very important with regard to some of the major faults. From there, you could go to the Imbricate fault zone, you could go to Drill Hole Wash, you could go to the Ghost Dance. You'll see these kinds of features very common throughout the changes that we made to the excavation plan over the years.

As I said, that was in the SCP, site characterization plan. As that was reviewed, there was a lot of internal and external discussion with regard for the need to see more excavation of various horizons. Here, I'm already misleading the new folks on the Board. It's 34 options, not 38. 17 times 2 is 34; our mathematics is a little odd today.

We went through the ESF Alternative Study. The preferred option was Option 30 which gave us some key attributes. Mechanical excavation over a total linear footage in excess of 65,000 feet; on two major stratigraphic levels; four major ramps; multiple testing alcoves; a NE/SW-oriented main drift at both stratigraphic level up in the repository horizon and down in the Calico Hills; and, of course, the testing was focused on the main drift, the south ramp, and that lower stratigraphic level.

And, again, going back into the past history, this is a diagram out of the ESF Alternative Study, Option 30, and again showing a very extensive network of explorations.

As we continued to modify and, of course, the ESF Alternative Study recognized the fact that there would be modifications to those various options as we got closer to actual construction. Some of the changes and the reasons for those changes were drift grade basically to get to grades that could permit access by rail. A north-south main drift that would basically avoid crossing the Ghost Dance Fault and run along parallel to it. Then, we would do access to Ghost Dance Fault via alcoves. By going parallel to the Ghost Dance Fault, we could maximize potential repository area and the ESF then could function as an access way in a potential repository.

The components of this modification were a north ramp, a north-south main again constructed parallel to the Ghost Dance, a south ramp, and basically alcoves, seven of them planned, and that's what we're working on right now.

The layout as we know it today, you've seen various versions of this. This is just a simplified cartoon again coming in on the north ramp, the north-south main, starting the turn as Rick reported here earlier, and then coming out the south ramp with these being the seven major alcoves that we're dealing with now.

What have we learned? Offset along the major faults and the complexity of the fault zones has been less than expected. As I pointed out going back even to the two shaft concept, we were looking at the details of these faults. For example, the Bow Ridge Fault was anticipated to be a rather wide zone. It was a matter of a couple of meters. We basically breezed through that. The Drill Hole Wash Fault was thought to be tens of meters wide. It's a difficult structure to find down there if you don't have the trained eye for looking at it. So, it's on a matter of a couple of feet wide with minor offset on it. The Sundance Fault was anticipated to be somewhat of a structure. It's again one of these small structures with a minimal amount of offset. And, now, we've gone across the strand of the Ghost Dance Fault which Steve Beason will talk about later today or tomorrow. And, again, we have a very minor feature with very little offset. So, we're seeing that some these major faults that we anticipated that we would encounter are a lot more minor features than anticipated.

Fracturing within the potential repository host sequence is likely stratabound. We see that when we see the change of fractures as we move from the lithophysal unit into the middle nonlithophysal unit of the potential repository horizon. You don't see the details of the fractures cutting across the contacts of these various units. Likewise, when we go to the section from 4200 out to 5200, we see a lot of fractures, but when we look at the surface-based testing, such as SD-12, we see that that fracturing is likely stratabound. It's only in that horizon. This will be very important as we start to put all these modeling concepts together to see how moisture moves through the mountain.

And, of course, we have not observed any free-draining fractures. Of course, we're always observing the excavation as it proceeds forward to make sure if we have any free-draining fractures. We recognize those early in the excavation process, but we haven't seen anything of that nature to date.

The rock quality has been better than predicted from the borehole information. I think this is something that we did expect because of the very disruptive nature of drilling coreholes especially with air. AS some of us know, whenever you drill a fluid, you have a tendency to dampen the impact of the drilling process. Your core doesn't break apart as badly when you do that. So, usually, when you go underground, you will find that your predictions on rock quality are somewhat conservative. Once you get under there, it will be a lot better than what you anticipated.

The ground conditions range from Category I to

Category IV. Our original Category V ground conditions had not been encountered. Category V was associated with the major disruption associated with faults. So, we didn't have major disruption associated with faults. Obviously, we did not have Category V ground conditions. The ground conditions have generally been within Category II and III range for the repository host sequence. But, remember that our ground category system is the Norwegian Geotechnical Institute Q System that is really based on the fracturing of the rock, the interconnection of fractures, the presence of water, those types of things. So, we do have a little difference in our ground conditions based on those pure numbers than the actual ground--or the ground support installed as is reported by Rick.

The TBM excavation has proceeded with little delay due to adverse ground conditions. From a geologic standpoint, we believe that constructibility has been demonstrated. The tunnel stability and constructibility, likewise, appears to be better in the lithophysal and the non-welded units. Those were two units that we had some particular concerns about going into this excavation in large part because of the predictions up here from the borehole drilling program.

Air permeability, and Russ talked about this at some length and we'll also have later presentations on this.

What we are seeing is that the air permeability testing from the bulk rock in the fault zones are measured in the range of Darcies to tens of Darcies; again, higher than we expected from the core measurements. We anticipated they would be higher than expected from the core measurements because usually your core measurements don't take advantage of all the heterogeneities that you see in a rock mass and you get that whenever you go into the larger bulk measurements.

And, likewise, we see that when we go to something like UZ-7a which is a borehole straight down the Ghost Dance Fault, because of the disruption associated with that fault zone, you basically short circuit the pneumatic system so you have air moving in and out of this fault zone; whereas, you don't have air moving in and out of the bulk of the mountain because of the barrier effects of the PTn unit.

Perhaps, one of the most interesting things associated--I'm going to grab water. I'm running out of air here. The fracture fill determinations. When you do a borehole sample, you pull the core back to the surface. Because of the brutal nature of drilling the rock mass, you tend to rattle the core around and you break off a lot of the nice little fine fracture fillings. When you go into the ESF though, you see all of these materials in situ. What our people that have been working on fracture fillings have been able to do is go for the very fine details of these fracture fillings and pull out these very thin skins of the most recent episode of that fracture filling and date that. We'll have a very good presentation, I believe, tomorrow on those particular sampling techniques and the age of those fracture fill materials and the very slow rates of deposition that are indicated by that. And, of course, since we've got underground, we identified the young bomb pulse Chlorine-36.

What does it tell us? Well, fast pathways exist. UZ-16, a couple of years ago, we had the presence of bomb pulse tritium in the Calico Hills unit at depth exceeding 1400 feet. Recently, we have data from UZ-14 of Carbon-14 again in the Calico Hills unit at depth exceeding 1400 feet. We've got Chlorine-36 at numerous locations in the ESF and we're picking up Technetium-99 which is another bomb pulse isotope in the Bow Ridge Fault in the ESF. Chlorine-36, technetium, June will talk more about that tomorrow in her presentation on isotopes.

Where do we go from here? Our current plan says we will complete the north-south main drift in FY96. Well, as Rick reported, we have completed the north-south drift. Approximately, 59 and change, that's where we start making the turn onto the south ramp. We will complete the south ramp by the middle of FY97, hopefully earlier. We will continue to map, sample, test, analyze, and report on these developments of drift and alcove investigations. And, we will complete our construction and initiate our tests in the '96 and '97 time frame for the thermal test alcove that will Bill will report on shortly after lunch. Russ mentioned the North Ghost Dance Fault alcove, the drilling and the testing in that; and, a similar set of testing in the South Ghost Dance Fault alcove.

The few minor conclusions that I'd like to draw from this, the project will continue with our planned program. We have all these things in our long-range plan, the testing and the observations in the ESF. The results of this testing obviously will be integrated into our design, into our process models, and into the performance assessment activities. Additional underground excavation has been considered in out-year planning. In fact, we have place holders for some of this additional underground excavation. It's in the long-range plan. We basically are planning on designing excavations, if necessary, in FY98 and do the excavation and testing starting in FY99.

Comments?

DR. CORDING: We have opportunity for questions.

DR. LANGMUIR: I think all of us--geochemists among us are most intrigued by the fast pathway information and the isotopy that's led us to that conclusion that we have fast pathways. You've summarized where you are with all this on overhead 15. Does that include by inference dating of the perched waters? Are they among those waters which we think got there by fast paths?

MR. WILLIAMS: As I recall, the dating of the perched waters, especially up on the north end of the repository area, the UZ-14 area, to get some of that water there implies that you had to have relatively fast paths coming vertically through the section. Some of the work, I think, that will be presented tomorrow in the north ramp in the hydrogeology report will give a component of flow that comes laterally from the Solitario, and I think that makes up about 30% of the total volume. As to the details of how you actually work those ratios out, 30% from here, 70% from there, I'll leave it to our colleagues tomorrow to explain that part of it.

DR. LANGMUIR: You can expect I'll be coming back and encouraging discussion of what we know about the amounts of water involved in these younger ages.

MR. WILLIAMS: Yes, yes. And, that's why I said this is a bit of a snapshot of some of the things that you'll be hearing later on in the other presentations because they're getting into a lot of details on these things.

DR. CANTLON: In your where do you go from here section, I didn't see any mention of the block thermal test. It's my understanding you're going to continue to do something I thought you had abandoned at one point. Would you amplify that?

MR. WILLIAMS: The large block thermal test?

DR. CANTLON: Yes, right?

MR. WILLIAMS: I guess I didn't put in here because I've been talking largely on the ESF. But, the large block test which is setting out on the surface, we are definitely continuing with that. That's in our '97 program. Bill Boyle will mention that in the thermal testing program shortly after lunch.

DR. CANTLON: Okay.

MR. WILLIAMS: Yeah, it's definitely in our program.

DR. COHON: This exchange you just had with Don Langmuir about fast pathways and perched water, does that tend to contradict the finding that fracture within the potential repository host sequence is likely stratabound?

MR. WILLIAMS: Most of the fracturing that we see appears to be stratabound. However, there are some set of fractures that do cross those boundaries. So, if we look at it from a generalized sense and you look at, say, the shorter fractures, you'll find them setting up in this thermal pile in a stratabound configuration. But, your large faults obviously cut across the entire pile and there is a large--there is a set of fractures that, of course, cut across the boundaries of these different units. But, that allows us then to start pinning down where--you know, how are the fast flow paths operating? We basically could possibly get away from that finer network; that's not going to be a problem for us. Then, we have to emphasize what's happening in the larger set. So, you can get it broken down to sets, to major structures and then you have a better understanding of how the mountain works with regard to the flow actually coming through.

One of the things I might mention with regard to flow paths though, the fast flow paths, there's been a lot of comments about whether or not this was expected or not expected or whatever. But, again, for the benefit of some of the new Board members who may not have gone back into the early '80s when some of the initial concepts on Yucca Mountain were being developed, one of the reasons this site was selected for characterization was the fact that it looked to be relatively dry and that it was likely free-draining. So, some of these findings shouldn't surprise us.

MR. MCFARLAND: Dennis, I wonder it you'd put up viewgraph 12?

MR. WILLIAMS: Viewgraph 12, ground class.

MR. MCFARLAND: I'm curious; what does constructibility mean to you? What is the definition? What is constructibility in your--how has that been demonstrated?

MR. WILLIAMS: You can excavate it, and you can support it. It will stand as a maintainable opening.

MR. MCFARLAND: At any cost?

MR. WILLIAMS: Excuse me?

MR. MCFARLAND: At any cost?

MR. WILLIAMS: I'm only a scientist. I leave the cost to the engineers.

MR. MCFARLAND: Then, constructibility does not have a cost factor involved in it?

MR. WILLIAMS: Not in my mind, but it has in the mind of others on this program.

MR. MCFARLAND: And, Rick presented earlier a chart that showed in the repository horizon production rates have been between 15 and 24 meters per day. Does that support your comments that excavation has proceeded with little delay due to adverse ground conditions?

MR. WILLIAMS: The lower production rates as I know it from the experiences in the ESF to date have been in large part depending on the machine and the type of support installed, especially in what is considered to be the Category IV ground support systems. As you would note here, we find that most of the ground conditions are within the Category II and III range which would indicate that you may have a lot more flexibility in the types of ground support you want to utilize in that particular mountain, giving you the flexibility to go to some other things as Rick has talked about.

MR. MCFARLAND: But, not knowing the conditions within the block, how do you at this point extrapolate to what your conditions are going to be so that you can generate a credible cost estimate for the repository? For example, your ACD shows tunneling at about $700 to $800 a foot, maybe at $900 a foot, for the 120 some miles of tunneling on the average. And, yet, we have seen tunneling here of the order of $3,000, $4,000, $5,000 a foot. How do you extrapolate from the information we have presently to a cost estimate that is defendable if indeed your statement is correct that we have established everything we need to know about constructibility?

MR. WILLIAMS: Okay. Again, I want to go back to my qualification of my remarks with regard to constructibility from a rock mass basis. The rock mass can be excavated. We have demonstrated that. We can support it and have a maintainable opening. As far as the cost considerations associated with that, that's probably better answered in the realm of our engineer, Rick Craun.

But, I just want to go back to this chart that I brought along for backup. It's not in the package, but this is basically the north-south main, from the north end to the south end--and, again, for you folks that aren't really familiar with a lot of these diagrams associated with rock quality analyses, we have our basic ground support class systems over here, 1, 2, 3, 4, and 5. We talk of a range from 4 to 5 and you can see as the points clock in, the actual points measured in the tunnel as we move through it. The red shows the type of support installed. We see a range, some down here in the 4, but the bulk of it in 3 and 2, a lot up here in the 1. Of course, the Category 1 ground support, the actual rock bolts and mesh, whatever used, and the steel set down here. So, basically, what I'm saying is if you use these measurements as your basis, there is a lot of flexibility for the actual ground support that can be put in.

MR. CRAUN: I thought I'd just add a little bit. The ESF has been very beneficial for us in learning what works well on this machine, the TBM, and what doesn't. So, to extrapolate the current machine into emplacement drifts, we need to really incorporate a lot of the lessons learned. For example, the muck pick up on the machine needs to be redesigned. The ground control installation systems need to be redesigned. So that when you're getting into a production mode into the emplacement drifts where you're looking at 130 miles versus the five mile loop, then that's going to be governing how we design the machine.

But, I think the lessons learned, Russ, really--the ESF has done exactly what it's supposed to and that's to afford us the information on what does work well and what doesn't. For example, when you shift from Category 1 ground control to Category 4, you have a three or four hour delay in machine operation. So, we're looking seriously at just one ground control type system. So, those are the types of things that I think are important.

DR. ALLEN: Dennis, you state the offsets along major faults, the complexity of fault zones, have been less than expected. I guess, that's the good news. Would it be fair to say or not fair to say, on the other hand, that the pervasiveness and the degree of faulting and fracturing at the repository level has been greater than you expected?

MR. WILLIAMS: I don't think so that it's greater than expected because we had the Sundance Fault that was predictable at the surface. I mean, we've been able to find that at the repository horizon. We did not expect that the Ghost Dance would extend to the north and cross the north ramp; it did not. We knew that we would have quite a few minor faults associated with the Imbricate Fault zone. I think we've had those, we've encountered those. We see them in the tunnel and we've got them on our maps. I know Steve Beason is going to give a presentation later on on what we've encountered in the underground, and if he feels differently about that, he can tell me that I'm wrong. But, for the most part, I think we're seeing what we expected from the identification of the structure. But, it is smaller, it has less offset, it doesn't have the disruption associated with the fault zone.

The Bow Ridge Fault, a lot of people thought we would have a great deal of difficulty in that because it could be up to several meters side, tens of meters wide. We had poor core recovery as we drilled through it. Again, those drilling techniques generally tend to give you a much more difficult picture than you actually encounter on the ground. So, I feel that we have a better situation underground than what we predicted we did.

MR. DOMENICO: On 15, you say that Chlorine-36 at numerous locations in the ESF. That you found Chlorine-36. You also found it in boreholes before we had an ESF. And, my question is is the strategy there to measure for it only where you think it might be structurally related? Or if this thing is almost ubiquitous, are we going to know that, as well, when this testing program is over?

MR. WILLIAMS: We're continuing with our program of systematic samples. And, I think that that was something on the order of every 200 meters, we take a systematic sample whether--and, I've got June back there shaking her head right. So, I'm on the right track.

MR. DOMENICO: Whether it's in a fault zone or not?

MR. WILLIAMS: It doesn't make any difference. It's almost a blind sampling. You walk up, you hit 200 meters, you take the sample. And then, also, on features that look like obvious candidates. We will do some more work around the Sundance. I wanted our folks to do some blind predictions on the Ghost Dance whenever we cut across it to look at the distribution of occurrence and then go back and sample the feature and see what we actually have there. So, it's a two-pronged approach; a systematic approach and a feature-based approach.

DR. CORDING: I know that in our visits in recent months there's been interest in doing more ambient moisture measurements of saturation, suction, conditions in the--the ambient conditions that you have to put some boreholes in for that. I understand that there's been some proposals on that sort of thing. Is there an increased emphasis on this and will more of this be done in the program to be able to test--in addition to the isotopic studies to be able to look at some of those moisture conditions back behind the walls of the excavations as part of an exploration program; not, in other words, adjacent to the major faults, but in other portions of the exploratory facility?

MR. WILLIAMS: What you're referring--I don't know for sure what you're referring to whether or not it's additional drilling associated with the isotope samples or--

DR. CORDING: No, I'm talking about--well, basically, more combined measurements in addition to the isotopic studies of the ambient conditions in and behind the wall of the excavation?

MR. WILLIAMS: Okay. One of the things that we're looking at is a larger block of rock sitting back in away from the excavation and doing what we call our large-scale perc test with the unsaturated zone to get the flux measurements in the unsaturated zone which would involve a considerable amount of drilling on a grid fashion so we have a better understanding of how the water is actually moving through this unsaturated zone. This would be in addition to the drilling that we're doing for the thermal tests.

DR. CORDING: Is that now in the plan?

MR. WILLIAMS: Yes, it is. It's in the long-range plan.

DR. CORDING: And, it seems to me that that sort of approach, perhaps not as major a grid, but that approach in other portions of the facility, going back into the walls, looking behind the surface, and various areas as we hit different features would be desirable. I know there's been some interest in doing that.

MR. WILLIAMS: Yes. One of the most difficult problems we have, of course, is understanding the percolation flux in the unsaturated zone. So, what we're trying to do based on the recommendation of basically the whole community of PIs associated with this is not only look at what the response is going to be in the thermal, but what's the ambient? You know, get a lot of details on the ambient condition, as well. And, by doing this larger scale demonstration, we hope that we can get good information on that, combine it with percolation flux understanding that we may get from Chlorine-36 and from dating fracture fillings and, of course, what happens to the water as it moves away from the thermal tests. All this should provide us a better understanding of what the ambient flux conditions are in that mountain.

MR. CORDING: Other questions, Board consultants, staff?

DR. BARNARD: Dennis, on your last slide, you indicate the possibility of doing some additional underground excavation. Could you expand upon that bullet for us and explain what additional excavation has been considered and what the schedules are and how will you decide whether you're actually going to do it or not?

MR. WILLIAMS: I like to think that as we move through something like the North or the South Ghost Dance Fault alcove and we start seeing the nature of the fracture associated with the Ghost Dance Fault that once we see how that is developing and if there is a need for additional excavation in those areas, to me that is my first priority to go talk to Rick and say, hey, look what we're finding here. Look what we're seeing with regard to this excavation. We can gain a great deal of knowledge if we extend this excavation another 50 meters. So, in my mind when I'm talking about additional excavation, those are the first things that pop up on my screen. What can we do in the existing loop and the existing alcoves to improve on the knowledge. Up at the PTn, up at the barrier, the thing that appears to be controlling the water movement through the mountain, can we do additional excavation up there to find out something that's going to help us understand the total processes of the mountain. So, that's what I think about when I think of additional excavation.

There is a lot of opinions, of course, on the program both on the part of the DOE folks, our contractors, and of course, on the part of the Board; a lot of discussion over the years of a necessity to get out in the west side and look at that part of the block underground. That's basically what this means here. The potential place holder for additional large-scale excavation with the time frame. Design it in '98, excavate and test it starting in '99. But, before you do that, have an understanding of what we've got to date. If we continued to find that these faulted features are very small and insignificant with regard to constructability, possibly with regard to how the mountain works pneumatically and hydrologically, we may not want to do this. But, we want that information before we make a final decision. But, we don't want to limit our options. So, we put it in the long-range plan.

DR. BARNARD: Thank you.

DR. DI BELLA: Dennis, one of the lessons learned that you gave was that there have been no free-draining fractures observed in the ESF. And, yet, the site was selected as you just mentioned because this is an arid climate and because of the free-draining nature thought to be findable there. Furthermore, doesn't the Chlorine-36 indicate that there's--that's been found in the ESF indicate that there are some points that drain. I don't know if you call that free-draining or not. So, I guess, my question is what is the significance--what is the meaning of free-draining and what's the significance in your observation?

MR. WILLIAMS: A network of fractures that allows water to move through it very quickly. One of the things we're finding with regard to Chlorine-36, it probably came through as a transient pulse of a very small volume of water. Some of our infiltration studies which Alan Flint will talk about tomorrow, too, I believe, talks about these transient pulses of small volumes of water actually moving through the mountain. And, on this intermittent basis, you would almost have to be in the very right place at the very right time with some delay after a surface precipitation event to see those things come through. Based on what I said about not seeing them, we must not have been in the right place at the right time to date to see these transient pulses coming through. However, we do have this large opening now that we will have access to over the years, over the next few years, before license application and then on out. Perhaps, we will see the effects of some future precipitation event that will give us one of these transient pulses of a low volume of water that moved through that system and find it in the ESF.

DR. LANGMUIR: I understand. I don't recall who did it in the program, but someone apparently we heard from. I learned this yesterday. It might have been Alan Flint. Put plastic sheets over the wall of the ESF and water returned to that wall once it was isolated from ventilation fairly quickly. I would assume you would have to do this to find any transient pulses. You'd never find them unless you put such a plastic sheet up and isolated the walls. What plans are there to do something like this so you could find them?

MR. WILLIAMS: We have discussed that in a variety of our discussions with the PIs. After Rick gets the facility done, then I think we'll have a lot more flexibility of potentially bulk heading off the best candidate for some of these things. Alan has talked to us about some areas that may be good candidates for observing these kinds of phenomena. Probably what you will see in the future is you will go into certain parts of the ESF and you'll see it bulk headed off to get us back to the ambient condition of moisture content in the wall and also in locations where you likely could see some of these pulses come through.

DR. LANGMUIR: Are these likely to be a problem if you've got steel sets and materials are already up against the wall to support? How do you deal with those sorts of areas? Or are you lucky and none of those areas are the places where this is likely to occur?

MR. WILLIAMS: Well, the likely place for it to come through is in areas where we have steel sets. We might go back and take out selected pieces of lagging, but it's a major operation both from a safety standpoint and a mechanical standpoint to remove steel sets out. But, that doesn't keep us from, say, breaking out some steel sets and basically going in with a small alcove. We could potentially do that. That's again one of the things that I would consider for additional excavation is additional selective excavation in places like this where we'd get the most bang for our buck.

DR. REITER: Dennis, back to the east-west drift in the ESF Alternative Studies, an east-west drift was shown in that particular option. As you're well aware, the Board has been pushing this. Let me try and paraphrase your view and tell me if I'm wrong about this. At least based on current information, you don't think that that kind of a drift is going to come up with anything that is going to significantly change your conception or get information about flow in the mountain either on constructibility or flow or anything important to performance in that part of the mountain. Is that correct?

MR. WILLIAMS: Based on current information, that basically is the opinion of Dennis Williams. And, of course, I've been wrong in the past, but I think the important thing to understand right now in these, we want to go through a couple of modeling runs on this mountain. We want to look at the unsaturated zone site model that's coming out of Berkley later this year to see how the mountain is responding. We really have to balance what we know against what we can expect to gain.

And, maybe going back to one of Russ' comments a little bit earlier about, well, you understand these rock conditions of the ESF now. How can you extrapolate that out to that western side? Well, if we have an understanding of the major and minor faults that are going through that rock mass in the areas where we've excavated within the ESF and we understand that, do we gain anything by cutting across another fault or two. Does that really increase our level of understanding? We have a few drill holes in that area out there. We have a few geophysical surveys. We know that the same rock units are extending out in that direction. We know that the core generally gives us a more difficult picture of rock conditions and actual excavation. So, we would know that we would have more of a conservative understanding of that site out there, but the important thing is capture the information that we have to date, see where we need the additional information, and if the additional information dictates that we need more excavation out there underground, then again I pointed out it's in the plan as a place holder.

DR. REITER: Yeah, but now I'm not sure what of this is Dennis Williams and what is the project. You said--you identified this as Dennis Williams' view, and then you talked of some more generic things. What does the project feel about these?

MR. WILLIAMS: The project obviously feels that we need to have a place holder for that type of excavation because we have it in our long-range plan. I guess, for the headquarters' view, I might defer to Russ Dyer. He's had some recent conversations with regard to that.

DR. DYER: Leon, I'll second what Dennis said. We've got it in the plan. It is an activity that we planned for the out-years. Of course, we're going to have to re-evaluate what our needs are, where the priorities are. It's possible that that might be accelerated. It's possible we might do it on schedule. It's possible it might be deferred. But, right now, we've got the resources committed through the planning process to go ahead and look at this.

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

We're going to proceed on. The next presentation is a joint presentation, Steve Brocoum and Jean Younker, on the Status of the 1996 Draft Program Plan and Updated Waste Containment and Isolation Strategy. Steve is making the first part of the presentation. He's the Assistant Manager for Suitability and Licensing of the program at Yucca Mountain.

MR. BROCOUM: When we started planning for this meeting, we originally were just going to talk about the waste isolation strategy, and then I think we were informed by the TRB staff that they wanted a few words on the program plans. So, I'll start off with the program plans. So, I'm talking about the status of the '96 program plans and then I will introduce the waste isolation and containment strategy. Jean then will pick up after me and talk about some of the details.

You know, after Congress cordoned off that money about a year ago, we adjusted all our planning to try to comply with Congressional direction. We de-emphasized interim storage. We focused our efforts at Yucca Mountain on the core science and the construction of the ESF. They became central. We came up with a new milestone to assess the viability of Yucca Mountain in 1998. We essentially deferred all licensing activities. The annotated outline was stopped. At the time, this is now last fall, we planned for significant budget reductions for '96, on. Every year the budget went down, if you remember, and in the year 2000, we'd have had about 100 million, and in a sense, we'd be closing down the project. And so, that led to obviously a major reduction in our activities, and it led to by the end of this fiscal year a reduction of about 850 FTEs of the project and about 200 FTEs at headquarters.

Last fall, we started a contingency planning effort to see what we could do under the constrained budgets. We tried to use the elements of the waste isolation and containment strategy which we will talk about a little more later. We emphasized--we thought how can we better integrate the project and make it more efficient and add lots of things like the project integrated safety assessment, the PISA. We, of course, had to incorporate the viability assessment, and we re-established milestones for a site recommendation and for a license application, though we slipped them by about a year. To do this, you know, we had to convince the project manager and the director of the program that this was doable under more constrained budgets than we had planned for in the original program plan in '95.

So, in a sense, the contingency planning now became the central planning of the project, and it led to the program plan we have recently released. That program plan was published in the spring. It was embargoed by--we were not allowed to release it because we had out-year budgets beyond '97 which were not approved yet. So, OMB allowed us to release it with a draft stamp and that's when it was released with a draft stamp on it.

Some of the rationale for what we are doing. We discussed some of this before. You know, we've collected a lot of data over the last 10 or 15 years. So, we think we can understand what we need to do still to show that the Yucca Mountain will contain and isolate waste. We have a better understanding of what information we have and what more is necessary and that gets into waste isolation containment strategy.

There's been a lot of initiatives in the regulatory area. We all know that the EPA is working on 41 CFR 197. We are eagerly awaiting for that to go to the OMB for the interagency review. At that point, DOE, the NRC, and other agencies will have either 30 or 90 days, depending on whether it's an extradited review or not, to make comments internally. Then, after that, EPA will publish it in the Federal Register for public comment and so on.

We are also revising 10 CFR 960. That's our regulation. We expect that to go into formal concurrence within DOE later this month. When it completes that concurrence process, it not only includes the project and headquarters, but includes the general council of the DOE. It will then be published in the Federal Register for public notice and comment and it will be in the form of rulemaking. And, of course, the NRC is required to conform their standard to the EPA standard and we understand that the staff is now working with outlines on how extensive 10 CFR should proceed. There's nothing to my knowledge that's formally been done or presented to the Commission at this point in time.

We've initiated a lot of project efficiency initiatives. We're trying to be more efficient. We have certainly rehauled the whole planning process. We've done our planning for this year and the out-years. The AMs and the senior M&O managers got together and we created this higher level milestone and the M&O took those and developed a long-range plan which is more detailed than the program plan. And, now, we are in the midst of doing very detailed plans for fiscal year '97 and '98. But, it's basically a top-down process now with a lot of DOE involvement.

We're putting more emphasis on data management accessibility to make sure we all have all different parts of the project that need to use the data, have all the most current data that's been properly blessed and has the proper quality of assurance controls on it. And, we're trying to use PA as an integrating tool.

The key milestones are shown in this chart. These dates may differ somewhat from those in the program plan because these dates come from the long-range plan which was completed after the program plan was completed. All the key milestone dates are still the same; you know, license application dates and those kinds of things. But, some of the more lower level dates may have moved here and there. For example, the Daylight TBM, as Rick said today, the best estimate right now is for March of '97. As I say, we're going through rulemaking. We're planning to publish a final rule in October of '97. We will have a license application plan in October of '97. That's one of the key elements of viability assessment. Russ Patterson talked a lot about the site process models today. Those will be completed for the next TSPA in November of '97. We'll have a TSPA-VA in August '98. That's another key component of viability assessment. The viability assessment itself will be done in September of '98. On the right, we have where there were recordable dates in the original program plan for comparison.

We will publish our draft EIS in July of '99. The way this is worded is a little bit misleading. This is NRC comment of site characterization. We're expecting them to use our PISA to start accounting on our adequacy. We'll publish the final EIS in August of 2000. We'll recommend the site to the President in July of 2001 and submit it to the LA to NRC in March of 2002. So, these are the current dates of our revised program plan and long-range plan.

Now, I'm going to talk about the waste containment and isolation strategy. Do you want to ask me any questions about the program plan or should I just go on?

DR. CORDING: Let's go ahead with some questions.

DR. CANTLON: Steve, in your Slide 6, the one just before this one here, where would a site suitability--which you sort of abandoned as a phrase--but where would that fit?

MR. BROCOUM: The formal declaration that the site is suitable to move forward would be encompassed in the recommendations of the President.

DR. CANTLON: Okay. That means site suitability decisions--

MR. BROCOUM: The site is suitable for intended purpose. At that time, presumably, all the regulations will be in place that we could make those recommend--you know, formal recommendations.

DR. CANTLON: Thank you.

DR. COHON: I'd like to pursue this further, this issue of suitability in the context of the new program plan. Suitability is a word that has been central to the DOE program up until this program plan. The reasons for altering the plan, I think, are good ones and you provided a good background and good rationale for that. But, one of the great opportunities for confusion here is the distinction, if any, between viability and suitability. Suitability seems to mean something, although it may mean different things to different people. But, it's a been a word that's been around for a long time. I found the draft plan to be written in a very artful way to get around this issue without hitting it head-on. And, I think that you need to hit it head-on or you're going to be hit head-on at some point as you move through this process.

Now, to expand further on John Cantlon's question and your answer to it, suitability seems to now be defined as the act of the Secretary recommending the site to the President. She would not do so otherwise unless she found it to be suitable. Now, that suggests that either viability equals suitability or something will happen between the viability assessment in '98 and the suitability determination by the Secretary in 2001. Which is it, and if it's the latter, what's going to happen in those three years?

MR. BROCOUM: The viability assessment is really a status. It's also a statement how we think a particular design will perform at a good site. In other words, it doesn't necessarily compare to a regulation; it may not be a regulation. It's just a statement by the Department with all the backup. Here's a site, here's a potential design, here's how it will perform, here's what it will cost, all that stuff that we've said in the past. That is not a legal finding. It doesn't depend on us having any regulations in place. It is just a status at that time in 1998. The recommendation is the major decision required by the Nuclear Waste Policy Act for the DOE to make. In fact, it's the only real major, aside from the EIS, decision that the DOE does. That's all that is required of the Nuclear Waste Policy Act. All the suitability stuff that we had in our program a year ago was all something we invented, if you like, to have more public participation and have kind of a step-by-step process. Now that Congress has spoken and told us to do more with less, less resources, we have rethought the program and we've decided to try to meet the intent of the Nuclear Waste Policy Act as efficiently as we possibly can. This is the program we have come up with. But, we don't think we're violating anything in the Act, itself. We're meeting the intent of that Act.

MR. COHON: I'm not accusing you of violating anything. I'm trying to understand because someone is going to ask us and you the question, well, is the site suitable after you find it to be viable. The word is everywhere. I notice that your title is Assistant Manager for Suitability. Not viability. You didn't change your title. And, in the program plan, here's one part I found especially striking. This is why I don't want to let you wiggle off the hook on suitability. The revised program plan says that consistent with fiscal year 1996 Congressional guidance, the program will make an assessment in 1998 of the viability, et cetera. And then, two pages earlier, I should say, you quote from the conference report for that same 1996 Appropriations Act which says, "The Department's goal should be to collect the scientific information needed to determine the suitability of the Yucca Mountain site", et cetera, et cetera, et cetera. So, the word "suitability" is everywhere. If viability is new and we're trying to figure out what that means, too, but it's key to figure out how you get from one to the other. And, I don't think it's enough to say that if the Secretary recommends this site, that must mean by definition suitable. Because we've been struggling, this Board has been struggling, since its creation with the question of what does technical suitability mean? How does one determine it?

MR. BROCOUM: Okay. One of the issues we've had with suitability is you cannot talk about the suitability of a site absent design.

MR. COHON: Here, here; that's the next in point.

MR. BROCOUM: And so, when 960, for example, was first promulgated, '84--I've got to check and look around the room--'84, it had all these criteria in it, guidelines. In a sense when you didn't know much and you were trying to figure out--and you didn't have a design yet--what type might you go, you might lead to what people at that time thought was a better design, a better site. We've learned a lot since then and one of the things we've learned is you can't talk about the suitability of the site in the absence of the engineered barrier and the waste package, the whole design. And so, in that sense that you can't talk about suitability independent of the whole system, yes. In that sense, we moved away. But, suitability, however you want to define it, suitability at the end means do we think that we have a design and do we have a site that we can get through the licensing process? And, that's why it's really tied in to the end when the Secretary makes it. At that point, we're far enough in our license application. We've completed much more of our work. So, we're confident at that point that we think we can get through a licensing process.

DR. COHON: Just to bring this to closure, I don't think you've abandoned suitability. I think you've finally come up with the correct definition when you say you can't talk about it independent of the design, et cetera. Are we in such a difficult political climate that you couldn't say that in your program plan and say this is what suitability means and this is what we're headed for? That's a rhetorical question.

MR. BROCOUM: But, the key thing about the viability assessment in '98 is that it's not dependent on a regulation being in place. That's the key thing. We're not comparing it to 60. It probably won't be in place by then. We're not comparing it to 960. You know, we may not even be comparing it to 197 once that's out, also. So, it's just a statement how the site will perform given a given design.

DR. COHON: Thank you.

MR. BROCOUM: Yeah. Shall I go on?

DR. REITER: Steve, again, what does compliance with 960 mean vis-a-vis technical site suitability and when do you plan to do that?

MR. BROCOUM: I think we have it in the program plan now for 1999. So, we have an activity. We evaluate the site against the new 960 in 1999. That will be part of the input. That will be one of the inputs the Secretary--

DR. REITER: What will that be called because in the past you've called that suitability, you've called that technical site suitability. What is that called now?

MR. BROCOUM: I think, it's a compliance report right now.

DR. REITER: It's nothing to do with suitability?

MR. BROCOUM: It's a part of the suitability.

DR. REITER: Is it technical site suitability?

MR. BROCOUM: We don't have a formal title, I guess, for it.

DR. REITER: But, that's how it was called in the past?

MR. BROCOUM: I think what we'll do is we're going to write a--we'll probably write a management plan how we're going to do that. And, in that plan, we'll define that.

DR. REITER: What does the spirit of compliance with 960 mean?

MR. BROCOUM: I'm not sure what you're asking.

DR. REITER: Well, what does that mean conceptually when you tell them that you're complying with the regulations? And, somebody asks you, well, what does that mean? What is that telling you about the site?

MR. BROCOUM: Well, you know, we're trying to revise 960 to make it truly relevant with this site. So, when we comply with 960, we must also comply with 60 and 197.

DR. REITER: Conceptually, what does it mean to comply with 960? What kind of statement are you making aside from complying with a regulation?

MR. BROCOUM: Well, we would like to make it as much-- see, we can't see ahead. So, we don't know what 60 is going to say, but we'd like to make it as much as 60 because we need to eventually meet 10 CFR 60. I mean, that's the licensing regulation. So, you know, from our perspective, we think the site will perform as intended. Okay? We think-- I'll use the word; we think at that point it's a suitable site.

DR. REITER: I think you've answered the question.

MR. ARENDT: Are you using the draft program plan now as a basis of the project?

MR. BROCOUM: Yes, but we--

MR. ARENDT: Even though it is only a draft and hasn't been--

MR. BROCOUM: Yeah, but it's a draft of the reason I told you. OMB wouldn't let us issue it in final form. But, we have taken that program plan and expanded it to several thousand--I think it's 5,000 pages--more detailed, long-range plans and internal plans. We're taking that for the next two years and expanding that. So, yes, if we don't do our planning now, we can't go into '97 with all our activities.

MR. ARENDT: And, all that 5,000 pages tie into this new plan?

MR. BROCOUM: That's correct.

MR. ARENDT: Thank you.

DR. WONG: Steve, I have an easier question. In your program plan, your core science increases from '96 to '97 by $17 million. So, what activities do you plan for '97, and what would you jettison if you didn't get that extra 17 million?

MR. BROCOUM: You need to ask a science person that on the budget.

MR. WILLIAMS: One of the big things that you're going to see in the '97 plan that's a big ticket item will be the thermal testing. I think over about a two year time frame, we're going to be spending something like $17 million or $18 million on thermal testing. So, that's the big thing that we're not doing now that we will be doing in the out-year planning that's a big ticket dollar item.

DR. WONG: And, what would you do if you don't get that extra funding?

MR. WILLIAMS: We'd have to go back and look at the plan.

DR. LANGMUIR: To followup on that question of cost which also brings me to ask the question of relevance, the cost of doing the thermal testing underground is obviously going to be in the millions. The cost of doing the block test which has been resurrected is in the millions. What's the relevance of a block test if we're going to learn about what we need to know from the--this is a loaded question, obviously--from the ESF tests of thermal loading, why do we need to do the block test?

MR. WILLIAMS: I could very easily say we'll defer that to Bill Boyle right after lunch, but basically what we're talking about underground is something on the order of $17 million or $18 million worth of activity; in the large block, we're talking about $2 million worth of activity. One of the things that the large block gives us, it gives us some information very early on validation of some models based on the water movement in the block. As you know, the block out there is isolated. We've got it sitting out there as a free-standing block of water. We should be able to understand where hopefully most every drop of water goes in that particular block. If we can understand that, then that will give us some understanding on how we can better--I'm using too many "understandings"--how we can better understand what goes on in those in situ tests underground where we don't have that kind of control. But, again, Bill will elaborate more on these things right after lunch.

DR. CORDING: Okay. Let's continue with our discussion.

MR. BROCOUM: I'll start on the waste containment isolation strategy. A viewgraph down the road, I'll turn it over to Jean.

In the 1988 site characterization plan, we had a top-level strategy for Yucca Mountain. The update to the strategy which still has the core of the strategy in the SCP--you know, we were hoping at that time and expecting that we would protect the waste package corrosion. They would be emplaced in an unsaturated zone. If you remember way back then, those were thin walled packages placed in borehole emplacements. We thought that along the flow paths there would be considerable potential for radionuclide retardation.

Let me just say a few more comments here. When we prepared the first draft in October of '95, that draft was written by a small team of authors. That went into a formal DOE review and comparison process which went on for many months. The end result of that is we could not get concurrence from all the assistant managers in DOE, and we could not get buy-in from all the national labs in the USGS. And so, that draft that we had out and we talked to the TRB about last October or so, that was the status. What we've done since then is we have reconstituted a larger authorship representing each national lab and the USGS all under the direction of Jean Younker and Martha Pendelton who is in the audience who works for Jean as the lead pinnacle person to try to bring the waste containment isolation strategy to closure. We have then produced this short summary which is consistent with the even shorter summary in the program plan. We were hoping to issue this as a DOE document Rev zero, but we haven't quite completed the internal concurrence process, though I think we have all the AMs brought into this. So, the plan now is for this team to move on and complete the waste isolation and containment strategy.

At this point, my plan is to turn it over to Jean who will tell you where we are and where we're going.

DR. YOUNKER: Okay. In your handout, there is a Page 7 and a Page 8 that I don't have copies of viewgraphs for. So, if you would just look at Page 7 which talks about the basis for updating the strategy for a moment with me.

The improved understanding of site conditions and processes that you've heard talked about already here was certainly one of the drivers that it was time to take a look at what we issued as a top-level strategy in the SCP. And, as Steve just mentioned, the new repository and waste package designs, the larger robust waste packages which allow for in-drift emplacement allow some other options to be considered in terms of other engineered barriers like backfill or like some of the other alternatives that are being looked at. Then, also, you will find that through time as we've used this improved understanding of the site, improved process models, and folded that into performance assessment, we've moved towards something that we think is a more realistic and probably a little bit more credibility in our performance assessment results. And, that also then allows us to map back into that strategy and ask the question do we have the right set of parameters that we're really chasing in terms of site characterization and design?

As Steve mentioned, one of the most important issues, the change in regulatory considerations, at the time of site characterization plan, we did not have the dose-based standard. As we've already talked about for the saturated zone characterization program, since the time of the 1988 site characterization plan, we clearly have had to take a look at that to see whether it's an adequate program to give us what we'll need for doing the calculations for a dose-based standard.

Steve already gave you the status that I hope is on

Page 8 of your--

UNIDENTIFIED SPEAKER: Page 10.

DR. YOUNKER: Page 10, okay. I have a different version, I guess. Let me have yours. Is that the one I gave you? We have two different versions.

Okay. The status that Steve already talked about, the top-level strategy is in the program plan, a short version of it. The highlights which you saw on the table back there and I think you probably have already picked up copies of that, the one that Steve held up, is the one that Steve mentioned as kind of the--I look at it as an executive summary of the document that Steve mentioned--is being prepared as the comprehensive strategy. I like to think of this one as the technical basis for this highlights document that we've prepared. What we've tried to do in the highlights document was to kind of move up one level of generality so that we could get a level of agreement on the general concepts and let the technical debates continue down at the level of what are the alternatives that are consistent with the current information. That was a lot of the debate really focused on and why we couldn't come to closure because there's still certainly a range of interpretations of the information that we have. So, I think in this 15 page document, you'll find some things that sound like assertions and they are to some extent. It's not heavily referenced.

It's not intended to be the really comprehensive basis that we hope to provide for you in the second level or second tier document that we're working on now with the very broad rewrite team that Steve mentioned.

Okay. Now, let me move back. The highlights of the updated strategy, as I already said, takes more credit for the robust waste package; explicitly considers potential for enhanced engineered barriers; continues to rely on multiple natural barriers which is an issue I know that's been raised in the earlier draft version of the strategy document last October. There was a lot of concern that we were moving away from multiple natural barriers. If you look at the wording in this 15 page and I think it will be supported in the comprehensive document, we've moved to being careful to not give up on the natural barrier, but also to be very careful to consider what it would cost you in terms of site characterization to characterize it adequately that you could take it into a licensing hearing and rely on it. You know, make the case; allocate true performance to it in a way that you will be able to defend it in a licensing hearing. So, I think much of the debate here has been what will it take to characterize adequately to use it as a defensible barrier. Then, I've already made the point about relying on dilution in the saturated zone.

The latest strategy is now stated in both the program plan and in the 15 page document, a short version. We stated as the goals being near-complete containment within waste packages for several thousand years, and the second part, acceptably low annual dose rates to a member of the public living near the site. We've tried to keep the strategy such that as the regulatory framework evolves, it is still a valid strategy. So, it isn't tied specifically to how the regulations come out. These are still, we believe, the right goals for the strategy.

I'll try to just move through this and tell you where there are some significant differences from what you've heard before because we have given you very detailed briefings on the technical content of the strategy before. The system attributes which we've called various things, but we've now kind of settled on calling them the attributes that are recognized to be most important for predicting performance are listed on this viewgraph. And, the rate of water seepage meaning how the percolation flux gets translated into what actually enters the drifts and contacts the waste package; waste package lifetime; rate of release of radionuclides from the breached waste packages; the radionuclide transport through the engineered system, whatever else you may add to the waste package and then natural barriers as you leave the engineered system; and then, dilution in the saturated zone. Those have been pretty stable. The wording has changed a little bit through time, but they're pretty stable, I think, and will continue to be as we evolve with the comprehensive strategy.

I think this one probably doesn't even need to be said. But, defining the key performance attributes provides the basis for focusing the testing and analyses program on what's important and looking at these attributes will hopefully aid us in confirming or revising the models that are used to predict performance. So, it's a real feedback loop. I think it's finally working. I know that over the years, many of you have commented on how important it is to make that feedback between the performance assessment, site characterization, modeling, process model development, function. I think we have evidence now, I think, displayed in the strategy that it is working.

Now, from this point on in the talk, I think you will see some evolution and some change as the comprehensive technical basis is developed. What we did at the 15 page level that you have available to you, the working hypothesis has been developed to guide the testing of the remaining issues that connect to each of or define each of those attributes that we just listed. They provide a basis for organizing, managing, explaining the rationale for testing and analyses such that I think our hope is that as we go forward with our annual planning that we can really map back to the work to answer questions like you all have been asking. Why are you doing this work? Well, it's because it maps back to testing. One of the hypotheses that relates to--helps us to find the performance of one of the important attributes. A very important point on this last bullet. Each hypothesis and attribute needs to be looked at in the context of its relative contribution to the total system. So, this isn't set up to be a failure criteria, and if one of the hypotheses turns out to be invalid, that the whole system is non-functional or it is not an acceptable system. You have to look at each one in a way of thinking about it as we did in the SCP days of performance allocation. If you found out that one particular area you were overly optimistic about the outcome of site characterization or engineered barrier performance that you would have to go back and look at this and see whether or not there were some tradeoffs that you could make.

Now, I don't want to spend the time, I don't think, unless you want me to since we're near the end of the time, but each hypothesis is in the short paper that you have for each attribute and there's some discussion, a little bit of discussion, about what we will look at for each of the hypotheses to be evaluated. I think, as the rewrite team works this, we may see some evolution and some development, further definition, better definition I would hope, of what exactly we will need to do to test these hypotheses and whether they're the right set of hypotheses. So, I think Steve and I fully expect that as we get the comprehensive strategy developed further and through review, this short version which is kind of the executive summary of that document will need to be updated and reflect the changes and the improvements that they make in the detailed version.

In terms of each hypothesis, I'll just hit a couple of them. I believe, they're pretty much the same sort of information that you've seen displayed before, but I'll mention the ones that are different. I think the first three are what you've seen before which just get you at how much seepage will you really get into the emplacement drifts. The other two that we've added during the last period of rewriting and pulling out this general document is that you can place bounds on thermally induced changes in seepage rates. We had a lot of internal discussion about--and I think it's already come up here--about, well, you can understand the ambient system, but gee, that's not will cause the real problem for the waste packages during the time that you're worried about them and their performance. It's what kind of thermally induced changes do we cause in the seepage rates. And, the impacts of climate. Likewise, you're interested particularly with some of the discussions that have gone on about the potential for a longer standard. One that the EPA had considered with the peak dose standard would drive you out into time periods where the probability of major climate changes would probably be one. So, you have to look at how you will bound that for your performance assessment modeling.

For containment, I think the only modifications or enhancements to the way this is presented is to certainly take advantage and expect that we will be able to take advantage of the low relative humidities and the good performance you get out of waste packages under the low relative humidities that you get during the thermal pulse. The idea during TSPA-95, we became very much aware that the double-walled waste package design that has now been developed gives us a significant potential benefit by the protection of the inner barrier by the outer barrier, particularly with regard to what is referred to as galvanic protection or if we call it cathodic protection before.

So, I think there's a little bit of additional information and you will find even in the text of the short version, minor changes have developed in the hypothesis wording that brings back in the importance of understanding the potential microbial effects. And, I might mention to you the actual wording in the text of the highlights document is more current than the wording on my viewgraphs. So, go by what you see in the text of the highlights documents. It does mention microbial effects.

Okay. I don't think the radionuclide mobilization--this one does mention microbial effects on mobilization of radionuclides. That one has probably been about the same.

For transport, this one may have evolved a bit in that we were criticized originally for perhaps having put too much emphasis on the engineered barriers and not enough on the natural barriers. So, we tried to balance that. I think in the rewrite of the comprehensive document, you will see that, as well.

DR. DOMENICO: Excuse me, Jean, but does that apply only to the unsaturated zone because I haven't heard the word "depletion" or "dispersion" used anywhere, so far. I don't even know what depletion is.

DR. YOUNKER: Yeah. The way we use depletion is defined very narrowly in the strategy to be the types of delay or almost permanent retardation, if you will. The radionuclides are delayed long enough that they decay or they're permanently stored. There are some radionuclides where we would--say, for the time period we're concerned about, depletion means it's removed from the transport system.

DR. DOMENICO: So, that whole statement then pertains to the unsaturated zone?

DR. YOUNKER: Well, when you get to the dispersion, clearly you will get some dispersion in the saturated zone.

We hope so. And then, evolution. I think this one is probably stated slightly different than you've seen it before, but it's still the same basic idea that's been in the strategy as the strategy has evolved.

We also added hypothesis for the destructive processes and events and you'll find these written up. The wording on these has changed slightly, but for tectonic and seismicity, we get at both the amount of movement on faults, as well as the ground motion related to that movement and whether that will have any effect or impact on--isolation.

Likewise, for volcanism, the statement as it is stated in the highlights document, "volcanic events within the controlled area will be rare, and the consequences will be acceptable". These are stated as hypotheses to be tested.

Okay. The results of the hypotheses may impact, as we talked about the feedback that I think is now in evidence of working, may impact and cause changes in waste package design and materials testing. It could have an impact on decisions about other engineered barriers depending on how you do your tradeoffs and how the validity of the hypotheses turns out. Repository design, particularly the whole question of the density of the heat generated by the waste in the repository or in the individual waste packages. So, you get at the question of what kind of thermal loading makes the most sense. The various types of modeling that is going on that helps us figure out what periods of dryout you will have, what relative humidities through time will look like. And then, back into the site program in terms of what information is most critical.

I think it goes without saying, but I'll say it, that the strategy serves as an integrating tool for design, site, and performance assessment. There's no doubt that there will be refinements as new information becomes available.

DR. CANTLON: Thank you, Jean.

Questions?

DR. CORDING: Yeah, in your overhead 17 where you're talking about containment, you mentioned that the short version that you have over there on the side table talks about microbial which isn't in your overhead. Is there something in there that talks about the role of rock falls as a feature of the containment question?

DR. YOUNKER: On Hypothesis #14, as that evolved in the final hours of agreeing to that draft, we did change the wording on that a little bit. The hypothesis is now stated as--let me turn to that page. "The severity of ground motion expected in the repository horizon for tens of thousands of years will only slightly increase the amount of rock fall and drift collapse." So, we specifically focus on that as the potential reason that you really need to understand what ground motion will do to you in terms of performance. That's how the hypothesis is stated.

DR. CORDING: This is based on G-Tunnel and other kinds of experience or what's the basis for the--

DR. YOUNKER: Yeah, I think, in general, people with that kind of experience like the person sitting on your right will tell you that ground motion tends to die out with depth and certainly at portals you have to worry more about it. But, when you're at depth, I think the idea of rock fall being probably a major increase such that it would impact waste isolation or impact containment performances is probably not too likely.

DR. CORDING: Whatever you do to make changes in time, that's when things tend to happen. But, it may be a triggering type mechanism. I mean, you're bringing up some rock falls that will occur, the seismic events may cause some loosening at that point. But, I think the caution is more to just general rock fall should be expected over the long-term.

DR. CANTLON: Particularly since you have a fair amount of void space inside the waste packages, as they weaken through corrosion and so on, they're susceptible to high strain on the sides after they receive any kind of thump. So, it would seem to me an issue that ought to be looked at.

DR. YOUNKER: Yeah, there are programs both--I think, Sandia is doing some work on that and there's also programs to look at kind of the potential effects. I don't know that we have a lot in our plans.

MR. WILLIAMS: One of the things that we'll be looking at in the thermal test, in the drift-scale test, is how the rock responds to the thermal load and whether or not we have rock falls for a variety of different support systems.

DR. LANGMUIR: Jean, I appreciate this is early-on in a process. But, looking at some of the items listed, for example, on overhead 17, you may disagree, but 6, 7, and 8 look to me to be essentially givens. I don't see that they're really hypothetical particularly. Another point--you may want to argue with that. I think some other things on the list are also largely givens, I think, you wouldn't find much disagreement from anyone on. One thing I did not see here which struck me as very important and it's missing is that the design of the repository itself greatly impacts the relevance of all of these hypotheses and whether or not they're important. And, some that might not be important with a low thermal loading or with a high loading and some may not be important if there's a mixing of defense and commercial fuel; others will be. So, the design of the repository is critical to whether these hypothesis--how you address them and it has to be key to how you're doing the whole thing. It has to all come together.

DR. YOUNKER: Very true. And, I think--

DR. LANGMUIR: And, I haven't seen any discussion of the repository design in your list or any thought of how it might influence these in what you've said, so far.

DR. YOUNKER: Well, I guess, I've always thought that the repository design is embedded in the whole thing or it kind of underlies the whole strategy. Certainly, the performance assessment that we plan, say, in '97 or '98 that will update the one that's kind of the most current basis for the way we think in this strategy is going to be based on as close to the current design as we can be at the time that we do the final TSPA. So, I think, then because this is all wrapped up with performance assessment results and being as current as we can with design, for the next round of total system analyses, I feel like it's embedded in it.

DR. LANGMUIR: Well, is there real communication going on between the M&O's activities and repository design and what you're proposing here?

DR. YOUNKER: Absolutely.

DR. LANGMUIR: Is there interplay all the way?

DR. YOUNKER: Yes.

DR. LANGMUIR: With what they're doing?

MR. BROCOUM: The most important thing that's come out of this whole evolving waste isolation and containment strategy is the fact there's been a lot of dialogue. I mean, there's been constant dialogue between engineering and science and PA and, you know, environmental and so on. So that we're not so worried that we didn't get this strategy done because it has forced us to have a dialogue and has forced us to confront a lot of issues that we needed to confront. So, the dialogue has really increased both in preparing this strategy and in the planning effort. So, I would say that the project is probably better integrated today than it has been at least in my experience with the project.

DR. LANGMUIR: Does the M&O retain flexibility? Are they still in a position to change how they might put the waste in the site? And, also, in that same vein, where are we headed with this potential of new defense waste to the mix with commercial?

MR. BROCOUM: Okay. The M&O does have a lot of flexibility. I mean, the lead technical work is being done for the waste isolation strategy and for the design by the M&O. With regard to other wastes, they were being considered and prioritized and there's a person--several people within the M&O and DOE dedicated--you know, Diane Harrison on my staff and several people in the M&O are dedicated or spend a large part of their time worrying about these other kinds of wastes. I think that activity is being looked at, also.

DR. YOUNKER: Let me mention one followup to Don. I didn't use the figures from the strategy, but there's a table at the end of the short version that you have that attempted to kind of get at what you're talking about. In some cases, you will noticed in the way the information is displayed for each hypotheses that we are giving you an indication we think we're much further along. So, if you'll notice the number of checks in any one of the boxes, it kind of is an indication of we think we're getting there for this phase of the program. I think we kind of have the information pretty much in hand from the source that is indicated by that column. So, although it isn't real explicit, it gives you a general indication of where we think we are. And, you can see to some extent whether that matches with your intuition about it.

On the point on containment, I think the issue on that one just to make sure I give a response to that. I think the issue is that everyone knows that you will get lower corrosion rates at lower relative humidities, but the question is what does that relative humidity, temperature, profile look like through time. You know that if you look at the various modeling results that we have right now, there is quite a bit of variability over a few thousand years out to probably tens of thousands of years where you're below the critical relative humidity for the--

DR. LANGMUIR: Yeah, my problem was that these were statements, but what you really need is to bound the processes--

DR. YOUNKER: Oh, absolutely.

DR. LANGMUIR: That's really what you have to have to get at this mathematically.

DR. YOUNKER: Absolutely, yes.

DR. CORDING: Okay. We're running a little behind. Thank you, Jean. We may want to continue some questions after lunch. So, if you would be available for that, we'd really appreciate it.

DR. YOUNKER: Okay.

DR. CORDING: We're going to break for lunch now and reconvene at 1:00 o'clock.

(Whereupon, a brief luncheon recess was taken.)

AFTERNOON SESSION

DR. CORDING: Would you please take your seats? We'll begin our session. Thank you.

Okay. We're going to continue with some questions on the previous presentation for Jean Younker. She graciously came back to the meeting to do that. I had to cut off some of the discussion of a very important topic, so we'll continue now with some questions before Bill Boyle's presentation.

Pat Domenico?

DR. DOMENICO: Jean, I have a question and an observation. I didn't see the term retardation appear anywhere in the things to be done. I saw studying dilution, dispersion, et cetera. Has the program lost track of retardation or does it no longer play a role in the characterization?

MS. YOUNKER: No, I don't think we've lost track of it, Pat. I think that the question--the way the strategy has evolved so far, and, you know, this may change as the comprehensive one is developed, but with the radionuclides that play the major role in contributing to the peak doses, especially for the longer time frames, are ones that are just not retarded for the most part. I mean, they have very low retardation factors.

So if you look at neptunium and technetium, you know, the ones that are the major bulk of the high doses, peak doses, so you tend to see the lack of emphasis because of that, I believe. And I think you may find, I believe, and I could ask some of the people who are working on the comprehensive strategy, I think what you'll find is that when the information to support this one is really prepared, I think what we'll do is give a better basis for why we know that say over the 10,000-year period, if you did get early releases, some of the radionuclides that would come out early will be retarded.

DR. DOMENICO: And that makes it important.

MS. YOUNKER: And makes it important.

DR. DOMENICO: Yes.

MS. YOUNKER: And so I think you'll see that case being made much better in the comprehensive strategy.

DR. DOMENICO: Yeah, I mean, the geochemical barrier, you wouldn't like to see that disappear.

The observation I recall, maybe in the '60s I think, that document came out that told us about site suitability, site selection processes where favorable conditions were cited and unfavorable conditions and that's when we were looking at nine sites. I think that was 35 years ago.

MS. YOUNKER: Yeah.

DR. DOMENICO: It seems that the waste isolation strategy is a restatement of each of those favorable conditions, that if met anywhere would be suitable for a repository. Is that a fair statement? Everything that's in the favorable conditions are stated there as being--

MS. YOUNKER: I haven't gone back and done that comparison. It would be interesting to do, but the favorable conditions in 960 as they were set up I think were pretty broad and generic. I don't think--maybe you've done the check, but I didn't do it, so I can't say this for a fact. But--

DR. DOMENICO: They're close.

MS. YOUNKER: They're close?

DR. DOMENICO: They're close, yes.

MS. YOUNKER: Well, it was coincidence. Or maybe it was that the original people who put 960 together had it pretty well figured out.

DR. DOMENICO: Well, yeah, we've come a long way in 35 years. That's just an observation, just an observation.

DR. CORDING: One of the questions I'd have for the geochemists and others in this related to--the geochemists, related to the retardation, you're describing things such as coal precipitation and things where there's permanent fixing of materials or the actinides. And I understand TSPA-95 is a totally reversible process of any retardation at all, and I was just wondering to what extent some of these other permanent affixing or precipitation of materials would be something one could take credit for. How much of a factor is it? I've heard discussions where not very much would come out at all. And to what extent is that going to be investigated or can be investigated as part of the strategy?

MS. YOUNKER: Yeah, I think there's no doubt about what the people who are doing the rewrite of the technical basis, the long one, are looking at that. So I know I've heard discussions about it. I don't see the right people. I see Bill Dudley back there, but I don't think he is aware of that part of it.

I know that when we looked at it for the first version, for the draft that went out last year, that for the most part I think the statement I made earlier is probably accurate. But when you begin to look at some of the early release scenarios, you know, the question of what happens to some of the radionuclides that could be released early, it may be that some of those processes will become important. But I'm not close enough to it to tell you, and I don't think I have the right people in the room to respond.

DR. CORDING: Don Langmuir?

DR. LANGMUIR: This is not a geochemical question, but I'm wondering to what extent the waste isolation strategy represents a cross pollination between TSPA and what you folks are doing. And I would assume that this should be going on at all levels in every major topic here that's considered important, is one that's identified in TSPA.

Another part of that question/comment is, I would assume also that every one of your hypotheses here that can be quantified, and I presume they all need to be somehow, if their significance is to be identified and if you're going to prioritize your work, you have to quantify each of those things. And presumably that's something that also ties into the TSPA exercise, which is a quantification of the uncertainties.

MS. YOUNKER: That's right.

DR. LANGMUIR: Yes, that's all I get out of that?

MS. YOUNKER: Yes.

DR. CORDING: Okay. Vic Palciauskas, staff.

DR. PALCIAUSKAS: Yes. I was just going to ask a question, and part of it has been asked already. But this is really, as before Pat mentioned, a list of site attributes primarily, and there were four of them, of course, associated with thermal management, basically of that nature.

I always thought waste isolation strategy would be how you combine the site attributes, which you have to verify, with potential engineered barriers in a long-term waste isolation strategy, which means immediately performance allocation and seeing which parts can be taken care of and so on. Do you agree with that and when will the second part come?

MS. YOUNKER: Yes, I agree with that, and I think there's actually some work going on in performance assessment this year to take I think the first step that is more closely tied to the way the strategy is casting performance. But I think what you described in terms of combination of the natural engineered barriers is exactly how I believe we are thinking about it. Do you see something inconsistent in what was presented?

DR. PALCIAUSKAS: I guess many people have talked about the stability of the system and the redundancy barriers. For example, if one cannot prove, for example, you'll have complete containment for a thousand years, there's always 1 or 2 per cent probability it will fail, what happens next? For example, in this paper there was no explicit discussion of that. What will we count on that?

MS. YOUNKER: Yeah, I think certainly in this short version, we avoided going into any of what I would call kind of failure scenarios. But that is one of the topics that they are addressing in the technical basis for this. And so I think your question is leading into some of the kinds of things that they're wrestling with right now.

DR. CORDING: Leon Reiter?

DR. REITER: Jean, I have a question about volcanic events. The statement to follow is "Volcanic events within the controlled area will be area, and the consequences of volcanism will be acceptable."

Now, I was a little confused by that, but upon reading the document, I think you're saying that even if you have a volcanic event in the repository, there are acceptable consequences. Am I correct in that?

MS. YOUNKER: Well, what the text of the highlight says, and you've probably looked at it, too, is that we've done enough field work in this area that we think we are pretty stable on the probabilities of the events. So now what we really need to still do are some consequence calculations. We haven't really done those.

And so in terms of--there was a little bit. There was a volcanism scenario I think included in the TSPA-93, but I believe our intent is to really make it a little more credible, and then do some actual dose calculations. Those were not done. And so it isn't--I don't know if the wording of this will be the way we'll finally word it when we get the technical basis developed, but the basic idea is we have the technical information pretty well established. There's still some controversy about that, of course, between some of the

NRC staff and our technical staff, but then the question is of using that and applying it through performance calculations.

DR. REITER: Well, it is one thing to assess the consequences, the other thing to say that the consequences in regards to their probability are acceptable. It's a very strong statement, and I think you're saying that.

MS. YOUNKER: Well, and that's what you're attesting to.

DR. REITER: And in your chart it says that you've essentially completed all the work on that.

MS. YOUNKER: Right.

DR. REITER: And I'm just kind of wondering whether you really can say that.

MS. YOUNKER: Well, if we did the consequence calculations, if we went ahead and did some dose calculations and found out that this statement was invalid, clearly we might drive ourselves back into the program and say we have to do some additional site characterization.

DR. REITER: Of course, clearly if you believe the consequences are acceptable, you wouldn't have to spend all that money which you did on the volcanic probability.

MS. YOUNKER: But you couldn't have made that statement if you had not done the site characterization work we've done. I guess--

DR. REITER: Well, how does probability relate to consequences in this case? I mean, you spent an awful lot of money with geomatrix and did this wonderful study on probability, and if you really believe the consequences are acceptable of a volcanic event interrupting it, then you need to calculate the probability becomes much less important.

MS. YOUNKER: Yes. Right, that's true.

DR. REITER: So I'm still not sure what you're saying. Do you really believe--does the project really believe that you have acceptable consequences? Does the information demonstrate that, or is that still up in the air?

MS. YOUNKER: I should probably have one of the PA guys comment on that. I don't know, Ed, do you want to take--I mean, I'm a little bit in over my head in terms of exactly what we've done and what we intend to do. There's not a lot left to do. That much I know.

MR. VAN LUIK: This is Ed Van Luik. I think the first thing I would say is that this is a hypothesis to be evaluated. This is not a declaration of suitability.

And I think the analyses that we have done of volcanism show the consequence were pretty meager, but that is the consequence multiplied by the probability of the event. And I think this statement, unless you have a better idea, I would say, we need to go back and look at this because it's the consequence, the risk of volcanism will be acceptable is the way I would put it.

MS. YOUNKER: Yeah, it should be risk. It should be risk.

MR. VAN LUIK: Yeah.

DR. CORDING: Okay. Don Langmuir?

DR. LANGMUIR: Jean, your hypothesis No. 12, "Water percolating down through the repository horizon of the water table mixes strongly." I realize it's a hypothesis.

I talked to Richard Luckey at lunch, and I'm wondering--we never really resolved it there either--how can you find it? For one thing--first of all, before we get to that, my sense is that all that DOE is hoping to have out of the saturated zone is physical processes of attenuation; am I correct? Only dilution and dispersion will be evoked as important processes to reduce radionuclide concentrations? Or is there also--I gather nothing is going on now, which would characterize the geochemical effects that might retard or eliminate radionuclides in the sat zone. The program is only looking or might be looking strictly at the hydrologic issues.

MS. YOUNKER: I think that's probably true of the current program. I know one thing that I have heard from the rewrite team--and I suspect that Bill Dudley could comment on this part of it. He is one of the members--I keep referring to Bill--of the team that's doing the rewrite, and he has said that he would not be surprised if the comprehensive strategy drives us back to take a really hard look to make sure that the saturated zone program we have in place is the right program, has the right parts to it.

So what you're bringing up is something that they are wrestling with. The way this strategy is written at this point, you're right, we don't really--we don't go into any detail about what kinds of testing we would really want to do, other than what would be necessary to test these hypotheses.

DR. LANGMUIR: And my sense is the only way you're going to find out how much mixing you've got is to go to geochemistry and look at stable isotopes and tracers and so on.

MS. YOUNKER: That was mentioned in the review, yeah.

DR. LANGMUIR: That's the only way you'll find out what the mixing is all about.

So the test work that's being done now in the sat zone can evaluate groundwater flow rates and transmissivities and hydraulic connectivities, and that's where--that's it. It will not tell you anything about attenuation or radionuclide--

MS. YOUNKER: I think Bill might want to just give you a lead into what they have been worrying about in the rewrite.

MR. DUDLEY: Yeah, Bill Dudley of the USGS, one of the members of the rewrite team working for Martha Pendelton.

We certainly have been talking in terms of strongly supporting some of the plans in saturated zone studies to look more closely at mixing and also at geochemical retardation of some sort, so that the non-conservative tracer tests C-Wells are credibly within reach during the period of time, and we I think at this point are leaning toward supporting those and the discussions of things that need to be done.

Certainly another thing that we have discussed a lot you already mentioned, Dr. Langmuir, and that is the sampling for isotopic uniformity or lack of uniformity by supporting the sampling of the WT holes, most of which don't penetrate too deeply. But there are other areas of some sampling that go to greater depth that could be compared with those.

We haven't gotten much further than that in recommending supporting various planned aspects of the program. I expect we'll get deeper into that as we begin to close on this document.

DR. LANGMUIR: Bill, refresh my memory. What traces are going to be used? What conserved and unconserved traces are planned?

MR. DUDLEY: For that one I would have to defer to the Los Alamos people who are working on that.

DR. LANGMUIR: I would assume they'll use some actinide analogues at least if they're going to attempt to get a handle on actinide behavior, otherwise it's not going to tell them much. But you don't know--

MR. DUDLEY: I don't know specifically.

MS. YOUNKER: June or Chuck, are you guys at all familiar with this? Who else? Oh, June was here. He was here, but he's not in the room, yeah.

MR. DUDLEY: Dick Luckey just mentioned sodium iodide and bromide?

MR. LUCKEY: Lithium.

MR. DUDLEY: Lithium bromide.

DR. LANGMUIR: For the conserve tracers, although lithium--we're not an expert in tracers here. Stan Davis is for that.

DR. CORDING: Okay. Thank you. Any other questions from the Board, from the Board consultants?

Pat, quickly.

DR. DOMENICO: Yeah, it's quick.

I don't know if it's an observation. It seems the strategy should say that we're going to build engineered barriers to contain this waste over certain time frames, and they will be designed commensurate to what we need them. We can say that the rate of radionuclide release is going to be small, if that can be a design variable as well. It seems like strategy should go for design variables, and you take the system as it is. You can't strategize the flux. Whatever the flux is, we're going to design a system that can accommodate that. I mean, that to me is a working strategy, and without going to strategize, things aren't how the natural system is behaving.

MS. YOUNKER: Yeah, there's been a lot of debate about use of the word strategy.

DR. DOMENICO: Well, maybe it's not proper to say things like that.

MS. YOUNKER: No, no. You know, the--

DR. DOMENICO: That means we're going to make Yucca Mountain good, whatever, maybe.

MS. YOUNKER: And what you say about the engineered system is, in fact, those comments have come from the engineering side. Just, you know, help us understand what it is you need from the waste package in terms of lifetime and what it is you need from an--

DR. DOMENICO: Exactly.

MS. YOUNKER: --enhanced backfill, if that's the way we go. But you can't do that yet. I mean, I think we're close to being able to do that, but we're just putting it together in a way that I think with this comprehensive rewrite, together with the information that's coming in the next couple of years, I think we'll be able to be very explicit about that.

DR. DOMENICO: You know, I think isolate the design variables, those things that you can design from, those things that you cannot, that you're stuck with, and then the strategy is to bring those design elements up to part to handle whatever it is that you find, seeing it's reasonable.

DR. CORDING: All right. Thank you very much, Jean. We appreciate your extra time.

We're going to proceed now to the afternoon program itself, and that's Bill Boyle's presentation on In Situ Thermal Tests Program.

MR. BOYLE: Thank you, Ed. Thank you, everybody's who's here coming back from lunch. I'll try and get us back closer to schedule, but I don't know that I can do that completely unless I just start taking questions now, which I don't think that's what you want.

I hope this is the last talk before the full Board with an absence of in situ thermal test data. If everything goes according to plan in October, we'll actually--if you want to see it, we should have some results.

Now, this talk on planned in situ thermal tests, we have a dry run, an agenda-setting meeting, and I hope I address the issues that were brought up. Generally what I'm going to talk about is the utility of the thermal test data, and you can look in the agenda. I think what needs to be addressed is how do the data from the thermal tests have input, viability assessment design and the viability assessment, TSPA, and towards resolving issues identified in the water containment and isolation strategy.

Well, these are specific items. Taking a larger view, what we're really trying to do in the thermal tests is understand heat-related processes and parameters. So if we do that, whatever we have up here, whether it's TSPA now or 10 years from now or a VA or an LA, we'll have information out of the thermal tests if we understand the processes.

Now, this slide I've been told something like this was shown at your April meeting. I wasn't there, so I'll take somebody else's word for it.

And for those of you who like graphical information, this shows the relationship of the thermal tests, the single-heater test, the large-block test and the drift-scale test with respect to Phase 1 design and when VA designs are done, TSPA for VA, for the viability assessment itself. You can go through here and see in general that depending on which--take VA designs, for example. The tests aren't done yet. Take viability assessment. Some of the tests are done, at least in terms of heating and cooling, although not all the analysis might be done yet, but the drift scale test isn't done.

What's to be gotten out of this is a sense that we will have information available at certain times, and the designers will use what's available as appropriate at that point, and as more information becomes available, they can--if they already have scheduled other designs, they can take that into account and modify.

And it's the same with TSPA, they have time to do sensitive analyses.

All I'm trying to say is, we're not doing things in a completely serial fashion here where all the tests are done and then the design is done, and then we go back and do new tests. They're going on in parallel.

Now, I'd like to address one of the questions from this morning. I think it was Dr. Langmuir's, and it had to do with the purpose of what is called here the single-heater test, yet in a slide that Rick Craun showed, the naming of the boreholes, it's ESF TMA, thermomechanical alcove.

I want to make--you know, another--a rose by any other name. You know, the name doesn't matter. But actually in the thermomechanical alcove, I went and looked. We have more holes for determining where the water is than we do for making mechanical measurements. Maybe the sum total of the thermomechanical exceeds the hydrology measurements, but we actually have quite a few hydrology measurements in what I call the single-heater test. At times it's been called the shakedown phase. It's also been called the thermomechanical alcove. The name really doesn't matter.

And I think the first time I ever made a presentation to the Board was a year ago last November, and I stated something to the effect, I wish people wouldn't get hung up on thermomechanical, thermohydrologic, that really this is the processes we're interested in, or the fully coupled processes, thermohydrological, chemical and mechanical, and we need information on all of them. And, therefore, that thermomechanical alcove is not solely for the purpose of making thermomechanical measurements.

For those of you who'd like your information some other way other than graphically, this gives the schedule for the heater tests at least, and I didn't put the design nor the VA schedule on, but the Phase 1 design is done about the summer of '97, with a cutoff date actually before that. They are not going to take information at the last minute. Same with TSPA for the viability assessment; that will be done spring of '98, but I'm sure they're going to have a cutoff before that. The viability assessment itself will be done September of '98.

So as I mentioned with the graphical slides, you can go through and see that depending on which milestone you're looking at, VA or TSPA, a test might be done or might not, but we will get by with what we have at the time.

Now, I'll specifically address the viability assessment design.

For it, and this is--now I'm actually talking about, you know, the completion date for the viability assessment design, bearing in mind that they're going to have an input date before that. But still in general, results from the heating phase of the single-heater test and large-block test will be available for VA Phase 1 design, and specifically the information that we'll have at that point are rock mass thermal properties by conductivity, heat capacity, thermal expansion, rock mass deformation properties at elevated temperatures, rock-bolt anchor performance at elevated temperatures.

For the viability assessment itself, you may as well put TSPA on here also. It just occurs a little earlier than the viability assessment, and I address TSPA down here, but I don't have a separate slide for it.

For the viability assessment, the large-block test will be complete, the single-heater test will be complete in both cases, heating and cooling, and one year of heating of the drift-scale test will be complete.

And this statement just summarizes what I've mentioned a few times already. Whatever information we'll have at the time will be used to enhance the credibility of whatever it is we're looking at, whether it's TSPA or VA or VA design.

Now, another issue brought up was how does the thermal testing relate to the waste containment and isolation strategy issues?

The waste containment and isolation strategy was written by others. This is my summary of the five issues. Briefly, the issues are seepage, waste package lifetime, release from the waste package, transport and dilution. I have a line through dilution because no one has come forward yet and said that the effect of the repository heat on the groundwater table has any bearing on the problem at all. So the thermal testing is not going to address this issue.

How does the thermal testing address these other issues? My reading of the document that is available, that 15-page document or so, my reading that for the clubs, effects of heat are explicitly mentioned in the text of that document. And so that's seepage, waste package lifetime and releases.

Effect of heat are implicitly mentioned in the document by reference to the effects of heat on fluid flow. That's for transport.

And the table that Jean showed earlier is the spades, and that addresses the seepage and waste package lifetime issues.

So that's my reading of somebody else's document, how the thermal testing is related to the waste containment and isolation strategy issues.

Now, back to what I've mentioned earlier. What we're really interested in knowing in these tests, whether there's a VA out there or a TSPA or whatever, is what happens to the heat, what happens to the water, what geochemical changes go on and what mechanical responses are there. And I'll say a little bit about each.

These certainly aren't meant to be exhaustive, but just to give an idea for--let me back up. That order I showed, heat, water, chemistry, mechanical, that's in a rough order of importance. You know, you could argue that the water is the most important because it's what's dissolves things and transports it, but what sets the water in motion is the heat. So I would rank knowing about the heat transfer the highest.

And the issues that we're looking to get at in all three thermal tests will address this in some way or other, is that heat is transferred by conduction, convection, radiation or heat pipes, as a specific example of a convective process.

Now, I won't go into a great amount of detail. I'll just try and set this up. These are not listed, and from here on out, whenever I have a list, they're not really in order of importance. These are actually somewhat in order of occurrences the heat is generated. But we have a decoder scale down here that explains the symbols, as you can see which test provides information about which of these processes.

And as you'll see in these slides, the test that supplies the most amount of information consistently is the drift-scale test, the largest test.

Geochemical effects. Here you can see all three tests contribute to an understanding of these chemical processes. You might ask, well then why do all three tests? Well, there's--one, they're in different scales, physically. Two, they're at different time scales, too, that we can get information out of the large-block test much more quickly than we can out of the drift-scale test.

Finally of those processes, thermomechanical. Some of this information is primarily of interest to the designers, such as drift stability and support-rock interaction. Other information, although of interest to the designers also, is also of interest to the process level modelers in determining the proper models to be used in the PA.

Now, all that information I showed you was sliced by processes, and I showed which tests applied. I'm going to show you all the same information--this is test-by-test, starting with the large-block test. Now, I'd like to explain what the primary and secondary refers to. It refers to this test. The primary reasons for doing this test are to get these items. The secondary things to be got out of this test are these items. It's not referring to the large-block test as the primary test out of all tests to gain an understanding of those items.

Now, I'll get back to this. I just mentioned it a minute ago. Why do the large-block tests? Some people may ask that. Based on one of the earlier slides I showed you, it's still the fastest test. We can actually get this one done before any other. It also has the best knowledge of initial conditions and boundary conditions for any test we'll do. So in terms of modeling and gaining an understanding, this has a lot of value.

This test is also the easiest to dismantle, if you will. We have a very beautiful fracture map of it now. We understand it better than any other piece of rock we're going to test at this scale, and we can take it apart when we're done much more easily than the other two in situ tests.

This test is also in keeping with our thermal test strategy, which laid out a philosophy of small to large, simple to complex, short to long durations. And so it fits in our strategy in that sense.

The next test is the single-heater test, and this gives an idea of the parameters to be examined in the test and what the primary things in the test were and what the secondary things were. And you can see some of these items, like water chemistry and mineralogic changes, they appear in each of the three test.

Finally, is the large drift-scale test. In addition to strength, we'll be looking at the deformation, rock mass properties, And you can see an awful lot of primaries, that there are many things to be gathered in this test.

Now, that's what was shown on the agenda, and I have one last slide that came as a request at the first dry run. If I understood it correctly, it came from Leon, but he tells me it's actually a question from Don Langmuir, a geochemistry question. And it has to do--now, this is beyond--I'm in the deep end on this one, and I assume the non-geochemists, we're all in the deep one on this one. It's are we looking at the clinoptilolite to analcime transition and the volume change, and I'll also mention the large amount of water that could be released. So I'll set a little bit of background here for the non-geochemist.

Both clinoptilolite and analcime are zeolites that occur at Yucca Mountain. With heat, clinoptilolite tends to go toward analcime, but it's a very complex problem that's a function of Ph, the constituents in the water, the vapor pressure of water. It's a horribly complex problem, but as far as I can tell, the scientists at Los Alamos have been working on it for many, many years, and as far as I can tell have done a very good job.

Why this is of concern is there's two types of water in clinoptilolite. That's one issue. One type of water can come and go reversibly with the heat. And as the zeolites in the Calico Hills unit heat up, we can generate this water and what effect does it have.

There's another type of water in clinoptilolite that as you heat it, and this is a very slow process, but it does occur, this water leaves irreversibly, and the clinoptilolite becomes analcime. This is of interest because clinoptilolite is apparently much better at capturing radionuclides. Analcime doesn't do as good a job.

Now, people have been aware of this for a long, long time. There's actually years ago, there was a maximum temperature placed on the vitrophere underneath the repository horizon, largely to prevent dehydration, if you will, of the clinoptilolite in changing it to analcime.

People are still looking at this, what should be the right temperature and those sorts of things.

I'll say a number of things. If it's decided to have cold repository, I don't think there's much of an issue here. This is really a heat-driven process, and if you were to keep things cold enough, the clinoptilolite for the most part isn't going to go to analcime.

Now, that I've set some of the background, let me address--there was an easy answer I think to all of this, and that's actually the first bullet. We're not testing it in any of the tests we're doing right now. The large-block test, single-heater test, drift-scale test are all in the middle non-lithophysal unit, has no clinoptilolite. We will not see this effect. The water moving, and as you drive out at the bound water, if you will, in the analcime transition, you actually have a volume change, too. We're not going to see any of that in any of our currently planned in situ tests.

If it was decided that we needed to have more information, we would probably perform more lab tests before pursuing expensive in situ tests, and if we did decide to do in situ tests, we would probably consider alternatives, such as P-tunnel to actually excavating down to the Calico Hills.

That was my last slide. I hope I addressed--if it was your question, I hope I addressed it some, but feel free to ask more.

DR. CORDING: Time for just a few questions. Don Langmuir?

DR. LANGMUIR: Actually, Bill, it wasn't something I wanted to press on. Maybe Leon would like to.

But I had related questions. To me, one of the critical issues that's going to be tough to resolve from the tests that are proposed, at least the ones that have closure, which are the single-heater test and the large-block test, those are presumably 21-month tests and 14-month tests or less because you've got a month or two to write a report.

I look at those times, and I think about the connectics of reactions in coupled processes. You're talking about, among other things, trying to address the issue of whether mineral will dissolve and precipitate and find where that's occurred and see what the effects of those processes might be on the transmissivity of the rock, for vapors or for fluids.

And a test of that length, you're getting yourself down to the time scales of reaction rates for aluminosilicate minerals. So you have to move things around, get to saturation with them and then precipitate them, and have something happen meaningfully on a time scale of a couple of years or less--a year. And I would kind of doubt that you're ever going to see any of these effects, these coupled effects, which would influence the reflection process, they'll influence a major mountain behavior around a repository. You'll never find out about them in these tests. You'll have to wait on the in situ test in the repository for that sort of information. And I wonder whether you even know how to measure it.

MR. BOYLE: I don't disagree with anything you say. And I'll tell the audience my own view, and I'm not a geochemist.

You know, Yucca Mountain is a natural analog to itself. This clinoptilolite to analcime transition has occurred, as has glass to clinoptilolite, analcime to albite. Some of these reactions, clinoptilolite to analcime is one of them, is very slow, and it's difficult to measure in the lab. It's going to be difficult to measure in the field. I'm going to have to defer to the geochemists on this, but I think they would say something is better than nothing, that they will try to make something out of these measurements, bearing in mind that some of the reactions are so slow that we may miss them or be misled.

But to just shrug our shoulders and say let's wait for the repository and get it in performance confirmation may not be acceptable.

DR. LANGMUIR: I think all you're going to do is reduce uncertainties, taking into account the reactions that are relatively fast in the mountain.

MR. BOYLE: Right.

DR. LANGMUIR: Some you'll have a somewhat less ambiguous system, but you're going to have to go to the repository sites itself for a long-term performance.

MR. BOYLE: And I don't disagree with that.

DR. CORDING: Any further questions from the Board? Consultants?

Russ McFarland, Board staff?

MR. MCFARLAND: Bill, a question on how this data is related to specific critical issues that you would like to better address before the VA, and particular as an example, in one of the earlier presentations, it was mentioned that thermal testing would provide to the repository designers information to allow them to better determine an aerial loading, perhaps even whether they should be a point loading, a line loading.

What data, specific data, out of this suite of information you just described would give them that information?

MR. BOYLE: Well, I would say, for example, in both processes and parameters, and we'll get information on all of those in all three tests. Take the heat processes. If the repository designers, if we can tell them as a result of our tests heat conduction, forget convection, forget heat pipes, that helps them. They will be able to calculate where the temperature fields are using, as an example, a specific value. If we know what thermal conductivity is based on some small little wafer, but we go out and we do these various in situ tests and find out, no, thermal conductivity at a big scale is something different, therefore your thermal envelope is going to be different, that would be of help to the repository designers.

And with respect to some of the thermal

measurements, we can actually get information on those questions relatively quickly; even in like the large-block test, we should be able to get some understanding of how much of the movement of the heat is due to conduction, how much of it is due to convection, heat pipes, something like that.

MR. MCFARLAND: Critical enough data of sufficient clarity to make a major design decision?

MR. BOYLE: I don't know. I suppose that would depend on the designers and what we would get out of the tests, but it's possible.

MR. MCFARLAND: Okay, thank you.

DR. CORDING: Thank you very much, Bill.

We're going to go on to the next presentations and through the next portion of the session that will be chaired by Pat Domenico. I turn it over to Pat.

DR. DOMENICO: Needless to say, this is a very important topic. We know that the most important factors affecting repository safety is the amount of water percolating down through the mountain that could reach the repository, corrode the waste packages, dissolve radionuclides and transport then to the accessible environment.

Although we have a lot to learn about the present-day hydrologic regime, it is even a greater challenge to determine what the regime will be thousands, tens of thousands, or even hundreds of thousands of years in the future.

In this case the scientific community does have at its disposal a number of tools, but first we have to reach some understanding about what is happening today. We can and have measured temperatures, the level and timing of precipitation. We can observe the way the water infiltrates into the mountain, measure rock properties, determine amount of moisture present in rock matrix and the unsaturated zone.

We could map those structural features that we believe facilitate fast pass and fracture flow, and we could try to capture all these different elements into conceptual and eventually mathematical marvels of fluent transport.

Determining future climatic hydrologic relationships would be less direct. We know the past has been definitely different than the present. Recent studies tell us that the most recent past has been a remarkably stable example of interglacial period. The more distant past, extending back thousands to hundreds of thousands of years, has some very rapid changes in both the long, cold glacial periods and in the short, warmer interglacial periods.

We can estimate past climate through the array of paleological studies of fossils and microfossils. We can try to estimate past flow regimes through geochemically and isotopic studies of minerals usually associated with deposition during times of flow. Of course, even if we assume we can adequately define those past hydrologic regimes, we still have to translate them to future behavior.

We can revise the famous geologic maximum and simply state that the future--what do we want to say here, what is that famous geologic statement? The future is adequately represented by the past, something of that sort. But what past are we talking about? Are we talking about the past of 12,000 years ago or 10,000 years ago or 100,000 years ago?

We need to take advantage of the modeling studies to provide insight on those aspects of the future that might be captured in the records of the past.

In the Board's eighth report, we suggested a strategy for resolving some of these problems. The strategy required, number one, an understanding of how climate change can cause the repository system to fail. Now, that's a quantitative question that needs a quantitative answer, and it's, of course, a model calculation. All we have to do is define what we mean by failure in this particular case.

Number two, primary reliance on paleoclimatic and paleohydrologic data to determine--or to put bounds on future scenarios. This was another recommendation.

Number three, the use of climatic modeling to determine the impact of anthropogenic effects on climate.

Again, probably another modeling question.

Four, the creation of a panel of experts to help guide the program in integrating data and models.

And five, setting as a goal not so much the ability to accurately predict future climatic scenarios, but rather determining whether or not these scenarios would have an adverse effect on repository performance.

No. 5 is obviously related to No. 1 there.

The Board is interested to see to what extent the DOE has used these recommendations.

Now, today and tomorrow we will hear about the basic work in this area. As part of the presentations, we have asked three consultants to provide us their perspectives on specific techniques, their uses and their limitations. These consultants are Ike Winograd of the United States Geological Survey. Ike, can you identify yourself?

Stanley Davis, from the University of Arizona. Stan?

And Tom Wigley? Tom is I don't believe here with us today, but Tom is with the National Center for Atmospheric Research.

Ike is a hydrologist of long standing, whose work on Devils Hold in Yucca Mountain has become a benchmark for reconstructing past climatic cycles in the southwest. He is also known as the father of the unsaturated zone. And I'm sorry, but I'm just reading a script--all right, and the originator of many provocative ideas.

Stan Davis is an eminent hydrologist who has laid out the basis for much of the use of isotopes in determining paleohydrologic regimes. Stan's studies are numerous and students are numerous, and they, themselves, have become important figures in the field.

Tom Wigley is an internationally-recognized authority on climate modeling. His experience also includes serving on the Center for Nuclear Waste Regulatory Analysis Expert Panel on Future Climate in the Yucca Mountain area.

We will start the meeting today with a presentation by Russ Patterson of the DOE on the Yucca Project Strategy for Addressing the Climate/Hydrology Issue.

He will be followed by Warren Day of the U.S. Geological Survey and Steve Beason of the Bureau of Reclamation, who will tell us about recent surface and underground mappings of geologic structures at Yucca Mountain. We are most interested in the 1,000-meter-long zone of closely spaced factors found in the ESF.

Ed Kwicklis of the USGS will then build on this information, put down his ideas on how geologic structure can affect the hydrologic regime.

Lastly, Alan Flint of the USGS will close today's sessions with a presentation on his and others work in describing present-day climate and infiltration. It is good to see Alan back here.

And we will continue this effort through tomorrow's sessions, in which I will be introducing in the morning.

With that, I think we'll start off with Russ.

MR. PATTERSON: If everybody can see okay, I think I'll start over--I'll start and use this slide for this.

Okay. During this introductory presentation, my idea was to talk to you about what we plan on doing to address the hydrology and climatology strategy for addressing the waste isolation attribute that you heard about earlier from Jean Younker on seepage. And one of the things that I do want to mention right up front is what you're going to hear for the next day and half is a portion of the hydrology program, definitely not the whole thing.

Our overall objectives for this strategy, if you will, for the hydrology climatology strategy for addressing this issue is to determine the spatial and temporal variability, as well as the magnitude of infiltration and percolation flux, determine the factors that influence infiltration and percolation, obtain the bounds on these influencing factors, and determine the likely impacts on the saturated zone, and ultimately transport of radionuclides.

Our overall strategy, use the geologic framework as a basis, understand the present-day hydrologic response to the present-day climate conditions, understand the past hydrologic response to past climate conditions, building on the climate conditions observed to date, provide future climate conditions that could affect repository performance, and model these hydrologic responses to future climate conditions.

And basically what you're going to hear over the next day and a half and what we're going to be hearing this afternoon is how the use of the geologic framework as a basis from Warren Day and Steve Beason, the geologic structure at Yucca Mountain. The present-day hydrologic response, basically you're going to hear about that from Ed Kwicklis and how the fracture pathways and flux for the UZ in the North Ramp. He's going to talk about evidence for fracture flow, percolation flux analysis.

You're going to hear about the present-day climatic conditions from Alan Flint. He's going to talk about how you determine the present-day relationship between the climate and the meteorologic factors and the infiltration rates. You're going to hear about past climatic conditions, the paleoclimate record, implications for future climate change from Rick Forester. He's going to talk about dates, amplitude, periodicity and paleoclimate mechanisms that link the global and local paleoclimate conditions.

We'll talk about the past hydrologic responses, the paleohydrology, age control, the U-series dating, the carbon^{14}. Zell Peterman and Jim Paces will be talking about the relation of the paleoclimate and paleohydrology and determining past timing of percolation events.

We should have an interesting discussion on hydrologic flow paths and rates from June Fabryka-Martin and Andy Wolfsburg. Talking about Chlorine-36, Tritium, fast flow path detection and the estimated age of in situ water.

And then we're going to talk about the modeling efforts and how you provide future climatic conditions, the future climate modeling by Starley Thompson. Incorporate anthropogenic factors into the climate conditions, provide probable scenarios.

And then we're going to have a discussion by Mike Wilson on TSPA and how the TSPA and the modelers will be using the data that we provide to them for TSPA-98, or TSPA-VA, or whatever you want to call it.

In summary, I want to catch us up because I'm whipping through these. Paleoclimate study determines climatic conditions. Isotopic studies identify the mountain's hydrologic response to those climate conditions. The present-day climate/infiltration studies identify effects of temporal and spatial variability of the climate conditions on hydrology. The future climate models will provide the climate scenarios that could affect the future hydrology.

And the TSPA will examine those impacts on future hydrology on the waste containment and isolation.

And that's basically our strategy.

DR. DOMENICO: Thank you very much, Russ.

Any questions from Board members on this introductory point?

Any questions from our consultants?

Incidentally, Tom Wigley has showed up, and, Tom, will you let people know you're here because we introduced you when you were absent.

That being the case, how about staff? Any questions?

Well, that's expected because this was just an introduction of what's to come. And I've got a few minutes--I've got just a few minutes after 2:00. We have a break scheduled at 2:05. Let's take it early, what the hell? Let's take a 15-minute break.

(Whereupon, a break was taken.)

DR. DOMENICO: There will be--attention. There will be a reception tonight at 6:00 to 8:00 in the Oak Room. That's just off the main lobby. Everybody is quiet now. There will be a cash bar, but snacks will be provided by who? M&O. M&O is going to provide the snacks. Does M&O know?

Where were we? There we go. Our next presentation will be by Warren Day of the U.S. Geological Survey, Steve

Beason of the Bureau of Reclamation, being with the new geologic map, geologic structure in the ESF, the correlations between the surface and the ESF mapping.

Who do we have presenting? Warren?

MR. DAY: Well, today Steve Beason and I have the pleasure of sharing the same time slot, so I'm sure we're going to run over, and so that the cocktail hour, Steve is going to pick up the tab for the first drink.

What's been going on in the last year here and a half on the surface and on the underground, is we've been working to prepare geologic maps to try and characterize the special setting of Yucca Mountain. I know this first preface may seem a little melodramatic, but it's really true.

The significance of geologic maps and the basic geologic research they support is routinely overlooked. They become part of the background environment. They're like water or air. They just exist to most people. But, in fact, the lives of every one of you in this room today has been touched by two maps, one by Pete Lipman and Gordon McKay, and the other by Bob Scott and Jerry Bonk.

If Pete Lipman and Gordon weren't interested in ash-flow tuffs, they would not have provided the first geobedrock geologic map of Yucca Mountain. If that map didn't exist, it was one of the key ingredients that was in the pot when Yucca Mountain was chosen for the their site characterization.

Bob Scott was hired away from Texas A&M to provide a more detailed reconnaissance geologic map, and I don't know where this project would be today without that particular piece of information.

So it's with this understanding, and the history that we've undertaken, the donning task to provide the project of bedrock geologic map in the central block area of Yucca Mountain, which includes a repository area. None of us in this room can begin to guess how this new map will figure into the future directions of this project.

I'm going to present the information, just some brief glances at the 6,000 surface geologic map we prepared. Steve's going to talk about structures that they've encountered in the ESF, and he's also going to try and present just an example of correlation of the bedrock with the underground geologic mapping, and that example is the North Ramp cross section.

Some of the highlights in my presentation, I'm going to try and just touch on some of the highlights that came to mind as I was preparing this talk. I'm going to compare our results with the earlier mapping, and then just briefly touch on some of the implications that I can think of, of the map for future and current use.

As scientists, we always have to be able to answer the question "Why bother with anything?" The purpose of Bob Scott's map was to help locate the repository within Yucca Mountain. It was a very good detailed recognizance map, but it was never intended to be used to the degree that's it's being used today, and it's just overworked, quite frankly, for the scale which they were mapping.

Last spring we determined through an effort to try and outline the dominant faults within the central block area, that some of the areas of Bob Scott's map, in fact, needed revision. It's always useful to have context of where the previous generation was so that you can know where you're going.

The GQ maps and Bob Scott's map define the major and--major block-bounding faults and the interblock faults within Yucca Mountain. They also established a stratigraphy which was useful at the surface, and basically at the formational and zonal level. But they were hampered, compared to today, by their base topographic maps are 1 to 24,000. Bob Scott just basically photo-enlarged his.

Well, we came along a year ago January with this huge corporate knowledge of the geology of Yucca Mountain, and we were asked to provide the bedrock geologic map. We're standing on some very tall shoulders here, and basically have been able to cherry-pick on a lot of the concepts and a lot of the work that's gone before us. We have a tremendous geophysical data set to work with, borehole data set. We have this fantastic ESF under our feet we're walking upon. There's a revised stratigraphy that correlates very well with the surface observed features and those in borehole and in the ESF, and we do have a fine 6,000 scale topographic base map to start from.

Bob's map, as I said, was a very good map for the scale and its original intent, and it did establish the location of many of the faults. And I'm going to focus our discussion primarily on faults because those are some of the main pathways from the hydrologist side of the house. I won't bore you with the details on how the stratigraphy varies over Yucca Mountain. I could spend the rest of the afternoon on that one.

Bob Scott, one of his main conclusions was a listric model for fault geometry for the main north/south faults, like Bow Ridge, the Solitario Canyon Fault. New seismic lines that we've had that Tom Brocoum put together do not support section model. Bob had a fault starting out essentially vertical and flattening out at about a kilometer below the repository.

He also introduced a concept of the Imbricate Fault Zone. I have it in parens, and I'll leave it in parens. It, in fact, is a fairly awkward/bad name for a series of faults, and I'll get into my reasoning for that in a moment.

Bob didn't recognize the members, the crystal-rich/crystal poor members of the Tiva and the Topopah, and those are tremendous tools for understanding the faulting and the fault geometry offsets at the surface.

And, you know, obviously, when the next generation comes along, you know, everybody has their own ideas, and there's just some natural variability based on the scale differences.

I'd like to touch upon some of the highlights and review for you just briefly the geologic map. We've been able to define the branching nature of faults, both vertical and their horizontal traces. Everybody is interested in the Ghost Dance Fault. It's just another fault. The Abandoned Wash fault is, in fact, connected to the Ghost Dance Fault, and I'll go through that interplay of those fault systems.

There's a connectivity of the faults, like the Abandoned Wash and Ghost Wash, the Dune Wash and the so-called Imbricate Fault Zone. And there's a northwest continuation of the Abandoned Wash Fault into the southern part of the repository area.

Another fallout of this of interest to this crowd would be a northwest-striking fault in the C-hole complex area that would connect them with at least the Bow Ridge Fault.

The new surface geologic map extends--by the way, it's going to be delivered, before I get that question, it's going to be delivered in August--extends from essentially Yucca Wash just north of G-2, south to Abandoned Wash, from about midway through Jet Ridge, east to just east of the C-Hole Complex and Exile Hill. It takes in the central block area of the--it includes the repository area.

Going from north to south, we were able to get a good detailed look at the Sever Wash Fault, the Pagany Wash Fault, see how the Solitario Canyon Fault varies along its strike from north to south. It's a very interesting fault. Pete Lipman and those fellows saw that it was, in fact, a scissors fault. We were able to, with it being downthrown on the east and the north and the west and the south, the plain of the fault dips to the east and the north and dips to the west and the south. The hinge point on your pair of scissors would be about here in Teacup Wash.

We were able to pull apart the various splays in the northern portion of the fault zone, and also gave it our best shot in the hanging wall deformation in the valley floor of Solitario Canyon.

Going across the southern part of the fault, the Abandoned Wash Fault starts here and continues to the South, comes up to the north, connects with the Ghost Dance Fault through the central part of the repository area. There's also a splay that Bob's got, didn't recognize, that continues further north, and I'll go over that in a moment.

We were able to unravel the very complex geology down in Boundary Ridge country. Over in the foot wall of the Bow Ridge Fault in the northern part of Bow Ridge, here we came up with a very interesting interplay of reverse faults and small-scale little grabens. In fact, they dropped the stratigraphic down 100, 200 feet, over a size of half of this room. I mean, they're just these narrow, canoe-shaped robins that are really quite pretty.

We were able to put together the geology of Exile Hill and see the northwest trending faults at Exile Hill.

I'd like to take a few minutes and talk about the South Ramp cross section, which this geologic map fed into to help provide the project with some sort of feeling for where they're going to go now that the ESF has made the turn and is heading for the home south portal, if you will.

This is an east-west cross section. Currently, in the 3-d geologic model, the cross section at this latitude is shown at the top, and at the bottom is our section we provided for the South Ramp area. Shown just briefly in colors here, Prow Pass, Calico Hills; Prow Pass in pink, Calico Hills is blank; Topopah Springs, green; the PTn units in this buff color; Tiva in blue; and some breccia zones that we're projecting to death in orange.

How does this vary? Well, the first round of the

3-d geologic model for the central block area, the workers knew that there were some problems in Boundary Ridge with Bob Scott's mapping. So they basically punted and put in an anticlinal structure over Boundary Ridge.

Well, what happens at Boundary Ridge is, in fact, a series of east dipping blocks, structural blocks that are just some good old-fashioned normal faulted blocks that we're going to come through the red line, is a trace of the ESF. When it makes a corner, we're going to be in the middle to upper part of the Topopah, and we're going to be skimming the top of the Topopah, intersecting the Ptn in several areas before we exit.

One of these canoe-shaped robins is, in fact--we're going to go through that in the middle of Boundary Ridge, and I'm really excited to see what happens with that, and I'm sure the design guys aren't so excited.

We're going to run across the Dune Wash Fault, another pretty good sized fault. It's a splay of the Dune Wash. It's about 250 feet throw along our traverse here.

To the east--or excuse me, to the west of the trace of the ESF, the new mapping has provided a little bit of insight of a branching nature of faults with depth. I've told you about the--the Abandoned Wash Fault comes north from here, and just keeps on going, and terminates. There's another splay that joins up with the Ghost Dance Fault.

You have good three-dimensional views of these two faults on these two ridges, and cutting across through here, this is this fault here, and the eastern splay is this fault here that grows up and become the Ghost Dance Fault.

When you project these to death, you can see this horse-tailing structure, or as Dennis was talking about briefly this morning, he was talking about the faults simplifying with depth. And that's one of the conclusions of this round of mapping, is brought out that, in fact, a lot of these fault zones seem to horse-tail as they come towards the surface.

Some other highlights that I think are pretty neat, better than neat, in Solitario Canyon Fault, there are several splays of the Solitario Canyon Fault, the die-up section. However, one such splay shows that the Topopah is offset greater than the units within the PTn, greater than within the Tiva, and there's an apparent thickness increase of the PTn over this fault.

Now, if this is true, then this is evidence for post-Topopah, pre-Tiva deformation faulting in this pile of ignimbrites, which should come to no one's surprise, being how you had to put a large volume of magma in the upper crust just north of there in the caldera so, you know, extension and volcanism were linked together.

But we are able to at least unravel some of that with this fine-scale detailed mapping we were able to do.

This is Solitario Canyon. The Topopah units are in green. Basically, the Pah Canyon and Yucca Mountain Tuffs, you can think of these as PTn in buff. And then the Tiva is in light and dark blue.

In Solitario Canyon, the hanging wall, as Bob Scott correctly pointed out, there is a series of west dipping structural panels in this hanging wall zone, but there's also quite a bit of tectonic juxtaposing of different units, and he punted and called it Tiva undivided on the map.

Well, we've been able to--we tried very hard to try and pull out as much information as we could. It's lousy exposure, and there's a lot of deformation going on down there, but there's also Topopah mixed in with the Tiva.

But back to the growth fault story. There are several little fault splays that come off the Solitario Canyon Fault that seem to die up section. This one actually keeps going on. But they're displacement decreases as you go up section.

There's one such splay that Ed Kwicklis is going to point out to you that plays into his discussion, that is, in fact, this growth fault I was talking about. The top of the Topopah in green here is offset about 50 feet, where the base of the Tiva is only about 10 feet, and, in fact, the fault itself dies out up section in the Tiva, and it doesn't even cut the crystal-rich member of the Tiva.

But what's interesting, there's an apparent thickness increase across that fault. It's down on the northwest side. So there seems to be some growth faulting going on associated with the Solitario Canyon Fault.

Okay. Just some examples, of the block-bounding faults, we've talked about the hanging wall deformation, the Bow Ridge Fault. Essentially, the traces remain unchanged because it's not exposed anywhere, so we didn't change it. The Sever, Pagany and Drill Hole Wash locations and displacements, we were able to fine-tune a little bit.

This map, I don't know that you guys have this because the colors wouldn't reproduce very well, but I gave you at least the basic geologic map.

The faults--our faults that we recognized are both --are in black. Bob Scott's faults and also his photolinears are in red, and the trace of the ESF is in magenta. This is just to compare the results of the mapping. And we did not see any evidence for the Yucca Wash Fault, so we don't put it on a map.

And the Pagany Wash--Sever Wash Fault is, in fact, composed of several very interesting fault splays that Bob Scott wasn't able to pull out because of the scale mapping he was dealing with.

The Pagany Wash Fault is very interesting. It terminates, as Bob and others had shown, at the Solitario Canyon Fault, but also terminates down here at a north trending down to the East Fault, associated with hanging wall deformation off the Bow Ridge Fault. That means that this fault is, in fact, we have good relative age control on the latest motion on the Pagany and the Sever and the Drill Hole Wash Fault.

I'm going to draw our attention to the intrablock faults, the difference of the map in here, and also what the Imbricate Fault--the mapping in the Imbricate Fault Zone has revealed to us.

Examples of intrablock faults, the Ghost Dance Fault, we've been able to fairly well define its splay, the width and the displacement variations along its trace. The Sundance Fault was originally not delineated by Bob Scott. It was recognized by Rick Spengler and Chris Potter and Bobby Dickerson last spring, put together a very detailed map of the extent of the Sundance Fault.

I'll talk just briefly about the orientations of the minor faults in the repository area, and just focus on the Sundance story.

Sundance is a very small fault of about three-quarters--it's 750 meters long. It's a zone of discreet, short discontinuous faults and sheers. The amount of displacement and the nature of the fault as seen at the surface is exactly what we see in the ESF at depth.

The Ghost Dance, itself, a very interesting fault. There's trace of the ESF, a little zooming in a little bit. These numbers indicate the amount of displacement. This is down to the west, normal fault displacement along the trace of the Ghost Dance and the Abandoned Wash Fault splay. It begins its life up in Cayote Wash and continues south. The north Ghost Dance alcove is situated about right here. And by the way, our mapping was integrated within the project to help cite that--both the north Ghost Dance and the south Ghost Dance alcove. So there's very good cross talk amongst the people worried about siting and working in these alcoves.

But it's a very minor fault. There's only about 15 to 20 feet of displacement. Where we see it in the northern part, the north Ghost Dance alcove, at least at the surface, is a very simple fault. It's only about two meters wide at best and about 20 feet of displacement.

South of--this is the Sundance in through here. South of this zone in here, the Ghost Dance picks up its head esteem, and there's a 40, 50, 90-foot of displacement along its trace, and then it starts dying out. The vertical displacement dies out as you go further to the south. It's this zone in through here with the maximum displacement along the Ghost Dance that Steve is going to talk about, the intense fracture zone within the middle of the Topopah.

We want to cite mapping at the surface. Where the ESF--a trace of the ESF crosses the surface expression of the Ghost Dance, there's only 10 feet at best of displacement. And Steve is going to talk about what they see underground, but I'll just feel his thunder. It's in the same--he sees about a meter or maybe 1.2 meters of displacement, same amount of displacement essentially. It's about--you know, as close as you can get, about 10 feet at the surface. So it's the same order of magnitude as seen in the ESF.

I touched upon the Imbricate Fault Zone. It's been--as discussed, in the eastern part of the central block area, there's a series of faults that are north-south turning faults. There are several disconnects between Bob Scott's original map and our generation of mapping in the northern part of that fault trace, up in the Azreal Ridge.

Scott and Jerry Bonk did a very good in the Yucca and the Antler Ridge, but down in the Boundary Ridge area where the south portal is going to be located, there again were some major problems.

Again, in red are Bob Scott's faults, in black are our faults. We recognize several faults that are essentially northwest trending, a few faults that are northwest trending in this area, as well as north trending. And the north trending faults seem to cut off the northwest trending faults. This is basically, if you look at the real map, is all query because the control is very poor.

But what our new mapping was able to show was that, in fact, there's a fault that comes from--we can trace essentially from Drill Hole Wash north and west, and that intersects usually 4 and 5. And that's an important hydrologic connection we later come to find out. There are a whole crew of people here that can describe this better than I can, but as the TBM is advancing, a lot of these drill holes are monitored for pneumatic changes, and you can see the effects of that TBM as they cross structures.

But anyway, the northern part of Azreal Ridge, some major disconnects here. To the south, and around the horn in the South Ramp country--I don't know that you have this particular diagram either. This is a very complex structural problem down here at Boundary Ridge, but let's just say there is a series of north-trending faults that connect with the Drill Hole Wash Fault, which is a major block-bounding fault, cut up through and then connect with the extension of the so-called Imbricate Fault Zone.

The problem with the Imbricate Fault Zone, as Bob described it was, he described it as a series of small down to the west faults that are laying--are imbricated together, are co-planar. In fact, our mapping has found out that at least in this area where he called the Imbricate Fault Zone, they are not co-planar. They dip to the east, they dip to the west, they go to northwest, they go to north. They're not imbricate. And, also, imbricate is used in the structural geology world and tied in with thrust faults. And they're not even--you know, clearly, this is not a thrust fault of country.

So we suggest--the problem is that these names get entrenched in the lure of the project, and they just don't go away.

So what are the implications of some of our work? So what, who cares? Well, the central part of the 3-d geologic model, we're working with a team that is developing that to try and tune it up and integrate the new geologic mapping.

From the tectonic hazards assessment group, it's important to know the location, orientation and widths of the faults and how they connect with the faults of no quaternary offset.

We've been able to feed information to the design team. One example is the South Ramp cross section. And it also--this kind of mapping provides the framework for discussions for possible expansion areas.

I work on a daily basis with my hydrologic colleagues in trying to put together to mesh the geologic story into the hydrologic investigations. And a basic bedrock geologic map is important to know for process models.

And last, but not least by any means, it provides Steve and his team a context for what they're seeing in the ESF.

DR. DOMENICO: Thank you very much, Warren.

Any questions from the Board?

Do the group of consultants have any questions?

DR. PARIZEK: I have a couple points. A question about Scott's photolinears.

MR. DAY: Yes.

DR. PARIZEK: They disappear on subsequent maps. Are they inferred fault possibilities or zones of fracture concentration? They can have also deep significance in terms of their water flow, even though they're not fault displaced.

MR. DAY: They are photolinears.

DR. PARIZEK: And those are real, the ones he has are real. Have they been looked for in terms of structure in the--

MR. DAY: Yes. Well, obviously, we looked at those as we were mapping. And our criteria for a fault, at least certainly in the central block area, over the repository there was a fault of displacement of about a meter and a half, something like that. So the faults that met that criteria were mapped and defined as best nature could allow us, you know, the exposure to map them.

They're not on our generation mapping because, you know, they're just photolinear.

DR. PARIZEK: But if you put photolinears on the tunnel and look for fracture concentrations, you'll see fracture concentrations in some parts of the tunnel under those photolinears or near the surface trace of the photolinears in my prediction, and it has hydrologic significance. It needs to be tracked down.

The other point to the west, there weren't as many faults in the central block area mapped. On the other hand--

MR. DAY: That's because they're not there.

DR. PARIZEK: On the other hand, you have faults that die out coming up section.

MR. DAY: That's true.

DR. PARIZEK: So it's very possible that exploratory tunnels going westward have a high probability of turning up faults that may be present at depth, but don't show up at the surface, and a hydrologically important observation can be made there.

MR. DAY: That's an interesting point. The orientation of the faults that you're describing would have to be essentially north-south. They couldn't rupture or emerge on the wall of Solitario Canyon or else we would have picked them up if they were significant faults.

DR. PARIZEK: Another possibility for the hydrologic gradients to the north of the repository.

MR. DAY: Yes.

DR. PARIZEK: Could that be the result of blind faults; that is, the faults that are present at depth, but don't daylight, and as a result, they act as a dam?

MR. DAY: That's an interesting hypothesis, too. Another hypothesis would be--and this is something that David Bush and Rick Spengler are working on, is trying to understand the volcanic faces change. You come from north to south, down underneath the Topopah and the Calico Hills. There's a lot of the flows and pyroclastic deposits that their thickness varies considerably right essentially at that hydrologic gradient.

So there may be a non-structural answer to that question, but you certainly can't rule out what you just described.

DR. DOMENICO: Thank you, Richard. Any--Don?

DR. LANGMUIR: Just thinking ahead about what your findings might be doing to the hydrologists, it seems intriguing to me as a non-hydrologist to think about you're displacing formations, and if the large displacement faults presumably allow a lateral flow in some of the formations and bypassing some of the low permeability zones in the mountain, I'm wondering to what extent your findings are impacting their thinking about flow in the mountain, whether those kind of displacements and the locations of them were things that Scott and Bonk had thought about or whether this is something new that is going to influence your analysis of flow.

Alan Flint is saying no.

MR. FLINT: I guess if you were suggesting that--I mean, we've certainly considered the offsets in all cases. What they have done is they have delineated those offsets so that we can get more specific information. But if you look at some of the Scott and Bonk maps, his fault said less than 10 foot, 10-to-20 foot, or greater than 20-foot offset, and we've incorporated a lot of that information already.

DR. LANGMUIR: That's not been changed by this analysis?

MR. DAY: Well, that's not quite true. A lot of Bob's smaller scale faults, we weren't able to verify, but the large scale, block-bounding faults that would be of interest to certainly the saturated zone people are well known, and our mapping has not changed that.

DR. DOMENICO: Clarence?

DR. ALLEN: Yeah, Clarence Allen.

You've now spent a fair amount of time on the ground. Let me just ask you what I asked Dennis Williams. Are you at all surprised by the pervasiveness of faults to the depth that you did not see on the surface, or do you have any comments on what you see at depth versus what you've looked at comparably at the surface?

MR. DAY: Well, I guess what I've learned is that, in fact, is that faults seem to simplify with that; you know, fault zones. Quite often at the surface there's some really rinky-dink faults at maybe 10 feet of displacement on Azreal Ridge that have 100-meter wide breccia zone. There's hardly any displacement, and there's no correlation between the amount of brecchiation at the surface and the width of the fault zone and the amount of displacement at that fault zone, either at the surface. But those zones seem to simplify with depth. That's something I've learned.

The amount of faulting at depth, it seems to be in keeping with what we map at the surface. In fact, Steve and I are very surprised at how well we correlate with our surface traces of faults versus those at depth.

So the density question you're asking about faulting, to me, is keeping at what we see at the surface.

DR. ALLEN: One other question, you mentioned that Scott and Bonk had--their map was efficient because of their assumption of the listric nature of the faults. Why did that--or how can that affect their mapping at the surface?

MR. DAY: Well, the evidence for that, Bob drew upon, which is correct, in the hanging wall zones of the major block-bounding faults, there seems to be a rolling over of the strata. That can only be accounted for by a curved plain in the fault. Straight, flat, would just be, you know, just a simple little thing, but you seem to see a rollover in the dip of the strata.

So he called upon a curved plain, which then, in the mid-80s was, you know, listric. Everybody was running with a listric model and a detachment fault model, and I'm sure they're proponents of that model today. But Bob's evidence at the surface is, in fact, that rolling of the structure.

DR. ALLEN: Well, I can see how it would affect the cross section; I don't see how it would affect the map.

MR. DAY: No, I'm sorry. It didn't affect the map.

DR. ALLEN: Okay.

MR. DAY: It didn't affect the map, no. But it's just the conclusions one draws from a map that trickle down through the thought process.

DR. DOMENICO: Jared Cohon, Board.

DR. COHON: I'm curious about uncertainty in your map. With no background whatsoever in geology, I appreciate you saying something about in your field what you consider to be good results. Are we talking about order of magnitude, fact of two, 10 per cent off?

MR. DAY: Well, nature has dealt us a very interesting hand of cards out there. The tools that we have to employ here--I'm going to get to the answer to your question. But the tools that we have to employ here is some very subtle zonal variations and these ignimbrite are pyroclastic sheets. Those zonal variations are distinct quite often, over an area of stratigraphic thicknesses to maybe a meter.

So you've got that kind of--that's your kind of level of confidence of where you know you are in the stratigraphy. That's assuming of a good outcry. Then there's quite often not so good outcry.

And you have to try--and we carry around a big ball of flagging with us, and we tie off, and we walk each and every contact that we can, you know, back and forth, and try and see if we can see that flagging jumping on us as we're mapping.

So we're turned in to the various subtleties of the stratigraphy; tremendous variations, but you have to know those.

Then there's a question of locating yourself on a map. I don't know if you tried to do that, I mean on a fishing trip or something like that, but that's not a tribunal exercise.

So our level of confidence at our location is essentially one contour interval, and what's nice about these series of EG & G maps is there's an underlinement. We've compiled, or composited the aerial photos. So we can pick out, by gosh, there's that yucca bush right there, and it's over there, and this is this boulder over there. And so you can put yourself in relative space to those photographic features. So we use altimeters and that kind of calibration, try and locate ourselves.

So our level of confidence, I mentioned maybe a meter and a half or one-half of--excuse me, half of a contour interval at 10-foot contour intervals, essentially is our level of confidence for the relative displacement on that fault as you're looking at it.

And there's the confidence of your location, and we feel actually better than we can get with the GPS system on that because of the aerial photos, and we tried GPSs out there.

DR. DOMENICO: Leon Reiter, staff.

DR. REITER: Warren, the Board has been talking about putting together--suggesting that it might be important to do an east-west drift because that's where most of the waste is going to be, through that western part of the block.

MR. DAY: Yes.

DR. REITER: If such a drift were constructed, based on, you know, your mapping, where do you think might be the most interesting and important place to put it, vis-a-vis repository performance, and what do you think we might see there?

MR. DAY: Boy, that's--I guess I fall back on what are the questions we're trying to ask before we get into that. What are the most important features. What are the features that are, if we found them, would either make the repository go or not go. Those are the questions you have to tell me the answer to first.

DR. REITER: Well, there's lots of--obviously, there's lots of flow paths. There's lot of water coming through lots of places.

MR. DAY: Right.

DR. REITER: We're going to have cause for concern.

MR. DAY: Right. Well, you know, I mean, let me just give you some tests that we could run, and without prioritizing those, if you don't mind.

If one ran an east-west drift--well, if we get the southern Ghost Dance alcove in, we're going to have one through this important zone of high deformation associated with the Ghost Dance. If we cut one essentially at the latitude, we'd pick up--we'd be able to see some of these north trending faults at depth.

If we cut an east-west drift to the northern part of the repository area, as outlined today, we would probably find a very coherent block of rock. I mean, that's the answer you want.

If you cut a north trending alcove up into an area that may be slated--may be at least a good candidate in our minds' eye for expansion area, then, again, you would be getting into a different--you would be trying to unravel what happens with some of these northwest trending faults. We have one example, the Drill Hole Wash Fault. There seems to be a southward progression of deformation associated with these, where the Sever Wash Fault has a high degree of deformation associated. When I say high, I mean slickenslides, mullions, breccia. It's just a wonderful little fault zone.

Then the Pagany Wash Fault has good really sheers associated with it. And then the Drill Hole Wash, as we've seen on the ground, seems to be a fairly insignificant structural feature until essentially the Sundance Fault, which I would classify in this genetic scenario I just proposed as the dying out of these northwest trending faults.

So if the question the group asked is what's the nature of these faults, then let's go north. If the question that we ask is how good is this block of rock, how far do some of these faults, maybe this growth fault, how far does that extend under, then an east-west drift would answer that question.

So I'm not copping out. I'm just imploring to the group if you're going to do an east-west drift, ask the question--ask some questions like, is this going to kill the repository or not, the answers to any of the questions you list. And then devise and design the east-west drift to those criteria.

DR. DOMENICO: I think John Cantlon wants to sharpen that question.

DR. CANTLON: Yes. The question you just raised, I think is a key one. What is the most likely place you could find the failing feature of the site?

MR. DAY: Right.

DR. CANTLON: Where would you look for it? Where would you look for the thing that would rule the site out? What's the east-west--

MR. DAY: I'm not sure that structures are per se-- hidden structures are going to rule it out, and that they're covered by the Tiva. And, therefore, they haven't moved since the Tiva time. That's what, 12.7 million years.

So hidden structures from a tectonic standpoint, a tectonic hazards standpoint, are quite frankly irrelevant, at least as my understanding goes.

DR. DOMENICO: Excuse me, but we've got--

MR. DAY: Let me just put that one out to the greater powers here. What are the criteria--what features would you guys--

DR. DOMENICO: Hold it. We're running a little bit behind here.

Steve, do you have a companion presentation?

MR. BEASON: Yes.

DR. DOMENICO: Yes. So I think we better relinquish the floor there to Steve and get his points on the geologic structure. We are a few minutes behind.

MR. BEASON: My name is Steve Beason. I'm with the Bureau of Reclamation. I'm the PI for underground mapping.

I'm going to try to cruise through these as quickly as possible, so just chime in, or whatever.

I'm going to talk about the characteristics of notable structures, and by notable, these are things that we expected to hit, such as the Drill Hole Wash Fault, the Bow Ridge Fault, Sundance Fault, all the ones that have names on them, and then some that didn't have names on them, the characteristics of the fractures primarily in the main drift and the fracture densities in the North Ramp and the main drift, so you have a little bit of a comparison between these two.

There's fractures that everybody has been sort of hinting around at that's several hundred meters long. We've dubbed it the Broken Limb Fracture Zone. This is not because they'll break my legs if I don't tell you about it, but it's because it underlies Broken Limb Ridge. And that's just an informal name so everybody knows what we're talking about. And then we're going to talk about the correlation between the surface and the subsurface mapping.

The major features we talked about, everybody obviously expected us to hit the Bow Ridge Fault. We had a lot of extensive explorations on the surface drill holes. We did our trenching up above there. There was numerous testing, different things. The offset along here is about 100 meters, which is almost exactly what David Bush had predicted from the USGS. The thickness in the tunnel was only about two meters, about that wide. It was a very simple zone. Maybe the most remarkable thing about it is we did not have an increase in fracturing going into it. The foot wall is relatively unfractured. It's in the lower lithophysal of the Tiva, and then as we went into the pre Rainier Mesa tuffs, the very soft, non-welded tuffs on the other side and reworked tuffs, again very little fracturing there.

As we got into the Imbricate Fault Zone, this is a fairly extensive zone of different faults. Some of them are very difficult to see, especially the one at 5+50, but as we got in there, we found that there were northwest trending faults. There was a number of faults that match up delightfully well with Warren's mapping on the surface. I'll talk about that in a few minutes. Various degrees of offset, up to five meters.

So some fairly significant faults in there. Also, some that had very wide open brushes; in other words, class with no matrix in them. So a fairly hydrologically significant area.

The Drill Wash Fault was maybe the most interesting thing underground. It had been predicted to be tens to even hundreds of meters wide. It turned out to only be just a few meters wide in the tunnel. It hits the tunnel at a relatively low angle. It first encounters the tunnel at Station 19, runs to about 1940 on the right wall. The zone itself is only about a meter, if you use your imagination. Most of it is only about 10 to 20 centimeters thick. And there's actually two faults that intercept right there, and these match also real well with Warren's mapping on the surface. I was, in fact, amazed that he had put two little branches right through there.

And the sense of the offset along there, the horizontal--or the slickenslides are primarily horizontal, but we've shown you what the dip slip movement is just so you have some kind of feeling. If you try to figure out what horizontal movement would give you, that relative offset, it's about 15 to 20 meters, or if you had just pure horizontal movement to give you the six meters down to the west offset along the Drill Hole Wash Fault.

The Sundance Fault was just composed of a series of discontinuous sheers and small fault plains. That's out over here. I've shown it almost intersecting the ESF. It was known to be that way by Warren and others up on the surface, and it matched very well at depth, except for it's over a little bit farther than what we had anticipated. I'll show you that on the cross-section in just a couple minutes.

And the things that we saw underground confirm what Warren had seen on the surface, and it's a series of just discontinuous sheers and small faults; not a continuous plain, not a wide zone of disturbance. As a matter of fact, in the tunnel you're hard-pressed to even trace it all the way across the crown of the tunnel. It's well exposed on both the right and left walls, but to get a continuous plain across the tunnel, one has to really use their imagination.

The Ghost Dance Fault we've just gone through within the last couple weeks. It's about Station 57 on the left wall, about Station 57 plus 30 on the right wall. It also matches very nicely with Warren's orientation on the surface, and about 1.2 meters offset down to the west, or down to the southwest underground.

I need to say just a little bit before we get into fractures on how we measure fractures so you know what in the world I'm talking about. Some people use strike and dip, and some people like to use dip and dip direction. We use azimuth and dip, which is essentially the same as strike and dip, except for instead of having like a north 30 west, or a south 30 east, we just use the degrees of the compass.

So this particular 110 azimuth would be a fracture that's striking in this direction, dipping to the southwest. If you were to express that in maybe more normal strike and dip terms, that would be like north 70 west, okay, dipping in this case--I didn't put a dip on it, but dipping to the southwest.

Okay. So whenever we talk about particular faults oriented a certain way, that's the convention we're using. Is that fairly clear?

Okay. We'll continue on.

I'd like to show you just a few stereonets. Those of you who aren't familiar with stereonets, this is a projection of fractures onto a hemisphere, in this case a lower hemisphere. And without going into a real hairy explanation of it, if you had a fracture plain, okay, oriented say north 30 west, and you projected it to lower hemisphere, it would give you a circle. If you plotted all the circles on this thing, you'd have a hopeless mess. You couldn't interpret it. So instead of doing that, what we've done is projected a pole at 90 degrees, at right angles to that, and then marked where that pole intersects the lower hemisphere. And then we contour them so we can see what in the world it is that we have there.

And this is a stereonet from Station 28, or essentially right where we come out of the curve at the north end of the ESF main. Okay, from there, that's Station 28, down 700 meters to Station 35.

And you can see in here--this is the scatter plot. This is where the poles actually intersect that lower hemisphere. This is the orientation of the main drift. Okay. You can see we've got a pretty wide scatter, but when you contour them up, you get a very distinct concentration right here. Okay, you can see that there are a number of other fracture sets kind of hiding in there, if you will, oriented, but most of them are high angle. If you look at the scatter pile, you can see that there's also a low angle set. This is a set parallel to foliation right here.

Now, if you can handle going from one to the next, I'll put them up so you can compare them here. Essentially we're looking at stereonets going from here to about right here.

This is the next one down. This is 35 to 40. Okay. You can see that we begin to lose some of these here. You see that you still have them, but as you're contouring them, what you're seeing is you're getting a stronger and stronger concentration.

Also, this one, the little set that is parallel to foliation shows up. Also, if you--I don't have it shown on this one, but if you just contour cooling joints, it will look very similar to this, these two orientations, with the low angle guys.

If you go a little farther to the south, now Station 42 to 4900, and what you see is all of a sudden, everything seems to disappear on the contour plot. And if you look at the scatter plot, you can see that you have lost a lot of these lower angle guys in the middle, but what you're getting is a stronger and stronger concentration around this. This is also just as we're coming into and really getting into what we've called the Broken Limb Fracture Zone. Okay, you still have these other guys oriented this way, but it is, is you've got so many along this one orientation, that the contour plot is just being obliterated by this orientation.

This is about, in this range, right about 120 to 145. In other words, it's southeast striking, dipping to the southwest.

Okay. And then as we look from 49 on to 56, you can see it becomes even more concentrated. Okay, again, you've still got fractures down in this orientation. These are southwest trending fractures. You've still got the ones parallel to foliation, but if you look in here, we've actually got some where we have 90 fractures plotted on one point in there. So there's a whole mess of fractures in there. Also, we're looking at over 2,000 poles on that one stereonet. You're maybe used to looking at stereonets that have one, two or three hundred of them. All of these have in excess of 2,000 fractures, so they're pretty big stereonets.

If we look at fracture densities in both the main and the North Ramp--let's see if I can do this now so it makes sense. This is starting from the very beginning of the North Ramp up here. This is the starter tunnel that will gap in there, it's not included in this data set, with all these funny-looking abbreviations for all the different rock units. This is the Tiva, right before the Bow Ridge Fault. The Bow Ridge Fault is laying right about right here. Tmbt is the pre Rainier Mesa tuffs, and then we lumped a bunch of them together there, the so-called Tuff X, the pre-Tuff X, and the upper part of the Tiva, the non-welded part, and then the Tiva crystal-rich, Tiva crystal-poor.

So you see a lot of data that will be coming out of here and out of here on some of the hydrologist information they'll be showing you. The vitric part of the Tiva, then the very low fracture density. What this is, is a moving average. The blue dots that are connected, it's a little easier to look at those than all the little red lines.

These are the bedded--so-called bedded tuffs, or the PTn some folks like to call it. The upper part of the Topopah here. Okay, then the crystal-rich, non-welded part of the Topopah, the transition zone between the crystal-rich, crystal-poor. This is all the upper lithophysal of the Topopah, and then we just start to get into the middle non-lithophysal, and here you can see the fracture densities jumping up to over four as soon as you get into the middle non-lithophysal.

Here we're out of the curve, we're starting out of the curve, and you can see we're running along about the same as what we were leaving off there. We have a slight increase here. The Sundance Fault is laying right in here, so nothing particularly remarkable around it. And then we have kind of a quiet area from about 38 to 42, and then we immediately at 42, the running--or I'm sorry, moving averages jump way up.

Okay. This is the beginning of the Broken Limb Fracture Zone. We do have a quiet area smack in the middle of it, from about 45 to 4670, where the running averages for fracture density drop back similar to what we had before we even got in the zone. They do persist. The same orientations persist through there, and then we get back into it about 47+00, and it continues on to about 52.

Okay. If you want to know what that looks like, in the back of the package you have there are a couple really poor looking photographs, but I have the color versions here. Russ Patterson sort of already stole my thunder on these two, but I'm going to show them anyway.

Let's start with this one over here. This is looking right along strike, or looking kind of northwest. The squares in the fabric are three inches by three inches. Okay. So that gives you some kind of an idea of the spacing. If you look across here, you can see they are just a few centimeters apart. And actually, looking right along strike is right here. So you can see they vary from about six to eight centimeters, or even 10 centimeters right there, and then you've got some in here that are almost one to two centimeters apart.

When you look at it in this way, and particularly in this one, from this view, they have kind of the view of columnar joining, but you can see from the stereonets, you don't have the fractures going back the other directions that you would have. If we had columnar joining, the stereonet would show you three nice concentrations, or if it was rhombohedral, you'd see two somewhat--two orientations somewhat at right angles to each other.

Okay. But that's not what you see. What you see is by far the dominant set is going northwest.

Okay. So it's really not cooling joints, or at least columnar joining in the way most people think of it.

I'd like to jump right into the characteristics of this particular fracture zone. It goes from 42+05 to 52+50. There is a break in the orientation. Remember, I told you it was kind of a quiet area in the middle. There's an orientation shift right across that. It's not dramatic. It's like 10 to 15, 20 degrees.

The thing that surprised us the most was that it wasn't observed at the surface. We're right under Antler Ridge, Broken Limb Ridge, Rail Back Ridge. We even went up after seeing this in the tunnel and said, well, maybe we didn't see it on the surface and we just overlooked it. So Warren and I went back and walked all over this area. There's not an expression of this as such at the surface.

Okay. The fracture densities locally, we had some that are actually greater than 12 fractures per meter. That doesn't mean that every .8--or .08 meters if there's a fracture, but that in some areas, you have very high fracture densities.

The fractures are generally long, two to four meters; smooth, which is maybe one of the more remarkable things about them, R5, and our roughness scale is 1 through 6, 6 being polished, very smooth, R1 being extremely rough, and they're very planar.

Okay. The other set of typical cooling joints we have in there are generally smoothly curving, but these are nice and planar. They're not anastomosing very much.

The fractures are typically coated with manganese oxides and/or vapor-phase minerals. Okay, and the vapor-phase part of that is what's important as we look to figure out how in the world this fracture set got there.

And, also, as we look at the video log from Drill Hole SD-12, which is just west of the ESF, we see that these fractures do not extend into the overlying and the underlying units. By that, I mean the upper lithophysal, lower lithophysal. They just are limited to the middle non-lithophysal.

We also looked at the lower non-lithophysal, which is quite a ways down the hole. They also were not evident in that. Okay, so they appear to be very much stratabound to the middle non-lithophysal.

As we begin to look at how these things got there, a lot of different ideas were put forward, and there's even more beyond this. We looked at, or tried to determine if there was a previously unrecognized cooling surface in the Topopah, one that, you know, maybe we just missed in looking at the drill holes or something. We talked to Dave Bush, to Tom Moyer, and that doesn't seem like a very plausible idea. There's no other indication that there's any kind of a cooling surface at the top of the middle non-lith.

Someone put forth that it might be related to the excavation by tunnel boring machine. I don't think that's a very good case either. If you've been in TBM tunnels before where you've seen fractures related to the boring, you know that they change from side to side. They're very distinctive. They don't extend way back into the wall generally. So that doesn't seem very much--or very likely to be the case.

Where this particular fracture zone, it just happens to be where the greatest offset along the Ghost Dance

Fault is.

And if I can find Warren's overhead here--I believe this is one he showed you earlier. Where the fracture zone lies is right in through here. Okay, and you can see this is an area where we have 40 feet, 90 feet and then 40 feet again of offset along the Ghost Dance. That seems like it's too much of a coincidence to ignore.

We looked at possibly this is formed by the Topopah when it was deposited draping over some pre-existing topographic feature. We think that it probably is related somehow to tectonics that were going on during eruption or right after eruption.

The fractures, because they're so smooth, so planar, because they have vapor-phase alteration in the walls, in other words, the walls are actually altered out a few millimeters either side, we looked at this possibly as being some sort of cooling joints. And then our catch-all phrase, some combination of all of the above.

The three that Warren and I are probably chasing after right now are the fact that it's related to the Ghost Dance Fault, some sort of syn-eruptive tectonics, and then possibly that they are cooling joints, or that they started out with widely-spaced cooling joints, and then because of the tectonics going on, fractured them up--are much more parallel to the pre-existing cooling joints.

I'll tell you right off, there's problems with every one of those theories. Okay, we've argued it back and forth. How we're going to resolve this, is the southern Ghost Dance Fault alcove goes right out from this zone into the southern extension of the Ghost Dance Fault; in other words, right out in here.

And as we mine out through this, we'll be looking at how this fracture zone changes. If we get on the other side of the Ghost Dance Fault and it isn't there, that will tell us something. We know that it doesn't extend much farther to the south. We have ST-7 down there, and the indications--I haven't seen the log from it, but the indications are that it's not present in ST-7. So we're limited pretty much to this area in this particular fracture zone.

DR. DOMENICO: Steve, we're running 15 minutes behind.

MR. BEASON: Okay. I'll speed it up.

DR. DOMENICO: I'm glad you didn't find seven more faults.

MR. BEASON: This is the generalized cross sections from the North Ramp and the main drift. Here's the Bow Ridge Fault right here. I'm sorry that these didn't come out better on your xerox copies. But here's the Bow Ridge Fault. Here's a series of faults through Azreal Ridge that Warren has put in the Imbricate Fault zone. These things match amazingly well between the surface and the subsurface.

Drill Hole Wash Fault is out here, and then here's the Sundance Fault. We've made it as discontinuous looking as possible on this cross section. And then out here, just beyond ST-7 is where the Ghost Dance Fault recrosses or crosses over the alignment of the ESF Main.

I'll throw these two up real quick. Since Warren has been on the job and since about the same time we started mapping underground, we've been talking continuously, comparing notes between what's happening at the surface, what's happening underground, so that both of us are able to refine our maps.

In the Imbricate normal Fault Zone, surface mapping helped to define faults that were obscured, especially the one at 5+50. This thing was completely lagged up underground. We didn't get a look at it all, other than to climb up in the head of the machine and see what was going on out there.

But we did have a good sense of what the offset was because we had a change in the rock units across there.

So Warren's map helped us straighten that out, and, also, the underground mapping helped show Warren several very small faults, okay, that would have been really tough to discern on the surface had we not mapped them underground. So we went back up on the surface, said, yeah, this--you know, this little offset really is a cross here. So we were able to tie those two together.

The Drill Hole Wash Fault, they agreed on the location. The underground mapping helped define the limited size of the faults.

The northern extent of the Ghost Dance Fault, Warren's mapping had showed that it didn't go up to the ESF. We confirmed that. It's not in the ESF. There's just nothing up there that could be the Ghost Dance. The Sundance Fault matched amazingly well between the two.

The Broken Limb Fracture Zone, okay, the surface mapping tells us, or helps tell us, that it is stratabound, that it's limited to something. Obviously, we needed the drill hole to tell us that it was just the middle non-lithophysal.

And the South Ramp mapping helps us as we go through these different faults to correctly identify them, to project them up to the surface. It helps us to know what to expect on the other side. And it's obviously very helpful for the design people as they're trying to come up with design estimates, numbers of steel sets and that sort of thing.

Okay. Thank you for your attention.

DR. DOMENICO: I'm going to hold questions here, and we'll have time at the end. We're 15 or 20 minutes behind, but we will have time to pick up any questions when this is all over; is that okay?

So right now, we're going to hear from Ed Kwicklis on fracture pathways and flux in the North Ramp area.

MR. KWICKLIS: I'm going to be talking to you for the next 25 minutes or so on fracture pathways and flux through the unsaturated zone in the North Ramp area of Yucca Mountain.

I am going to talk about basically three topics. One is the evidence we have for fracture flow in the North Ramp area, and this included the fact that we have evidence for a locally fractured non-welded Paintbrush Tuff in the North Ramp area, some corroborating evidence for fracture flow provided by the water potential measurements from instrumented boreholes in this area. And there's further evidence from the geochemical evidence from the perched water and from the Calico Hills, based principally chloride concentration, strontium data C-14 activities that also provide evidence that there is fracture flow in the North Ramp area.

Following that, I'll be presenting percolation flux estimates at two locations. The first percolation flux analysis is based on perched-water occurrences in the Drill Hole Wash area, based principally on data from using 14. The second percolation flux estimate is based on analysis of heat flux in Pagany Wash at boreholes UZ-4 and 5.

I think both of these analyses are interesting from the point of view that they represent effective long-term averages and that they include the fracture component of the percolation flux.

The second application may be of additional interest for the purposes of this meeting in that the UZ-4 and UZ-5, as Warren Day indicated, are located in a fault-effected zone. And so inferences of the rock properties and percolation fluxes at UZ-4 and 5 may provide some indication of what fault properties may be like at other unsampled or untested faults elsewhere, in the North Ramp area and elsewhere.

The data and analysis that I'll be presenting are from the most part taken from this report, which has just been revised in response to colleague review, and it's been returned to the reviewers for their concurrence.

This was really a team effort involving geologists, geochemists, modelers. All of these authors contributed to various sections of the report, and the authors' list is too extensive to identify the individual contributions of each of these people, but suffice to say it was a team effort involving a lot of people.

This figure was taken from the North Ramp hydrology report. It shows the borehole location and fault structures in the study area of the North Ramp area. I'll be keying in on just a couple of these boreholes, namely the UZ-4 and 5 boreholes in Pagany Wash, for which the percolation flux estimates based on the temperature data were made, and also the second--actually, the first percolation flux analysis is based principally in the UZ-14 area.

But many of these boreholes have been instrumented in the past year, UZ-4 and 5, NRG-7a, NRG-6, have all gotten water potential measurements, pneumatic pressure measurements and temperature measurements from the in situ monitoring strings. UZ-1 was instrumented around 1984 and also included the similar types of measurements. SD-9 had some measurements, may have pneumatic pressures as well.

But I'll be concentrating principally on UZ-4 and 5 and UZ-14 areas, as well as making passing mention to NRG-6 and UZ-1.

So the evidence for the existence of local secondary permeability in the PTn includes isotopic evidence from tritium data within and below the PTn, inferred field-scale permeabilities based on the pneumatic pressure record that were substantially higher than the matrix permeabilities as measured on unfractured core samples, and the chloride and C-14 concentrations and strontium data from the perched water reservoir at UZ-14 that indicated the fracture flow origin for the perched water.

And I'll quickly go through each of these lines of evidence.

These figures show the tritium data for borehole UZ-4 and similar data for borehole UZ-5. UZ-4 is located in the main channel of Pagany Wash. UZ-5 is located on the side slope.

The data show that tritium related to nuclear testing has found its way through a depth of at least 45 to 50 meters in UZ-4, and to a depth of about 30, 35 meters in UZ-5. The UZ-4 data in particular indicates that there is some local fracturing within the PTn. The fact that we observe these tritium data where we do, however, implies that the PTn is to some degree effective in capturing rapid fracture flow through the overlying rock and mitigating that fracture flow.

However, as I'll show later, it's not a perfect barrier. It is a barrier that's effective to some degree as evidenced by the fact that we find this tritium data where we do have them.

In this case, I should point out that the PTn is about 80 meters thick in Pagany Wash. It thins dramatically between Pagany Wash and Drill Hole Wash, but it's only about 25 meters thick in Drill Hole Wash and areas to the south. And Don Langmuir had a question earlier about fault offsets, and I think that the fitting of the PTn across the Drill Hole

Wash Fault, combined with the greater offset of cross faults towards the south sets up the situation where it's possible to completely disrupt the lateral continuity of the PTn and create windows through the PTn, and I think this is what was seen at some of the UZ-7--in the UZ-7 pneumatic record where some of Warren's work is shown that there is a continuous pathway around the PTn at UZ-7a.

This is a plot of tritium data. By the way, all of the geochemical data that I'll be presenting were essentially from Al Yang, a section in the North Ramp report edited by Joe Rousseau and others.

In NRG-6, we see that tritium data with--tritium with concentration of about 150 tritium units occurs in the non-welded upper part of the Topopah Spring, and so it's made it almost entirely through the PTn at this location, and this may be related to the fact that at this location, it's only 25 meters thick as opposed to the 80-plus meters thick in Pagany Wash.

So we see that there is evidence that the tritium has penetrated almost the entire thickness of the PTn, and presumably, some may have made it into the Topopah all together.

This is similar data from UZ-14. The only unambiguous evidence of bomb pulse inputs occurs probably in the upper part of the Pah Canyon member at a 40-meter depth.

There is some--it's not clear whether some of the other peaks in a deeper stratigraphic intervals in the Topopah Spring and Calico Hills do or don't reflect bomb pulse inputs. There's some concern that because atmospheric tritium concentrations remain at approximately 10 to 30 tritium units that possible exchange of vapor in the laboratory air with the samples, doing the sample handling, may have some led to some higher values in some of the samples.

Skeptics of that theory, including myself, wonder if that's the case, why all samples weren't similarly effected and why we see, you know, some points have fairly consistent trends.

And in light of recent findings of the chloride 36 studies and the fact that the Calico Hills C-14 data shows C-14 concentrations as great as 95 per cent modern at the base of the Calico Hills, there's ample reason to re-evaluate the uncertainties surrounding some of that tritium data.

I think the C-14 data in the Calico Hills and UZ-14 is a very significant finding and one of the most significant findings reported in the North Ramp report. The 95 per cent modern value at the base of the Calico Hills represents a water that's only 500 years old, and it's unlikely that these samples would have been influenced by C-14 in the gas phase at nearby UZ-1. Many years of sampling have shown that at the base of the Topopah Spring that the gas phase C-14 concentrations are about 25 per cent modern, and so it's clear that old gas can't make water look younger than 25 per cent modern. And these values range from 65 to 95 per cent modern.

It's also clear that a past water table rise hasn't left these--hasn't affected the Calico Hills samples. The C-14 values for the saturated zone from nearby H-1 are about 21 per cent modern and obviously far below the 65 to 95 per cent modern values that we see here.

DR. CANTLON: Before you put that off, where is the water table on that--on B?

MR. KWICKLIS: The water table, I believe, is around here in the upper--

DR. CANTLON: Okay. Thank you.

MR. KWICKLIS: Yeah. Another line of evidence for fracturing the PTn comes from the pneumatic pressure data that's complected it at numerous boreholes. This figure shows the instrument station, locations for boreholes UZ-4 and 5. Basically, stations to monitor the pneumatic pressures were placed in the upper--at the upper and lower contacts of each of the major stratigraphic intervals. And these intervals define--these stations define intervals for which permeabilities can be estimated based on the way that surface barometric pressure changes prop date through the subsurface.

Those pressure changes enable estimates of pneumatic diffusivity to be made, and if drained processes can be estimated through independent means, those estimated pneumatic pressures provide estimates of the drained--the air permeabilities.

So the first panel here shows the data for UZ-5. You can see that as one moves from shallower to deeper monitoring stations, there's a progressive shift in phase and reduction amplitude of the pneumatic pressure signal as it moves through the subsurface.

The other three panels show the match between a one-dimensional model, gas diffusivity model, with the measured data at various stations. And the match between the one-dimensional model using barometric pressure changes at the surface as inputs was able to very ably match the measured pneumatic pressure data. And from this match to pneumatic pressure data, we obtained estimates of permeability for the intervals defined by the monitoring stations.

The permeability estimates for UZ-4 are shown in the red. The estimates based on pneumatic pressure for UZ-5 are shown in the blue. All of these--all of the estimated values exceeded about 10^{-12} meters-squared.

Based on permeabilities measured on unfractured core samples from the same boreholes had permeabilities approximately two orders of magnitude less, and the difference in the magnitudes of permeabilities from the field data and the core samples reflects the fact that the permeability estimates based on the pneumatic pressure measurements incorporate the effects of fractures. And it's not surprising, given the fact that we are in a fault-effected zone, and also not surprising given that we saw some tritium data deep within the PTn at least at UZ-4.

DR. COHON: I'm sorry, could I just interrupt for one second?

MR. KWICKLIS: Yeah.

DR. COHON: Could you go back to the slide before that? I just didn't get it with regard to how this shows the effect--the variation in depth of pressure variation. Could you just go through that?

MR. KWICKLIS: This is the shallow station, and we go from H to A. We go to progressively deeper stations. So the station at the surface shows a large amplitude signal, and the stations deeper in the subsurface show that there is a separation in time with the signal at the surface and also a reduction in amplitude of the signal.

So it's basically--it's basically showing you that as you go deeper into the subsurface, the signal with increasing depth shows increasing lag to the surface, the signal measured at the surface, and also it has amplitude reductions. So that's just getting your sense of the character of these things.

DR. COHON: Thank you.

MR. KWICKLIS: This figure shows water potential measurements measured at NRG-6. The data for water potential at NRG-6 are fairly representative of what we've seen at instrumented boreholes elsewhere in the North Ramp area.

What you see on the right panel sheer is measurements made at three different times, and what we see is the sending material progressively coming into equilibrium with the surrounding wall rock. What is interesting about these profiles is that as they near equilibrium, they are very wet, and that they have pontentians (sic) greater than minus 5 Bars.

And while this doesn't prove fracture flow per se, it does suggest that conditions in the rock, at least near the fractures, are wet enough that capillary imbibition of water moving through the fracture system is not going to be a very effective process in mitigating fracture flow through the fracture units.

And this was essentially observed at all the instrumented boreholes in the North Ramp area.

Another bit of evidence for the fracture flow origins of--for fracture from the North Ramp area comes from the chloride concentration data, also from Yang and others, which was another milestone report that Al has published this year. It includes both chloride measurements from water squeezed from cores, and that's shown by the red squares, and the chloride data for the pumped and bailed samples from the perched water body.

The PTn samples showed chloride concentrations in the range of 40 to 100 milligrams per liter, with an average of about 77 milligrams per liter. The chloride data from the perched and bailed samples showed about eight milligrams per liter.

The fact that chloride is essentially a conservative tracer, that's very hard to get rid of until you reach brine-like concentrations and it begins to precipitate out.

So it's clear that water from the PTn matrix never became perched water, inferring that the perched water is, in fact, water that moved through fractures of the overlying PTn matrix and did not reach as matrix water in the PTn.

Also of interest is water squeezed from the core from the same interval as the perched water. It showed chloride concentrations of about 80 to 130 milligrams per liter. So it's clear, again, that the perched water didn't originate from matrix flow, at least not through the Topopah, and also that there's considerable disequilibrium between the chloride values in the fractures that were pumped and bailed than in the adjacent matrix.

The chloride values from the underlying Calico Hills are intermediate between the perched water and pore water samples, and one might--that they simply represent a mixture of the fracture water and the matrix water. However, the Calico Hills pore water, as we said earlier, has C-14 concentrations that range from 65 to 90 per cent modern, whereas the perched water from the fractures had 25 to 65 per cent modern carbon.

So, again, it's clear that there are secondary inputs occurring between the perched water body an the underlying Calico Hills that have contributed to the water in the Calico Hills.

So in summary, the data indicate fracture flow through the PTn and Topopah, at least to the level of the perched water body, with secondary inputs of water occurring between the lower Topopah and the Calico Hills.

This next slide summarizes the data, the chloride and the C-14 data, and also points out that the perched water samples based on analysis by Zell Peterman and his colleagues had strontium ratios that were very nearly those of the surficial and fracture coatings, and quite dissimilar from that of the Topopah Springs rock matrix, indicating again that the perched water didn't arrive as a result slow seepage through the rock matrix.

So I'd next like to present to you some very simple calculations based on analysis of the perched water body that used the residence times and volumes of the perched water body to estimate the permeability of the perching layer, as well as seepage rates through that layer, and ultimately produce estimates of percolation flux through the overlying Topopah Spring Tuff.

The general conceptual model for perched water-- perched water, first of all, may be defined as a zone above the regional water table where water pressure is greater than atmospheric, and outside of which water pressure is less than atmospheric. And so it's a zone from which water must flow freely when intersected by a borehole or tunnel, which serves as a constant atmospheric pressure.

So perched water occurs when the percolation rate exceeds the transmission capacity of the perching layer, which is a function of its vertical permeability and the hydraulic head gradient across that layer.

We believe as part of that conceptual model that the perched water in the North Ramp area is widespread, if not ubiquitous, but because of lateral diversion related to the dipping of the layers, it doesn't accumulate in significant volumes, except where the appropriate stratigraphic or structural conditions exist. And so positive heads do not develop except where structural or stratigraphic conditions lead to the formation of a trap.

The analysis that I'm going to present to you depends upon the validity of the so-called growth fault model for perched water, which is not universally accepted within the USGS primarily because as Warren Day explained to you earlier, a growth fault is one in which displacement occurs simultaneously with deposition so that displacement in the lower stratigraphic horizons is much greater than displacement in the shallower stratigraphic horizons.

And so based on--in order to explain that you have apparently greater thickness of perched water at UZ-14, the growth fault that Warren and others had identified along the Solitario Canyon was projected northward into the Drill Hole Wash area as a possible explanation for the apparently thicker accumulation of water at UZ-14.

In cross section, this is how the conceptual model of perched water looks, that this fault has created an offset between--across the basel vitrophere, between the west and east sides of the fault and allowed the perched water to accumulate where it was accepted by UZ-14.

So basically the analysis that I'm going to present assumes that there's two components--that there's two components of the water that has accumulated here. One is water that is flowed vertically downward through the Topopah

Springs, and that there's a second component that flowed vertically downward through the Topopah Springs in the up-dip areas, the so-called adjacent contributing areas, and then flowed laterally into--and openly found its way to the perched water body intercepted by UZ-14.

So the logic that I follow in estimating percolation flux based on this model is that if you know what the residence time of water in the perched water body is, and you know its volume, you can calculate the seepage rate through the perching layer. If you know what the seepage rate is, and if you know what the head gradient across that perched layer is, you can calculate what its field scale permeability is.

Under the assumption that it's laterally homogeneous, and under the assumption that because significant positive head stone developed in the upper lying areas that you have possibly integrating it, you can then estimate what the seepage rates through the perching layer are in the uptive (sic) gradient--uptive areas.

So then you say that the total flux, the total volumetric flux in the Topopah Spring is equal to the total flux through the basal vitrophere in the area of the NEPA-approached water body and in the uptive areas, and through that water balance equation, you can calculate what the percolation flux through the Topopah--vertical percolation flux through the Topopah Spring is.

Furthermore, if you say the total flux seeping from beneath the perched water body is equal to the really weighted fluxes flowing vertically in the area above the perched water, plus the diverted--aereally weighted diverted fraction in the uptive areas, then you can calculate what the component of the vertical percolation flux that's diverted might be.

So there's some uncertainty in this in that we don't know if there's the same degree of alteration in the uptive areas. We don't know what the effect of some of these splays of the Solitario Canyon Faults are.

So we've seen two cases. One is that the contributing area extends from here to the Solitario Canyon Fault, and the second case, it seems that the contributing area is essentially zero.

And I'll just run through these very quickly, just give the results, since I've explained.

So based on this, we calculated a reference time of between 5,150 to 11,000 years. We estimated that the seepage rates through the basal vitrophere beneath the perched water reservoir were surprisingly low, .0014 to .29; that the permeability of the perching layer ranged from 5.9×10^{-19} to 2.8×10^{-21} m^2.

And we determined that the average vertical percolation through the Topopah Springs that's compatible with the volumes and residence times of the perched water are extremely low, .001 to .29 millimeters per year. The caveat here is that based on all the isotopic evidence that I presented earlier, that this volume arrived as a result of fracture flow and not matrix flow.

And finally, the diverted component of the flux was also very small, .015 millimeters per year at most, and because the contributing areas are potentially much larger than the area of the perched water body itself, that resulted in a maximum of 30 per cent of the water in the perched water body at most arrive through fracture flow.

So I'm running out of time. I'll zip really quickly through the second analysis, which was a calculation of percolation flux based on an analysis of heat beneath Pagany Wash.

Again, I think it's interesting because it incorporates the fracture component of flow and because it lies in a fault-effected zone of high permeability that may have indications for what the permeability of similar fault zones that haven't been tested might be.

We did a number of sensitivity analyses that looked at the effect of topography, contrasts in thermal conductivity between the non-welded and welded tuffs, conducted subsurface air circulation. We didn't examine this explicitly, but for heat flux in the saturated zone, we looked at papers on regional heat flux that is published by Frederick, Dudley and Stuckless.

We focused our analysis that I'll show you primarily on what are percolation rates. So as water moves from shallower, cooler environments to warmer, deeper ones, it consumes a certain amount of heat in order to maintain thermal equilibrium with the surrounding rock, and the amount of heat consumed is related to the percolation rate. And we're going to use that principal as a means of estimating what the percolation rate is.

This is the temperature data for UZ-4 and 5. The profiles look similar, however, the temperatures in the upper part of UZ-4 are approximately a half degree to 1 degree C warmer than those at UZ-5.

In cross section when these temperature profiles are contoured, they show a convex upward curvature to the temperature contours, and these indicate that there's a lateral component to the heat flux from UZ-4 and UZ-5 that based on some purely conductive model simulations, we believe is related to the low thermal conductivity of the alluvium.

And as a consequence, the heat flux in the interval increases as we--upward in UZ-5 as we go from the Pah Canyon to Yucca Mountain to the Tiva Canyon members.

The real story here, though, is that the heat flux in the Pah Canyon at both UZ-4 and 5 is only 15 1/2 milliwatts per meter squared. And this is a substantial reduction in the heat flux estimated for this area based on the regional heat flow studies, as well as on analysis of the deep heat flux at NRG-6, NRG-7 and NEZ-1 and UZ-1, which provide estimates of about 32 to 40 milliwatts per meter squared at the base of the unsaturated zone.

So there's a significant reduction in the upward-- in the upper conductive heat flux at both of these boreholes, and so we're going to use that information to try and estimate what the percolation flux is between the Pah Canyon and the water table.

The next slide is just a written record of what I've just described to you about the contours.

Basically the liquid percolation, the downward liquid percolation flux can be related to the upward conductive heat flux at any elevation through this equation. The equation assumes steady state flow of both heat and water.

So based on information that suggests that the true heat flow at the water table is about 32 to 40 essentially milliwatts per meter squared, and that the temperature difference between the Pah Canyon and the water table is about 10 degrees C, we estimate that there is about 12.4 and 18.4 millimeters per year moving between the Pah Canyon and the water table in the Pagany Wash.

We also set up a couple of model of water and heat flow using TOUGH-2, and based on a trial and error fit to the measure of temperature data at both of these holes, obtained estimates of water percolation rates of about 18 millimeters per year at UZ-4 and about 5 millimeters per year at UZ-5.

These estimates differ slightly from those I just calculated because these estimates are conditioned to some of the shallow temperature data. We're aware from the previous slides that there are some non-vertical components of heat flux in the upper parts of these holes, and so the estimates at UZ-5 are probably underestimating slightly the upward heat flow and the downward percolation rates. And the model fit at UZ-4 is probably overestimating slightly the downward percolations and upward heat flux.

But overall, they're in very reasonable agreement with the analytical expression--the results from the analytical expression I showed you earlier.

So in summary, I'll wrap it up quick. I think there's ample evidence that fracture flow occurs within and through the PTn. The occurrence of the perched water reservoir in the Drill Hole Wash area is compatible with relatively small amounts of fracture flow of .001 to .29 millimeters per year.

In calibrating the site 3-d model, LBL, identified a typical matrix flux of .02 as being a value that seems to be compatible with most of the borehole saturation data across the site, and the range of fracture flow that we calculated based on the perched water analysis encompasses that matrix value.

The inferred heat flux deficit in Pagany Wash at Boreholes UZ-4 and 5 implies a long-term deep percolation rate of, we're going to call it 10 to 20. There's some uncertainty in these estimates--10 to 20 millimeters a year in the general wash environment.

Whether this is, in fact, related to the fault structure that we know occurs at UZ-4 and 5, or whether it implies a greater role for the wash and infiltration processes than current climatic conditions might indicate is not year clear.

So that's it.

DR. DOMENICO: We have time for a few questions. Does anybody on the Board have a question? Don?

DR. LANGMUIR: I think we are all racing to stay up with all this information. It's tough to get it and digest it all quickly.

But one thing you passed over at the very beginning, which I would like to talk about first, has to do with the point that the rock was wetter now, so that fracture flow--this is my paraphrasing--was not limited by matrix imbibition.

And this--you know, I'm not in this business all the time, but my sense from years ago in this program was that we were very concerned about losses from fracture flow into the wall rock by imbibition as a limiting factor on the movement of fracture flow downward in the mountain. If the matrix is full and we're at saturation for most of the depth in the mountain, then anytime the water hits the fracture, it's all going to go down, right? Isn't that--how do you read this? This is my sense of the implications of that, which is very important I think to water movement downward in the mountain.

MR. KWICKLIS: I think that's a logical conclusion to draw, that there's not going to be very much imbibition in the Topopah Spring matrix. There are very small capillary pressure gradients compared to what we once imagined existed, and I think that matrix imbibition can't really be appealed to as a very effective mechanism once water has gotten to the Topopah Spring. And this is indicated by the--you know, we saw that there was chemical disequilibrium between the Topopah Spring Tuff matrix and the perched water with regard to the chloride concentration. So it appears that based on the water potential data, that--exactly as you said, that there's going to be very--capillary imbibition of water is going to be a very--have very small--

DR. LANGMUIR: This, I would presume, is consistent with the observation of the 50-year old bomb pulse is a shot right straight down through the system. You haven't lost it by imbibition in the matrix.

MR. KWICKLIS: Right.

DR. LANGMUIR: And you couldn't otherwise get it, right?

MR. KWICKLIS: That appears to be the case.

DR. LANGMUIR: Another question on your last overhead. It sounds to me like you've gone a long way, at least towards preliminary integration using the different techniques you've chosen. How much water is going down fractures, and how much water is going through matrix, based on the thermal test work you've done and the other measurements you've made? It's suggested that Pagany Wash is getting a heck of a lot of the water, getting 99-something per cent of any water nearby it, and you're getting infiltration rates of 10 to 20 millimeters a year there. And the other infiltration rates you cite in the next to the last bullet are tiny fractions of that going through matrix and other fractures towards perched water.

Are you saying, then, that the bulk of the infiltration through the mountain is going down the washes, and the very small amounts are going down elsewhere?

MR. KWICKLIS: I'm not in a position to say that. I don't know if--

DR. LANGMUIR: Is Alan Flint going to say that? Is that--

MR. KWICKLIS: I think Alan's going to say something.

MR. FLINT: I'm going to say just the opposite.

MR. KWICKLIS: Just the opposite. We have all the bases covered.

Again, I'm not sure whether this is unique, this estimate is, you know, related to the fault properties, you know, caused by the fault and the fracture PTn at UZ-4 and 5, whether we're measuring different time scales with these different kind of measurements, whether these temperature measurements are giving you some kind of long-term integrated average behavior for the wash, and, you know, what's being measured at the surfaces is maybe more relevant to the past 100 years. I'm not sure exactly why the numbers disagree to the extent that they do, but my own feeling is that we may be looking at the effects of a temporal averaging in the case of the percolation rates inferred from the--

DR. DOMENICO: Ike Winograd has a question.

MR. WINOGRAD: Ed, on your UZ-14 cross section, Figure 4.3.2-1, at this site, is the Calico Hill massively zeolitized. Is that why you have to squeeze to get pore waters?

MR. KWICKLIS: It is zeolitized at UZ-14.

MR. WINOGRAD: Okay. Then I'm at a loss to understand why would pore waters have C-14 ages up to an order of magnitude younger than the overlying perched water?

MR. KWICKLIS: Yeah, I kind of glossed over an important point. I mean, what we infer based on the age inversion is that there's additional influx of water, perhaps from the Solitario Canyon Fault or associated fault splays that very rapidly gets to the level of the bedded tuff lying above the Calico Hills and flows towards the borehole environment.

Some of the early work that was done at the site at H-1 also indicated that there's a very permeable zone in the upper part of the Prow Pass that has a permeability of about four to five times 10^{-12} meter squared, which is a very permeable rock that may also be contributing water to the UZ-14 borehole environment.

So I think you have to infer some kind of lateral inputs based on this east-west cross section. It's a likely candidate inside the Solitario Canyon Fault and associated fault splays. If I had a north-south cross section, maybe I'd be pointing to the Drill Hole Wash Fault as the likely source of input.

MR. WINOGRAD: What's the permeability of the Calico?

MR. KWICKLIS: It's fairly--I think it's something like 10^{-17} meter squared, versus 10^{-12} meter squared for this zone, and probably 5 times 10^{-13} for that zone. So it's the low permeability material.

The zeolitization is stratigraphically confined once you get to the Prow Pass to the previous vitric zones, the zones that were previously vitric. There are de-vitrified zones, such as the interval I'm pointing to, that were never altered to zeolites and maintained their initially high permeability.

DR. DOMENICO: Cantlon, Board.

DR. CANTLON: Yes. Is there a role for water vapor flux in both the dynamics of the perched water and for the heat flux model?

MR. KWICKLIS: Yeah, we did some very schematic modeling of what kind of condensate reflux one would expect due to reflux of the vapor because of the thermal gradient, as well as conductive air flow sweeping into the mountain and picking up moisture and bringing it to the crest, and then shedding it as it moved down the geothermal gradient. And those flux estimates were very low, on the order of .003 millimeters per year, something like that. So about an order of magnitude less than .002.

DR. DOMENICO: Any further questions from the Board, the staff or the consultants?

Thank you very much.

And lastly, then, Alan Flint is going to talk to us about present-day climate and infiltration.

MR. FLINT: To speed things up, I'll skip right past the introductory slide.

I do have, for those of you interested in following, there is an outline that you can just take out and keep it aside so you know where we are all the time. What I'm going to point out is I'm going to give a little historical perspective of some convert variability information, the mechanisms of infiltration, then how we distribute that infiltration specially, how we distribute it in time and some ideas on modeling infiltration under future climate scenarios.

The objective of the work that we're doing now is to convert climatic variables of precipitation and air temperature into infiltration. So if we get that information, we can make an infiltration map.

From a historical perspective, there are several ways in which one can estimate infiltration and have done that. There are transfer equations based on variables like infiltration. You can use geochemistry information. You can estimate discharge or look at water balance and soil physics techniques.

I have a slide. I won't spend too much time on it, but these are some historical estimates that have been made from Gene Rush's work in 1970 for an area near Jackass Flats to the father of the unsaturated zone in Ash Meadows, Sedan Crater. But if you look at some of these numbers, the two in particular ones, if you'll look at Winograd's number and if you'll look at Nichols' number of .04 and two millimeters, you'll see those a little bit later.

But this is just some idea of how some of the estimates were made. I'll also point out some of the Lichty & McKinley work, too, that we use later on.

DR. LANGMUIR: But they're from all--so which ones are relevant to Yucca Mountain?

MR. FLINT: These are all relevant to the Yucca Mountain area.

DR. LANGMUIR: All of them are?

MR. FLINT: All of these. All of these are relevant to the Yucca Mountain area, and I'll show you--

DR. LANGMUIR: The 320, the bottom one?

MR. FLINT: Specifically, the Jackass Flats is nearby. This particular one by Scott and others was the first estimate, I think, for Yucca Mountain directly. And, of course, Montazer & Wilson, an estimate; Czarnecki's estimate all based on sort of a Maxey Eakin type technique, which I'll talk about. Nichols' nearby. The Flint & Flint was specifically Yucca Mountain. Fabryka-Martin was specific to Yucca Mountain.

Okay. I should have done the detail like I avoided the first time.

I want to talk a little bit about climate variability, the climate, and in particular the El Ninos, are they anomalous or are they typical? El Ninos are very important for infiltration at Yucca Mountain.

This is the region that we're doing our analysis in for regional climate. The area you see here in red is the area of the groundwater flow modeling that's being done, the three-dimensional groundwater flow modeling.

And a lot of these stations you see are where we are collecting data from and have some pretty good historical information.

Currently, this is what the climate in terms of precipitation in general looks like. This is a creaking map. It's not as detailed around the site, but it shows more rainfall to the northeast of the site. Although equal in elevation to the northwest of the site, there's less rainfall, and that's due to the rain shadow effects in the Sierra Nevada Mountains, very clearly presented here with a lot less rainfall down in the south. You can see the Spring Mountains and the Sheep Range, but I'll show you more details in this area. But this is a generalized view of precipitation.

And I want to show a little bit about probabilities in terms of rainfall that caused this to be the way it is. We see about a 20 per cent probability of rain on any given day for the northeast. For the south, we're looking at about a 4 per cent chance of rain. It's not that the storms are particularly bigger, it's just that they have more of them. A lot of this is due to clouds that are moving at a certain elevation, and higher elevations intersect those clouds.

But we use these probabilities, and we have to start considering when we look at future climate scenarios, does the amount of rain change or does the probability of rain change, or both?

Here is an example of average annual precipitation for 1993 in terms of a percentage of the rainfall, and I'll just put this back up. This is actually the amount of rain we're looking at. This is for an El Nino year, and what this points out to us fairly clearly is that in an El Nino year, you have an increase in precipitation to the south, over two-and-a-half times, 260 per cent. You have zero, or actually a deficit to the north. This is for 1993. 1992 was the same way. Starley Thompson is going to show some data that he has from this area, which in their modeling would give the same kind of information.

What we're seeing is an increase in the probability of precipitation during an El Nino year. We've just had more storms. We did have more intensity of storms in the south. But this also points out very clearly, when we talk about climate change, we have to be very careful when we say the rainfall is going to double. You have to ask the question where. In the Spring Mountains, in the Sheep Range, in the valleys? You could double--I mean, what does it take to double it here? We're looking at 40 millimeters of rain. So you get 100 millimeters, or 150 millimeters, you've made a great difference, but no more recharge. So it's important to know that there is a distribution.

Is this typical for an El Nino year? This is a graph that shows the water precipitation versus water year going back to 1940. The 13 NTS stations, the most reasonable for representing what we say Yucca Mountain. The El Nino years, you can see by their peaks. Also, these two years, '92 and '93 were El Nino years; '95 was an El Nino year.

And then looking at other data from farther away from the site, we can see that there's a correlation between these peaks, so we would assume that this one, this one, this one, this one definitely was an El Nino year in '41. So El Nino years come in about every six years, and they are fairly significant.

When you start looking at bomb pulse kinds of phenomenon, if you look at 1963, the peak of the tritium, we really don't see a good bomb pulse probably until 1968 or 1969. And these events are most likely what I think gets infiltration pulses moving which might move the bomb pulse signal through the system.

So these are not unique. They happen quite often, and we have to account for those.

Okay. That's sort of just a general overview, and now I'm going to talk about infiltration.

The mechanisms of infiltration are fairly straightforward. When we build our conceptual model, we have to consider precipitation, runoff, infiltration, evapotranspiration, redistribution. The initiation of fracture flow is a very important part, and I'm going to spend some time on that. But we do have some hydrographs, some neutron holes data. I won't show too much of that, but I will show some.

This is an example of a borehole. I think the Board has colored pictures. Everybody else has black and white. But this shows--and you have to take a little bit of time to study this, but these are very, very important to us in our analysis. This is depth from the surface to the bottom of the borehole. In this case, it's 12 1/2 meters. This is what LOTUS puts out in terms of time, but basically it's days from--this is about 1984 to 1995.

The colors represent water content, so you're looking at sort of space time relational information.

To point this out, if we look at a particular time in '84, 1985, we can see changes in '86, '87, and if you look in here, you see it stays fairly dry. That's the drought of '88 and '89. Not significant infiltration in this particular borehole. This is in a wash.

You can see the pulse moving down and over in time. What we're seeing when we see it go down and over in time is we're looking at the infiltration wetting front moving over about six months worth of time. But you also notice that after awhile, it just disappears. It's evaporated. It's being used by the plants.

If you look here, you'll see this is the '92 and '93 El Nino events, no runoff of any significance, '94, but 1995, a very significant runoff event from that El Nino year; one of the wettest winters we've had, actually the wettest winter we've had on record. And you can see that we've gotten water to go all the way down to the bedrock. This is the Yucca Mountain member right in here, and this is probably a small bedded unit, and there's no Tiva over this particular hole.

If you look at the sequence, it's kind of interesting to note that when we drilled the hole, it looked very similar to that. That's because in 1984 before the hole was drilled, there was a runoff event. So we can see an '84 El Nino event and the '95 one, that these two were not as significant as this, although they were El Nino.

So how long did it take to dry this system out to it looks like it's not changing? It took maybe five or six years. So this hole will stay wet for another five or six years, most likely.

So in this particular case, the shallow--or the deep alluvium has made a significant deterrent in keeping this wetting front from reaching the bedrock interface and getting down below the root zone, and this water is used up by evapotranspiration.

So that's one of the reasons thick soils are not as significant, unless you have a runoff event.

This is a very important slide in terms of understanding our conceptual model of infiltration at Yucca Mountain. This is from a Borehole N63, and Pagany Wash is on a side slope. It has about two meters of alluvium. What we're seeing is the 1992 event in which water infiltrated down and perched itself under unsaturated conditions at the top alluvium interface. And for a long time, even though you can see it stays wet, that water stayed there, and it was persistent for probably six to eight months.

At another time, it happened again, and this was the first storm and then the second storm in '95. But you can also start to see in here that we're looking at changes in saturation. Water is moving into the bedrock. We don't know how much is in fractures. That's probably bypassed the system, but we can see that the matrix is actually changing, so we get a signature. It's the duration that this water stays in contact. It's saturation water potential that tells us how water is going to flow into the fracture rock below.

If we knew the fracture properties and how long it stayed wet, we could make an estimate of infiltration, and that's just what we do.

There are some problems with neutron hole data and it causes some of the high fluxes that you hear about sometimes, and we're trying to eliminate that problem, but this is one of the example.

If you have this mechanism which is true, you get a ponded condition at interface, water goes down the fractures. Put a borehole in there, put a lot of fracturing because of the borehole, especially if you leave a big gap around it, pond it up because you have a half of meter of soil, all the water goes down the borehole. And you can see that we can get flux down to 12 meters in about a day. So that's a fairly high flux rate, and I don't know how long it would take to get to the water table that way, but I imagine it's pretty short.

So we have these high numbers, two or three times the rainfall rate, because we're draining a large area. So you have to be very careful with some of the flux calculations when making neutron holes because this is a serious problem, and we're working real hard to get away from it by doing some modeling and some further analysis on the borehole data.

Intermediate scale, and this is kind of another--I think the last of these graphs. We see an infiltration event, and this is in 1994. Maybe it moved fairly fast down through here, but you can see the long duration that it takes to move through this bedrock. And this is over two years, but the wetting front has penetrated down another--well, from this point, probably another five meters. So it moved five meters in two years. You can make calculations from that.

Two more major events and further penetration. I don't know how much leakage there was around the casing. In this particular case, it was in a channel, and it had about 7/10 of a meter of alluvium. So it was very, very wet.

If we add up the amount of water that's in the borehole in the top alluvium interface right here, below that we go about two meters, and then we add the water that's left. How much water came into the system? And that's how we make the calculation of flux, one of the ways we get a point measurement. That's what this graph is. It simply says the volumetric water content, which are the red squares, average through the borehole.

So at 12 per cent water content, we're just moving along, and then all of a sudden it goes up to about 14 per cent. Take that, multiply it by the depth of the borehole, and you get a flux estimate. In this case--and I use different kind of filters, but in this case we're looking at about 200 millimeters of water went into that borehole. In another case we had 300 millimeters of water going to the borehole, and then the third one was another 200 millimeters. So there's 500 millimeters in one year, nothing in the other year, 200 the year before that. So maybe we're averaging 200 millimeters, 300 millimeters a year in this channel, shallow soil. Maybe it's some leakage, maybe it's not, but these are the way we calculate some of the numbers.

This particular borehole was eliminated from our flux model because of the high flux and the leakage, or what we thought was leakage.

If I look in the absence of a borehole at a similar location, I want to provide some additional data. This is looking at water potential with time, and these instruments were put out about a week before the first rainstorm in 1995. Very fortuitous for us. We established some water potentials in the soils. It had been waiting, but not very much, of around somewhere between a tenth of a Bar and a Bar. And these are for different depths, and there are some temperature fluctuations, and the calibration has some problems at the high end.

But basically, if we look at this particular graph that shows at about 40 centimeters deep, that's the purple line, and that's at the top alluvium interface, the system is saturated.

In fact, if we look at 35 centimeters for a short period of time, although you can't see this clearly, the system was saturated, which means we had about 30 centimeters of standing water sitting on the fractures in this particular location.

If you see how fast this rises, which means the water potential is changing, if you were to calculate how much volume of water that is, you would see that that's higher than the evaporation rate, which is what I did. I added up all the water in the profile and came up with a change in water content.

So the red data is the total amount of water in that profile using the water retention curve in those water potentials. I put some green diamonds on here so I could make calculations between these points, and those are the final blue values, which is the change in water content in millimeters per day.

So I'm looking at the early time here, somewhere around six to eight millimeters a day of water is leaving that system. It's a flat soil. It's not in the channel, and it persisted for--actually, it persisted for about 30 days. The total amount of water that went away from that system is at least 150 millimeters. So that's a high-flux value, no borehole.

The evaporation rate is down in this range, so now we're looking at water that is probably just being removed by evapotranspiration. This is definitely above the evaporation rate. It's probably draining into the fracture. So if you go up there and look at this system, you'll see that there are quite a few fractures.

So the potential for getting 150 millimeters under shallow soil is very high. How often those El Nino events occur, how often you get that kind of rain is what brings your averages down, and there's some significant things about the fracture densities that we have here that we have to talk about.

So we have a way to make these calculations using the neutron holes, but that's at a point. How do we distribute that in space?

Well, there are several ways we can make estimates of spatial distribution of infiltration. We tried to look at it from a regional perspective first to get an idea of where we were at Yucca Mountain. And we used the Maxey Eakin technique, which are just simply what we used to distribute infiltration spatially. It's a dynamic static system. It's really static in that you use average annual precipitation for a long record, but if you were to change that for a future climate, you could use the technique.

So we need a rainfall map, first of all, because that's what we're going to use as our correlation. This is probably the best rainfall map we have of average annual precipitation for this region. We put quite a bit of effort into this, quite a bit of work into this based on a lot of stations we have, and it's the best record we can come up with.

You can see where Yucca Mountain is in here, looking at about 160 millimeters a year right in the middle of Yucca Mountain and higher up in the mesas.

You take this map, and you have a model of how you rate infiltration--or rainfall to infiltration. That's what this model is.

This is the Maxey Eakin model, modified by myself, and these points are the relationship that they developed between precipitation and recharge in a large area.

You can see the work by Lichty and McKinley. Here's one of their water sheds and the rainfall amount, and here's another one.

We have modified a model through this more current data, plus the older data of Maxey Eakin, and have developed a model so we can estimate the distribution of recharge with precipitation.

Looking at a little closer detail, we can see--and this is just going up from 100 to 250 millimeters, this is the first part of that curve, the two different models, and these are some local estimates of infiltration. Some of this was done by EPRI, Austin Long, Stewart Childs. Here's a

Nichols' site, one of our data point calibration sites, some stuff that Lorrie and I did. And so we have some support for these numbers based on some current models that are being generated.

You put the rainfall model then on it. Here's the rainfall distribution for the area, and now here's recharge estimate, for regional estimate of recharge.

We think this is a good starting point for saturated zone modeling. An interesting thing is right about in here is where Eakin did his work up around Sudan Crater, and we get a number of around two millimeters a year based on the rainfall for a large scale. The Beatty site is out in here, and on a large scale, you're somewhere less than a millimeter to .1 millimeters. It's a little higher than what was estimated there, but fairly consistent with this.

And then, again, high values in the Spring Mountains, Yucca Mountain. For a regional basis, we're looking at about two millimeters a year--from a regional scale. That doesn't mean that it's averaged everywhere two. It could be 20 on the rich tops and zero everywhere else, and that's how you could get the two.

But this is one way to distribute the information spatially, and this is for a large scale analysis because the Maxey Eakin technique is a large scale analysis.

Another way to do it is the flux map approach, and we've used the flux map approach. That's a static approach. It's just your best guess at a current point in time, and it's difficult to make a change daily or seasonally. You can do it on properties, in situ conditions and soil physics, or you can use the statistical distribution.

We've done two of those. The first one we did using physics, most of you probably have seen, that was the Flint and Flint flux map that we had in high level waste a couple of years ago, and it was just based on looking at the physical properties of the bedrock, what the water contents were assuming integrated calculating flux.

And so we have the high numbers in here. This is--actually, it's not appropriate now, but it's where Ed was talking about where we had the high flux in Pagany Wash. But this is one way that you can get at the problem. This shows us that infiltration is spatially distributed.

The second flux map was the one by Hudson and myself. That's what this diagram is as best we could estimate, and this is static, and that is the last several years in time, three El Nino years, very significant events causing some of these higher fluxes. Some problems with the borehole still exist in here, but this is the scale we're looking at, and somewhere around 20, 30 millimeters on the crest, and then we get down to lower numbers. On average, this is probably around seven or eight millimeters a year, higher than we would estimate for a region based on the Maxey Eakin technique, which you can see the channels showing up, and channels do have significance in this model.

What we did is we looked at everything that we could correlate to the neutron hole fluxes, and then we spatially distributed those using a GIS system, and then used this large equation and calculated this value.

One of the things that we could do with this that was interesting to us was to see if this would help us with fast pathways. This is sort of an aside, but if you look at the--and we used the Scott and Bonk map. If you look at the Scott and Bonk map of faults, if you can see there, and what we did is we overlaid the Scott and Bonk fault map on top of the flux map. And we said, well, wherever the flux is higher, we're going to call that a fast pathway because the fault likely penetrates the PTn and opens up the system.

And then this is the map that we end up with. You can see that quite a few of the faults disappear, and those that are under very thick alluvium disappear because there was no infiltration in the alluvium, so there was no infiltration in the fault. And the red ones are more than 20 millimeters a year because they're in the high flux zone, and the green ones are 10 to 20 millimeters a year because they're in a lower flux zones.

But you can see and identify some of these pathways that would be considered fast pathways because we can get high fluxes in them, they would go through the PTn.

So this was one additional thing that we had put into this analysis, was trying to identify these fast pathways. But you can see that most of the fast ones are up to the northwest over the first water body and the steep grading.

Okay. Now, another way to distribute infiltration spatially is to use a numerical model, and that's a very dynamic approach because you can do it on a daily basis. We use a simplified bucket model, and we use a more complex Richards equation model. I'm going to show the Bucket model right now.

To do this calculation is we use daily precipitation. It can be real data, stochastic simulation. You can use a climate scenario. We do hourly evapotranspiration. We include a solar radiation model, which takes into account the physics of the site; slope, aspect, elevation, blocking ridges. We also use the Priestley-Taylor Equation that we've calibrated and a soil water limiting function and a root function.

For water storage terms, so I can explain how this model works, what we basically do is we look at field capacity of the soil. We have a residual water content, soil thickness and this Bucket overflow term. If we exceed field capacity, the water drains out. If there's bedrock underneath it, it hits the bedrock, and it sits there, and we take it out by evaporation or we let it go in by gravity drainage. And the drainage is dependent upon the permeability of the underlying matrix, the underlying fractures, the fracture density, the fracture properties and whether they're open fractures or filled fractures. So it's fairly simple. It's a very simple approach.

And in looking at how it works, if we make a calculation of net infiltration using this approach, and we used actually a 100-year stochastic rainfall simulation, which I'll talk about later, we can see the net infiltration versus precipitation, and these purple dots are fluxes we estimated in the neutron holes. We think those fluxes are a little high because of the problem with leakage around the casing.

If I took those same locations and put them in the Bucket model and ran the model, I get this same kind of pattern increasing and not too bad on most of the neutron holes, a little bit lower than that.

And then the green is the Maxey Eakin model. You can see we have higher fluxes, but again, the Maxey Eakin model does not apply to point measurements. It's a larger scale.

Well, one of the things this shows, which is important on itself, is that there are years in which you can have a high infiltration rate with a low precipitation. If you were to follow this along, you could see that you could have 300 millimeters of rain and have less infiltration than if you had 150. It depends on how the rain comes. That's why you can't just look at average annual precipitation. You have to look at how that rain comes about.

Well, to put this together, we just took the information we had in a spatial scale. An example of that is a field capacity map. What's the storage capacity of the soil? And an interesting thing to note, 40-mile wash, you can see very low field capacity; young, sandy soils. They can't hold a lot of water, so they're going to drain quickly.

If we get to the site, you can see a little bit higher we find some of these older, more developed soils with a lot of clay in them. We have high field capacities.

So we have a field capacity distribution now for our model. We can also take depth to bedrock model, which we've developed, and these are in classes. So we're looking at zero to half a meter. That's in here. Then a half a meter to three meters, three meters to six meters, and greater than six meters. Anything deeper than three to four meters has little influence on infiltration. After it gets past that, it's pretty much infiltration.

So we take this depth to bedrock map, our field capacity map, we multiply them together, and we can come up with a storage map. So here's how much water I can store. So if I get a meter of water in my profile out here, I can store it. It's going to slowly drain because field capacity isn't the best term to use, but it's a reasonable term. I can hold a lot of water here. I cannot hold as much in the channels. Here I can't hold anything, right on top of Yucca Mountain, because there's no soil there, so where's the water going to go? It's going to go sit on top of the bedrock. What's it going to do when it sits on top of the bedrock? It's going to go into the fractures, or it's going to sit there and wait for the plants to take it out. They can take it out at a couple millimeters a day at best in the rock; on the other hand, where the underlying soil can take it out at some different value.

So now I have a model of rock permeability with fractures and the fracture properties. If we look at this on a large scale, we don't see a lot because these--if you get below six meters, what do you have out there? More soil. What's the permeability of that? Really high. So I say once it gets below six meters, it keeps going, there's nothing to stop it.

But if we look in detail around the repository area, this is the kind of thing we would see, that highlights the fact that we have different geologic formations as we go through the units because of the weathering sequence. There are some areas that have low permeabilities. The water doesn't go in very fast. So if it's sitting there ponded, it's being evaporated at a millimeter and a half, two millimeters a day, and it's going in at .06 millimeters a day.

But there are other areas where the infiltration rates are very high because there are vapor-phase corrosion zones in the top of the Tiva where they have high-fracture densities, and water can go in quite quickly. So we have areas out there where if you have ponded conditions, it's going to go in at four millimeters a day. So you're going to get a lot of flux down into the fractures, and the roots only go about a meter, so they're not going to get most of that--a lot of that water out.

DR. LANGMUIR: What's the white area?

MR. FLINT: The white area is anything greater than five millimeters, and that's mostly just the soils. So it's deeper soils, is what that amounts to.

For sort of a reference, ESF kind of goes I think right through here and down through this way and then back here. And then if you wanted to drill an additional borehole, you know, these are the areas you can see, the large transition and ability to take on water, and that becomes important later on.

So you put all that together, run your Bucket model for however long you want to run it, 100 years or 1 year or 15 years. With current data, you get something that looks like this. So this is what develops in terms of a flux map.

And we can see the high values of flux to the north. Up in here we have another area that's highly fractured. We can see--I sort of made the channels work, but not the way I want to. But you can see some of the major channel features, and you can also see areas where we don't have much infiltration in.

And so this is a specially distributed flux map. This number--this map, I could make one a day. People complain that I make too many flux maps and why don't I just get one that's right, and I just have to know what day they want it on.

If we look in detail at this, and then point out a few things, and this is something that I'm sure everybody will probably understand when they get to get my conclusions, they're going to see infiltration is spatially and temporally variable. And if you look at Ed Kwicklis' two points of measurement, what you'll see is Ed's measurements are actually up in here, in the wash itself, although my GIS system wasn't working quite right, so I lost some information.

But you can see that we have high infiltration in the channel and low infiltration on the side slopes. We're looking at about five millimeters, where Ed in his measuring using--about five millimeters. And we're looking probably at 20 or 30 millimeters in the channel where he said he had about 10 or 20 millimeters a year.

So we were in fairly good agreement in here, but you also have to look at where this is in relation to everything else at Yucca Mountain. Where Ed had the low values is up in this area right in here, where I actually have fairly low values. But we have to consider that that's alluvial material, and we don't know the extent of the recharge area.

If we simply go up to the side slopes a little bit, and particularly on this side slope, we're going to get a lot of infiltration in there. It's going to be through the fractures, and that water may be significant to that perched water body, but it is not underneath the channel where we took our borehole data. We have to be very careful when we analyze borehole data that was taken in the bottom of a channel, a fairly thick channel, in applying that to other places.

So here's two points that Ed measured, and then here's what the rest of it might look like. We're not inconsistent in two locations, but it is important to note that there are some areas of high infiltration rate along this ridge.

And again, this is an area where I think that we-- if we were going to put some boreholes in through the Tiva to look for these high flux zones, do some temperature measurements, that's where we'd look.

Of course, if we look at a typical year, not the high rainfall year of 205 millimeters, this is what we see. So we're looking at infiltration rates on the order of less than five millimeters for this particular year. No runoff in the channels and--but still, where we see the steep gradient, we have still fairly infiltration rates. There may be a relationship between the high infiltration rates because of the rock type, the thinness of the soils, the high rainfall rate, and we also have some areas up in here that are high. So it is variable in space and in time.

Well, how do we distribute it in time? Well, one way, we can simply use the 10 years of site data that we have from the neutron problems to look at 10 years. We could use the 50-year regional precipitation data. That's another way to do it, or stochastic models. We can match some of our data and use stochastic models to make 100 year or 1,500-year simulations, that we can look at how current climate and what kind of sequences might occur, to whether or not these are typical for Yucca Mountain of the 10 years that we have of record.

Well, I won't go through and explain all of this, but I will just point out one or two graphs.

This is from a Markov chain analysis for a stochastic model, and it's a probability analysis of having it rain. For instance, let's look at the red square. It says that if you have three days of no rain, based on the record, and this goes back to about 1940, what is the probability that it will rain on the next day? Three days with no rain. And that number is down here, less than 10 per cent in all cases.

If you had two days without rain and then it rained, what is the probability that it will rain the next day? You can see that it goes way up over 50 per cent. That is if it rained today, it's probably going to rain tomorrow in January.

Now, here in June, it says, well, there's a 25 per cent chance if it did that.

And you could continue on through this analysis. You know, what are the chances it's going to rain--if you have three days of rain in a row, that it's going to rain on the fourth day? In June it never happened. Back to 1940, it never rained four days in a row.

And you can add all these up and look at probabilities because when you try to continue on the probability of any one of these occurring, you have to add all the--or multiply all the previous probabilities, so it gets really low.

But we can develop this model now that we have and have a realistic representation of the winter precipitation storm, following storm. Those kind of events are very important. So now we can predict whether or not it's going to rain.

The second thing we have to do is we have to predict whether or not we're going to get how much rain, or how much rain we're going to get. And so this is a probability once it rains, how much do we get? For instance, January, February and March, there was a 20 per cent chance that it was going to be less than a millimeter of rain. There is an 80 per cent chance it's going to be bigger than a millimeter, and a 20 per cent chance that it's going to be bigger than 10 millimeters. So we get--most of our storms in the winter are between 1 and 10 millimeters on an individual day.

And we look at different seasonality, and we can see the probability of rainfall. It's hard to see right down in here, but if you look at July, August and September, although they're low probability, they also have the highest volume rain. They don't have many big storms in the summer. In fact, we don't, which is surprising. But when we do get a big one, it can be where we can get the biggest ones. And

I think that's--the low probability is important.

Well, we can now make a stochastic model, and there are some problems I have with this because I don't say what the probability of rainstorm size, given that it rained the day before and didn't rain yesterday and, you know, today. I just do it for one day at a time.

And here's what 100 years stochastic simulation looks like. And this is just looking at the total yearly average with a mean model of about 170 millimeters. You can see there's quite a bit of variability. It's a log normal distributed data set with higher probabilities. The tails go out further toward the wet end than they do on the dry end.

If we take this stochastic rainfall model, then, and plug it into our Bucket model, what do we see? For infiltration, this is based on the 90 neutron hole location. Some years at those 90 neutron holes, we get very little flux. Other years we get fairly high flux, 25, 30 millimeters a year.

Well, the question that we're trying to answer through some of the PA work, some of the work that Andy Wolfsburg is doing, or some of the stuff that Bo is doing is looking at some of the cycling. And in particular, Andy is spending a lot of time looking at these on a daily basis to see whether or not using the mean value, we'll get water into the Topopah at the same time as if he uses these big jumps.

And he'll talk about that a little bit later.

But these are the kinds of simulations that we can get, long periods of no climate and then big pulses.

One of the next questions is whether or not we can look at future climate scenarios, rather than just simply using this current stochastic model.

One of the ways that we want to try to do this is first of all, evaluate the infiltration response to determine what influences infiltration; precipitation, event frequency, duration, seasonality, air temperature, cloudiness, that kind of thing.

And in particular, this is important because if you're going to do a climate model, you can't just say you doubled the rainfall. I could put--in my model now, I could have it rain a millimeter a day, 365 millimeters and have zero net infiltration. So it's not--it's the distribution and how you do it. That becomes very important.

We could use past climate records to try to do some scenarios, using specmap, using the Devils Hole data, or using Grid. Grid is the high-frequency stuff that may be real interesting, or we could use the NCAR global climate model with the MM4 model, and Starley will talk a little bit about that, to do some scenarios.

An example, just an example of one that we did and are looking at is this case. This is sort of a regional recharge map for Yucca Mountain. And the way we did this, is actually I used Eakin's data, and then I used the specmap data because the specmap was a little bit longer, and I had something that went to zero, and I could put a tag on it. I used that, but they virtually give the same kind of picture.

What I did is I used the--for instance, climate differences, where it's either very wet or very dry. I assigned the very dry value--here's to a precipitation rate of 170 millimeters a year. I assumed Spaulding's estimate of 40 to 60 per cent increase in rainfall for the Eagle-Anna (phonetic) Range, which is near Yucca Mountain, maybe a little higher in elevation, I'm not sure, and increased the average of Yucca Mountain to this value of 225 or 260 millimeters a year. That's a regional number. And then I used the Maxey Eakin model, and I simply said, well, if we had that, how much recharge would we get? And here's our current value, a little bit less than two millimeters a year, and we can see that we go up to 10 millimeters and down and up and down for the last 600,000 years, with the mean value at about five millimeters a year.

This does not say that at a specific location on the crest it's going to be the same number. It's not. You're going to have low infiltration areas and high infiltration areas. But overall, the whole general area, if we believe the Maxey Eakin model, if we believe my assumptions about climate change and all of that, we can make this model.

If you put this model in your--the red squares in your top code that Bo's running and ran it, or the blue one, and you didn't get any difference, then we have to ask how significant is the fluctuations in climate change, or how detailed does the model handle them? I think both cases have to be considered because sometimes I think it's the way we do the model that controls whether or not we open up. Under these conditions, some of those big, giant fractures could be opened up and flowing versus this condition.

So anybody what to guess what my first summary conclusion is? Any guesses? Infiltration is temporally and spatially variable. I don't know, I guess I've said it enough. It's controlled by the daily variation in precipitation, the depth of alluvium, the hydrologic properties of the underlying bedrock and the topographic position.

The topography is important, especially to the north where you have these slopes that are facing north that hardly ever see the sun. And if you've ever done any work out there, it gets pretty cold, in the wintertime particularly.

In the development of these scenarios for climate, we have to account for the frequency timing and the spatial distribution. But the most important thing, I think, is that the infiltration modeling, the way we're processing it now, we can convert these climate scenarios if we have a precipitation distribution air temperature.

And I think Starley will talk a little bit about what he has done, but his data is perfect for the work we're doing, and we can take that and turn it into fluxes for whatever scenarios he has developed.

And that was it. I tried to go over with Ed because Ed had 30 slides, so I wanted to have a little less than twice as many, but in the same time--we're in competition, Ed and I. Don't invite us together again.

DR. DOMENICO: Well, do we have any questions from the Board here? Jerry?

DR. COHON: It seems to me that the conceptual model, the approach you're taking using the simulation approach, is very promising, especially given the point that we all got, that you've got to take into account spatial and temporal variability. But there are a couple of assumptions I wanted to pursue and see if you're pursuing them. That's even more important.

Did you consider other representations of precipitation than Markov?

MR. FLINT: As far as doing the simulation, no. Markov analysis was the one that we did to get at least a simulation done that we could work with, but we haven't done that, and we have not done a yearly Markov as a baseline so that we can have three dry years in a row or three wet years in a row. Although when we look at 100-year simulations, that shows up in the record on its own. So it may be that the seasonality of rain on a year-to-year causes long-term drought, but there's nothing in particular that causes the drought because it happens with the yearly data. But, no, we haven't gone beyond that yet.

DR. COHON: Yeah, Markov will produce short-term persistence like that, but I'm struck by the disconnect between the modeling assumptions you made and all that you were telling us about precipitation records early on, the very strong periodicity, about ever six-year effect.

MR. FLINT: And we actually see that in the model. We see this reproduced on El Nino event. In the first 100-year simulation we ran, we had 12 El Nino events, and they were spaced in such a way that we had three in five years, and in reality we had three in four years. So we could lump these together, but then to keep that record, we didn't have any for a long time.

DR. COHON: Okay.

MR. FLINT: And so we have not come up with a way to make the long-term record in Markov because we're only dealing with less than 50 years of data.

DR. COHON: Fine. You know about more about this than I. I'm just aware that there are other models out there, and they're probably worth looking at, given--I mean, even though you have only 50 years of data, there's probably quite a bit more--

MR. FLINT: There are probably more models out there. If that becomes a limiting factor in the analysis, then we can go on to that.

DR. COHON: There are. You know, I imagine the stuff we hear about the paleo things tomorrow will affect this.

Just one other question. To what extent do you see over year effects in your simulation model; that is, the effect of a dry year this year affecting--okay, you've got that.

MR. FLINT: Yeah, we do see that. The model actually takes, if you start off with the initial conditions, whatever you want to assign them, it takes probably three years to get rid of those, and that's not inconsistent with the data that we see because we have low evaporation rates. It gets deeper down. We have a root function that goes down, and we do see three or four-year histories of that condition.

So if we look at it today, we can sort of get a feel for what it might have been the last three years on a long-term average.

The new model that we're doing, and actually it's going along with this, I didn't show, is a more dynamic model than that, and I think more important because if you have three years of dry conditions, a lot of your plants die, a lot of your roots die, and your first big rainfall after that has a lot more infiltration than the next two or three because it takes a while for the plants to recover. But that's in the model. It's a dynamic model, and we're working the environmental plant guys trying to come up with the right parameters. But we are trying to tie several years together because you have to do that in this analysis.

DR. DOMENICO: Don Langmuir, Board?

DR. LANGMUIR: I wanted to get you to talk back a little bit to what's coming out of thee studies at depth. I suppose that Ed's approach, Ed Kwicklis' approach, was to some specific spots and a larger view that you've taken of the whole site.

I'd like to have you, though, since you went down just 15 meters, conjecture at least about what you think is going on deeper than that. Are we looking at--when there's a major change in climate, are we looking at increases in the flow through matrix materials, or are we pretty much restricting these flow to drains down the faults and fractures? This is very critical, obviously, to how much climate is going to change anything underneath the surface, and critical to our analysis of the flow. Any conjecture you can make on that?

MR. FLINT: Well, I think that in looking at some of the things we see in the ESF, some of the measurements we've made, you know, there's a likely that even under the current climate, we see flow probably from El Nino events in some of the smaller, moderate-sized fractures. And if we have an increase in climate change, a wetter climatic condition, we'll have those flowing, will probably open up some of the bigger fractures to flow through the PTn--or through the Tiva.

Now, once we go into the PTn, my guess is, is that these fault zones will likely be pathways, just like they are today, and we're going to increase the saturation of the PTn. I don't know what a 10-millimeter or 20 or 30-millimeter flux will do in terms of the saturation, but it's likely enough to get it wet enough, not fully saturated. I don't think there's any evidence at all that the PTn has ever been fully saturated so that water can pass through it. Once it gets to the top of the Topopah, then most likely it is a fracture flow process.

And I agree a lot with what Ed's argument is about the matrix being saturated to the point where we probably don't have fracture imbibition into the rock matrix, and we probably have more or less a separate system that has some integration, but maybe 5 per cent, 10 per cent integration with time. So the water is going to continue on through the fracture network.

I don't know how much more we can involve the matrix, but we definitely have to involve it in the PTn. It will increase the flow through the PTn. Lateral flow will not be able to explain away all of that water. In fact, as you increase the flux, you're going to have more vertical flow as a percentage, and so you're going to have these high numbers.

There are a whole bunch of zones that Steve pointed out in the top of the Topopah which are very, very significant, and that's these broken zones that exist on what, a 10 or 20-meter scale that will be acting as little drains to get us flowing again into the top of the Topopah. But most likely, the system will be fracture flow throughout, with the faults still contributing, maybe a little bit more than they were before, maybe some lateral flow and maybe some interceptual lateral flow in the faults, but it will be more or less uniformly distributed. I don't want to put everything down just in series of drains.

DR. LANGMUIR: Can we find a block in the repository horizon that will be unaffected by climate change?

MR. FLINT: No, I don't think so. I think that--actually, one of the things that Dennis Williams talked about, and this is something that we want to do, I believe that the precipitation that we saw in '92, '93 and '95 will move through the repository while we're there. It may be going through there today. We're evaporating water at a half a millimeter a day through the PTn. That's, you know, 200 millimeters a year. If we're talking of fluxes of 100 or 10 or 5, we're not going to see that.

And we've talked about maybe having an alcove that we can go in, instrument, lock up and watch for five years and see when this pulse goes through.

There may be zones that are--there are fractures, certainly, that don't contribute as much, and I think Zell will talk a lot about that and so will June. Some fractures may flow more often than others. Some may flow periodically. But I don't see how you would separate--you know, I mean, sure, you're going to have small pockets in small zones like that, but for the most part, I don't think you could find these things. You're going to have--I think you have to live with that.

DR. LANGMUIR: How is the program coming on this idea of putting, as you say, alcoves, and closing off the wall, and being able to get a handle on the infiltration measure which you see at the surface?

MR. FLINT: Well, I mean, yeah, that's--

DR. LANGMUIR: Where are we in doing that?

MR. FLINT: Yeah, that's one of the purposes that I had for trying to get under there and do some of this dry-out study, was to get some of the measurements before they dried out too much; one, to analyze the dry-out, but the other was to see what the water potentials were.

The water potential at the base of the Tiva was one Bar, and two weeks later it was 100 Bars because it had dried out. We put plastic on it after three or four months of drying out, and within a week, it went from 200 Bars to about 40 Bars, and then another week, it went to about 8 Bars. So it's recovering fairly fast.

So that's our first bit of information I talked to Dennis about. We sat in a couple meetings and talked about ways we might do this, and it hasn't progressed beyond the fact that we think it's a good idea and we want to try to incorporate that, but we're trying to put the rest of this data here. But I think there are areas that we need to do that.

We're going to go into the main drift next and put in instruments and put these big sheets of plastic up, just like we did in the alcove, and try to wet these zones back up. And we're going to go to fault zones, where June saw bomb pulse chloride and see what we can see, and then go to areas where we don't see it and see if we can ever detect a difference between the two. Those are long studies that we're just starting to think about.

DR. DOMENICO: Ed Cording, Board?

DR. CORDING: Yeah, those sorts of studies just seem to me to be very important, and I'm glad you're, as you said, moving down into the--from the surface down at depth here to look at those--

MR. FLINT: I'm just following--

DR. CORDING: --following the open areas of space down there.

One item that--you're describing very high infiltration--or higher infiltrations to the west than you are towards the east. Does that mean--if we're going to have relatively lower fluxes down at depth, does that mean that we really have to have a pretty strong lateral flow on the PTn towards the east, you know, if our idea is that most of these drains are the major faults? What do we expect to see down there in most of the footprint of the--of emplacement drift here is. What would you expect to see, or what do we need--what has to happen in terms of all the water you've got coming in once it hits the PTn. You have to get a lot of lateral transfer in order to keep the flux low in the western portions; is that correct?

MR. FLINT: You would have to have lateral flow if you wanted to keep it that way. It may not be that way. You may have more in one location than another.

If we looked at map like this, where we see the infiltration rate, it's probably high on this side and lower down here, this would probably be persistent. In some of the work that Bo has done, it looks like it's persistent in his models through to the water table. We won't see evidence for it, I don't think, in the Topopah. If you were to go in the Topopah and do an east-west drift, you'd never see that because it only goes through the fractures, and we're not going to capture that. The matrix is probably not going to be any more saturated.

If you wanted to see whether or not these high fluxes, which may be very significant, are there, you need to go vertically through the top of the PTn and into the Calico Hills and do core analysis, do a temperature profile analysis and the kind of calculations that Ed made, and then do borehole instrumentation to look at what the water potentials are. That's the way you're going to be able to see that, but you won't see it in the fractures.

If it exists and we don't see lateral flow--and we have a study that we proposed for next year that's looking very specifically at the PTn for lateral flow and lateral redistribution, to try to answer part of the question, But if we don't see that lateral flow, and under higher fluxes, we probably won't see as much of it, then this is the way the recharge would probably occur. Fairly low numbers in essence for this area, but still higher on one side than on the other side.

DR. DOMENICO: Tom Wigley has a question, I believe.

MR. WIGLEY: Yeah, two questions with regard to the stochastic simulation, and these are standard problems I'm sure you know about, and I'd just like you to comment.

The first is that in modeling infiltration, it's not only determined by precipitation, but also by other variables, and you mentioned cloudiness and temperature and so on, related to evapotranspiration.

Well, you have to use some correlation method so you've got significant relationships between the different variables. So do you use the standards that are richest in approach for that, or what do you do? That's my first question. You know, how do you account for any correlation between the climate variables?

The second question is that the method you're using is for a single site, but if you're going to produce future climate maps like that, then you have to account for spatial correlation between--from site to site for precipitation variability on a day-to-day basis, and I wonder how you plan to do that, or do you plan to do that?

MR. FLINT: Well, the first question was about how we correlate--you said the Richard's Equation. Are you talking about property or are you talking about climate?

MR. WIGLEY: The climate. Yeah, I'm just talking about the fact that day-to-day variations in temperature and cloudiness, for example, are correlated with each other and with day-to-day variations in precipitation. So how do you account for that in the variable correlation?

MR. FLINT: Okay. First of all, in terms of the rainfall simulation, we do the rainfall simulation straight out by itself. For air temperature, we use a--it's just a model that follows that mean air temperature distribution during the year. We don't have day-to-day variability in air temperature. We're looking at a maximum ET rate right now because we don't have any cloudiness because we haven't added the stochastic generation, but we want to tie the stochastic generation of air temperature with cloudiness.

Now, Lorrie and I did a paper a couple of years ago that showed how you could take air temperature and predict cloudiness. We want to try to use that to look at cloudiness for the long-term record that we have and develop a stochastic model for air temperature, therefore one for cloudiness, and tie that into the rain.

So we are going to--we are trying to do that. That's something we're working with Jack Istock at Oregon State University in doing the stochastic model for the spatially-distributed or for the temporal distribution for rainfall.

Spatially, we're working very hard, and we've done a lot of work on average annual precipitation from a spatial distribution. We have looked at storm-by-storm spatial distribution, too, so that we can take a distribution from a storm data set and look at, you know, a two-kilometer thunderstorm. What we haven't been able to do yet, but we're working hard and hoping to finish by the end of this year, is a combined stochastic simulator that will give us temporal and spacial distribution that is consistent with the temporal and spatial distribution data we have.

Right now, if you were to take your 30-meter Grid over this whole site and do a stochastic simulation on each point, it would range somewhere every single--it would rain in 20 per cent of those squares every day. So it would always rain on your site. So we have to have a way to see the system to make it rain in the summer and then make two-kilometer storms, four, two-kilometer storms, winter storms that are 20 kilometers. We know the spatial correlation of individual storms. We just haven't figured quite the way out to make it happen. But if we could do that, we'll write a paper on it for sure.

DR. DOMENICO: Let me make an announcement before I continue this.

I'm urged to remind you again that there will be a reception between 6:00 and 8:00 in the Oak Room just off the main lobby. There will be a cash bar, but snacks will be provided by Jean Younker, I believe. Did I get that right?

Any further questions from the staff? Leon Reiter?

MR. REITER: There's one thing that is always a little confusing, and maybe it's a simple thing. Very often you plot precipitation versus net infiltration, which I assume is what's happening just below say five meters, or something like that. But then we use the Maxey-Eakin, and you talk about recharge. And recharge, I'm assuming, is something that's happening at great depth at the water table. And is there any mixing up there, or how do you jump from one to the other, and what are the assumptions there?

MR. FLINT: In terms of the net infiltration values, we use that to keep where we are in the system and the calculations and where the model fits. The Maxey-Eakin technique was more definitive towards recharge, and we assume in a sense that net infiltration will become recharge, but we don't assume that the spatial distribution of the net infiltration will be the spatial distribution of the recharge. In fact, we're pretty sure it won't be.

I don't think that if you take these Pagany Wash channels and put 20 millimeters a year in there, it's going to be 20 millimeters a year right underneath that location at the Calico Hills. It most likely is going to be a wider area. How much wider? I don't know the answer to that.

But I am using Maxey Eakin as a recharge estimate because that's the way it was developed in mine as a net infiltration, although I think net infiltration is likely to be, and I think that was Question No. 1 to answer, is whether or not net infiltration--I think what was it I heard somewhere earlier--it's 20 millimeter going at the top, then a miracle happens, and .2 comes out the bottom? And if you put it all down a fault you can do that, but otherwise you're going to have to explain that.

DR. REITER: So essentially what you're saying is when you spatially average, net infiltration is recharge?

MR. FLINT: I think it is because there have not been any credible mechanisms that we know of that can bring five and ten millimeters of water back up from greater than a couple of meters down below the bedrock interface. There may be some that we haven't thought of, but if you look at the thermal pumping, if you look at barometric pumping, we're stuck with a half a millimeter a year at best. So we can't get those numbers out.

DR. REITER: Is there any sort of scaling factor, will you say, that meet a minimum scale of size of an area where you can assume that that equation exists or below a certain scale--

MR. FLINT: Well, for the Maxey Eakin scale, I'm not really sure what the scale of these water sheds are, the kind that might work.

Is Pat McKinley here? What's the scale of the water sheds that you worked on Pat, the two that you did?

MR. MCKINLEY: One was about three square miles. The other one was about a mile and a half.

MR. FLINT: So those are the size. I think if we were looking at something like Yucca Wash, if we were to take the Yucca Wash water shed, that might be the scale that we would think of, or even the Drill Hole Wash water shed is probably --maybe a little small, but maybe not unrealistic.

And if you were to take those flux maps that I showed and take the average over that site, it's three millimeters a year, which is very close to the Maxey Eakin technique. And so that's why I think that technique applies. For the saturated zone, I think it's a great technique for looking at climate change. I think it's one of the best they're going to have.

DR. REITER: But we were hearing some numbers. Maybe it was that the average infiltration over Yucca Mountain was 10 to 20 millimeters per year.

MR. FLINT: It's all a scale question.

DR. REITER: Okay.

MR. FLINT: And that's what this kind of shows. If you're looking at a flux map estimate, you know, an estimate like this is about three millimeters a year. On top of Yucca Mountain, it's 20 millimeters a year. Over the repository area, it's 10 millimeters per year.

So when you hear about infiltration rates, remember it is spatially variable, and it depends on who's telling you and what scale they're thinking about. When Ed gives a number, he's talking about a very, very small confined area. So if you were going to do an analysis, and like I pointed out here, this is the area that is where Ed was talking about, the numbers that were fairly low. And these numbers, if you would average them, probably is less than a millimeter a year., Or in an area like this where we have a channel, you can get high rates, and you're surrounded by low rates. In some areas you can have high rates. So it is an area we have to deal with.

Now, we're dealing--and from a repository perspective, I think--although I was told to put this on here, I forgot to do it. The south end is about here, and the north end is about here, and it goes from this side to this side.

So this area, you can see that there is a lot of spatially-distributed infiltration across there, but I really do think that, one, this model has not been field verified. The assumptions in this model about flow through the fractures have not been field verified. I am convinced that as we increase in elevation, the amount of fracture filling decreases, so that if we got all the way up to Rainier Mesa, there probably is very little fracture filling. But as you go down in here, these fractures are filled, and if we get up to the north--when I did my model up at the north, I had .3 millimeters a day infiltration. When I did the heat dissipation calculation, I came up with eight millimeters a day of flux.

So I have to correct my model. There's likely to be more filtration to the north than I even have in this model, but I need to be able to find a way to test these numbers, and I haven't done that yet.

DR. DOMENICO: Thank you very much, Alan. We have one more task here, and that's to open it up as much as we can for the questions and public comment. And I have a request for Hal Rogers to give us some sort of comment. Is Mr. Rogers in the audience?

MR. ROGERS: My name is Hal Rogers, and I'm co-chairman of the Nevada Nuclear Waste Study Committee. We have about 15,000 members throughout the state of Nevada, the majority, a large majority down in the Las Vegas area, and relatively few in the northern area. I'm co-chairman for the northern area.

We have a couple of comments. I did have a couple of questions, but I got answers to those during the various intermissions; for example, whether or not the study of the disposal container was including the credit for the fuel cladding. And the answer is, no, not yet, but I understand this is going to be looked at.

I might add that the Committee strongly supports renewal of funding for the Nye County and Lincoln County, for example, other counties' survey of what DOE is doing.

I also want to add our emphasis to something that we have spoken of before, and that is that the studies that are being made should concentrate on what is needed to reach a conclusion about Yucca Mountain. That was brought up earlier, and we just want to add our reinforcement to that.

The other comment is a comment about what the Board is doing, but more directly as what they do in their documentation. The annual report to the Congress and the Secretary is obviously well done. I like it. But the average member of the public who might have an interest in what is being done will not understand it. I think this is very obvious. Even the specialized documents that come out every so often are over their heads.

We had a recent example, though, which is the February letter to the Secretary. That is a letter that is receiving rather extensive distribution, and it's being understood. And this is the kind of thing that we want to encourage.

The Technical Review Board has a high standing in the public domain. Unfortunately, DOE does not. They are improving, I think, but they've got a long way to go to overcome some of the things that happened years ago. But the Board is well thought of in general, and I think that documents from the Board that can be easily understood are a very important aspect of what you are doing.

With that, I thank you very much. Yeah, thank you very much.

DR. DOMENICO: Thank you very much, Mr. Rogers, for your comments.

Even though no one else has made an official request, if there's anybody else out there that would like to make a comment, can you please identify yourself and do it?

(No response.)

DR. DOMENICO: We're adjourned until 8:30 in the morning.

(Whereupon, the meeting was adjourned, to reconvene at 8:30 a.m. on Wednesday, July 10, 1996.)