UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING BOARD MEETING
April 21, 1993

Holiday Inn
Reno, Nevada

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman
Nuclear Waste Technical Review Board
Dr. D. Warner North, Chairman, Morning Session
Dr. Patrick A. Domenico, Chairman, Afternoon Session
Dr. Clarence R. Allen, Member
Dr. Garry D. Brewer, Member
Dr. Edward J. Cording, Member
Dr. Donald Langmuir, Member
Dr. John J. McKetta, Member
Dr. Dennis L. Price, Member
Dr. Ellis D. Verink, Member

APPEARANCES

Dr. William D. Barnard, Executive Director, NWTRB
Mr. Dennis Condie, Deputy Executive Director, NWTRB
Dr. Sherwood Chu, Senior Professional Staff, NWTRB
Dr. Leon Reiter, Senior Professional Staff, NWTRB
Dr. Carl DiBella, Senior Professional Staff, NWTRB
Dr. Robert Luce, Senior Professional Staff, NWTRB
Mr. Russell McFarland, Senior Professional Staff, NWTRB
Mr. Frank Randall, Assistant, External Affairs
Ms. Karyn Severson, Congressional Liaison
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

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P R O C E E D I N G S

8:30 a.m.

DR. JOHN CANTLON: Good morning, and welcome to the Nuclear Waste Technical Review Board's spring meeting.

My name is John Cantlon. I'm Chairman of the Board. In my former life, I was Vice President for research and graduate studies at Michigan State University, and my professional background is environmental biology.

Let me introduce the other Board members here. Clarence Allen is professor emeritus of geology and geophysics at the California Institute of Technology. Garry Brewer is professor of resource policy and management and dean of the School of Natural Resources and Environment at the University of Michigan. Ed Cording, professor of civil engineering at the University of Illinois; Patrick Domenico, David B. Harris professor of geology at Texas A&M; Donald Langmuir, professor of geochemistry at the Colorado School of Mines; John McKetta, the Joe C. Walter professor of chemical engineering at the University of Texas at Austin; Warner North is consulting professor of engineering and economic systems at Stanford University, and a principal with Decision Focus, a consulting firm.

Dennis Price is a professor of industrial and systems engineering, and director of the Safety Projects Office at the Virginia Polytechnic Institute and State

University; and Ellis Verink, Distinguished Service Professor of Metallurgical Engineering emeritus at the University of Florida.

Our technical staff is over here on my right, your left, and, as most of you know, the Nuclear Waste Technical Review Board was created by Congress in the 1987 amendment to the Nuclear Waste Policy Act. The Board is charged with providing an unbiased source of expert advice on the technical and scientific dimensions of DOE's work in high-level nuclear waste management.

During the next two days, the Board will be hearing about the Department of Energy's process for resolving difficult issues associated with assessing the suitability of the Yucca Mountain site for a repository for spent nuclear fuel and high-level waste. Such difficult issues are those that are complex, and for which there will be residual uncertainty.

Warner North will chair the first session of presentations on that topic, but first, I'd be pleased to announce that we will be hearing from Lake Barrett, the acting director of the Office of Civilian Radioactive Waste Management, and from Carl Gertz, the project officer of the Yucca Mountain Site Characterization Project Office. Dr. Barrett will present a "Report on the Status of the Waste Management System," and he will be followed by an update on the Yucca Mountain Project by Carl Gertz, so, Lake, it's all yours.

DR. BARRETT: Thank you very much, John. It's a pleasure to be here this morning to try to update you all on things that are going on back in Washington.

As you know, the new administration is taking hold back in Washington, and things are happening at a fairly rapid clip, and what I would like to do this morning is to kind of quickly go through some of the things that are happening, and then throw it open quickly to questions from the Board.

What I'd like to cover is the program priorities on what we're planning to do here for '93 and '94, go over the FY 94 budget request. I'm testifying next week before Congressman Bevill on what we're basically talking about there, and some of the recent Secretarial activities that are presently underway, so let's move along with that.

First of all, the Secretary has the program under active review. We've briefed her several times on various issues. She has a great interest in it, and her immediate staff is also assisting her in that, and I'll mention some of the major players there. Mr. Daniel Dreyfus is an Assistant to the Secretary. He is likely to be appointed to some administration job in the future. I don't know what that is. I would take a guess it would probably have something to do with this program, possibly either the Director or a position above that, but he is a very knowledgeable person in the program and he has been advising her, and Rich Rosenweig is her Chief of Staff. He also has had input to her, as well as her own personal knowledge of the program.

Now, the way she is basically going at it is that she's reviewing the entire program, and she is going to outline in a memo from her to me of April 6th of what's she's going to do, and basically, she's putting things into three different categories: Certain activities that should go on as presently planned--and I'll go into an example of that in a minute--other items and policies and decisions that she's going to make right now, that she will make based on input that she has already received--and she has received a lot of input, and I'll get to that in a moment--and then items and issues that need further more formal reconsideration in a consultation/collaboration process with all the various external parties before things go forward with that, so she is going to be putting together issues along those lines.

Now, she has made some decisions already. One of those is to move ahead with the tunnel at Yucca Mountain. She went into considerable detail on the tunnel, on what our plans were on that, also including the Board's special report and previous reports from the Board concerning the tunnel, as well as she heard from others on that as well. She has made the decision that we could continue with the tunnel. The law is fairly clear that we are to characterize Yucca Mountain. It's a pretty much unanimous scientific opinion that we need to get scientists underground in Yucca Mountain, and the Board's views on that for the last several years clearly were very important to her, so we are proceeding ahead at this point with the tunnel. And she's also directed--and I'll get into that a little bit later--that some of our prime priorities are the scientific evaluation of Yucca Mountain.

Also, she has told us to continue to evaluate the concept of the multi-purpose canister, standardized canisters. I think you all know that that is something that I think pretty much everyone, again, agrees is a valuable thing to study and possibly use in the system. It has advantages for reactor storage, it has advantages in transportation, has advantages in the federal storage system, whatever the federal storage systems may be, and we need to be very careful that it is compatible with the engineered barrier system in the final geologic disposal.

As you well know, those are all complex issues that have to be done with a lot of care as we go through that, but we believe that has great promise for the nation across the board, just even beyond the federal waste management system, going back into the utilities and the storage as we go along.

Some other things that she has made clear to us--

and this goes along with one of your recommendations in your special report--is that we're not to be unnecessarily schedule-driven. When we go into the '94 budget, basically, the 2001 license application date is probably unlikely to be achieved, and I'll go into that a little bit more, but she has made that statement, that high-quality scientific work comes first and is not to be compromised by schedules.

Now, going into a little more of the details in the '94 budget, first of all, I think you've all been kind of following some of the federal budget. It's an austere budget, it's a realistic budget. President Clinton has had a mandate from the people for change, and I think the budget clearly shows that.

The RW program is $380 million in '94. That's a slight increase above the $375 million in '93. That actually is quite good, considering what has happened in many of the other departments on nuclear-related programs. So even though it is not the increase that some would have liked to have had, it is an increase, and it is actually a fairly good budget. It does support scientific investigation of Yucca Mountain. It does not allow a lot of things that we would like to do, and I believe that you would all like us to do, as we have to basically allocate that money in the best way, and you're going to find a lot of different sectors of the program is going to be funding-limited. We are basically funding-limited now that we have access to Yucca Mountain.

But even with the nominally-constant budget, we're going to increase the emphasis on Yucca Mountain's scientific activities. To be able to do that with a nominally-constant budget, we are decreasing the emphasis on the MRS and transportation, and basically bringing that back to a bare minimum. When you do that, you basically shift some funding from the east to the west so we can get some more money on Yucca Mountain.

We're going to continue to try to find ways to do the site characterization work better, smarter, more efficiently. I think some of the views that the Board have will be helpful for us on that; for example, the main topic of trying to resolve some of these issues that you are going to be focusing on here over the next couple days, I think will be helpful and right along that line.

Now, in her initial part of her review, she has made some other decisions besides proceeding with the tunnel at Yucca Mountain. She has directed us to establish a Chief Scientist position for the Yucca Mountain Project. This stemmed along from a letter that we received from folks at Stanford, and I think has pretty much been--again, I have heard no one object to that as I kind of checked around and made the recommendation to her that we do so, which she rapidly embraced.

She has directed us to enter into negotiation with governments on the Payments Equal to Taxes, and we are doing that, and we are discussing that now.

She has also, for the--we call bin 3, you know, issues that need a more collaborative process and an involvement of external groups, to come with a plan to do that. You may be aware that there is a plan underway by some of the state regulatory Public Utility Commission groups with the state, and through their National Association of Utility Regulatory Commissioners, the NARUC group, to do that. They would focus primarily on the front end of the RW system, including storage at reactors. It would be the transportation, multipurpose canisters from reactors. Transportation, the would be the RW part, and also, federal storage at the front end.

I use a very general term, federal storage at the front end. That could include an MRS, that could include interim federal storage. It could also include even the LAG storage part at the front end of a repository, so it's be the federal receiving part.

We would basically integrate our activities in with that. I would expect that we would pick up the back end of that; that is, the repository area, where we could look at repository-related issues. They, states and the public utility regulators, would not look at the back end so much.

We would look at the back end and we could explore such issues in the consultative process; things such as the alternate licensing strategies that I believe many of you are aware of, so some of those issues would be discussed in a mechanism that we are yet to come up with on how to do that, and that's something that we'll be talking to many of the stakeholders about how to set up that process.

She is also clearly aware of the funding situation. As I said, the $380 million is nominally a constant from last year. Now that we have access to Yucca Mountain, we are now funding-limited, really, for the first time, and there is a need for the program to have system-predictable funding levels that are higher than what, historically, for, say, the last five years that we've had in the program.

This gets to be a very complicated federal budgeting process, with deficit reductions and controls, and scoring with OMB, but we have been talking to both houses of Congress, with the OMB, with the utility ratepayer commissioners about the need for us to get into a different budgeting arrangement than what is currently, in FY 93 and FY 94, were in place with the Deficit Control and Deficit Reduction Acts. So I expect that when we finish with this, that over the next year, anyway, that there will be proposals from the administration to try to go to a revolving fund, which would allow us to have predictable, consistent funding levels. It would be greater than what we presently have now.

I think this will be very important to be able to the quality scientific work that she's directing us to do, and I think that you all have been recommending in many of your recommendations, and will probably be discussing here over these next couple of days.

I think I've already mentioned this, but clearly, the front end of the system, she wants that to be discussed by the stakeholder's Board, so that includes the 1998 waste acceptance requirement. Also, in the discussions here, it would include the issue of potential compensation if we are not capable of taking material in 1998, and also, the MRS siting activities, be they from the negotiator or federal siting, or whatever viable siting mechanisms that we can come up with.

In conclusion, what we're going to be focusing on will be scientific work on Yucca Mountain, the primary point there being the ESF facility; multi-purpose canisters. That's basically what Ron Milner and the storage folks will be doing. We still want to continue, even though the budgets have gone down somewhat, in the systems. We want to focus along on this. This is along the recommendations that you've had again, to try to keep our steering and our overall systems, you know, integration and systems analysis.

Again, I can't over-emphasize that we must do the highest quality scientific work. We cannot be jumping ahead because schedules say you must do something by a certain time, and they want a formal process for including all parties and all stakeholders in the program development at the early stages.

Now, as far as numbers in the budget, I wasn't going to really dwell on this unless you would really like to, but the budget breakdown from a programmatic point comes out basically like this: 380 is our total. Our federal staffing will be about the same. Actually, we'll go down by three people, to 245 people, but Yucca Mountain is basically two-thirds of it; nominally, $262 million, the largest; the MRS and fuel management basically at $15 million. Transportation--this includes the MPC part of that, that's the transportation overpath, which is a fairly complex issue, $16 million there; waste acceptance, working with the utilities on the waste acceptance issues, basically constant into '94, $3 million; quality assurance remains basically constant. As we do more scientific work on Yucca Mountain, we must make sure that the quality assurance program stays on top so there's no reductions in quality assurance.

Systems integration and compliance, that has come down substantially as we're getting sort of over the hump on some of our document hierarchy; also, as we kind of shift more toward site suitability, kind of away from license application dates, we can make some changes there. However, you cannot do site suitability totally independent of the licensing. They are very intertwined, so we must do enough licensing repository work that's necessary to support a site suitability determination, and there's a lot of that, and I think that's not as widely understood by many.

Program management, we are going to reduce some here, but there's a lot of work there that's necessary to support Yucca Mountain and that above.

There's a nominal, less than a million dollars on the Civilian Waste R&D. This basically is to continue some of the dry cask work that was done out at Idaho, so that's less than a million dollars, so that's fairly minor.

Now, Carl will be going through and giving you more detail on the Yucca Mountain budgets later on.

That's kind of all I had for a start. Are there any questions from the Board that you might like to talk about? I'm not quite sure how you work this.

DR. CANTLON: Board questions?

(No audible response.)

DR. CANTLON: If not, thank you, Lake.

MR. BARRETT: Okay. Thank you all.

MR. GERTZ: Dr. Cantlon, members of the Board, once again, I appreciate the opportunity to update you on the Yucca Mountain Project. It appears that we're even a little bit ahead of schedule, and that's the way we'd like to keep projects and presentations and everything, so I'll--there's lots to update you on, so I'll take some time, but I'll certainly allow some time at the end for questions, so I'll move pretty quick.

What I intend to do is talk about some things that we're doing, new progress, show you a just very brief video that some of you have seen that were at the mountain. I appreciate the fact that some of you found time in your busy schedule to get out there. Certainly, any time you're moving west, staff or Board members, we'll be glad to take you, Saturday, Sunday, any time you're traveling through the Las Vegas area, please stop and we'll take you out to the mountain.

And I will talk about '94. I certainly will remind you that we have a balanced program; surface-based testing, three drill rigs operating sometimes the last couple of months, getting ready to go underground. We need to solve a national environmental program, as Lake pointed out, and in doing that we have work underway at several locations. I'll talk about and show you some photographs, but our focus over the last several months has been the ESF, getting ready there, and doing the engineering drilling to design the packages that follow on.

We did complete some of our drilling. We completed

UZ-16. I know, Dr. Domenico, you talked to me about the time it took, and it was 186 shifts, I think, to complete UZ-16. We are now at UZ-14, and we hope--we have 200 shifts planned there. Next year, depending on funding, we hope to go to multiple shift operation.

Dr. Flint's, 23 of his 24 boreholes are done, and there's lots of things going on, and I'll show you some photographs of that. In the book, there's even a list of 19 other site characterization activities.

There is our UZ-16. That was just completed. Instrumentation's going in the hole, and the rig is now at UZ-14 and did start the other day. We did some angle drilling, just gathering design information. This was at NRG-2, North Ramp Geologic Hole 2. We will do a two-way. Here are some of our photographs for getting ready.

The sequence of construction, this is earlier in the cut and bringing the pad up to grade. As we got into the high wall, this is putting stabilizing rock bolts in. We filled the cut back up with this ramp so we could do the top half of the tunnel excavation. We're excavating--I'll show you the sequence later--in a method called the Australian, New Australian tunneling technique, and this was an important step for us. It happened at 4:15 p.m. on April 13th, and this was our first blast of clearing out the tunnel. We're now about 25 feet in from the face with our first drift, and

I'll very briefly show you this minute and a half video that captures it. You see the fire on the video, but it's a very short fire. You see the smoke, but you can actually see the fire.

(Whereupon, the aforementioned videotape was shown.)

MR. GERTZ: That's, of course, inserting the explosives. The fire will be right there. You can see some of the rock came all the way out onto the pad. After the explosion, you can see it piled up just like it was supposed to. Then we went in the next day, of course, to start to muck it out and start drilling the next sequence. We've had, I believe--I'm looking at Ted's status from yesterday--we've had six blasts completed so far, and we're about 30 feet in, and I'll just talk to you a little bit more about what that means when I say 30 feet in.

Now you're seeing after the blast. You're starting to see the tunnel being developed. This was last Friday, I believe; yeah, April 16th. This is a view from the Exile Hill, looking the other way. Many of you have walked up to Exile Hill with us and looked over the pad. It's about a ten-acre pad, and that's the equipment being mobilized, storage modules for the equipment, for drilling rigs, and there you can see the ramp and the slot right there.

A little closer view, looking in the start of the tunnel, and as I said, we've emphasized ESF. I talked to you a little bit about the drilling, but the scientists and engineers didn't want to let you forget that there are many other activities, SCP activities that are going on out there right now. I'll just give you the list. They're in the book for you to look at, and I'll talk about a couple of them later on, but if you're out to the site these days, there's lots of activity; vehicles moving around, people working. Any given day, there's as many as 400 people out on Yucca Mountain doing something associated with the site characterization program.

Briefly, I'll remind you of the organization. It hasn't changed. The M&O is taking a much greater role now in the integration; project management, project direction of the program. They also have a role as a performer in some areas, and in the last part of the presentation, I'll talk to you about how we've handled their integration role and their performer role, but that's the groups that are still working at the project, and their areas of responsibility right now.

I don't want to go too far without talking about our oversight organizations. Of course, you are certainly one of them, but there are many others, and this has become a very significant part of our day-to-day operations. It's particularly increasing in two areas right now. The IG, within the Department, has full time, I guess, four reps in the Las Vegas area--right, Max--and six traveling in and off, and another team of six from the Washington area looking at special areas. We understand NARUC is going to be being more aggressive with their oversight. EEI has now badged themselves on site, and they're on site much more frequently. We just had a meeting with them yesterday, so we spend a lot of time of each work week dealing with one, two, three, or many of these organizations, and I think that's consistent with the Secretary's philosophy to managing big projects, is keep the people involved, let them know what you're doing, and, of course, we try to do as much of that as we can, but it does take resources, there's no doubt about it.

It takes time and money and effort, and Max, in part of his cross-reduction studies, is looking at what this all costs, and we think somewhere between $5-10 million we're devoting to this oversight activity support, and that's the meetings and the travel and the preparation of briefing materials, not the science that goes behind it. That's under other categories, but just conducting this type of activity.

It's what the law said, though, and it's not an issue with me. In fact, I think it leads to public trust and confidence, and I'm very supportive of it, but it does take resources, and those resources then get, sometimes, diverted from the scientific program, and you're well aware we have a site characterization plan. It's our plan, but it is not as rigid as many people think.

Just to point out, I got some facts addressing this bottom line; controls in place to revise the plans as needed. In fact, maybe even Dr. Flint might mention about how he's been able to revise his plans.

In '92, we were doing limited work, but we, out at the FOC, processed 73 changes so we could do our work, in '92. In '93, already, we've processed 165 changes, so we're able to process changes, get on with the work, change the plans and do what the scientists or engineers need in a controlled manner. We have to document it and have to be controlled, so the site characterization plan, while it is a plan, it's very flexible and on line. If we change big studies or big activities, we need to run that by the NRC, who has, in effect, endorsed it, and there's a process called the semiannual progress report that does that, so that's our plan and we're carrying it out, but I just wanted to point out that fact, that we have changed what's in place that allow us to be flexible as we do the work, and we have to be that way.

This is an old chart, and I continue to remind my staff and everybody I talk to that what we're doing out there, the site characterization program on this side is driven by regulations. Although 191 is not in place at this time, we have ideas of what it was and what it may be. 10

CFR 60 and 960 are in place, licensing and characterization, and they ask or they set several requirements which create questions--or issues, as we call them--which then we develop programs to answer those issues.

We haven't changed our approach. That's still it. The regulatory requirements create questions, and we develop programs which answer these questions in order to meet the requirements.

I'll now move to the ESF a little bit. This is still our basic design. Right now, though, our implementation plan for this design is a five-mile loop starting here, with very little, if any, interruption in that TBM process as it goes down, across, and up and out. That's our current planning process, and I'll show you the dates on that. Later on, we'll come back and do some more excavation and drifting.

As we do this, there are certain tests that can be conducted in process, and I'll show you those. Calico Hills, of course, is still in our plan. It's down here. We're debating whether that is still the appropriate thing to do, but right now, it's still in our baseline plan.

This week, we had a comprehensive design review being conducted in Las Vegas over Design Package 2, which is the ramp from the starter tunnel down to the repository horizon. I know you had some attendees, your staff, Russ, was there with some other people this week. The counties have been there, the state's been there, so we're conducting a comprehensive design review of Design Package 2. We've got 16 design packages to go, so there's lots of activity.

Dale, how many people were at the design review, total?

MR. FAUST: I didn't see the final list, but it was about 55, I believe.

MR. GERTZ: Okay. So this is another activity that takes time and money, but I believe is necessary in the construct of this program. But, where we are today is just up here, 25 feet in or so.

Design Package 1(a) was what we had designed and what we're implementing, and if you get out to the site, it looks something like this right now, and we're pleased with that. Our goal is still to be 200 feet into the mountain by the end of this fiscal year. September 14th, as a matter of fact, is the date I've given the project staff to try to meet, and that includes 200 from that high wall you saw, and there'll be 120 feet or so of cut and cover tunnel. So, end of '93, you'll be able to walk into a tunnel about 320 feet long or so.

This is the schedule I've started with last October. Some milestones we've met, some we've not met, but we have it up here to show you where we're at. We did start site prep, as we said on the 30th. We've completed lots of site prep activities, and on April 2nd, we started the peripheral holes, and April 13th was our first blast for the 100 feet.

On the TBM, we were late in issuing our RFP. We did receive proposals, and our award date is going to slip to mid-May. We had four proposals. We have some, we think, offers that are more beneficial within those proposals to the government, and as a result, we've extended the time to evaluate that, but we hope to award the contract on May 15th to that TBM supplier.

One thing that's not on here is the underground subcontractor. We have chosen to negotiate with and are being audited by the defense contract contracting agency, Peter Keiwit to be our major underground constructor. Under a REECo sub, he has, as a designed subcontractor, Parsons, Brinkerhoff, Quade & Douglas, so we think we have some of the world's best tunneling expertise on the program, what with Peter Keiwit, what with the M&O's team, which consists of Morrison-Knudsen. We think we've got the market pretty well cornered so that we can carry out the rest of the ESF schedule.

I'd like to point out one thing here, the TBM delivery is to be determined. We're going to try and negotiate dates with them at that time. You saw here, our goal was cut and cover the entry tunnel, and then get 200 feet, and there's a little line here and a small asterisk, and what it says is, we may drill and blast beyond 200 feet for some reasons. Some people believe it'd be wise to drill and blast to the Bow Ridge Fault, about 700 feet, and start the TBM in a little better rock, as opposed to in rock that we think is not quite as competent as we'd like it to be to start a TBM.

The second reason is, is if we delay the TBM, why not keep the crews busy moving forward and gathering scientific information, and moving the program forward? So, right now, we know of no scientific reasons not to continue about 700 feet drill and blast, but we've not come to a final conclusion on that, but that's our plans.

Let me talk to you about our tunneling sequence. We've had trouble trying to put together graphics to describe how we're tunneling, so let me just--and I know this is not that good, either. It's called the Australian Tunneling Technique, and it's a sequence of operations as you move forward. And what you saw, our first blast was just number one here.

It's kind of a rectangle with a little arch at the top, and number one is going to be carried on about 50 feet or so before we do anything else, and then we'll come back and do No. 2, the north slash, and then we carry on No. 1 and

No. 2, and then, after 2 is about 25 feet, we start No. 3. So, at any one time you'll have three faces being drilled and blasted in sequence, with this being the deepest, this trailing it by about 50 feet, and this trailing it by about 25 feet. The advantages of this is you can put in ground support, you can measure your ground conditions in small increments, and it's a more stable way to drill and blast. We will do the first half of it, either about 100 or 200 feet, before we come back and do the bottom half.

Then we come back and do the bottom in the same sequence of three faces; No. 1 here, with 4, 5, and 6, and as a result, that'll get us in until we finish our starter tunnel all in one face, where then we can insert the TBM and work against that one face. So, that's the sequence. That's what we're looking to do. That's our approach. We are working, by the way, around the clock, three shifts on that operation right now.

We know the mission of the ESF. It is to get underground and do in situ testing, both at the Topopah Springs and Calico Hills. Some of the objectives of the test program, because we want to remind everybody, the reason we're doing it is to conduct tests. It'd be otherwise unobtainable. If we can get information both above and below the horizon, and within the horizon, we can study the process and timing for water and gas movements and, of course, the in situ effects. It's a continuously early look at the natural system we can continuously look at as we move through the five-mile loop, and then it has access to special features, be it the Ghost Dance Fault intersections, different hydrologic or mechanical or thermal tests that we want to put in, so that's our objectives, and I guess the reason I wanted to put that up is I wanted to lead to the next one, which said: "Here are some of the tests that we're doing, in effect, right now."

We're mapping as we tunnel for stratigraphic and thermomechanical determinations. We will be monitoring the excavation. Sandia will be doing that. We'll be studying hydrologic properties, we'll be doing sampling for various principal investigators, and we'll be evaluating unsaturated zone percolation, so that's what we call early or constructive-phase tests that are ongoing as we move the excavation forward.

After construction, of course, we'll come in and do detailed testing in alcoves and large-scale in situ testing, and I'll talk about some of the waste package or thermal block, thermal heat testing, although we're not doing it underground because we're not to the Topopah Springs yet. We have an activity underway in Fran Ridge, with a large block thermal test this year, and I'll talk about that a little bit later.

You've seen this chart before. There's been various dates and various versions. As you note, on the bottom left here, this is April 20th's version of the dates, and what it says is, sometime in spring of '94, we'll start the TBM on the north ramp, move to the repository horizon, come across the repository horizon and exit, our best guess is, summer of '95. Some people believe we can make that loop in a year, some people two years. It depends who you talk to, and we won't know until we get started, really, so this is kind of a mid-term schedule. Eighteen months, I guess it is, but that's our approach.

If we have money, we certainly have the capability. We may start other excavations at the same time, other drifting at the same time, as long as it doesn't interfere right now with the main thrust of our investigation, which is to get the loop done, do the five-mile loop.

Some of our near-term dates. We did start site prep. We started tunnel construction. We award TBM 15th of May, and we complete starter tunnel 14th of September; that's 200 feet, is that definition. That's up there for us to shoot at.

This is a little longer term. It goes through '94 and '95 about the different activities that are going on. It doesn't include all the ESF activities, but it does talk about the ramp design from the surface to the Topopah

Springs, it talks about surface facilities that are going to go on, and you see out there, we're very basic right now with trailers and dumpsters, so to speak, and we just need to get a little more permanent facilities for the staff that's going to be working there.

And this talks about our power package. It talks about the mapping and testing that goes, and puts bar charts on the other chart that I had.

I talked about the TBM. Lake talked about the budget. Our top priority is going to make sure we can support major TBM operations in '94. That's going to come first on our budgeting allocations.

Before I go into some of the specific activities, let me move to the issue of what we're doing to keep the public informed, how that may or may not be being accepted around the state, and these are just some "gee whiz" figures, over the last six months, the kind of thing that the staff has done.

I'd like to point out that most of this is done by the scientists and engineers and staff on the budget, not by the public relations group. Ace Robison and Bea Reilly certainly line out the programs, but the actual people working on the project are doing most of these kind of things; set up exhibits, run our information offices, have update meetings. We have a tour coming up this Saturday, and I guess that I don't even see the tour just on here. Oh, yeah, here it is; I'm sorry. We have special group tours, 2,000 visitors, six open houses. We expect 400 people Saturday, or so, to come tour the mountain, and we do educational outreach, also. so that's in there for your information.

Our public meetings have been well-attended in Las Vegas, 300 and 500 or so at the last two, respectively. We've developed a format that seems to be user friendly for those people wanting to gather information in a closer-type session for a couple hours early on, and then a group question and answer afterwards.

We are concerned about when people take the tour of the mountain, what they think, what they'd like to hear different. We do provide them a questionnaire. I show you this questionnaire almost every time I come up here. The results of it are over 4500 people have filled out the questionnaire. This is not meant to be a poll. There's polls all over the state saying this and that. This is not a scientific sampling, not a poll, but it is survey results from people who went to the mountain and talked to the staff, and most people, after they do that, seem to be supportive of the studies.

But you can get polls and headlines that'll say almost anything about this project. This was a Review Journal headline in a state poll that said there's 60 per cent or so opposed. A more recent state poll that I've heard of this week is the number's back to 71 per cent or something opposed. There's another poll that was conducted by the nuclear industry that asked different questions in different ways, and had different answers. That's provided to you just for your information.

I also provide you some thoughts of some editorials out there that say move forward. There's other editorials that say, don't move forward. There is newspaper articles that say we should be stopping and rethinking it, and there's newspaper articles out there that say perhaps we should be negotiating or doing something else.

The state legislature is in session, and they may or may not address the issue, but I guess I want to point out that for the staff working on the budget, it is an every day newspaper story. You don't get up in the morning without seeing a story about the project in the paper here and on the news.

I want to move on to my challenges. We're midway through the year. I showed you these challenges before. They're still the same, they haven't changed. One of them is, we're going to have to work with the National Academy and respond to their needs about what they want us to do for the energy legislation requirements, as we had to work with the EPA on the standards. We don't believe our program, over the next two or three years, will be changed, but we're also making sure that we don't do anything that would not be consistent with whatever might come up.

We do have adamant political opposition and intense media attention. That's just a fact of life of doing business. Lake already addressed this sufficiently. Adequate funding is a challenge, because we don't have adequate funding. The state is not holding us up right now. We are funding-limited. We could be doing a lot more than the 19 activities that they're doing, or the three drill rigs if we had more information.

As Pat always talks to me about UZ-16 and 182 shifts or whatever, well, we're now moving to UZ-14, and that's going to be the second hole. Well, we have 39 holes planned, and at one every ten months, we won't get there very quick. We need more drill rigs and more things to gather that information.

And the last issue is about issue resolution. We need to start closing issues. Our first issue that we sent to the NRC was on extreme erosion. We believe that process is working pretty well. We will be moving on some other issues in the near term, but we've set up a process to do that, and we've set up lots of people looking at us being cost-conscious. Let's make sure we're being cost-effective.

My '93 priorities you've seen before. You can check them off. We're doing most of those things. I will say, though, this may give you a false impression; start advanced conceptual design for waste package and repository. We did start, but it is started at a minimal level. There's not a lot going on in either of those activities, so "start's" a pretty easy milestone to hit. "Complete's" a little more difficult.

We are working issue closure. We did a Total System Performance Assessment (TSPA). We intend to do another one. I know Dr. North is interested in this. It won't be done this fiscal year, but we hope to get it done by the end of this calendar year, sometime after Christmas, or early in the '94 year.

Other priorities are, of course, are to maintain the environmental program. I know you were out there, several of you were out there on a tour the other day, and the environmental program was an issue, and a couple of you remarked to me this morning about our Desert Tortoise Program, and I just would like to point out, as a timely piece of information, that night after you left there was a news story, a major story in Las Vegas, and it was about Silver State Leasing and Desert Tortoise, and unfortunately, they killed a desert tortoise in the process of building a condominium, and they are now looking at a $100,000 fine for the death of a desert tortoise and not following their procedures.

And all of us in southern Nevada have set up procedures with the Fish & Wildlife on what we have to do before we move forward, and it costs money to follow those procedures, and these people evidently didn't do it, and are trying to settle at least for $100,000.

The other activity talks about cost consciousness and our cost schedule control system. They're up there. I've talked to you a little bit about them.

That's the way we have spread our enacted budget. We may do some mid-year adjustments to change it. I haven't changed it right now. What I'm going to do now is, I've showed you some of the physical progress, the drilling at ESF. I'm going to hit some of these other work breakdown structures, just highlight a few items, and they may or may not be an area of interest to you, but just to let you know they're out there, then should you or your staff want further information, feel free to give us a call, or even have a Board meeting or a workshop on any of them.

I'm just going to go through the work breakdown structure, 1 through 15. One is our systems engineering. One of our big things here of interest to you, of interest to almost everybody is the thermal loading and performance allocation part of our advanced conceptual design. What kind of thermal loading strategy or bounds are we going to place on it? This is being done under the Systems Engineering process. We expect a product to be delivered by the M&O team in the end of October, and that's a big issue, a big item in this one.

Other things are normal things you do in systems engineering at a project level, including configuration management program and life cycle costs, and requirements documents.

The waste package, we have increased the funding in this area this year; not as much as we'd like, not as much as the $20 million we had in there several years ago, but keeping consistent with looking at site suitability as a priority, we still were able to allocate some funds here. We're starting an advanced conceptual design, but the big new one here is the laboratory large block thermal test, and it's not laboratory anymore. It's going to be done in situ at Fran Ridge, and we're pleased to be able to do that right on the mountain at Fran Ridge in one of our test areas there at an outcrop area. So we hope to see some results from that.

Lawrence Livermore and the M&O are working together on this activity, and that will help us understand Tom Buschek's models a little bit better, help us understand how the rock behaves to various thermal loads, try to validate and verify models, if you want to call it that. This is our first step in that process, and, of course, we have ongoing studies of degradation of fuel and waste package activities.

Site investigations, I already talked to you about most of that, so I'm just going to go through that, take that off. A lot of money, though; $48 million.

Repository, not much going on there. We do have to move our repository design along with the ESF to make sure they're compatible. That's required by 10 CFR 60, so we have a small activity of repository design going on. In this area, we also look at rock mechanics, because although the ESF openings we have now are not going to be thermal loading, those same openings will be thermal loaded should Yucca Mountain be suitable, and so we have to understand what effect that may or may not have on the underground openings, and that's really--most of the repository work is in support of ESF right now.

The regulatory area, lots of things going on. On one view graph, I can't attempt to capture it all, but we're working on reports for issue closure, a working paper on calcite-silica, a technical paper on volcanism. What have we called that technical paper on volcanism, which isn't in here; topical report on erosion is complete, is in at the NRC. I'm not sure if we're going to make the seismic hazard topical this year or not. It's still there, Tom? Okay.

We do an annotated outline for the license application. We have a compliance plan. We did calculations getting ready for Total Systems Performance Assessment II.

We are working with the next generation of engineered barrier system model. Our next total systems performance will have in it an in-drift waste package, is that right, like with an MPC, so that's something that we hadn't had in our previous modeling, and we will have that in our calculations.

This work breakdown structure takes care of the technical bases, also, and we do do a very important aspect that's a semiannual progress report, because that's where the NRC keeps track of our open items on what we've changed in the plan and what we've accomplished.

ESF, I think we've talked enough about that, so I'll just let that speak for itself.

The test facilities, probably we should change to test support facilities, because these are the laboratories, warehouses and things that are necessary in the area of Yucca Mountain, be it around J-13, where we're thinking of expanding some facilities, or in the Area 25, whether it's the field operations center, a hydrologic research lab, sample management facility, but this is kind of the maintenance cost and capital expenditures necessary to run a major project that's remote, 100 miles remote from other facilities.

1.2.9 is our project management activities. We, as part of the DOE system, have to have a baseline that's approved by the Secretary of Energy's Advisory Acquisition

Board, that's an ESAAB process. It flows down from the old big defense contractor days. Major systems acquisition, is what it's called. Our baseline is inconsistent, our current approach is inconsistent with our approved baseline of about a year and a half ago, so we have to work on that. That includes independent reviews, independent cost estimates.

We have monthly project control meetings. Dr. Dyer and other scientists are learning about cost schedule variances. They're able to report variances. They're able to discuss not only their science activities, but also how they're performing from a cost schedule and technical scope performance in these monthly meetings.

We have, what I think is probably one of the Department's best project control systems, and then we have compliance reviews in other appropriate functional areas; self-assessments, we call them in this particular project management.

Financial assistance speaks for itself. It's the direct payments to the State of Nevada and affected counties, universities, and also has some money set aside for eventual payments-equal-to-taxes in that category.

Quality assurance is that work that's being done by the participants in addition to the oversight quality assurance that Don Horton has in Lake's budget. All our participants have quality assurance organizations that come up to about $10 million worth of activities, verification activities, audits, surveillances.

Many years ago, I know, we all debated about the effect of quality assurance on science, and I think--at least most of the scientists that I've talked to--we've overcome some of those stigmas and the scientists and the QA professionals are working together with realistic procedures and procedures that also meet NRC requirements, so I think my focus for my team is just to keep on track, and don't let this get off track right now, but we're carrying out the program. Most all of our audits are going fairly well. Minor things are always found. We haven't found any major things.

In the area of information management, this is an area that is difficult sometimes for me to comprehend, because I have trouble running my VCR at home. That's what I have an eleven-year-old daughter for is to handle VCRs, but we're spending money updating our computer systems. We are operating project control systems.

Just as an aside, I'm giving you some "gee whiz" figures here, but in four or five months, as we were doing site work, those changes I talked about generated 85,000 page changes, which meant we then had to reproduce those and get them out to the project, whether it was specs or whatever. That's not Xerox copies. I'm talking about changes of some sort, maybe only a small change on a page, but 85,000 page changes that had to get into the system somewhere.

So these are the kind of things that a complex program, I guess, creates. It's not much different when you're in aerospace, I know that.

Environmental safety and health, you saw part of the environmental program the other day. We do conduct pre-activity surveys, some like 40 of them in the last six months. We do the environmental and socioeconomic monitoring. We are now, of course, getting much more focused on our safety and health, now that we're going underground. Internally, that creates many hazards of its own, and we must assure ourselves that worker health and safety is of paramount importance, so we want to make sure we're safe any time we go into that tunnel, and we've conducted 300 project safety surveillances in the last six months, but we've got lots of people looking over our shoulders, and that's appropriate, but there's lots of people out there looking at us.

The institutional program, I already gave you a highlight of that before. That includes the information offices and the tours, and our support services includes the rent for our Las Vegas office space, minor amount of rent for the information offices, motor pool, telecommunications, graphics, clerical support, and training happens to come in this work breakdown structure.

That's some of the things we're doing and completing in '93. Lake gave you the bottom line budget over here for '94, 262 or so. Here's what was enacted in '93. Our budget is a little higher because we had some money we didn't spend in '93 that we moved over in '94, so this is what we're looking to spend in '94 if everything works out perfect, according to plan, and here's where we're going in '95.

You can see much of our money, add-in money has gone into ESF and site investigations; also, in EBS. Other areas are kept constant. We're constantly looking at trying to find ways to optimize our way of doing business. Did we really need 85,000 pages changed? Do we really need those kind of procedures? And we are making steps, but it's a fairly detailed and slow process to do that.

We didn't want to shortchange ourselves on QA or safety or procedural compliance at the initiation of the project. Now that we're moving a little further along, we're finding ways we can shortcut things, and I have a list--not with me, but we call it continuous improvement. We've saved about $6 million in doing things; combining record centers, eliminating some procedures for graining, doing other processes, so I don't want you to think that we're not looking at ways to save money. We've got several task forces set up to do that, but it's still a complex project that requires complex documentation.

Our work breakdown structure hasn't changed. I just put it up there to remind you that that's the way we do business. We keep track of our accounts, plan our work, schedule it, and report on it in accordance with the work breakdown structure. My division directors get a monthly report, I get a monthly report that is, in essence, management by exception; where are the variances, what needs to be looked at, does it affect anything, can we switch money from one program to another, what's happening?

Let me point out about costs again. You know, we've had a discussion, and I have it with the GAO, with the IG, with you all about infrastructure costs and things like that. We drilled UZ-16 and we spent 182 shifts on it, whatever it was, and we know how much money that drilling was, and that came out of part of 1.2.3 called drilling. Down at the fourth level, there's also lots of other things in 1.2.3 that went into it, but in order to do that drilling, all other elements of the work breakdown structure supported it in one way or another, except ESF and waste package, but you had to have requirements to do drilling, you had to make sure the drilling was compatible with the repository design, you had to review study plans, you had to include it in the annual report, you had to make sure you had copies of the reports.

We took lots of people on tours, that just takes time from somebody's day to brief on it. We had to do all the safety and health things that are required, collect the information, do the QA audits. There's plenty of oversight out there right now. Both the counties and state are frequent visitors. We have to have plans and procedures, and we have to have power, fences, and those kind of things, environmental programs, so while our report may just show X amount for drilling in our accounting system, there's really more than X amount for drilling, and whether you call it infrastructure, required costs, discretionary costs, it could be debated forever. We have all the numbers. We let people look at them, and they can make judgments as to what they want.

I know I think your staff, Russ has been out to our place and looked at our PACS system in depth, and our accounts are about that thick for a year, so it's there for anyone to look at, and we do use it to manage.

Speaking of manage, I want to move into one thing now about how we're interacting with Dale Faust and his staff in Las Vegas, and we call it the management integration model for DOE, the M&O, and the participants, and we look at it as kind of a pyramid, with DOE at the top, the M&O a slice across the middle, and the participants carrying out the science and the work at the bottom. Let me expand on these parts of the pyramid.

We, in DOE, are staffing-limited. While Lake has also told you we're budget-limited, we're also staffing-limited. We're not going to get many FTEs in the future, so, therefore, we have devised a way of doing business that allows us to manage and accept responsibility for the project with limited people, and in doing that, we're going to use the M&O for management integration of the project.

This first part of the pyramid is me and my division directors and support organizations kind of depicted. The M&O, at this level, will have a management integration office, a separate office, we think, reporting to Dale. We're still discussing that with Dale, and he has the responsibility to get back with us on exactly how he's going to implement to provide management integration all across the program, across repository, waste package, ESF design, and site characterization, to manage and integrate all those activities and make sure they're integrated with all the controls and support, regulatory, performance assessment, systems engineering, all those kind of things.

Management integration includes day-to-day management in most all areas, includes oversight and integration in all areas. It doesn't include day-to-day management yet in permits or outreach, but it does include integration and oversight, so that's what we look at the M&O to do in this role as an integrator, but they also are--John, I'll get right back to you--in this role, they are also a performer in many roles.

They're doing the ESF design, they're doing licensing activities, they're doing construction management, so they serve a dual role as an integrator and as a performer, and there's a challenge to make sure there's not a conflict of interest when they do that, and it can be done, and it's done in all industries where integrators also do performing work. It just so happens that that's the way we've constructed this program, and that's kind of the way it's laid out.

Now, this is simplified in lots of areas, but that's our approach, that the participants conduct the work in accordance with our letters. Day-to-day management integration is provided by the M&O integration team. It's a hard concept to implement, not--pretty hard to put on paper, too, but it's also hard to implement.

John, I'm sorry.

DR. CANTLON: No, that's all right. Cantlon; Board.

The question is, have you given thought to where the chief scientist would fit in this operation?

MR. GERTZ: Okay. We have given some thought to the chief scientist, and we believe the chief scientist should be at the top of the pyramid in the DOE office, adjacent to the project manager. In other words, he is my man telling me that the science is good or bad or should be focused here or there, so that's where we put him, right near the apex of the pyramid.

And I guess that almost gets me to the end, just reminding you that our focus for '93, which we're halfway through, continues to be getting ready and ordering a TBM, doing drilling, keeping the public informed, taking care of the environment, and I think, Ellis, you were looking for a view graph. Something like that would do you good if we got to that? Okay, and monitoring things that are going on, and, with that, I believe there's still some time for questions.

DR. CANTLON: Questions?

DR. ALLEN: Yes.

DR. CANTLON: Clarence?

DR. ALLEN: Clarence Allen.

Carl, did I understand you to say that there was some debate as to whether to even go to the lower level in Calico Hills?

MR. GERTZ: Calico Hills? Yes, there is. It's both a cost-conscious debate and a scientific debate. Our Calico Hills risk benefit analysis study--and Max and someone can expand upon it--indicated that we ought to go there. In examining potentials for cost reduction in the program, as

Senator Johnson has asked us to do, that is one of the potentials, is can we get that information with a sufficient level of confidence some other way. I don't know if we can or not, so debate may be a little stronger word I shouldn't have used. There's a consideration.

It's being evaluated again whether it would be significant enough savings to not go there, and once again, if you don't get the data by going there, then that lowers the level of confidence and data you have as you try to close certain issues, and we recognize it's the primary barrier to radionuclide migration, so maybe debate is the right word.

Again, no decision has been made. I want to make it clear. It's in our baseline, we're going to Calico Hills unless we can come up with sound rationale that says we don't need to go there, or we get the sufficient information otherwise.

DR. CANTLON: Don Langmuir?

DR. LANGMUIR: Carl, some of your better scientists seem to be losing their jobs, in effect, within the program, and some of them are losing activities, losing responsibility. It's going towards the M&O, and the M&O seems to be taking over those activities, and some of these folks have a long history of productivity in the program.

My concern, at least, is what are we doing with regard to maintaining that productivity if we go to the M&O

and start all over again developing the expertise that these folks possess in the M&O? I wonder who's in charge of making the decision that individuals in the labs lose their jobs, and it goes towards the M&O?

MR. GERTZ: Okay. Let me talk a little bit about that, and I'll ask Dale to talk about it. Dale is here, so he can just--think about your answer, Dale, while I go on, but that's a very fair question. We get that asked all the time. My scientists ask me that all the time.

Our intent, and it's not always perfect, our intent is for the scientific work to be done by the national labs and the USGS, not to be done by the M&O, the scientific work. Now, you might debate scientific work, performance assessment or things like that, but the bulk of the scientific work, Los Alamos, far-field geochemistry, volcanology, those kind of things; Livermore, materials--the Livermore area probably is one of the areas that's subject to most reduction, because while material studies are scientific, some of the engineering aspects have been transferred to the M&O, and we think that's appropriate. Design is more of a commercial activity, as opposed to a laboratory activity.

Sandia is still supporting development of PA, but the total systems PA responsibility has been transferred to the M&O, and the other one would be the USGS, and I don't believe the M&O is addressing any of the things that USGS has primary responsibility for; however, where there is transition, we've asked the M&O to look at existing people. If the work is transitioned to the M&O, that's a decision that we make in the DOE, what work is appropriate to transition for effective project management, can they use the people, can they pick up the people with the labs? Some of the people may not want to go, may not want to change. You're right, that's a risk. We're losing some people in this transition.

Now, that was my short answer. I don't know if Dale has anything to add.

MR. FAUST: Dale Faust, Manager of the M&O in Nevada.

Mostly what I want to do is just say that I'd support what Carl said. For the most part, we have not transitioned scientists to the M&O. We have a few scientists. Our staff there is primarily in the oversight role in Tom Statton's area. What we did do, however, was find that there were some design activities, some other analysis activities that were in the labs. Some of those have been consolidated into the M&O, and I think those are well within the expertise of the design staff that we have.

But the perception that I think some people has been there's a significant transition of scientific work to the M&O, and, in general, that has not occurred.

DR. LANGMUIR: I guess I'd like you to comment on a specific individual that the Board knows quite well, and that's Tom Buschek. Our understanding is that he's losing his support, and that he has to train people in the M&O to replace him, and provide his software to the M&O.

MR. FAUST: I'm not aware of him training anybody in the M&O, and as far as him being out of budget, I'm not sure who's made that decision, but it's not been me.

MR. GERTZ: Larry may have something to add on that.

MR. HAYES: Larry Hayes, USGS.

I'm not going to comment on Buschek's comment because I can't, but I will say we do have continuing debates in the Survey with Livermore over some of that work.

I'd like to comment on the Survey work, and whether or not the M&O is taking any of the work that we're responsible for. That is not correct. They are not taking work the Survey is responsible for. Tom does have a very talented technical staff that is helping integrate some of that work.

My concern is not losing scientific work to Tom's staff. My concern is with the limited budget Carl has, the increasing needs to move some of that budget to the M&O for work they are rightfully responsible for. I'm very concerned that we continually do not get enough money to do the scientific work we need to do, but it's more of a budget problem, I think, than an in-fight among participants.

MR. GERTZ: Don, let me just take it as an action item. We'll look into Mr. Buschek's concerns. Certainly, I believe Tom's developing those models and that is a proper role for the laboratory, is to develop models, not for the M&O. However, it is the role for the M&O to exercise those models in doing "what if" studies and everything, so perhaps Tom does have to show people how his models work, and I think that's appropriate. I mean, I don't want scientists with only them knowing how to run a model. I think other people have to do that, too, so there's all kinds of, always, "hall talk" and rumors about anything, but that's the best I know, and I'm trying to see who's here.

Russ, do you have any other thoughts on that?

MR. McFARLAND: I don't intend to get involved in it.

MR. GERTZ: Max, any; Jerry?

MR. BLANCHARD: Yeah. I have been among those who've been involved in preparing transition plans and getting the two parties, the TPO of the organization that's transferring the work, and the TPO of the organization that's receiving the work to reach an amenable agreement to be sure that there's no data lost, and that adequate records and training and files are kept, so that the quality assurance pedigree of the work continues on and is in the records for future use in the license application.

And, while what you indicated may be the impression from some individuals, the theme we've had for the transition work is to improve the program. We've never, so far as I can recall, simply said, "Let's make a change for the sake of a change."

As Carl mentioned, we tried to bring in seasoned people who've done it before in the area of design for waste package repository underground construction. I think the evidence is there that shows we've done that.

In the areas of management and integration, Carl has talked a great deal about how to improve--at least our view of how to improve the management and integration of a long-term, complicated, science engineering public-type program. You can see large presence of the M&O staff in an M&O in the management integration role there.

Other areas, where we've brought them in that some people have been displaced, are areas where we think the program was not functioning well from a management or a technical standpoint; for instance, I think we've heard comments from you all and others that the performance assessment program that we had was not all that well integrated, conceived, managed, and focused, and that's one of the reasons why we wanted the M&O to provide that leadership.

Now, they didn't just come in and wholesale move out everybody. They evaluated those different contractors and the individuals within those contractor staffs to identify where the unique expertise was, and make sure that we didn't do a detrimental thing that would cause them to leave just because of some action that we've taken.

We still have a very active performance assessment program. We still have a lot of contractors, a lot of scientists, a lot of math models, but we also have a program that's more focused, and it costs us less, and you'll hear from Jerry Boak this morning discussing that.

MR. GERTZ: I don't want to downplay the issue. Transitions are difficult. When you're moving people in and out, it's difficult, and there's probably not a day goes by that I don't have two or three meetings in my office about transitions and people wondering what is right or what is wrong, so it's difficult. It's just a process we're going through.

DR. CANTLON: We'll take one more question from Ed, and then I think we'd better move on.

DR. CORDING: Carl, I wanted to follow up on something Clarence said, and I had one other comment if I could squeeze it in, with our Chairman's approval, but that was in regard to the, looking at the Calico Hills, and I know one of the things that we've commented on over the past several years is looking at an exploratory facility in which you can look at structures, the joints, faults, et cetera, in the non-welded units both above and below repository level, and I think understanding different matrix joint characteristics in those layers compared to the more jointed, high joint permeability layers at the repository level, which was an emphasis, and I'm wondering if that's something that we, as we proceed and as you develop your test plan and sequencing, if that's something that still ought to be on the table and looked at.

It seems to me that, for example, that there might be an opportunity to have a higher priority, for example, to go to the Calico Hills than perhaps to even construct certain of the later cross drifts at the repository level, recognizing that the ramp down and across and up, out is, in itself, which is the first item, will be encountering the Ghost Dance Fault twice, the Imbricate Fault at least once, maybe a little bit above repository level and the ramp, but also an early encounter at the Bow Ridge Fault as you come down, so you'll be hitting some of those units and faults, and perhaps the Calico Hills, at least looking at the options and costs and things like that, that the Calico Hills might be even a higher priority than perhaps some of the other later drifting, so it seems to me that ought to be kept on the table.

MR. GERTZ: You're absolutely right. I don't want to give you a false impression. It's worthy elements of cost reduction, like several items are on there, and we're not going to take it off the table unless the scientific community is convinced they don't need that data, or they can get it somewhere else; if you have to go to one of the softer units above it, or if we do some other kind of drifting or access to a faulted area below it, you know. So it is still part of our plan. We have not taken it out of our plan, and before we take it out, there'll be a pretty good debate and scientific consensus would have to be achieved.

MR. HAYES: Larry Hayes, USGS. Maybe to take a little heat off Carl, and perhaps put some of the heat where it should be, I think I'm probably as strong as anyone in saying, "Let's revisit tunneling into the Calico Hills," and part of the reason I say that is the costs. We're looking at approximately 300 million to do that.

I hope I don't insult anyone on the Board, but I'll be candid. I feel the Board is giving strong emphasis to getting underground, sometimes, in my opinion, to the detriment of the surface-based program. I do not hear as much support from the Board as I would like to for this surface-based program. I hear a lot of support for getting underground.

The more emphasis that is put on getting underground through drifting, the less emphasis that goes to the surface-based program, and it's been a fight every year, in my mind, to keep a balanced program, and I'm concerned, this continued emphasis, "Get underground. That's where you'll get all your answers." Every year, you and I, Carl, are going to have more battles about a balanced program.

MR. GERTZ: Unless we find a way to get higher resources so I could have less battles.

MR. HAYES: That's right. I guess if the Board would please all of us, help give you some more resources, but we are looking at how to characterize the Calico Hills. We're not saying, I'm not saying, at least, it is not important. It is terrifically important to do these things you're talking about and to characterize the Calico Hills, but my concern is, can we afford to tunnel into the Calico Hills, keep a balanced surface-based program, and so forth. If we can't, I feel we have to look at other ways of getting that information.

MR. GERTZ: I guess my commitment to the Board is, before we do anything like taking the Calico Hills out of the program, we'll surely discuss it with you at some length, and we must discuss it with the NRC in open forum.

DR. CORDING: I agree that one needs to look at the cost of these programs and the tradeoffs, and that is a concern of ours as well, what costs it takes to do certain exploration. I think there's deficiencies there that need to continue to be pursued, and I'm sure we'll be discussing those further.

I think the other that--one other point and challenge that you have are emerging concepts on the thermal issue, the large canister possibilities, drift emplacement, those things are influencing an ultimate repository configuration, as well as the slopes in the repository. Perhaps we're using things such as rail. Those are going to have to interface, as you pointed out, with the ESF, and it seems to me an area there where one--it's going to be difficult to try to tie all that together when we know the repository design at a fairly early stage, and I guess one concern is some of the gradients, perhaps even within the repository, the main drift, as to how that will fit the actual future repository, whether that main drift then, if there is a future repository, could be used, or whether it would have to be--if it's in a position where one would have to consider not using it for part of the repository; for example, for a two-level level repository.

MR. GERTZ: You're talking about this drift, of course, right here, and that's one of the elements--we're only in the 50 per cent design review. I don't know if I've got my designers on here. There's just mostly scientists here today, but one of--NRG-6, the drill hole that was drilled in plan view right about here is looking at the repository horizon, and my information is there's a high likelihood that we can raise that repository horizon and, therefore, lessen the slope. Am I saying that right, Max? Yeah, and therefore, addressing some of your concerns.

We haven't made that call yet, but it's one of the things we're looking at, and that's why we're trying to integrate repository design. The repository, I think, is lower down here than here--excuse me--it's lower here than here, and then if we raise this up, it would level it.

DR. CORDING: Yeah. I have understood that it's still going to be relatively steep.

MR. GERTZ: Four per cent or so.

DR. CORDING: Four per cent.

DR. CANTLON: Well, let's take our break, and then we'll start with the main program.

Thank you, Carl.

MR. GERTZ: Okay, thank you.

(Whereupon, a brief recess was taken.)

DR. NORTH: Good morning. We're resuming. Welcome to the second part of today's program, and the first session on the main topic. This topic is: Resolving Difficult Issues; Examples, Infiltration and Future Climates.

My name is Warner North, and I will be chairing the session this morning.

Infiltration, or the movement of groundwater into the unsaturated zone, is difficult to predict and model because of the complexities involved, both in making field and laboratory measurements, and in developing appropriate models. This general statement is especially true at Yucca Mountain, where the groundwater flow is both in fractures and within the rock matrix itself, precipitation often comes in infrequent, severe storm events, and variation in both terrain and rock strata are important in channeling surface and groundwater flows.

If one wants to predict infiltration over the next 10,000 years, then changes in climate must be considered. Substantial changes in precipitation have occurred in the past. In the future, we will have an atmosphere significantly altered by human activities, such as the combustion of fossil fuels.

So the challenge is awesome, but there is no way to avoid the issues. To evaluate repository performance, it is necessary to consider the hydrogeologic setting of the repository, which depends on infiltration, which in turn depends on future climates.

How much information is enough? We are not interested in the details, but rather the process of managing the scientific investigations to obtain the needed information. What information is crucial for assessment of site suitability, the license application, or program decisions such as repository design?

One answer is that information is crucial if it is called for in the site characterization plan or in the study plans. That answer does not provide opportunity for learning and adaptation of plans as new information and insights are obtained. The Board would like to hear about the process by which scientific studies will be managed as new, and possibly unexpected information becomes available.

This morning, Carl Gertz, in his comments, described flexibility in the plan and gave some numbers for the extent of the changes, including such data as 85,000 pages. Clearly, the management process is a very complicated and formidable one, and the Board is trying to understand this. We also want to understand better how the information is going to be used. How will the information from the scientific studies be integrated and evaluated? At what point should studies be terminated because further measurements or modeling are not producing information that is worth the money for the Yucca Mountain project, and certainly, the discussion we had about Calico Hills is a very major case in point.

We're not just interested in information because it's useful, publishable science. The issue is supporting the goals of the Yucca Mountain project. What we're interested in finding out, using infiltration and climate change as examples, is how the management process is going to work. At what point should study plans be revised or new studies initiated because information needs were not adequately foreseen? What is the role of performance assessment in providing guidance to this management process? How does this process stay on target, both with respect to high-quality science, and controlling costs, making reference to two aspects of the Secretary's guidance, as Lake Barrett just described it to us.

The program and the science have evolved considerably since 1989, so the targets need to evolve, also. How does this evolution work with respect to the site characterization information?

From this meeting, which is focused on infiltration and future climates, the Board hopes to understand better how the Yucca Mountain project is managing the site characterization effort in general, and with respect to infiltration and future climates as difficult and important site characterization issues.

We will be hearing from program managers and from the scientists and engineers conducting the studies. I will reiterate instructions given to them previously: Please keep the technical detail in your presentations to a minimum, and focus on the management aspects and the process, how the Yucca Mountain project will accomplish its goal of providing information that is sufficient, but not superfluous.

I will remind everybody that to stay on schedule, we're going to limit questions and comments to the Board and staff. I remind everybody to please identify themselves for the purpose of the transcripts. At the end of the session, we are going to have the opportunity for questions from the floor, time permitting.

Our first speaker will be Max Blanchard of the Yucca Mountain Project Office, who will give us an overview of the Yucca Mountain program process.

Max?

MR. BLANCHARD: Thank you, Warner. That was a very accurate introduction of what I intend to cover, but I will be providing, like you say, an overview, and other speakers today and tomorrow will cover this in greater detail, especially Jerry and Dennis, and tomorrow, Russ, Tom Statton, Jean Younker with respect to the management aspects from the project office view.

It's my pleasure to be here. I don't think I've spoken to you in about three years. During that time, I was a Director of RSED, the Regulatory and Site Evaluation Division. Now, bigger and better people are handling that--Russ Dyer is--

(Laughter.)

MR. BLANCHARD: --and as a scientist moves on his career, they eventually migrate out to other pastures, and now I'm involved in focusing on just the very thing you're talking about. Working as Carl's deputy, Carl and I and Dale have spent a great deal of time over the last three years, certainly the last two years, trying to sharpen the focus and make improvements on the process whereby we plan, implement, and evaluate what we're doing.

What I'd like to do is to share that with you today, and give you a better understanding of what that process is, and I'm going to try to do this with some icons, and I'm going to simplify some things. Like Carl has mentioned, the process when you're running a $250 million program, is fairly complicated and what I've tried to do is to leave the complexities and the exceptions behind, and focus on what the bottom lines are from the top level management view point, but we will be addressing resolving the difficult issues, and I'd like to share that with you in five ways.

One is, I'd like to explain the concept, the simplest version of the concept we have for managing the site characterization technical part of the program. Then I'd like to address, again, in a simple way, what are the applicable requirements, how do we use them as tools to manage the things that are flowing down?

Then, as you're managing, the things that you have to do to meet the requirements, certain things need to be controlled. Well, what is it? What is the management concept we have for controlling those requirements, and then the work that comes out of those requirements? And, of course, the baseline comes in. Lots of people use the baseline different ways. The financial people call a baseline something different than the technical people, and the change control of the configuration management group have very specialized definitions. I'll try to avoid getting hung up in that semantic issue. I'll show you the baseline concept that I'd like to have for the dialogue.

I would be remiss without saying that quality assurance is ever present in our mind of managing the program, and I'll show you how it comes in. It's all-encompassing, from the outside, and from the inside going out.

Finally, I'd like to show that we manage the process of planning, implementing and evaluating, and it is ongoing. It works now, but everything I'll be giving you is a snapshot in time, because as the requirements change, they get modified, the plans and procedures get changed and modified, the specs get modified, and, of course, the actual tests get modified, and so what you have to have in this program, just like if you were in a program where you were building a nuclear submarine, or an ICBM, or an airplane, is that you have to have a flow-down system, kind of a domino effect so that when you have a black box here, you know what the requirements were that were in effect when that was designed and tested, and if there's a new change that comes in anywhere in the system above it, you can do an impact analysis and then decide whether or not you need to change that black box so it'll function the way you want it.

So you have to have an orderly process for configuration management. We have that. I will present it to you as a snapshot in time, like I said. Bear in mind that all these things are changing all the time, and so change control is very important. And, as Carl mentioned, continuous improvement is one of our themes, and that means that our change control system works especially as we get things into the field condition.

Carl mentioned to you that there were 162 field change control requests processed at the field change control board so far this year. It is the way you get the work done. It's important, and it's part of our process.

Now, what I'd like to share with you is something that you've seen before. This was first evolved in the EA days. It was part of the SCP. It's been part of our program since then. I'm not going to belabor the details on this, I just want to talk to segments.

In the green, what we have is a synopsis of the earth science program. It's producing reports that describe the nature of what we've learned about the site characteristics, especially those things that address the processes that are changing that could affect waste isolation, and the magnitude and recurrence interval of those events.

The reports are used by the group that does performance assessment and by the design group that's designing the repository and the waste package as we move from the conceptual design, go through a Title I design, eventually a Title II design, and finally, a design for the license application.

This information is being used by these two groups, but also a third group, an ever-important group, is the regulatory group. In the process of analyzing the information that's coming out of these reports, and as these two things--the PA and the design--mature, we're looking at site suitability, we're looking at building, a draft environmental impact statement, and we're updating with the program semiannual progress reports, and we conceptualized a long time ago a series of reports which were topical reports in an annotated outline that, as we moved through the program, we had interactions with the oversight bodies, in particular, the NRC on issues of--from time to time we've changed the name of these, but topical reports, issue resolution reports, whatever they are. They're reports that allow us to make an argument for saying we think we have this kind of information, and this convinces us that we're ready to comply with this particular sub-part of a regulation.

Once we've had the interaction with the NRC, we've listened to their comments, we think we're ready to take that and move that into a building block, update the annotated outline so that at some point in time, we don't have to go back and rewrite everything. We've got all these building blocks in place for shifting the annotated outline to a license application.

Of course, something that's an issue resolution report that the NRC and the Department have addressed a long time ago could be changed by realizing new information is available that might affect your conclusion, and that would be updated as well. Okay, so that's the overall scheme that's been in place for over a decade.

We've tried to improve our view or our management focus on what we call convergence. Convergence means to us that the site recommendation report, the license application, the environmental impact come together in a neat way that people can understand, and that the information gained during site characterization, and the evolving performance assessment calculations on the predictions of releases from the site, as well as the evolving design, using the annotated outline and the issue resolution process, provide adequate building blocks so that everything dovetails together if and when the suitability decision is made about Yucca Mountain.

During the process, key elements are study plans, the exploratory studies, alternative studies, the early site suitability evaluation--and we have a number of other blocks. These are just icons. We don't know how many we'll have. Approximately every 18 months we expect to be doing another site suitability evaluation. We have prioritization things going on to help us decide where do we want to spend the money we have on certain testing areas of the program, based on those features of the site that we think might reveal characteristics that would suggest the site should be disqualified.

Of course, all of this has to be done under the aegis of the Waste Policy Act and the regulations that are the principal regulations that apply to the program.

Now, okay, there's an orderly way that we have applicable requirements. This is part of a systems engineering management plan. Like I said, things are updated. This, in effect, in my mind's eye, is now out of date. In a few weeks we'll be going through a baselining process where we'll be changing this version to another version, but the one that we're working under right now--and I want to focus not on the management side, but on the technical document hierarchy side--flows down from a waste management system requirements document at the program level, which divides into transportation, MRS, and MGDS.

This Volume IV has the high-level requirements. They flow down into this box that has stipple, and, simply speaking, this is the essence of what our technical document hierarchy requirements are, and we're trying to do everything to show that we are meeting these requirements all the time we work, and if we're doing some work that isn't a requirement, we shouldn't be doing it.

We have the system requirement documents for the MGDS system and a description of that, so that the two go hand-in-hand. We have a design requirements document for the waste package and for the repository. We have the equivalent for site, the equivalent of a design requirements document, but we call it the SCP planning basis. It's different from the SCP because it's the essence of what we're going to do while we do characterization; in other words, it's an abstract of Chapter 8. This is our plan. It has the goals and the test objectives in it.

Then, there are two things from an engineering standpoint that have to occur; design requirements for the ESF and design requirements for the surface-based program. So, in essence, it's these few documents that form the basis of our requirements, and everything else flows from them.

Now, to move on into what parts of the program need controlling, and what is the technical baseline, what I'd like to do is to have something kept in mind by you all that I won't be talking a lot about, but it's pretty logical, and I think it will make sense to you.

We define the technical scope from the requirements. It's clear we can do that. Of course, there are alternative ways to do that, but how you meet that technical scope and the rate at which you meet the technical scope depends upon the resources that are available from the system that funds you. We have only so much money available, only so many FTEs available. Therefore, the combination of these two determine the schedule. That's the way the program is managed right now.

I won't be talking about the schedule or the cost. Carl and Lake have both given you a perception of what the costs are for next year and what the costs are for this year, and I don't think we really know what the allowable resources will be, so we can't really go much farther on that. After that, we're crystal ball gazing with respect to how much of that technical work scope we can accomplish.

Okay. What is this technical baseline? Well, it's a set of documents that are systematically developed and formally approved, and the approval process is encompassed at the highest level within Carl's office, and at the highest level within the program. It has the objectives of the site characterization program, and this icon here represents that program planning basis. It is, like I said, the goals and the objectives of the tests. It reflects the 106 tests that were in the SCP.

The requirement documents for the repository, the ESF, the engineered barrier, the waste package, and the test facilities that go with that, and a description of the engineered system. At this stage, it's only a conceptual description of the repository and the waste package, and that will evolve.

Because they will evolve, and because the details of these change, these documents get updated in a very systematic way, and what I've brought with me, but which is not in your package--and I'll pass some copies around for you to look at; start here, Pat, and here's one for you, Ellis--this quarter-inch thick little pamphlet describes our master control document list.

It has the baseline documents that come from RW, which is the headquarters for the whole system. It has the baseline documents at the project level within the MGDS. These are documents that are controlled by the change control board, of which I'm chairman. It identifies the study plans. It has the spectrum of implementing procedures which provide the way we do business, the way we organize and manage change control processes. It identifies the job packages, which are the instructions to the contractors, to work together with different disciplines to accomplish some goal in the field of test or laboratory program. It includes the quality assurance grading process, whereby we identify things that are important to safety and waste isolation, and then it has a series of general plans.

These are the control documents in our system that we use as management tools. Not all of these are part of the CCB process, only the ones that are fundamentally important, that are part of the Q process, important as safety and waste isolation are part of the CCB process on the technical side of the program. I'll be glad to discuss these at some later time with you today or tomorrow.

Now, how is the baseline controlled? Well, it's controlled by configuration management, and configuration management includes a major aspect of change control.

First, we identify and document both the functional and the physical characteristics of the item that needs to be controlled, and when we say item, we mean that our analysis tells us that this is an item that's going to be important to either worker safety or to waste isolation over the long term, and don't do anything to that that would have an adverse effect without first very seriously considering, is that adverse effect worth the risk?

We make changes only through a controlled process whereby we include review and approval on all the documents that are in the CCB register, the requirements and the procedures in the flow-down documents, and we record and report the status of the changes.

A bit on the change control process. A change request comes to us from a design organization, a testing organization, a regulatory organization, and the first thing we do is we have a procedure whereby we govern the process to consider the change request. We do a multi-discipline impact review. We get cost schedule, a technical analysis, a regulatory analysis, safety, quality, and institutional. Everybody comes in with their analysis that says, if you're going to change this requirement, of if you're going to change the way you design this, here's what we think would be the impact.

Then it goes to the Change Control Board with all these impact reviews, and the Change Control Board reviews it, either approves it as it is, or suggests a modification and rejects it and sends it back, and then, once it gets approved, if it gets approved, we implement the change, and the way we implement the change, again, is by an ordered process.

We have documents that identify the interfaces required to do this. We go through the process, implementing the mechanics of making the changes to design documents, to procedures, to plans, to specifications. What falls out of that are released, revised documents that are a controlled document, and a set of documents that are revised, updated, new baseline documents. They go back to the Change Control Board, and we want to see and verify whether or not the appropriate changes to the requirement document has been done.

Now, how important is quality assurance in this process? My conclusion is, as a manager, it's crucial. It's extremely important. First, it plays a role in convincing us whether or not we have an orderly implementation of the requirements. It provides a check to see that our workers are indeed following the procedures. It ensures that the people have been trained to follow the procedures and trained to do the work within the discipline they're working, and that we have a records package that shows this was true.

In order to be successful ten years from now or 15 years from now, if we go into a licensing process with this site, we will have to have objective evidence. When I look around this room, I see--now, I've been in this particular program for 13 years now, and I can count on one hand the number of people that have been in the program that long. Even in our office, we change out the number of staff by about 30 per cent a year.

One thing's certain. When we get into the licensing process, there'll be darn few people in the program that collected the data in front of the NRC explaining what a data point means on a graph, important to safety and waste isolation. We have to provide objective evidence. We must know, to defend that data on that graph, who did the work, where the person was educated, what kind of training they had, whether or not they had specific training.

If the data came out of the system, was the instrument calibrated? What was the procedure that was used to calibrate? What was the test procedure that was used by the person that conducted the test, and then, what was the raw data that came out of the system, and then, how was it refined? This is what I mean by objective evidence. This is the picture we have, and the quality assurance, by conducting audits on all of our contractors and us--and over the last three years, we have had two one-week audits, which include quality assurance, staff, auditing the people that implement all the procedures in this system, including observers from the state, the NRC, the affected counties, and the utility industry.

Now, a bit into what is it we're trying to do with respect to planning, implementation, and evaluation? Again, the process you'll see, the APs, the numbers there means that we have a procedure that's been in place for a long time that may have gone through a number of revisions, it may have a number of engineering change notices, it may have been rewritten four, five, six times, it may be going away and being replaced by another document, but nevertheless, this is the way it works right now today.

In test planning, when we created the SCP we were going through this. We defined the test program and we identified the controls that would limit adverse impacts on the site as we "insult" the site. By that I mean we're changing in an irreversible way the properties of that site, so we could be having an adverse effect on waste isolation if we knew whether or not those properties lend themselves to contributing to waste isolation.

We then assure that we have an adequate planning basis. We do that with a procedure, and we check with the SCP planning basis to make sure that the study plan that's been prepared and submitted to us by the investigator, who wasn't in the program when the SCP was written, that he is not changing those goals. He is not changing the test. He is not changing the test objective, unless we've, in an orderly way, already gone through and revised those test objectives or goals at a higher level, and decided that's what we need for the best of the program.

We prepare a job package so that we get the right group of science, engineering, and administrative people together to make sure that the road is there, the power line is there, the Port-a-Potty is there. We're going to do it two or three shifts a day. They're all there, ready to work as a team. If test facilities need to be constructed, they're prepared under that, and then we collect the data, and the data goes into our data management system, and these procedures govern the flow of the data into either the reference information base, or the technical data base.

At some point we're over here into evaluation, and we have some going, in our program, some of the tests are going through this. A lot of it--and Carl gave you a long list of study plans--are in this phase. Some of them have worked their way down here; extreme erosion, seismic hazard methodology, volcanic hazards. Those are down here. We're preparing a report, or a topical report to get the process going with the NRC so we can build that building block.

We have procedure reviews, we have peer reviews, and we have technical assessment reviews to help us evaluate the results and decide, are the results adequate for the intended purpose, given what was in the SCP planning basis and what's in our requirement documents. If the answer is no, then we go back and continue testing, or replan and reevaluate. If the answer is yes, we go here, and in the process, we have things coming out that system now, out of the end of that box.

To look at that side in a little bit more detail, we have two processes we use in the evaluation. We go down both sides. This side is the more formal side. It's the side we'd like to go through when the experimenter comes to us and says, "Hey, I think the test objectives have been met. I've evaluated the results with your multiple discipline team. The information's been fed into design and performance, and we're at a process now where we're ready to answer that question."

If the answer's no, we could disqualify the site, or we go back to continue testing or to modify the testing, and the extreme erosion came through the process that way. The investigator said, "Hey, I think I don't need to do all this stuff on extreme erosion." On the other hand, there's lot of other things that are going down this side called interim data evaluation.

We take input from the NRC and our oversight groups, especially a Board like yourselves. We look at the issue resolution strategy that was the highest issue strategy in the SCP. We look at the test strategy, the basis and the instructions and the controls on the test. We determine whether or not it's appropriate at this stage in time for a peer review, a technical assessment review, or something else. If it turns out the particular topic would be something that we need to discuss--it's not a unilateral decision on our part to change or to stop or to reorient, then we go back and check with the affected parties, like yourself, and especially the NRC, who has that SCP and they are using that as their algorithm for the way we're going to conduct site characterization.

If the answer is yes, go ahead and make a change, then we follow one of these two processes. So just to show you that that right-hand side works, here are some examples of the evaluation activities that have gone on, using those procedures. The goal is to add a series of different products together, to help get the managers to make a decision, either this is enough, or, let's go back and do more, or, we don't have all the facts here. We have enough of that, but we need more design or more PA.

Right down here is our bottom line. We want to be preparing as many topical reports and updating that annotated outline in a sensible way, and in a prudent way that is about as fast as we can do it.

The issue resolution strategy process has been with us for a long time. It was originally defined in the draft SCP in 1987. It hasn't been updated, because the logic is well thought out. It's very fundamental. It's not likely to change.

Other people have talked with you about issue identification, performance allocation. In the program, we're down here. We're conducting investigations, analyzing the results, and trying to establish whether or not the information needs are satisfied, and some of the things that are coming out that we're testing and probing the system for interactions with the NRC are down here at the top of the report level. Russ will describe this in greater detail with you tomorrow.

In summarizing the current issue resolution process, one of the things Warner asked, how will we use performance assessment and regulatory analysis? Well, this diagram is meant to give you an idea of that, but Jerry will talk to it in much more detail.

The efforts that are going on here in site characterization and engineering design are feeding information to performance assessment and regulatory analysis, what they're doing--allows regulatory analysis to do things like site suitability, identify issues, identify future topical reports. It's these two things together that allow us to go back and look at our annotated outline, which has constraints from the NRC of what it should include, and update it from Rev. 1 to Rev. 2, and, at the same time, identify those things that we think might be sticky wickets, and prepare a topical report so we can solicit input from our oversight bodies to see how well we've justified our position.

As we go through that process, we really need input from the NRC staff. It's very important for us to pay attention to that, so that if we really intend to make these building blocks, we need to revise these topical reports so that they have the right flavor with respect to demonstration of compliance.

Okay. Now, in summarizing what I've tried to show you, is that I've tried to give you the overview of the process that Carl and I and Dale use in an attempt to manage a complex program in a simplified way, but the process we're using is really those fundamental, simple things that I've shown you, even though they're in icon format.

We have an issue resolution strategy that's in that SCP. We have an orderly way to change it. It is the baseline of our program. The program must be executed in a controlled environment. First and foremost, at the front of our thought process is, let's don't spend money on things that we can't use later in the licensing process in the technical arena. We have to have a configuration management process and change control is a way of life, continuous change; updating all the procedures.

We even have TQM groups involved in it. They come to us saying, "Gee, these two procedures don't seem to be working quite well. It takes too long to get through them." Well, we don't know how to change them because they're different departments, different disciplines, and different contractors. The first thing we say is, "Form a TQM group, here's a TQM facilitator. Change the procedure in a way that will get it done faster, more meaningful, more direct, and simplify it if you can. Bring us the product." That's ongoing. That's what Carl was referring to when he said continuous improvement.

We have a management process to plan, to implement and to evaluate. It works. We have numerous internal and external audits and reviews. Carl showed you a view graph of a plethora of oversight bodies that that come in for program reviews, as well as a quality assurance department, and the DOE orders also have internal reviews at our level, and internal reviews from headquarters people coming out to check to see if we're doing what we're supposed to be doing in addition to the quality assurance program, and then there is other groups like the IG and the GAO.

This process works. It's been demonstrated. We have the objective evidence in our records department, and I guess that's the essence and the simplest way I can explain to you what my new job is now and has been, and what we try to do to manage the technical program.

I'd be glad to answer any questions if you have some.

DR. NORTH: Dennis Price?

DR. PRICE: Max, I believe a number of years the issue of a change control board came up, and we asked about it as a potential stumbling block because it would be a choke point if there were a lot of changes coming through and approval, and at that time, it seems to me I remember that the answer was there are different levels of change control from the field on up, and I thought there was about three different ways--committees or boards--that change control could be managed, and maybe I missed something here.

MR. BLANCHARD: You bet. I explained the overall change control process with that one diagram. That functions at three levels. There is one at the program level, which look at the systems, you know, MGDS transportation and MRS.

There's one in Las Vegas that functions across the requirements, the plans, the procedures, the design drawings, the specifications, and then there's one--and this is the change control board process--for test implementation. There's a group out at FOC that operate one. I know every day they have changes. They're the ones that process through changes in an orderly way. Like Carl mentioned, there was 162 changes so far this year, changes in either specs or drawings as we move along.

So those three are functioning now. They work. I'm not saying that a year from now they'll work the same way, because we're changing them, but it does work and the field change control is absolutely essential.

MR. GERTZ: And they all have thresholds; cost, schedule, or technical baseline thresholds where you have to bump it up to the next level.

MR. BLANCHARD: And, of course, it goes all the way up if the change could have impact on waste isolation. Then it goes all the way in the system, even if it's only a very low dollar one.

DR. DOMENICO: Domenico; Board.

Max, I have a question about the quality assurance in the field program. Now, whether you're drilling a shallow hole or a deep hole or, as now the case is, you're getting ready to enter with a tunnel, you always have a project scientist on each of those all the time, I'm sure.

Who has the ultimate authority in these operations, the driller or the project scientist, or in the case now, the drill and blast team, or the project scientist? Who has the ultimate authority on saying, "Stop it, we want to make measurements," or whatever?

MR. BLANCHARD: Well, if it's an issue that would affect adversely the goal or the objective of the test, then it's the project scientist. If it's worker safety, then it would come into play for those that have the responsibility for saying, "This is unsafe. You have to stop here because we're jeopardizing the worker."

If it's someone in the cost control area who says, "Hey, guys, you were supposed to get this done for $100,000, and you're twice or three times over the budget. We've got to take a look at this if we want to continue to spend that." If it's a desert tortoise that walks across the path, say, "Oh, oh, hold it. The law says stop."

So the answer first and foremost to your question is the project scientist if it has an adverse impact on him achieving his goals. If, on the other hand, it doesn't have an adverse impact on him achieving the goals, but there's some other requirement or law or permit that we have to comply with and we're not, or we're not sure that we're complying with it, then other people come into the picture and say, "Hey, guys, I think we better talk about this," and we have a procedure that governs the way we do that.

MR. DOMENICO: My concern is, you know, with the drillers like to make a hole and tunnel-boring people like to bore, and I presume you have a project scientist on the ESF 24 hours a day and he will have the authority to at least, the authority, really, to look at windows of opportunity. That's my concern.

MR. BLANCHARD: You bet, and because we're concerned about ensuring that that occurs, we've taken a person within --Arch Girdley is now out there full time just to make sure that we are meeting the objectives of the science program, and whatever PIs are there, or representatives of the PIs, regardless of if it's a 24-hour shift, maybe the PI isn't there, maybe Arch isn't there, but there are designated officials there to take that input and decide, "Let's make sure that we meet our objectives."

DR. DOMENICO: One other question. Who provides these project scientists? Does it come from Raytheon, or if it's a USGS project, the USGS personnel all the time, or--

MR. BLANCHARD: Well, we hold the author of the study plan and the person responsible for spending the resources. That's the project scientist, in our view, and so we go to Larry and say, "Larry, you tell us who your project scientist is. Who is the man currently in your shop responsible for this particular study?"

DR. DOMENICO: Thank you.

MR. GERTZ: I think we've experienced some real life activity, in fact, with Flint on that, where he said, "One, I want the hole deeper," or, "I want the hole stopped until I get this done," and that's just the way the procedure should work and has worked over the last year. We're not drilling for drilling records, we're not tunneling for tunneling records. The only reason we're doing either of those activities is to provide access to the scientists.

DR. NORTH: Do we have other questions or comments from Board members?

(No audible response.)

DR. NORTH: I'm going to recognize Carl Johnson, who has some questions he would like to put to you.

MR. JOHNSON: Carl Johnson, State of Nevada.

Could you put up, Max, the slide that has the question, "What are the applicable requirements?" It was about halfway through your talk.

(Pause.)

MR. JOHNSON: Basically, the way that the, as I understand this flow chart, is it flows down from the technical and management document hierarchy, down to the lower tier, but if I look at the documents that are in that upper tier program level--

MR. BLANCHARD: You mean over here?

MR. JOHNSON: --both technical, and on the management side, those documents are pretty much in limbo at the present time.

Given that those documents have essentially not been finalized, what, then, is the validity of these documents then at the lower level?

MR. BLANCHARD: I'm not sure I know what you're referring to about not being finalized.

MR. JOHNSON: Well, you've got a mission plan up there that has not been issued, as at least amended to reflect the current program. I'll just use that as an example.

MR. BLANCHARD: Okay. Well, first, there is a mission plan. It was published. It has been updated. The most recent update hasn't yet been released.

There are other documents that are in different stages of evolution. All of the documents from here down have been released. They're in that list. They were prepared two or three years ago. The documents that pertain to the MGDS project plan and the project charter and the project management plan are several years old, have gone through small but important changes, and a number of these documents are updated during the year.

There's nothing here that affects waste isolation or safety. This is the system that we apply the most rigorous process to from a CCB change control process. The suite of documents that are here, this one's driven by the law. Everything else that's here, except for the quality assurance requirements, which are really over here, not here --in the new version, the quality assurance requirements are over on the technical side--all of this is driven by DOE orders. They're there because we have an order that says if you work in the Department of Energy, you have to follow this process.

MR. JOHNSON: Okay. My point is, though, that the upper tier documents do not reflect the current program; therefore, your lower tier documents, which some of them you've just said are three or four years old, can't be valid any longer.

MR. BLANCHARD: Well, I mean, that may be your conclusion. It's not mine. I feel that I have enough authority to manage the program. I have procedures that implement these, I have plans. I have requirement documents, I have specifications. All of these things are in our program. We're working to them and we have objective evidence from the audits and the oversights that they're functioning.

Are you referring--I mean, you couldn't be referring to documents down here, because they're listed in that product.

MR. GERTZ: Max, let me just interrupt. I think what Carl is saying, while we don't have a current mission plan, then how--

MR. BLANCHARD: We have a current mission plan.

MR. GERTZ: I understand, but it doesn't reflect the current mission that the Secretary has set.

When a new plan comes out, we will have to revise the documents, check if they have any impact. That's how we implement new changes to the program. Until the new one comes out, we proceed under current documentation and guidelines, but when a new one comes out, we have to make sure everything is now consistent and what changes may have been effected by that.

MR. JOHNSON: Let me go on here because I don't want to take a lot of time here. Let me make a couple of--another point is I would take exception to your definition of objective evidence that you put forward.

It is my understanding the main parts of a quality assurance program is, one, defining procedures for an appropriate test, then conducting the investigation under that particular set of procedures, and then, thirdly, developing a record of documentation that can be traced from the development of procedures right on through to the investigation.

Now, what that does is define a pedigree of the evidence. It does not say that the evidence is objective.

MR. BLANCHARD: I'm not sure. Are you asking me a question?

MR. JOHNSON: No, I'm not. I'm just making a comment. I have a different definition of quality assurance than you seem to have.

MR. BLANCHARD: That's fine, because you're--perhaps you've evolved into that definition because of your past involvement in certain type of nuclear activities that you have supported.

On the other hand, I think Warner said he wanted to find out how the Department manages this program, and the issue resolution process, and if you read the QARD, it starts: "Criteria 1 in QA-1 is organization." Who's responsible for what, and how is that delegated? The quality assurance program starts with the creation of the manager and how he delegates his work, and how he plans his work, how he allocates his resources.

Procurement, resource allocation, roles and responsibilities are all part, as is corrective action, of the management side of the QARD. It includes a lot more than just those things of, how do you do your work, did you follow your procedure, and does the procedure show that you were trained and it's in the records department.

You're right that a part of that is a very rigorous thing that the quality assurance audits of all our contractors cover, but those audits also cover all of those other aspects. They're a kind of, if you will, it's like a birth-to-death process for everything you do that affects waste isolation and worker safety in the technical side of the hierarchy.

MR. JOHNSON: My point, though, was I took exception with your definition of objective evidence. I don't have a problem with what you've just said about QA, that all QA does is define a pedigree to the evidence. It does not say anything about whether the evidence is objective or not.

MR. GERTZ: I think what Max was trying to say--and I'll put words in your mouth, and correct me, Max--is that we have records that is objective evidence that something existed.

Whether the evaluation of that data was objective or not is always a matter of debate.

DR. NORTH: I think we should avoid getting into further discussion on this detail. We're about 35 minutes late at this point, and I would urge that we push on with Jerry Boak.

DR. BOAK: I'm Jerry Boak. I'm the Chief of the Technical Analysis Branch of the Yucca Mountain Project. I'm going to talk about how the defining issue we've taken as our item to track through this process, climate, climate change and infiltration, how we get from a regulatory basis for that to the technical issues, and how we maintain some kind of contact between the technical scientific issues we want to talk about with respect to that, and the ultimate analysis that gives us some comfortable feeling of reasonable assurance that we can meet the regulatory requirements defined.

And so, I'll talk a little bit about the regulatory basis for the issue we're talking about, then comparing--contrast to views of that issue, the technical and the regulatory, and then talk about how performance assessment tries to provide a bridge between those two so that we can evaluate the suitability of a site like Yucca Mountain with respect to issues like climate change and infiltration.

The regulatory basis does, in fact, come from the regulations that govern this process, the repository development, 10 CFR Part 60, the NRC's technical criteria, and 10 CFR 960, the DOE's general siting guidelines, and it's important to point out that there are significant relationships between the two.

The overall system, of course, does to some extent guide our assessment of the performance of the Yucca Mountain site and issues of climate change, scenarios for climate change, and the geohydrology are certainly importantly wrapped up in not only the total system performance, but in the subsystem goals that we have for the evaluation of Yucca Mountain.

But then, in 60.122, there's a list of favorable conditions which we must evaluate, and potentially adverse conditions which we must evaluate, and these, then, are reflected, also--and, in fact, the development of 960 was conditioned by the insistence of the NRC that we make sure that we, in fact, have some intercomparability, what's sometimes been referred to as nexus, between the 960 guidelines and the 10 CFR Part 60 criteria. So there are these conditions in 122 which are, to some extent, parallelled in 960.

I've given you the text of those guidelines with respect to climate change and, in part, with respect to hydrology in the unsaturated zone as an additional slide in there, or as an additional text page in there, but I won't be putting it up as a view graph slide.

The site characterization plan, then, attempted to capture the essence of those regulations, identify the relevant issues that were involved, and break those down successively into sections; geohydrology, in this case, and climate, and then, ultimately, down into information needs which were intended to be captured by the various study plans on unsaturated zone infiltration, percolation in the unsaturated zone, and then on the climate side, regional climate, future climate, and the Paleoclimate.

There are, of course, others that will provide some information for aspects of this, but that is, in a schematic way, the way the site characterization plan attempted to capture the relevant regulatory requirements and parcel them out to the technical disciplines.

In the early site suitability evaluation, which we conducted in the course of 1990 and '91, we evaluated--we tried to pull together some kind of picture of the status with respect to all the issues that are addressed in the siting guidelines, and tried to pull together the information we had from various performance assessment exercises at the time, to try and capture a picture of where we stood with respect to some of these issues.

Where the site suitability team felt that we could make a relatively high-confidence finding, there are two of those high confidence. One is unsuitability, and the other is high-confidence suitability. There were recommendations made. DOE has not reached a final position on how it feels about those, and I think we will probably be continuing to evaluate that.

In between was the one finding that constituted a finding of uncertainty. It was the lower level suitability finding, which meant, simply, suitability for further characterization, and a demand that clearly indicates that uncertainty continued to exist about these issues, that the issue was not resolvable at this point, was the fact that the demand was made of the people who put together the suitability report that they indicate what data needed to be gathered in order to actually reach a resolution on that issue. It was a point that has been widely misunderstood by reviewers of the site suitability report.

For the climate issue, those areas of study which were identified included further detail on the role and magnitude of future climate change. That's in a relatively primitive state as it relates to Yucca Mountain.

Then, given some kind of idea of the expected role of climate change, what, then, would be the effects of that climatic change on the surface and subsurface geohydrologic systems, and then, finally, what consequences do those changes have for performance of the site?

When we look at the study plans that are there to resolve some of these issues, they tend to be focused on relatively low-level technical details, issues about what likely changes we might find in the water table, what changes in the rainfall, changes in the character of the soils or in the vegetation on those soils that might affect net infiltration and recharge through the repository block to that water table, wherever it might stand.

On the other hand, the regulatory objectives are much higher level measures of groundwater travel time, waste package performance or release from the engineered barrier system, and then, finally, of course, the total system releases, and it's not always clear, the connection between the two.

This is the performance assessment objective to provide that connection. I'm going to go into the bullets that are on this diagram in substantially more detail in the next five view graphs, but, in essence, performance assessment has this series of tests that it must do in order to get from what we have learned about the behavior of the site, to some kind of estimate of the performance against regulatory criteria.

The first step in that is the determination of how performance assessment can go about getting an estimate of performance against the criteria, and that involves abstraction, gathering together what we do know about the site, and figuring out what are the really critical parts, where, in fact, are the likely failures is one of the questions we've asked, because we're wanting to identify those areas where the site might be likely to fail, where the site might be likely to be unsuitable, and that abstraction process can go to pretty substantial extremes.

In fact, in our most recently completed total system performance assessment, we took the entire issue we're talking about today, climate change and infiltration, and rolled it up into a single distribution which represented what we estimated would be the percolation flux from the repository block downward. So we essentially did away with everything up above the repository and represented it by this distribution, which represented an expert judgment based on interaction between performance assessment and site people about what the likely range of recharge fluxes through the repository block would be, given our estimates of what the current flux might be, and what we might expect to change in the course of the next 10,000 years.

That was then input into a pair of relatively simplified models for how the site might perform, given that input, to produce the result that we ultimately got. It was, of course, combined with a great number of other sub-models. That's the second task of performance assessment, which is to develop the various models and codes that are needed in order to derive the performance estimates, and you see here, of course, we did have to address issues of volcanism, human intrusion, and then, of course, the aqueous and gaseous releases, which connect more directly to the hydrologic issues we're talking about here.

An important aspect, of course, of the combined curve that shows up up here is that our combination approach was intended to make sure that we do, in fact, capture the fact that volcanism, when it occurs, will be combined with some hydrologic system, so that the combination process--which I won't go into in detail--is described in the total system performance assessment reports, but it's an important aspect of it that we do, in fact, combine to get some kind of representation at least for those aspects of the site that we were able to model in a reasonable time for this iteration.

The next step, one which we have actually only completed in the course of the last year or so, actually, after we got a fair part of the work done on the total system performance assessment that was published in the Sandia and PNL documents, has been looking back at the large mass of data that was generated there, and trying to determine what are the really critical, sensitive parameters of the site, and this one showing the strong correlation between percolation flux and total system releases; in fact, 97 per cent of the variance in total system releases is directly tied to the flux value.

That's, in part, because so many other variables are strongly affected by the flux that goes through the repository, but, in essence, this is the critical thing. We've known all along that hydrology was important. How much water gets on the waste is the critical thing. It's simply captured very carefully there, and it's that that allows us to assign some priority to what kind of data needs to be acquired from the site, and it's also that that allows us to go back and refine our models, to try and capture more of the relevant details, look at some other alternative conceptual models so we can hopefully move from the kind of very simplified model that was portrayed in the site characterization plan, where we have relatively homogeneous, laterally extensive units that are thick, and are assigned essentially uniform properties.

We can move towards something where we understand some more of the details of how certainly these units in here are subdivided, how some of the details of the ways in which they behave and some of the detailed properties of them strongly affect how we would go about modeling the hydrologic properties of the site, and how we would carry that through.

For example, something that Alan Flint has talked to you about, this trend of increasing saturation reflects, actually, a systematic variation in the rock properties in this Tiva welded unit that reflect, in fact, the genesis of the rock itself, and application of some of the insights we've gained from that, we think will help us fairly substantially constrain the way in which we model those hydrologic processes in our next iteration of total system performance assessment. The regional consistency of this trend has really important implications for the way in which you might go about modeling influx into the repository zone.

Then, finally, performance assessment intends to provide increasingly better assessments against the regulatory compliance criteria, so that we can move from something as simple as the not quite back-of-the-envelope calculations of Sinnock, Lin & Brannen back in 1984, to something that we think is a little bit better, reflects more effectively not only the variance we understand to be there in the properties of the mountain, but also a little better capturing of our uncertainties about some of the properties which we captured in our 1991 total system performance assessment.

That leads to some questions about where and when are we actually carrying out some of those interactions that I described in those last five steps that involve negotiation and conversation with the site characterization personnel, so that, in effect, our models are, in fact, improving as site characterization goes on.

I'll mention just one. During the course of the PACE-90 exercises, one of our performance assessment people identified an interest in looking at the way in which boundary conditions for the models that we had for infiltration and for recharge affect the results you see at the repository horizon. She began to work with Alan Flint on a program of sampling, characterization of those samples, and then modeling of boundary conditions, of different boundary conditions along a series of cross-sections through the mountain, one of which begins in Solitario Canyon and runs over along one of the east/west valleys here, another of which actually is a transect down Pagany Wash to look at how variations in influx and recharge in a wash might change our view of how the system performs, as well as a cross-section across that wash to look at the fact that we have alluvium in the valleys, we have a mix of talus and some bedrock exposure on the slopes, and often, quite often bare rock exposed on the ridge tops.

Does that have any effect on how we ought to be modeling the site performance. In our TSPA-1991, Sandia's approach was to look at six different columns, each of which had different thicknesses of lithologic units beneath the surface. This was an attempt to try and understand how some of the regional variations that we might see, and where rain actually falls and how it gets into the mountain, whether that would affect, whether that might change our view of that original distribution of flux that we showed totally homogeneous across the site.

That study has been a little bit on hold; in part, due to other programmatic priorities, and in part, because the person who initiated that work with Alan has subsequently left PNL and gone to graduate school, and then taken a leave of absence from graduate school. So some parts of it have been a little bit on hold for that, and for other programmatic reasons.

There are a couple of other places where we think that kind of interaction has gone on and been quite beneficial to us. One of those has been in the volcanism program. From the time I came on the project, there was a fairly strong interaction between Los Alamos people and Sandia people doing the performance assessments to try and capture and refine and get estimates of the effect of volcanism on the performance of the site.

That, I think, has reached a fairly mature stage. There are still some things that performance assessment needs to do in the way of modeling, but I think we've captured, at least for the simple volcanism test case that we've done, most of the information that's available from our colleagues at Los Alamos.

One other place that might, in fact, produce a substantial change in our estimate of performance is something that got mentioned earlier today, the change in the low corner of the repository. A fair number of the higher-release calculations that we have actually come through that one column that has the thinnest section between the repository and the water table. The elevation of that, by a substantial distance and changes in the thickness between that and the water table might substantially affect the estimate of performance that we would get out of that, and we have been working with the people on that horizon, and trying to refine our estimates of the thickness for those estimates.

We also have been conducting a series of performance assessment road shows. We took a fairly summary presentation of our total system performance assessment and spent a day sitting down with people at Los Alamos, and another day with USGS-PIs and saying, "Here's what we did last time. Here's how we're going about our next iteration of total system performance assessment. Tell us how we could improve that. Tell us what you know now, what you've gotten in the way of data or what you've seen in the way of the models you've been running that says we ought to be modeling it differently."

I think those were very fruitful discussions. We learned a great deal, got some interesting ideas about ways we might actually improve that next iteration of total system performance assessment, and some longer term views of how the subsequent iterations might be improved.

How do we get to decisions? How do we provide input from performance assessment to managers about where issues stand? In my first physics class at the Taft School in Watertown, Connecticut, our physics teacher took the first day, the entire first lecture of the course and put a big T up on the blackboard, and that was truth, and then he put a T with a prime after it, and that was what science can manage to do. He pointed out that the objective is to provide successively improved approximations of that ultimate T up there, and I didn't realize that that was my first introduction to the questions of model validation and confidence that have plagued this program at least since I've been involved in it. It was also my first introduction to realizing that maybe these--when I thought about this recently, I realized that he was introducing me to an issue that's been around for several hundred years, and we're now encountering it in this program in spades.

I think what I want to point to is that as we go to progressively improved degrees of realism, from what I might call the Las Vegas model to the Monte Carlo model, we do get some improvement in confidence. There probably are ultimate limits to that. Management's desire is always to have something that rather than lying here, probably actually lies up here beyond the achievable, but it also has a desire to produce that at a cost that probably only gets us over this far, and you can see that the intersection of those two desires lies somewhere off the curve of reality, and it's that that drives us to abstraction.

That leads me back to the second pyramid you've seen today. I began to think about that it is a 10,000-year problem, but I'm not sure that you should be seeing quite so many pyramids. This one actually dates to a slightly different archeological epic. Felton Bingham assures me it's been around since the earliest days of performance assessment.

What allows you to take something that doesn't have all the details in it and still get a reasonable answer is this abstraction process. At the bottom, we develop very detailed mechanistic process models for all the relevant processes that we can think of. These are limited in scope. They don't answer the whole question. They can't give you an answer about performance, but they can give you an answer about what's relevance and what really matters, so that you can hopefully simplify a few things, maybe even ignore a few details, leave them behind, or represent them in very, very simplified manners, and produce sub-system models, and, ultimately, a total system model whose view is comprehensive.

It covers all the bases, all the relevant bases, but it doesn't have to cover them in the excruciating detail that they are covered down in here.

And if this abstraction process is carried on appropriately, then you will have a reasonable basis, a solid pyramid, probably constructed of paper, down here that will support the conclusions you draw from this model up here at the top.

So my way of portraying that is to say that, in the end, the performance assessment's job is to ignore most of site characterization. However, before you go and ignore, before you turn your back on a detail, you have to know that it's not the little demon that's going to jump on you and chew you up in a license application. So it's not, strictly speaking, ignoring the details, it's saying, "I no longer need to pay attention to you, because there are other little demons that I need to pay attention to."

That's probably the hardest job in here, is making sure that you feel comfortable, and I would say that probably it's a question that is never answered in a fully quantitative manner. It always comes down to a reliance on certain judgments, certain very careful judgments that are made about when you've gone far enough.

And my type example for how far is far enough has been, over the past few months, an article I read in Science

News, in which a group of investigators have demonstrated that Newton's formulation for the attraction of two bodies for one another was good, to better than two parts in ten thousand.

Now, for most geophysicists doing gravity measurements in basin exploration and minerals exploration, we've been quite happy to rely on expert judgments about gravity for quite some time, and even for orbital calculations, we've been able to rely on those things for quite some time, but it was interesting to read another article which said that recent calculations of orbital motions for the earth, if you try to carry them beyond ten million years, are chaotic, and you can't predict the orbital motions. Nevertheless, they've been stable for four and a half billion years.

Luckily, the problems we have to solve don't involve long time frames or untested models, so we don't have those problems.

Thank you. Are there any questions?

DR. NORTH: Questions?

DR. LANGMUIR: Jeremy, you talked about percolation flux and how one had to characterize the mountain in order to get a total system performance analysis. I didn't hear anything said about how the effect of thermal loading will impact infiltration.

The sense I got when you were done with the analysis to performance, when you determined how much infiltration might occur, if you could, but obviously, if you add heat to the system, that's going to affect, probably in a largely unknown way, whether this is going to perform adequately or properly.

How do you factor that into how you're doing this?

DR. BOAK: I guess I would say, again, there is this negotiation that goes on with people who are involved in the site modeling, and in--well, it isn't simply the site modeling, of course. We have to incorporate an understanding of the waste form itself, of the waste package, and all of those involved interactions, not just with site modelers, but with the people, the engineering and materials people to get those models in there so that they're appropriately incorporated.

I would say that that's another avenue where we're going through that whole series of steps. How should we model this? Talk to the people who are responsible for that, and a critical part of that is, how should we model the thermal effects of the repository? So that there's no doubt there's an effect there, and to get a reasonable estimate of performance, we end up requiring a full understanding of--an understanding of the system behavior so that interactions like the ones I've talked about with Alan have to go on with the Livermore people at Livermore, and the M&O people who are developing the models for corrosion, but also developing models for--it involves interaction with folks like Eric Ryder, who are doing the thermal calculations.

I haven't had a performance assessment meeting where Tom Buschek wasn't there, and so interaction with his aspect of it, which is actually funded primarily through the near-field environment part of the waste package, is another critical part of that, and we certainly listen a lot to what Tom has to say. It follows a parallel track to this.

Again, the total system evaluation ultimately relies on having all of those pieces properly represented, and properly abstracted.

DR. NORTH: Further questions? Leon Reiter?

DR. REITER: Jerry, the talk is about this issue of climatology, hydrology, and the effect and the use of performance assessment. I have sort of two related questions.

One of the things you pointed out was use of performance assessment to identify key parameters. Now, the 1992 Energy Act seems to be thrusting everybody towards individual dose. We don't know to what extent that will actually be, but certainly, it looks like it increased emphasis on that, and we've often heard the statement that if you go to a individual dose criteria, a saturated site is favored over an unsaturated site. Do you have any--and I've looked at your various measures.

Do you have any indication or plans to look at how individual dose might affect the sensitivity of different parameters, and when it could lead you to different conclusions than you have now?

And the second question again has to do with performance assessment. I couldn't help notice that in the response to the National Academy letters, a letter from the Department said that, in response to concerns about drilling more holes in the saturated zone, that the response said, "Well, we don't really rely much on the performance of the saturated zone. Therefore, though it would be interesting scientifically, we don't think it's worth the money to expend the extra effort beyond that which has already been stated."

You know, back in the Calico Hills risk benefit study, one of the conclusions that came out was that the saturated zone was an extremely important contributor to waste isolation. How is performance assessment looking at these kinds of issues, these sort of hydrological issues?

DR. BOAK: Well, we do have the saturated zone. There was a limited modeling of the saturated zone in TSPA-1991. I don't know that it was sufficiently advanced to really start answering some of those questions particularly well.

We are improving that in the course of the next iteration. I hope that we can address some of those questions about how much performance should we be putting on that saturated zone. It's still, at least in the SCP baseline, we're still not counting on the saturated zone for a whole lot of performance.

On the other hand, not only the Calico Hills, but also the hydrology peer review team said we ought to be looking back there because there may be better performance available to us there, and I think Dale Wilder, in a paper for the high-level waste conference next week stakes out the radical high ground on that, claiming that we ought to place a very strong reliance with respect at least to the groundwater travel time on the saturated zone.

But we do intend to look at that. It's another one of the sensitivities I'd like to see evaluated in the course of pulling this next iteration of total system performance assessment together.

DR. REITER: Does that mean that the response to the National Academy is not yet a final response, that you may change your allocation?

DR. BOAK: I think in the way of allocations, there's always the opportunity for change.

MR. GERTZ: But you're adding you'd like to be a little more aggressive in that. I think we'll be changing our allocations continually as we gather more data about the site, so I think as we gather more data, we will be changing allocations.

Our response to the National Academy, I think, revolved about the number of holes we have now planned, and, of course, we did a hydrology peer review that they were, I guess, unfamiliar with at the time, by Freeze & associates, an extensive peer review, and they thought we probably were gathering about enough data for where we stood now. But, yeah, the program probably will change.

DR. BOAK: Yeah. Actually, as I understand it, the National Academy panel was not aware, and that was partly, as I understand it, a function of Carl's desire to have the National Academy--to not be too close to the National Academy panel, so we only recently discovered that they had not seen the hydrology peer review report, and that might change their attitude. We don't know for sure.

It seems to me that in the saturated zone, that the questions that we have about it, we tend to think that that tuff aquifer is the main one we're going to be concerned about; that there are some things we want to understand about it, but that it's still, at this point, given our current strategy, not likely to be something we count on for a major part of the performance of the site.

Now, the first question?

DR. REITER: The individual dose.

DR. BOAK: Individual dose. TSPA-2 will involve individual doses, so presumably we will be looking, at least at a first cut, at what the effect on individual dose is. I think that the main thing that happens is that the whole focus that we saw in our total system performance assessment, where the highest releases we saw were from the gaseous phase, if you start cutting that--if you start putting that into a dose calculation, Carbon-14 kind of vanishes as a concern because of the dilution factors.

So you'd be looking at a whole different suite of radionuclides of concern. We might be, in that case, actually more interested in what goes on in the saturated zone, because the nuclides of interest are, in that case, nuclides like Iodine-129, possibly neptunium, depending on the solubilities, and technetium. So I think you'd see very different sensitivities. You'd be asking questions about very different parameters if we went to a dose-based standard.

I hope that we'll have a little better view of that when we get done with the next total system performance assessment, but it's quite a big can of worms to start looking at how we're going to go about modeling doses. You get into a whole range of questions that have been raised internationally about what's the right biosphere to model them to when you start getting into trying to figure out how to model the doses for 10,000 years or more. I expect we'll be fairly simply in what we do in the way of dose modeling this time around.

DR. NORTH: I think in the interest of time, we want to cut the discussion off here. I personally have a lot of questions I would like to ask about how the second iteration of total system performance assessment is going to change as a result of what you've learned. You've mentioned in your presentation the effect of terrain. You got a question from Dr. Langmuir with regard to the thermal loading issue. We've had some more discussion of the saturated zone.

But I'm going to try to heed my own advice and and stay out of the detail, and describe this essentially as a coming attraction to get more information on the interaction between the site characterization effort and the plans for iterating the performance assessment, and I feel very encouraged that we seem to be getting at those details, identifying important issues for further study, and that would suggest that the management process is going in the direction we'd like.

So, with that, let's go to the next speaker, Dennis Williams. I think we will try to do one more talk, and then break for lunch, moving the last presentation by Thomas Statton over into the afternoon.

DR. BOAK: Well, I certainly hope our sequel can live up to expectations better than many sequels can.

MR. GERTZ: I think this is Dennis's first time in front of the Board, and for those of you who remember Euell Clanton, Dennis is kind of Euell's replacement, and I don't if that's--how is that possible? That's right. I don't know, but we're glad to have Dennis on board.

MR. WILLIAMS: I'm in site characterization. I supply the demons that Jerry has to deal with.

Dennis Williams, Chief of the Site Investigations Branch, as Carl mentioned. The topic is integrated site program. Possibly, to help you out a little bit on your schedule, we did re-format this presentation a little bit. It's going to be a joint presentation between myself and Tom Statton. Tom is the Manager of the Site Characterization Group with the M&O at Las Vegas. We had anticipated that this joint presentation might show, in some small way, how we integrate the site program, and we'll give it a try at making this presentation.

DR. NORTH: At the expense of integration, however, could we try to find a natural break point in 30 to 40 minutes?

MR. WILLIAMS: We may be done in 30 minutes.

DR. NORTH: Great.

MR. WILLIAMS: A little bit of the overview on it, I'll talk a little bit about the framework of our integrated process, the planning process. We'll get into some details of the site investigation elements and interaction with other elements. That'll be largely the area that Tom talks about.

I might mention that because it is a joint presentation, we'll be both available to entertain questions at the end of the session.

I'd like to put it back into Max's framework. He gave us a little bit of an overview of the entire program. Basically, where we fit in is in the part on test planning, defining controls, how we get it to the field, preparing our engineering and scientific instructions, the test facility that we construct out there, be it a borehole, be it the ESF or whatever, collect our data, monitor the impacts, evaluate our results, interact with everyone to see whether or not the results are adequate for our uses, and then roll it into the PA that Jerry was talking about, other topical reports, and things like the design. The design of the ESF is one of the big areas that we're dealing with at this time.

We basically roll everything back around and start it over again in a reevaluation of the objectives if we haven't satisfied them the first time around.

In here on the front end of the process, we'll talk about it a little bit more. We have our long-range plan, we have our annual plan process, consolidated work scope, all these different issues up in here that I'll talk about preliminary to actually going to the field with our test planning.

As Carl pointed out earlier, we have a lot of challenges associated with this project. I don't know whether challenges is a good word for it. Some of it's a little bit of a nightmare, but we do have a framework for planning and integration. We have a process that we use. This talk will emphasize the process. I like to think that planning and integration can never be defined as final products. The emphasis is the process, and we have a constant changing level of detail that we roll through as we get in and out of resolving various issues.

Kind of a simplified flow of major elements of integration or an interaction process includes the project framework, a planning process that flows down from that, site investigations. We have various influences on our prioritization. I put that in a great big arrow because that has a great big impact on what we do.

The flow is very simple. Whenever we start getting into the elements, though, I think you can see that it is not so simple. We have a variety of things that drive us. We have the program milestones, not really a schedule-driven process, but a milestone process. We look at what milestones we have to satisfy somewhere along the way in the program. A lot of these are associated with the environmental impact statement, site suitability, license application. We have regulatory issues that we have to provide input to, performance assessment, as Jerry pointed out earlier.

Additionally, we have the things that really fall more into my bailiwick and background, the design, exploratory studies facility, the advanced conceptual design, license application design; into construction, the construction inputs into the exploratory studies facility; and, of course, the site investigations program, supporting our ESF testing, and supporting our surface-based testing. Larry likes that part of it.

To do this, we basically deal with integration at all levels. That's the way we try to think of ourselves, as the ultimate integrators of the program. There are various high-level requirements, especially in design and PA, that's starting to be integrated more carefully by the M&O. The area that I really work with and is more or less my baby is the site characterization program integration. I wanted to point out to you a few of these integration efforts, a couple of them that are quite mature, they've been around for awhile, and some of the new efforts that we have underway.

Hydrology integration has been with us. I think they were chartered in late '90. Claudia Newbury is the DOE lead on that, or DOE representative on that. I think Dwayne Chesnut of Livermore is the Chair right now. It's a rotating chair basis. They meet quarterly. The integrate the UZ and the saturated zone hydrology programs. They've had things like groundwater travel time workshops, and they work carefully with the, or closely with the geochemical integration task.

The geochemistry integration, that's led by Ardyth Simmons, also of DOE. It involves all of the technical participants. They have monthly telecons, and they try to integrate the geochemistry program in part with the hydrology program and the waste, or the thermal technical participants.

Geophysical integration, that task force is a relatively new initiative that we've undertaken in site investigations. It was chartered last October. Mark Tynan of DOE is the chair on that at this time. It involves the SAIC M&O and USGS technical participants. A couple of major efforts that they've undertaken in the last few months has been a VSP workshop, and they're preparing for a NRC technical interchange in June of this year.

Drilling integration, this is something that we got started last fall. We had a couple of workshops; one in October and December. It's led by Bill Distel out of the M&O. Basically, what we're trying to do is get all the relevant participants in the drilling area together, such that we can tie the drilling to the Level 2 and Level 3 milestones, get us a new workup on the near-term drilling schedule such that we can more efficiently carry out the drilling operations.

Integrated core logging, we are attempting to get the core log to a position where we have basically one participant develop the basic log, and at that point, then, we will have the other technical PIs overlay their particular requirements on that core log and give us a much better logging product for the project. This happens to be the brain child of Dave Kessel. He's our new PI out of Sandia for soil and rock. He and Tim Sullivan of the DOE are leading up that effort.

In activity integration, we started last year our work-scope consolidation, which sets up on the front end of our test planning process, such that we will look at every element of potential activity in a borehole or in a particular test in the ESF. We will combine all that information, and that's what we will carry forward into our test planning and our job package process. This is one of the areas that Tom will talk to us a great deal more about in the end of the presentation.

Data integration, we have technical product feedback, both to and from the users. Typically, here in the last few months, we've had issues of boreholes that encountered rock quality conditions along the ramps. What we've been doing is working with the designers. That leads us to additional drill holes. In the case of the Bow Ridge Fault now, we have the first drill hole in there. We have the rock quality conditions. NRG-2 was drilled along the ramp alignment.

I'm sorry. Tom, would you like to take this over for me?

MR. STATTON: I think where we were headed was the discussion of sort of the iterative feedback, based on the information gained, and then new information requirements.

Specifically, NRG-2, which is a north ramp geotechnical borehole, was drilled looking for ground conditions in the vicinity of the Bow Ridge Fault, finding some less than desirable ground conditions near the ramp location. An additional borehole was planned to help better define the extent of conditions so that we could better define tunnel requirements or support requirements if the tunnel was to progress.

The program goals are being defined for us in a variety of ways. First, we've got a suite of milestones that are set out; and second, we've got a new articulation of a long-range plan. That new articulation of a long-range plan isn't to say we didn't have a plan before, but it says that each year that the layout and sequencing that went on is fulfilled or unfulfilled as a function of either our readiness to perform a task, our ability to perform a task because of permitting, or our ability to fund an activity; i.e., it coaxes a change in the long-range plan, and that's a process that's underway at present and it's key in the element of our planning process that we'll get into when we talk about the annual planning process.

Part of what we wanted to talk about today is the suite of prioritization schemes, things that influence the way we prioritize what tests are to be run and how we deal with those tests, and finding the feedback between the tests that are put in place, and the fulfillment of a long-range plan, the long-range plan being derived dominantly out of the SCP.

Now, we'll start with this in terms of long-range plan, and we'll come back to it in a couple of other forums. The first forum was to say that in developing our long-range plan, we decided it's easier to capture the progress being made, and it's easier to relate to other segments of the program if we can sort of capture or state our current state of knowledge periodically throughout time.

Now, we intellectually did that, saying, well, let's begin with some preliminary models, some interim models, and some final models, suggesting that this is where we're headed in the short term, and this is where we're headed at the conclusion of the project.

Well, in taking a look at what these models are--

and I don't want to confuse those models with the ones Jerry was talking about at all. These are, in fact, conceptual state of knowledge reports that the scientific community likes to talk about in terms of their conceptual model, but there's a whole suite of sub-element models that fit into that concept, and each one of those sub-element models--in fact, here is a sub-element of the hydrology model--has sitting in it a whole suite of topics to be addressed, and the plan has as its basis development or understanding of those topics, and then, as you'll see as we get along a little farther, it's the sub-elements that fulfill these topics fitting into the model, that then provides the basis for the project to move forward.

It's the description of the contributors here that, in fact, provide a mechanism for us to integrate this program, the investigative of the program into the greater program; in other words, laying out a distinct plan such that the performance assessment activities can target in time when our state of knowledge is going to change, and what that change in state of knowledge is intended to convey.

Here we've taken just the notion of the interim plan. We've taken a look at the, for example, the inputs to that, the preparation of that plan, and what the outputs of that are to be, and the fact that they go to the performance assessment development, that they go to the annotated outline, that they feed back into the program and say, my expectations were or weren't met, and I either need to carry along as planned, increase the effort in a specific area because I didn't learn all I expected to learn, or based on some unexpected returns, I need to revise my attentions in the near term for that program.

I think where we were headed here was that the prioritization effort that we take in looking at these annual bites, or these annual plans--and maybe I should set that up a little better. The annual planning process, per se, is to take an annual slice out of a fairly well-developed long-range plan, and that's really the intent of that, is to articulate what is to be achieved in any given year, and we've got a variety of influences on that.

One of the influences can come from an outside peer review, for example, and as we wander through, we'll see that a peer review that was conducted back in the '89-'90 time frame suggested that we knew very little about the feed stock for the flux at the waste package, and that was a prompt, obviously, to get into the infiltration studies, which are the infiltration studies that I think Alan will talk to us a great deal more about.

For purposes of time--and we'll skip a later view graph--the next step in that was that we began that process of putting in these natural infiltration or neutron boreholes to examine the mechanics of this infiltration, and, in fact, we found that we had a specific opportunity in that we had a wetter than normal year, and the natural infiltration program was kind of discretely broken into two parts.

Well, given the opportunity provided by nature for a wetter than normal year, we dragged the second part of that natural infiltration into the present, such that we could get both segments of that program going at the same time: Number one, providing additional information to the modeling efforts that were going on in performance assessment, but, number two, to sort of expand the opportunity, expand our database based on the fact that the year was, indeed, wetter than normal.

There are a couple of other things. Max, this morning, talked about test prioritization tasks, the early site suitability evaluations. I know you've heard before about the integrated test evaluation. To that, we've added some design requirements and some performance assessment requirements, trying to get us to where it is we need to be prioritized, but each of these contribute into the way one prioritizes a specific set of dollars into a specific set of tasks to be conducted in any given year.

Now, there's very clear influences on prioritization that come out of the budget. I mean, given a fixed number of dollars, the menu gets significantly shorter as to what can be accomplished. We clearly have outside influences that come from review bodies or oversight bodies, such as yourselves, and we have outside influences based on what our findings are in the field in terms of modifying the program or moving it into a new direction, based on expectations either being met or not being met, or surprises being found.

So, clearly, the process has to be sensitive to all of those, and the pulling into the present in the schedule of our exploratory studies facilities activity, in fact, also have a great deal of influence on our prioritization studies.

Now, I think what we sort of wanted to do--I was wanting to do multi-media--is to talk about what our planning process is and sort of put it in two graphical forms. One of the--I noticed that the title of my portion of the talk was to be the M&O role, and I think to open that, what I really want to say is the M&O in this project, in the management and integration activities that are ongoing, are, in fact, an extension of the DOE, and that's really--that sums up what our role is, and from here on out, we'll talk about some of the mechanics of things we do, but I don't want to be able to draw the line quite as discretely as one might have it, where DOE performs Task A, and the M&O performs Tasks B and C, and a participant performs Task D.

One of the things we've spent a great deal of time doing is pulling together a team that allows itself the luxury of group intelligence, as opposed to individual intelligence, but, in the broader sense, there is a very broad planning basis, and that planning basis comes to us, for example, in requirements or directions given to us from headquarters.

The directions, for example, that we were given from headquarters in 1993 in our planning efforts, and in 1994 in our planning efforts, were--and Lake has gone--they were very specific directions: "Do ESF and, to the degree possible, do a very aggressive surface-based testing program," back again to Larry's balanced program idea.

Well, those are great words, but they don't translate to tests particularly well, and so part of our job is to translate what those broader directions are, what our long-range plan has given us as a more intellectual process of moving forward into specific activities that we're going to do in a given year.

Well, that's down into this annual planning process. How do I translate "Do ESF and do an aggressive surface-based test program," into where it is we're headed here, and we've derived a scheme to do that. Dennis alluded to the work scope consolidation, and I'll try to show you where in this process that fits, and why we have it there.

But the broader planning basis comes from outside organizations, such as yourselves, and from such clear guidance as we get from the Director, and we now had to find a process to get into our annual planning. We had to supplement that process with a way that says, "By the time I've identified a discrete task to be done, I want to make sure that it still fulfills what the intent was that I had in my more global plan to be achieved for that year, and we get into test implementation and, in fact, the evaluation and review process of those tests.

How do I know that, based on the funding and the discrete description of where we are, that the outcome of that test either changes my expectations, changes what I need to be doing, how do I feed that back into next year's annual, a modification of the current fiscal year annual plan, or how does that modify, in fact, the long-range plan which will end up playing back in my global planning basis?"

Some of the things I think we want to walk through are--we'll get back to the role of the long-range plan and where we come from, but how we get our annual project priorities, our interfaces with the other parts of the program, and then we'll look at some of the prioritization schemes.

So if we were to do that, for '93, we identified a suite of priorities, trying to translate, "Do ESF and do an aggressive surface-based program," into some mechanism of now focusing down on which of these 106 study plans, which of the myriad of activities beneath those study plans are we, in fact, going to empower or put in place in the field?

And this is the mechanism we came up with to prioritize, or to translate "Do ESF and do a surface-based testing program," and where we did that is, we said ESF is an extremely important facility to us. We need to put this in the context of the fact that we're still trying to pursue site suitability. Well, what is the ESF? Well, it's a terribly important tool in addressing ourselves to site suitability.

Is it terribly important because of a whole suite of tests that are going to be run in it, per se? Well, maybe not necessarily. I think the most important attribute of ESF in our lives is, in fact, the observational value of getting underground and being able to observe firsthand, not an experiment that we're going to run, but the experiment that Mother Nature's been running for millions of years. How do we get down and how do we read where we are in that experiment, to guide us in the future? And I think that's an important context for us, and that's why getting ESF underway as quickly as possible is important to both the surface-based testing community and its program, as well as the underground testing program that's planned.

So as we run down our prioritizations, we took all of the studies that we had and we put them into a suite of bins, and those bins said, this group of studies supports this activity, and each of these has a description to it, and that description is, in fact, articulated to a better or worse degree in our Fiscal Year '93 planning book.

Now, I do want to say that we were behind the power curve, perhaps, in the development of our fiscal '93 planning book, but we are ahead of the power curve in the development of the fiscal '94 planning book which is underway now, and it's out, in fact, being worked by entirety of the program.

So our translation said, "We understand 'Do ESF,' and the first part of that is to deal with the support activities and development of the ESF."

Next came into mind the fact that we've got a long-term baseline program in place, that redirection of funds can be fairly damaging to the program. For example, if I were to turn off because "Do ESF..." doesn't say, "Run a seismic network," had I shut down the seismic network at an inopportune moment, it would have been a Little Skull Mountain that we weren't quite so sure where it happened.

So, there are parts of that program, whether it's the surface runoff portions, whether it's the rainfall portions, whether it's the seismic monitoring portions that, in fact, need funding.

ESF testing in '93 was not as focused on the specific tests because there were few at that point, and those are being emplaced now. That's the mapping, that's the being prepared for the encountering of perched water, it's looking for water and gas flow activities, but it's more to get those studies thought through clearly, put on the table, and through the review process.

In the process of developing study plans, as technology changes with time, the plan has been to get those study plans in place not years and years and years before they're going to be implemented, but just before they're going to be implemented, so that it is the PI who is going to run the test that, number one, is developing the study plan; and, number two, has the benefit of current knowledge.

Next was our focus on, in fact, the site suitability issues. Out of early site suitability evaluation came one single disqualifying condition in the geotechnical sense that did not have a higher level of finding by that group, a higher confidence finding, I think, as Jerry put it. That effort was, in fact, the groundwater travel time effort.

Now, we talked a little bit earlier about what are we doing about the saturated zone. Well, this focuses, in fact, a great deal on the saturated zone. This is what brought a tracer test, a multi-well tracer test to the forefront and said, that is important to us, it's going to get in the plan, we're going to develop that and we're going to get that underway, and, in fact, that is being developed and about to get underway.

Then we addressed ourselves to other suitability issues, and then we had a suite of other topics that actually came a little lower in our prioritization. One was a directive that came from the then Director, John Bartlett, that said, "Focus a program so that you opportunistically take advantage of the Little Skull Mountain earthquake, its data set, and its ability into play." The limited activities that were then required in the field information gaining sense, in terms of issue resolution, some model development activities and other activities--when I say other SCP activities, it doesn't mean they're unimportant. It means they're unimportant in today's context in the long-range plan.

Now, the annual planning process starts with the capture of a specific study through a WBS element and an activity into a bin that says, this is where that study applies, and, in fact, it's more than just an X. It also tries then to evaluate the amount of resources in terms of real dollars and real people required to get that accomplished, or at least get accomplished the goal that was defined in the priority bin, and the process we then took was to say, more than just putting something on a table, we've got to describe what it is we're doing and where we're going.

So we took it upon ourselves to get not only a description of that study in some real detail, but, in fact, the rationale behind that study, you know, why is that important in terms of accomplishing my annual goal? Why is it important in terms of my progress in the long-range plan? So we need the purpose or the rationale behind that study, the dollars required to do it, and then the other attributes of where we are.

For example, out of the integrated test evaluation, where and why was that important? Was that ranked very high on the list? Am I doing it because it's easy to do and I can't think of anything else to do, or why am I focusing on that study?

The next process in the management process, then, is to define the deliverable, and the way we've approached that is to say, what I need in these deliverable descriptions is a progressively increasing detailed description of the content of that deliverable. Well, that's not only important for a management reason, it's important, in fact, for the long-range planning reasons.

If I want to address myself to an issue, a topical report for issue resolution, if I want to address myself from a different part of the program as to what it is I'm doing, what am I going to do with my next step in performance assessment? Well, I need some way to forecast my progress in a different part of the program, and the intent is to be able to do that through, in fact, a description of what's going to be included in each of those deliverables.

Now I want to go to the one chart that I've been told violates all rules of graphics, but it makes a great deal of sense to me, and I'll walk you through just a piece of this.

This is, in essence, the characterization of that long-range plan. This is, in essence, the intellectual framework for the development of the scientific program, and its feeds into the other parts of the program, so this is, in fact, the testing or site investigation portions.

Now, there's a whole suite of activities that, in fact, are subordinate to that, and they feed to that, so that once we've got an intellectual framework, we can go back, then, and organize all of the pieces and find where they roll up and describe, in fact, what my state of knowledge will be with regards to hydrology at this point in time, given the implementation of that plan.

One of the important parts of that plan is that the users or the demands on that system are placed by a lot of other program elements. Design, for example, has a suite of demands for its progress that come, in fact, out of that long-range plan. I can't, for example, begin what's termed

LAD within the program, without a few concrete design elements. I can't complete, for example, an advanced conceptual design without a few hard-core elements coming out of this program, so it's the interaction or the negotiation through the description of the deliverables, and then the negotiation of the required content of those deliverables that allow this program and this process to allow design to proceed at its scheduled rate.

The same thing happens, in fact, here with the performance assessment. Performance assessment has, through time, iterative cycles. What performance assessment needs to be able to do is plan how it is going to hone various parts of the code being developed, whether it's a total system code, but, more nominally, the lower-level codes that feed that, need to be able to know what's going to be focused on and developed, and they're going to find that, again, through the detailed description of the content of the deliverables coming out of this plan.

The same thing happens with our ability, then, to make the interim site suitability evaluations that Max alluded to this morning, and the feeds from this program to that, so that it can focus on the next important topic to get further development of that evaluation, and, clearly, the annotated outline process is a way of rolling up one state of knowledge.

Now, one thing was pointed out to me this morning, is this little chip here that says "data freeze," and before somebody runs off with the wrong idea, I want to describe what that is.

At the end of this process, we have a requirement of this program, given a successful investigative program and given a successful finding at the site, of getting a nod prior to making a site recommendation report that the data set that has been assembled is, in fact, adequate to carry further, given approval by the President.

Well, in fact, that's exactly what this is. This is trying to say, at this point in time, we are targeting getting a data set complete enough to send out for that review to gain that nod.

I did want to take a minute and talk about work scope consolidation, that Dennis alluded to. And, in fact, what this is, is this is the process of detailed definition of any testing activity, whether it's the neutron borehole program for infiltration, or any other portion of the program. This is the process of defining the specifics of that testing program, testing them, or ensuring that, in fact, they are following the intended purpose as stated in the plan, and then providing that basis so that we can work through the empowerment documents, which are the prerequisites to any of our field activities.

We can then conduct the test, we can get into the evaluation process that we talked about, the feedback from which not only goes to the data catalogues that are being compiled as these activities are going on, but, in fact, the feedback goes back to the long-range plan and the annual planning process so that we can move into the next year.

Now, we don't have to spend much with this. This is, in essence, the same diagram we just saw. Again, the long-range plan in its intellectual sense, one of its sub-components--in fact, the topics that are there, and, indeed, a variety of deliverables that are described within the system in some detail--or will be--that feed to those various topics. So this is now the construct of building this deliverable. So each one of these is nominally another pyramid in the process of building our state of knowledge.

I'll take one view graph out of order, and sort of show you the process by which we began to construct that intellectual framework, and I will say, we're not finished. One of the questions that I don't want to address at the end is what is my integrated drilling program.

The integrated drilling program is currently being examined now as an overlay on top of this long-range plan. So this is where those things come in. Now that we've described the state of knowledge we want to have, we want to go back and fit the resources, which then give us our cost schedule profile.

I think the next one we can skip. An example of both the process and the success of the work scope consolidation, I think, comes out of this one, called SRG-5. Well, we had NRG-5, which was the north ramp geotechnical hole. That's a south ramp geotechnical hole, which is headed in similar place to the map we saw this morning, at the turn at the south end of the proposed repository horizon ESF portion. This hole is on the books because one wants to define the level we're after.

Well, in looking at that in particular, we had a specific drill which said for me to conduct this ramp excavation that I'm currently targeting, I need that borehole. The thought process says, well, when I come back for the Calico Hills loop, I'm going to need another borehole nominally in the same position, only it's going to have to be deeper. So we took the requirements for the immediate demand of the upper ramp design, translated them to the lower ramp design, saying, in order to limit the number of boreholes being put into the mountain, I'm then going to want to extend that.

Well, given that I extend it, what else can I do? I've got few dollars in the program, and I need to be able extrapolate across many parts of the program. So we went back and said, we can take part of the systematic drilling program and we can now roll up all of these things, and we can move the drilling process forward if the planning is done correctly. If I get this borehole drilled with sufficient diameter that I can complete it down at the water table, then I've now consolidated two or three different boreholes into one, which is exactly what Carl was talking about in terms of our cost savings.

So then we looked down and we said, okay, here are a whole suite of activities now that we intend to get information back from for that specific borehole, and my last view graph is that complex one we started with, but where I want to sort of sum up, is that the program we're trying to design is not only cognizant of the various other elements of the program, the annual planning process allows us to identify those ties specifically, and to address the needs of each one of the the program elements, such as the investigative program serves all masters.

Thank you.

DR. NORTH: Thank you. Thank you for speeding up and getting us much closer to our schedule.

Do we have a few short questions that we might address?

(No audible response.)

DR. NORTH: Wonderful. We will then adjourn for lunch, and resume at about one-twenty.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

1:30 p.m.

DR. PATRICK DOMENICO: Good afternoon. My name is Pat Domenico. I will be chairing this session, which deals essentially with the workings of the hydrologic cycle, both past and present, at the Yucca Mountain site.

We will have five presentations, following which there will be a 30-minute discussion. I would ask that the audience reserve their questions for that discussion period, and I humbly ask the presenters to try to stay within their allotted time.

Dwight Hoxie will be leading off and speaking about the integration and use of climatic and hydrologic data and models, so we understand that models do require some sort of data.

(Laughter.)

DR. DOMENICO: Dwight?

DR. HOXIE: I feel like I should have some fancy comeback, but I can't think of one right off the top of my head, I'm sorry, so we'll go ahead talking about data and models, and models that require data.

Actually, the title of my presentation is a bit of a misnomer, because what I'm really going to talk about is setting the stage or the framework for the talks that will follow mine. So what I'm going to try to do is to provide you with a very big picture, and then the other speakers will focus in on particular issues.

So, the objectives that I would like to try to accomplish in a brief time this afternoon is, first of all, is what is the technical issue that we are using as an example to try to resolve? We want to demonstrate the process of resolving a difficult, technical issue, and it is the process that DOE is using and the scientific community within DOE, working with DOE, is using to resolve this issue. I want to define what that issue is going to be that we're going to use as an example.

Then I want to identify why that issue is important to anything at Yucca Mountain site; and then, finally, what is the process that we're going to use to actually resolve it?

Well, the issue actually has to do with climate and infiltration, and when somebody mentioned that to me, when I first heard about that, I got to thinking, and said, well, what does that have to do with anything, and I said to myself, well, but it actually does, it has something to do with geohydrologic system at the Yucca Mountain site. So then I said, well, if you took the very broad view, what are the natural processes that could possibly affect conditions at the Yucca Mountain site; the geohydrologic setting, essentially, at the Yucca Mountain site, and there's two, I would submit.

First of all, we have tectonism; that is, the active process of mountain building, which would affect the geologic framework, which in turn determines the system geometry and the boundaries of the system that we're dealing with. Tectonism can alter the pathways for fluid movement by creating new pathways or closing old pathways. We're talking about tectonism in mountain building. We're talking about faulting and folding, these kinds of processes.

And, if we have active magmatism in the area; that is, volcanism, and so forth, we can affect the ambient thermal conditions at the site, the thermal field at the site. But these are not the issues that we're concerned with today. We want to look at climate and infiltration and its effect on the geohydrologic system.

So what does climate change do, another natural process that can affect, actually, the hydrologic state of our system within the boundaries determined by geologic framework? The hydrologic state includes the moisture distribution within the geohydrologic setting itself, and also, the flux of groundwater that is moving through the system, either down through the system, or up through the system, or in whichever direction it might be going, and what I'm talking about here is the specific reference to the unsaturated zone, below which lies the saturated zone, where we have to deal with climate change effects on the water table configuration; that is, raising the water table, lowering the water table, and also on flow rates of groundwater movement and the direction of the groundwater movement within the saturated zone itself will all be determined by climate.

Well, let me introduce a few terms. I'm going to introduce them very, very generically, very schematically. These terms will be defined in more detail later on by other presenters, but this is just a very schematic cross-section through a mountain with a couple of yuccas on it, so maybe it has some semblance to Yucca Mountain, but we're starting off in our cross-section, we're starting off with climate up here.

You can see the precipitation falling on the site itself. Some of that precipitation will end up, if we have an intense enough storm or a long duration storm, will end up as runoff; that is, water moving on the surface, surface water flow into the ephemeral streams, and so forth, and then disappearing from the system.

Some water, however, presumably will enter the unsaturated zone as infiltration in the near surface environment. Of that, some of that water will be returned to the atmosphere as evapotranspiration, which includes transpiration from our yuccas, as well as direct soil or bare soil evaporation from the site.

But, some of that water also may escape being transmitted back to the atmosphere, and move into the deep unsaturated zone, where it may then become percolation. That's what we call percolation flux, say, at the repository horizon. And, finally, some of that water may actually make it all the way down to great depths, down to the water table itself, entering the saturated zone as recharge. So these are the terms that you'll be hearing this afternoon, and, as I say, they will be defined more explicitly later on.

So, what is the issue that we actually are going to consider? The issue is we have climatic change occurring which will affect the rate and distribution of net infiltration or infiltration over the Yucca Mountain site. That, in turn, will affect the evolution of the geohydrologic system within the site; and, finally, the question, then, what's so important about that? Well, we're mandated by the regulations to answer the question: Is the state of the geohydrologic system and is the evolution of that system compatible with the performance objectives established by the regulations for a repository system? So that's the final performance assessment question that we need to answer.

We're really going to be focusing only on the climatic change, and, essentially, the infiltration rate and geohydrologic system evolution in the discussion this afternoon.

Well, it all looks very straightforward, but there's a rub. There usually is in science, I guess, and the probable is, is that we can talk about climatic change, we can talk about infiltration. We know that there is some kind of relation between the two, but it's not simple, closed form. There is not some little magic formula or black box where we can put in climate change and come out with what the consequent infiltration rate is going to be, so that's the problem.

But, we have an approach. There is a way out, and if we take all of the nuts and bolts, and in regard to these nuts and bolts, the field and laboratory data that we collect at the site--data, right?--then we can feed this into models regarded as tools to use that data to resolve the issue, and let me try to demonstrate how we might do that.

First of all, let me just--and, again, these will be concepts that will be further elaborated during the subsequent presentations, but we have a couple of types of models that we will need to deal with in order to resolve the issues.

The first kind of models are process models, and these are generally small scale--by small scale, I mean small spatial scale, laboratory scale, or maybe a small field plot, something like. They're focused. They only are looking at one or a very few number of particular kinds of processes that might be occurring, and they're usually heuristic; that is, we use them as learning tools, to learn about a process, to test a hypothesis, or something like that. An example would be trying to model and describe the flow in unsaturated fractured porous media, and Ed Kwicklis will be talking about that tomorrow morning, I believe, that kind of a model.

Then we have the systems models. These are large-scale, site scale, regional scale, global scale, perhaps. These are integrated in the sense that they involve many different kinds of processes that might be taking place, and, generally, they are predictive.

These are models that we want to use to predict the state of a system as it is now, as it was in the past, or as it may be in the future, and to give you an example, our geologic framework model that we have for the Yucca Mountain site is a system model. The geohydrologic system models that we developed for the unsaturated zone and the saturated zone, these are systems models. We can predict the future state of the geohydrologic system at the site, for example; and, of course, the climate models that we'll be using to resolve this issue are also system models.

Okay, so what are the specific models that we are going to need to resolve the issue at hand, climate and infiltration? Well, first of all, we have to have the geologic framework at the site, and we, for the purposes of resolving the issue, we've generally assumed that we knew what that geologic framework were, and that we are not going to consider the coupling of tectonic changes with climatic changes, that's all I'm saying by saying that it's assumed given and non-varying with time, just to make the problem simpler.

We need climate models, and these will be both global models for the earth itself, and regional models that would focus or narrow in on the Yucca Mountain region itself. We need to look at climates at the present time, because this provides us a way to calibrate the climate model; the regional model, in particular. We need to look at past climates and model past climates, because that gives us a way to test and validate, or at least partially so, our climate models, and then, of course, we need to look into the future, into the crystal ball of what climate change in the future may do, and as it would affect the Yucca Mountain site.

And, finally, we need to feed all of this into our geohydrologic system model and our process models to develop an entire or complete model for the site itself.

Well, just looking at the system models, what are the data that we actually need from the site to feed into these models? Well, for our geologic framework model, we need the stratigraphy and the structure, and as I've already indicated, we would assume that to be essentially known, and we have a lot of information on that right now.

Geohydrologic system models? Well, we need hydrologic properties, porosity, hydraulic conductivities, this kind of thing. We need to know what the existing conditions are at the site. This provides us a way to calibrate the model. It also provides us with the initial conditions for making predictions of future geohydrologic system evolution. And, of course, we will need boundary conditions on our model, at the land surface, at the lateral boundaries, and, of course, somewhere down below. If we're talking about the unsaturated zone, the water table, of course, provides a very nice lower boundary for the system.

In terms of the climatic models, what we need are forcing functions; that is, what is it that is causing the earth's atmosphere to change, climate to change, such things as solar insulation, astronomical parameters, this kind of thing. We need to know what the land mass distribution over the surface of the earth, and what the elevation of that land mass is in order to predict what climate is going to be at various points on the earth at various times, and, again, we need the present and past climatic conditions as input to the models, to validate the models, to calibrate the models, and to essentially test them.

And I would like to emphasize that there is a very fundamental connection between the climatic and the geohydrologic system models, and the thing is, is that climatic change affects infiltration. Infiltration is an upper land surface boundary condition for the geohydrologic system model, so they are intimately linked. It's not just a matter of a casual link, but there is a causal link as well.

And so, we need to know what climate change is, we need to know how that will affect infiltration. Infiltration then becomes our land surface boundary condition, but we can't rely all together on models. We have to recognize that there are some limitations that we need to take into account.

First of all, our models, regardless of whether they're process models or system models, are really idealizations of real-world kinds of systems. They're also simplifications of those systems. We have to be sure that our models are representing the true governing processes of whatever system it is we're trying to model.

We're always faced with this problem of conceptual uncertainty. Do we really understand the system? Have we identified the really controlling processes, and are we representing these in the models correctly? And then, if we look at the massive amounts of data, hydrologic property data, rainfall data that we would be collecting out at the Yucca Mountain site over the period of the site characterization program, we realize we have an enormous amount of data that's going to have to be handled statistically, and so we, perhaps, represent this data by probability distributions or something like this. How do we fold those into our models?

So that's what I'm labeling here as statistical data uncertainty. How do we handle the variances and the spreading of distributions and all this kind of thing to take those properly into account into our models, which are actually asking just for something like the hydraulic conductivity for this sequence of beds here at this particular point in space and time. How do we handle the statistics of all of this? So these are some of the model limitations that we have to take into account.

But, given all of that, here I just, in a somewhat complicated diagram, would like to try to show you the process that the DOE and the scientific personnel within the DOE program plans to use to resolve this issue of climate change and net infiltration, and its impact on the geohydrologic system at the Yucca Mountain site.

So, starting out at a top/down kind of approach, we start off with the global climate models that have been developed for application to the earth as a whole. These provide us with boundary conditions to apply to regional climate models, which will be looking more at a scale appropriate to the Yucca Mountain site itself, and Starley Thompson, from NCAR will be discussing these kinds of models later in the presentations.

Feeding into the regional climate models is the Paleoclimate studies, which provide a mechanism by knowing what the past climates have been, by using the models to predict what those climatic changes were back in the Ice Ages, and so forth, we can provide a way of validating the climate modeling itself, and John Stuckless will be talking about the Paleoclimate studies that feed into this.

But coming out of our climate modeling studies, then, we get climatic parameters, essentially, for the Yucca Mountain site. By climatic parameters, I mean things like precipitation, air temperature, solar radiation at the site, wind speed and direction, the kinds of things that you would need to put into a watershed model, which would then allow you to predict what the net infiltration would be for a given climatic regime, and Alan Flint will be talking about how this kind of process will be examined.

And then, again, net infiltration, of course, is the land surface boundary condition for our geohydrologic model for the unsaturated zone hydrologic system at the Yucca Mountain site, and Bo Bodvarsson from LBL will be describing the cooperative LBL/USGS site unsaturated zone three-dimensional groundwater flow model which would be using this information as input.

And, as part of this, Ed Kwicklis will be talking about some of the detailed process modeling that is being done to make sure that we are adequately incorporating the relevant processes into our geohydrologic system model.

And, finally, coming out of the geohydrologic system model would be a description or a prediction, essentially, of the distribution of moisture content, water content within the deep unsaturated zone, and also, the amount, rate, and distribution of groundwater flux through the unsaturated zone, and Joe Rousseau is not going to be talking about model results. What he's actually going to be talking about is results from the program to actually monitor and determine the present day moisture conditions, the state of the system at the Yucca Mountain site.

But this is important because we need to know what the present state of the system is, as initial conditions for the modeling of future states of the system, and we also need to know the present state of the system to provide a calibration on the models. So this data is very, very relevant.

And then I just want to indicate, we don't really stop here, because once we know what the moisture distribution and flux is within the unsaturated zone, this feeds directly into site suitability evaluations; for example, looking at the groundwater travel time issue, or doing performance assessments; that is, looking at releases of radionuclides to the accessible environment, and I probably committed a slight error here, because it looks like this is an end in itself.

These are decisions that have to be made. I'm showing these in little boxes that indicates that somebody has to make a decision whether or not the site is suitable, or whether or not the performance is adequate, and so, this implies that if the decision is no, there must be some kind of feed that goes back up and runs through this process again, iterative, like Jerry Boak was talking about earlier today.

So, this is kind of the overall framework that the process, as I conceive of it anyway, that we are going to use to resolve this very important, but highly complex and difficult issue, and I would entertain any questions.

Thank you.

DR. DOMENICO: Any questions from the Board?

(No audible response.)

DR. DOMENICO: Staff?

(No audible response.)

DR. DOMENICO: Well, I have one question on the last slide. Perhaps Alan should answer this, but I'm asking about the watershed model. Is that similar to the, let's say, the Stanford watershed model or other such models that take rainfall and route it through the hydrologic cycle?

DR. HOXIE: I think I will have to defer to Alan.

DR. DOMENICO: Okay, thank you.

DR. FLINT: Can I defer that to Dwight?

(Laughter.)

DR. FLINT: We're developing the system now, and one of the arrows that doesn't point on there is Ed Kwicklis's hydrologic process model and Bodvarsson's geologic model both go back and point back to that watershed model, but it will incorporate the hydrologic cycle. We have to make modifications to what exist. There aren't a lot of models that work well with arid zone systems where you don't have a constant stream flow, so we have to adapt some of those ideas, but there are some U.S. Geological Survey models, large-scale models that we're going to start as baseline models, but we're going through a review process now.

DR. DOMENICO: So it's a lumped parameter model?

DR. FLINT: Basically.

DR. DOMENICO: Things change over time, not spatially, like the Stanford watershed model, or Texas A&M watershed model. Everybody's got one. No?

DR. FLINT: We're going to have a better one. Ours is going to be right, maybe.

(Laughter.)

DR. DOMENICO: Any other questions from anybody on the Board?

(No audible response.)

DR. DOMENICO: Thank you, Dwight, for giving us that fine introduction on things to come.

Our next presenter would be John Stuckless, talking about the evidence of past climatic conditions.

DR. STUCKLESS: Thank you, Mr. Chairman. You read that just like I wished it had been written. The IC on climatic here is abbreviated as an E.

I'm going to start with where Dwight left off and just tell you where Paleoclimate studies fit back in, which is up in here. They are part of the data that hopefully will calibrate and test the models that are developed down the road. The data actually could be showing as feeding into any of the current modern models as a test, if you like.

A little bit of an outline to tell you where I'm going. I'm going to start with the climate program and its goals, a little structure of the data collection, some examples of past climatic change during the last Pluvial, the last 25,000 years, just to give you an idea of how dramatically things have varied in the western United States. We've give you what limited results and plans we've got, and perhaps present some conclusions; rather tentative, I might add.

The climate program is a rather broad program, but it is not strictly within the USGS. We are the lead in it. The Desert Research Institute here in Reno provides us with a tremendous amount of on-site work in southern Nevada, plus, of course, they have a very large database for the area. The University of Arizona--this, by the way, is Paul Wigand, who is actually one of the authors of this, but the cover sheet only provides one space. Rick Forester is the principal person from the USGS. I'm giving this because they would be too technical, and they were not available.

Jay Quade, of the University of Arizona, who used to work for Marty Mifflin, and has done considerable work in southern Nevada; Thuri Serling, who was Jay Quade's thesis advisor some time ago at the University of Utah, is doing some stable isotope work for us, and, of course, the Los Alamos National Laboratories are doing a lot of mineralogy and chemistry in this program.

Well, why do we need the climate program? Well, within the last 23,000 years, we have abundant evidence that the climate has been quite different at Yucca Mountain. There are fossils of some little creature called the Arctic Vole, which is a rodent, and it is not very far traveled, and if he could live down there, it's not like he wandered great distances into a climate where he didn't belong.

A little less conclusive would have been the Columbian Mammoths, which have been found as recently as--in sediments--as 10,000 years old. Once again, they require a tremendously different vegetation than exists there today, but they are a little more far-traveled than the Arctic Vole.

We have found ample evidence of a totally different vegetation around this site, down to 1200 meters. We've found things like white fir and willow, which, of course, require having their roots in water; limber pine; and all of these suggest that in the past there have been some much wetter periods than we currently have today. In fact, the evidence collected thus far suggests that we may be in an anomalously dry period.

So the purpose of the climate program is to provide the data needed to estimate consequences of a climatic change on Yucca Mountain in both the unsaturated and saturated zones. In meetings I've been at previously, there's been quite a focus on the saturated zone, without people thinking about the consequences in the unsaturated zone. The current climate program hopes to shift that emphasis to some degree.

The approaches to be used, that is, to reconstruct climate during the last one million years, maximum, emphasis on the last 200,000 years, and, in fact, as we come forward in time, becoming more and more detailed, and, hopefully, more and more accurate.

We hope to reconstruct some of the unsaturated zone hydrology from uranium series and carbon dating; also, some fluid inclusion work, and in that regard, I neglected Harvard University, where Ed Roedder is doing fluid inclusions.

We hope to reconstruct Yucca Mountain saturated zone hydrology using various geochronologic, isotopic, geochemical, mineralogic techniques, looking both at deposits within the mountain, and deposits in regional aquifer discharge; tufa mounds and playa lakes and such.

Some of that, we hope to be able to reconstruct the atmospheric climate conditions based on both the paleontologic and isotopic data.

Goal. Well, hopefully, we will get fully integrated with the various hydrologic models, and with the regional climatic model, both as a test of the modeling efforts, and in some sort of iterative process where our data are considered in their models as they move them back or forward in time.

Fairly ambitious, how do we plan to collect the data? There are three main thrusts to collect the data. One is called terrestrial paleoecology, and here is the study of pack rat middens and their pollen. This basically tells us what plants were existing on the surface of the earth in the immediate vicinity of the pack rat's home. We get a long-term climate response by looking at the actual fossils that are in there. We can also get snapshots of the short-term climate responses by looking at isotopes within the fossil remains of both the plants and the animals, so if there have been little spikes in the long-term record, we very likely will see them by this method.

Vertebrate paleontology, we can look at the response of the entire vertebrate community, and, once again, in the vertebrates, we can look at carbon isotopes in the mammal dentition, and that will tell us what type of plants are there. There are two basic groups of plants, depending on climate. One's a C-4 cycle, and the other is--I've forgotten the other one--C-3, and whichever plant is dominant there at the time will be dependent upon climate, and what the animal eats will determine what stable carbon isotopes go into their teeth.

The second thrust of the climate program is the lakes, playas, and marshes, the aquatic side of the equation. Here, paleontology is a major player and, in particular, ostracodes. Ostracodes are little micro-fossils that look like tiny lima beans to an isotope geochemist, and they're different from most fossils that are used in paleontology in that they haven't evolved much as a function of time.

Hence, as an index fossil telling you how old something is, they are essentially useless, but they are very, very sensitive to environmental conditions. Some of them like to live, will only live in lakes that have big temperature fluctuations through the year. Some of them live only in areas where groundwater discharge assures them an absolute constant temperature through the year. Some of them like very salty water, some of them like extremely low total dissolved solids water, so by looking at the total assemblage of ostracodes, we can pin down quite tightly what sort of water they lived in, carry it back to the climate. The trace metals in their shells also are climatically and temperature dependent.

The stable isotope geochemistry, oxygen and carbon isotopes, once again, give us a good feel for what plants were feeding the system, and also, what sort of precipitation, regional precipitation was feeding the hydrologic system.

We'll use tracer isotopes, strontium isotopes, in particular, in both the inorganic and organic carbonates to give us something of an idea of flow paths in the system.

There will also be a look at the sedimentology and stratigraphy, both in surface and in core materials. In fact, DRI has been out this last week doing considerable coring in the Las Vegas sediments; and, finally, a data synthesis to pull all of this together to reconstruct the past climate and hydrologic conditions.

Calcite and opaline silica started off as a Trench-14 study, and we found out that we actually could get some other information that was perhaps more useful to the project. Uranium series dating will allow us to date materials between 10,000 and 400,000 years, and Carbon-14 dating, with some assumptions and some tests yet to be done, allow us to get back to about 50,000 years.

We can determine the ages of fracture fillings in the drill cores, and if anything is found in the ESF, we can do it there as well. We can determine ages of discharge deposits.

There'll be some fluid inclusion work done to give us compositions of fluids and temperatures at which carbon was precipitated.

Continuing on, stable isotopes and fluorescence studies. The fluorescence studies are a serendipitous sort of thing. We have found that we can tell the difference between vadose zone precipitated calcite and saturated zone precipitated calcite, and they'll also give us tremendous amounts of looks at structures within the minerals, which may have a real time-dependent record in them. And, once again, the stable isotopes will help us with temperature.

Tracer isotopes again, flow paths telling us, are we looking at a constant flow system which is fixed with time, or has it changed? Has there been a different recharge area bringing in a different composition of tracer isotopes; so on and so forth.

Mineralogy. Mineralogy, of course, has turned out to be useful in differentiating between petogenically started precipitated carbonate and that precipitated by groundwater.

Now, then, let's look at some examples of climate change during the last pluvial, just to give you an idea of how dramatic they have been. We're going to start with the growth of the polar ice sheets around 25,000 to 26,000 years ago. We'll look at some of the lakes in the intermountain west, and they all began to expand around that time. Distribution in vegetation came down in elevation by as much as about 1300 meters, and this is work previously done at DRI.

And if you look at the Great Salt Lake, it's probably one of the best-studied examples of climatic changes in the last 25,000 years. We'll start here with the Stansbury time period, and that's shown here in comparison to the current Great Salt Lake; considerably larger. Polar fronts, which many people believe governed how the climate was reacting, probably sat seasonably down in the northern Great Basin, all the way down to the southern Great Basin.

The water table in Kawich playa, which is just up north and east of the test site, actually stood at the surface. So the groundwater table intersected the surface near the test site at that time. White fir was found down to elevations as low as 1500 meters.

Now, a lot of the climate people typically try to relate what they find in the fossil record to elevation. There are two ways to do this. The other one is to go and look at a similar assemblage in a totally different climate, but at a similar elevation where you are, and find out what that climate is, and the ostracodes are our basic way of doing that.

Okay, at 20-22,000 years, we're still in Stansbury time. The polar front merged with a subtropical front. Lakes got to their maximum level well to the south of the Great Salt Lake, from Texas down to California. We had discharge from alluvial fans in the vicinity of southern Nevada. We had extensive marshes in southern Nevada, and the water table at Kawich playa was probably still at the surface. It was not a dry playa yet.

Brown's Room, which is a cave which you can only access by diving at Devil's Hole, was probably filled, and I think, if I remember correctly, that's about a 10-meter water level difference down at that area.

During Bonneville Time, the Great Salt Lake reached its maximum extent, where it is now about 2500 square kilometers, it was then around 52,000 square kilometers. It is now about 20 meters deep. It was then around 300 meters deep, so it was a sizable lake at that time. It was also very fresh, very low total dissolved solids, in the range of a few parts per million, presumably.

By this time, things are starting to dry up down here in southern Nevada. Kawich became a dry playa, and it has remained so, as far as we know, through to today. Water levels began to fall at Devil's Hole, and marshes became seasonal and ephemeral.

At 12-15,000 years ago, we went to Provo lake levels. This was not due to climatic change. This was due to a failure of the dam that basically held the lake back, and we collapsed to a much smaller level. The southern pluvial lakes; that is to say, Texas, southern California, began to contract. Marshes became smaller still, and, in terms of what we see in pack rat middens, juniper and pinon replaced the limber pine, so we're getting into a much dryer phase at this time.

Around 12,000 years ago, polar fronts were probably pretty similar to the modern position, and the Great Salt Lake was at the stage of Gilbert, and from then on, of course, it's going to continue to contract into its current configuration. Alluvial fan discharge in southern Nevada was basically terminated. In a few cases, it became seasonal. Marshes essentially disappeared, except for very valley centers.

Finally, the Holocene, we see what we see today, modern. The Great Salt Lake has collapsed to where it is. It's only real variations are things which are minor, little effects, such as the little Ice Age, or large El Nino events, where the southern hemisphere's westerly trade winds basically lose power, and the weather fronts migrate in to shore and come up in here and give you all kinds of water.

Alluvial fan discharge in southern Nevada is now extremely rare. Marsh lakes are not in evidence. We've yet to find one, anyways, and the valley centers may have a few wetlands.

All right. Plans and what little we know so far about Yucca Mountain. You will find an error in your book. If we look at modern ostracodes in the area, and then try to match them to where we find similar assemblages of ostracodes in the United States, where climate is well known, there are about five such locations. They average around 110 millimeters per year of precip, and around 10°C. There are undoubtedly more than five such locations, but that is our database.

If we look at them from a 15,000-year-old deposit around there, we see an average rainfall of about threefold more, 340 millimeters per year, and at an average temperature of around 17°C. So, as recently as 15,000 years ago, we have had a major climatic difference.

Calcite and opaline-silica. The calcites that we see in the drill core, as I mentioned earlier, we can differentiate depending on whether they form above or below the water table. There are two forms that calcites occur in. I don't mean to imply here that the visual is sufficient to tell you where they form. In both cases, you see them as vein fillings above and below the water table, such as this. In both cases, you see them as a vug filling, very coarsely crystalline calcite such as you see over here.

The difference is that the ones formed below the water table fluosresce with this orange, or actually fluorescence, and ones above the water table fluorese with green and blue. This is actually this part of this drill core, so you can see the correspondence between the two. This is undoubtedly due to minor differences in trace element geochemistry, and Los Alamos is currently working on that.

We do see, of course, differences in carbon isotopes, oxygen isotopes, strontium isotopes, lead isotopes, so it's not just a visual thing. We really do have a pretty good key as to the differences.

Based on that, we have found calcites that look like they formed below the water table as much as 80 meters above the current water table, but not higher. The studies at Los Alamos, based on mineral and glass/water table interactions come up with a similar depth. I believe they are projecting like 65 meters above current water table in the drill hole data they have so far.

The very existence of calcites within the unsaturated zone shows that at some time in the past--and we yet do not know when--water moved through the unsaturated zone, and we have no idea how much water. Those are two major questions that we need to answer, I think you'd all agree.

One of the things that we expect to find in the calcites in the unsaturated zone is something that will look like, perhaps, this. With the limited data we have, it does not look like it's a uniform distribution. Calcites have not been forming as a function of time at a uniform rate, as they did at Devil's Hole, which is constantly saturated.

What we expect, then, is that there'll be some climatic function here. Now, obviously, if there is no water moving through the site--as Alan believes--today, you shouldn't be precipitating calcites, and maybe that's the time when we'll have a frequency distribution of zero. We have some indication on the faces of calcites that there has been periods of dissolution of calcite by moving water, so maybe some of the periods when no calcite was forming actually represents maximum flow of water through the site, and it never hit calcite saturation.

But anyway, we're hoping to be able to take this sort of record, once it's developed, and correlate it then with the surficial climate record that we will have. The current ages that we have range from about 23,000 years to greater than 400,000 years. As a geochronologist, I have very little faith in some of these ages. These samples were not chosen for site characterization work. They were grab samples to see if this would work.

So if I could finish up with a couple of conclusions here, we hope that the climate reconstructions will provide a framework to understand past hydrologic behavior in both the unsaturated and saturated zones. We have carbonates and other minerals that do show past saturated zone water table fluctuations, and once again, you have an error in your handout. That should not be 100 meters, but up to 80 meters; and finally, on this page, preliminary data suggests that the carbonates in the unsaturated zone may contain dissolution surfaces, which could imply that downward percolating waters were under-saturated with respect to calcite at some point in the past. That one, by the way, I don't think is going to be datable, because we can't date when something dissolved. We can only date when something precipitated.

Anyway, again, dating the minerals that we do find, trying to get a cause and effect relationship, I have shown you that we've had some pretty major changes just within the last 25,000 years, and the data that we get will fit both empirical models and some of the numeric models, so, Pat, we will have data in the final models.

That's it.

DR. DOMENICO: Don?

DR. LANGMUIR: John, you mentioned that saturated zone calcites were found at 80 meters higher than in the past, currently, and you also mentioned the ostracodes were suggesting that you had three times as much precip, perhaps. Do those things correlate time-wise? Do you know whether those were both 15,000 years ago?

DR. STUCKLESS: No. First of all, we have very limited data on ages of calcites in the drill core; and, secondly, the youngest calcites that have been dated--and these were done by Barney Szabo, eight-nine years ago--is 23,000, so we do not have any such correlation at the moment.

We have a considerable effort going on to see if we can, in fact, use mass spectrometry to date the carbon in the calcite so we can date very thin bands, but that's in a testing stage at the moment.

DR. PRICE: What would the data have to look like, or your models look like to drive you to the conclusion that the effect is such that Yucca Mountain is not suitable?

DR. STUCKLESS: Well, fortunately, that's outside our realm. We are data gatherers, and we concede that to performance assessment, and if they come back and ask for more data, we'll get more data. If they say it's sufficient, they'll be the ones who will have to make that decision. Geochemists, I think, are going to be out of that loop, except as consultants.

DR. DOMENICO: Board questions? Staff?

DR. REITER: John, I think you indicated that the largest rise in Devil's Hole was 10 meters?

DR. STUCKLESS: No, that's to the top of Brown's Room. So if it had gone above that, there would be no evidence for it because it would have gone up a little fracture or something like that, okay?

DR. REITER: The reason I'm getting at that, and I think you talked that possibly Yucca Mountain could have been 80 meters higher. Sometimes people refer to changes in Devil's Hole as some sort of indicator as to the extent of the water table rise that occurred at Yucca Mountain. Is there a problem with that?

DR. STUCKLESS: There are many problems with that. I probably almost ought to let a hydrologist handle it, but there are simple enough answers, so I'll give it a shot.

First of all, Devil's Hole represents the discharge point for the carbonated aquifer that drains the east side of the test site and points east of there, and is not connected, insofar as we know, to the carbonate aquifer that's beneath Yucca Mountain.

Secondly, the aquifer that is immediately beneath Yucca Mountain is in the Tertiary. It isn't even one of the carbonate aquifers. I suspect--I, unfortunately, work with the tracers and data enough where I neglect to make that distinction to people.

And then, finally, Devil's Hole, once it rises just so far, is going to overflow, and you will never get a higher elevation for the water table than the lowest discharge point of Devil's Hole. So there are multiple problems with trying to use Devil's Hole as an absolute place of discharge.

There is a past discharge program which has been looking at areas around the test site in the Armagosa Valley where discharge of the Tertiary aquifer has occurred. We have one of these sites at Site 199, south end of Crater Flat, which is approximately 100 meters today above the current water table. That was a discharge point, apparently, around 30,000 years ago.

DR. CANTLON: As you look at these different types of data, geochemical, the ostracode, the pack rat midden, and so on, are the data more or less all in synch? Do they all point in the same direction to the same time, or are some of them sort of creating a blur in the motion?

DR. STUCKLESS: Well, at the present, we have very little age control. What little age control we have, it does look like we're going to get consistent answers down the road, but we haven't gotten far enough along yet. As I said, I think in the middle of my talk, DRI has actually been out coring these deposits for the first time this last two weeks, and that's where we'll really start to get our detailed data.

DR. DOMENICO: We are actually ten minutes ahead of the schedule. Now, I don't know if it's due to the clarity of the presentations, or the skill and cunning of the session chairman.

(Laughter.)

DR. DOMENICO: But I would like to open up any questions for a few minutes that the audience might have because of this.

(No audible response.)

DR. STUCKLESS: I either dazzled them with footwork, or totally confused them.

DR. DOMENICO: Thank you, John.

You may have noticed that the total program, except for our next speaker, is the United States Geological Survey people, and, of course, Marty Mifflin, on next, does not work for the Survey, and even though I've known him for some 30 years, I found out just last night that he once worked for the Survey for three months, continuous employment at about the time of Oscar Meinzer.

But Marty's going to talk to us about the problem of converting precipitation into infiltration.

MR. MIFFLIN: I'd like to apologize to the Board that they had to thread their own notebooks there for my presentation. I made a value decision about four, four and a half weeks ago, when I got the letter saying that there had to be 100 copies. I was about ready to leave the country for South America, and I decided that that was more important, so...

The other apology is, is that for many years, I've been trying to use vados zone rather than the infiltration, I mean, the unsaturated zone because I like the idea that maybe there might be some saturated zone within the unsaturated zone, and vados might be a little bit more ambiguous, and then it turns out that when I get a chance to use it in front of the Board, why, I see that I was dozing and misspelled it.

(Laughter.)

MR. MIFFLIN: Actually, I have a better excuse than that. I was trying to make a re-entry from one language to another and in Spanish, why, S's and Z's are almost the same.

I'm going to try to cover some rather complex material, and in the handout, there's a supplemental section which I won't get to, but which I tried to give in a similar presentation before, and I found I didn't cover about a third to a half of what I wanted to say, so you have some modifying and follow-up discussion in the actual handout.

This is kind of a general concerns list that relates very directly to the problem of trying to characterize the waste isolation properties of a given site, and it relates very closely to how much infiltration and percolation occurs. Yucca Mountain, either in the present climate, or in a modified climate, much of the potential waste isolation characteristics are related to climate, and--or may be related to climate. So, therefore, this topic is really quite an important topic, and it also is a rather difficult area to assess or characterize accurately.

I like this. This is kind of a model that came out of the USGS out of hydrology, and I added, at one time, a slight complication that tried to illustrate--not trying to be gloom and doom, but illustrate the complexity of the characterization problem by showing, based on core count fractures, what numbers of fractures, in rough orders of magnitude, result when you calculate for the volume of the unit underneath the actual repository area of a couple thousand acres, and you get a feel, for example, in the Topopah Spring here, we would have something like 76 billion fractures if the core counts per volume are correct.

So, and each one of these fractures creates a complication with respect to how we characterize the hydrology of both the vadose zone and, of course, in the saturated zone, and the question, of course, always becomes whether we have enough infiltration and percolation, whether it's fracture flow, or whether it's matrix flow or some combination of either. Ike Winograd calls this my gloom and doom figure, and I think it's a pretty good name, primarily from the aspect of confident characterization.

I'm going to go through this a little bit on the fractures to show why accurate assessment of what precipitation really means with respect to infiltration and percolation in terms of potential site performance. This is just an old slide showing the types of data that exist with respect to fracture flow, and there's a better database. Alan Flint probably could give more information of how much evidence for fracture flow actually exists from his database that he's developed, but there is some evidence that there is fracture flow, and there's lots of evidence that other places there is no evidence of fracture flow. So it's probably a minor amount of fracture flow with the current climatic situations if the very small sample that exists right now is valid.

Moisture contents, though--and this, again, is old data, but it suggests that there is quite a bit of moisture in some of these units within the vadose zone, and, of course, the question becomes what that moisture represents in terms of the dynamics of the actual flux through the system.

I present this briefly as a kind of a simple-minded analysis. Here are some literature values. There's three different literature sources for each unit showing the saturated hydraulic conductivity in units that we could easily see what those units mean with respect to rates of uniform infiltration.

So if we have a unit that has a .31 mm/yr saturated hydraulic conductivity, and we had 1 mm/yr of average infiltration, it would force fracture flow in a fractured unit. In other words, the rock could not transmit the available infiltration; therefore, fracture flow would occur, because this is smaller than the amount of moisture entering into the system.

This analysis shows, in a very rough way, if we believe the hydraulic saturation conductivities that have been measured in a laboratory at this point in time, and everybody kind of disagreed with each other on what type of values were valid or representative, that some of the units you would have fracture flows, for example, if you had 5 mm/yr of average infiltration, and on the same unit would maybe fracture flow, maybe matrix flow if it was smaller, like .5 mm/yr of infiltration and percolation.

Now, you've probably read that one of the favorite numbers is something like .0-something mm/yr for a average flux rate for the vadose zone. I would move that decimal point in a different direction if I was going to give my estimate, but the key point is that nobody knows, and it's very hard to determine.

The criticality of making a good determination both for current and future climate change is that you can see that many of these units would probably, if you have any significant flux rate, would go to a fracture flow state.

Now, into how do we try to get to the point where we can, with some type of confidence, estimate both future infiltration and percolation, or even modern infiltration and percolation. The terminology that's commonly used by the Paleoclimate, or Paleohydrology people, effective moisture. Basically, that's a term used that is what's left over after evapotranspiration; in other words, what enters into either runoff or infiltration after a precipitation event?

The problem that comes up in trying to deal with effective moisture in the Great Basin climates, both modern climates and pluvial-type of climates, glacial climates, is that we're dealing very much with kind of the edge of what can be measured with most site-specific methodology.

This is thrown in just to give some type of an idea of one approach to trying to deal with quantitative measures or estimates. If you start out with a continuity equation, or precipitation taken over a long enough period of time, so don't worry about changes in storage, it's going to equal some amount of runoff, some amount of evapotranspiration, and some amount of infiltration.

In the Great Basin climates, the potential evaporation is always greater than precipitation, even in the highest of the mountains, I think even at the very highest alpine areas. So that if we took the engineering or standard approach of just playing with numbers, we should not have very much runoff, or we should not have very much infiltration. Most precipitation should evaporate.

Unfortunately, the--or fortunately, actually, it's very fortunate that precipitation is not equally distributed in time and space, and so that we do get some infiltration, and we do sometimes get runoff, but it's very hard to measure because it's very small quantities, and it's concentrated both in time and space, and it's dependent on weather conditions as much as climate, in some cases.

So if we go down and what we really know, we know that evapotranspiration is always greater or equal to, less than or equal to precipitation, and so on down here. We see that we always are in the state of uncertainty, but we have evidence here, for example, sometimes on the--we have ephemeral water on the bolson playas, which means that there was runoff and it all didn't get evapotranspired, and we have dynamic groundwater flow systems and groundwater discharge, so some water has to be getting in, and there is no, to my knowledge, at least, there's no area in the Great Basin and south central Nevada around Yucca Mountain that there isn't some evidence for dynamic flow systems. That means that there is some infiltration somewhere in the region, but we can't measure it very accurately.

Well, I'm going to try to shift gears a little bit and go into, then, trying to see what type of quantitative evidence there may well be of effective moisture in the Paleoclimates that persisted in the Great Basin on a periodic basis over the last million and a half years.

The easiest period of time to evaluate with accuracy, or at least some quantitative accuracy, is the latest plenipluvial climate that occurred in, say, 30,000 to 10 or so thousand years before the present. The deposits and the features associated with that particular period of a more moist climate or a pluvial climate are still preserved at land surface in many areas, and that preservation gives good opportunity to actually establish the spatial distribution of pluvial features like pluvial deposits, such as marsh, and spring deposits, and former lake deposits and lake extents.

Yucca Mountain, however, is up on a ridge, away from much of the actual Quaternary-type quantitative evidence. It's in an environment that is kind of uncertain with respect to both modern and pluvial climate recharge, and so that probably the majority of evidence that will be established at or very near the site will be more of a qualitative nature of processes, but not very easily to quantify.

Well, we're pretty sure that there have been some major climatic perturbations in the past. This is the type of evidence that persists in the hydrographically closed basin, all through the Great Basin, with the exception of the southernmost extent, which begins right about the latitude of Yucca Mountain. So if you go north--and there'll be another figure here that I have--you'll find in the basins that are north of the drainage divide, of which Yucca Mountain is part of the basin divide, you have evidence of former pluvial lakes in the bolsons.

South of there, you have evidence of greatly expanded groundwater discharge, but no standing deep water, or relatively deep water lakes that have stability, enough stability to form shorelines. These are shorelines. These are mappable, measurable. This happens to be Long Valley. I picked it just to show how nicely they stand out and can be mapped with considerable accuracy in most of the basins.

Now, based on this kind of evidence, quite a few years ago, many people were always fascinated--have always been fascinated, even from the mid-1800s, the first people that came into the Great Basin that had any geologic or natural scientific background recognized that there had been former lakes with these features that look like beaches showing up, and, of course, everybody would see these and stands out in the middle of the desert or up on a high hill looking out over a desert, on former beach, is fascinated, wondering what it really means, so there's been lots of analyses, or attempts at analyses of what that climate might have been which produced these pluvial lakes.

This is a map that was established back in--I guess this particular map is originally from Roger Morrison's work back in the sixties, and then I modified and took out some lakes that he had in, but it basically shows the extent of many, many of the basins that were hydrographically closed, holding very large lake bodies.

And the basins that did not overflow--here's Yucca Mountain, and here we see some small ones right here, right to the north of Yucca Mountain. We see nothing in these basins down here until we get over into a Death Valley/Mono Lake type of system, where there was basin spill-over from the Sierra Nevada.

We see that, at least to this extent, right here to the north, that at the so-called plenipluvial or full pluvial climate, there was enough runoff reaching the bolson to maintain stable lakes of relatively small size.

That type of evidence in more recent years, actually, stimulated somewhat by the Yucca Mountain proposed repository, made us look a little more carefully. Here's the lakes, the southernmost pluvial lake extents, and here's Yucca Mountain right there, and then these areas are areas where there is preserved deposits of what we call Paleo groundwater discharge deposits.

Basically, when they're examined in great detail, the micro fossils and the textures, and so forth, strongly suggest a wet meadow, small springs, channels, small channels, in some places, standing marsh, in some places, ephemeral marsh, a whole variety of very wet, but varying of Paleoecologies associated with groundwater discharge.

There are very, very good modern analogues of what occurs, and the actual sedimentation patterns that are preserved in the deposits here in Las Vegas Valley, for example, and Pahrump Valley over here, that occur in northeastern Nevada at the present time; in other words, it appears to be, in terms of the flora and fauna, almost--and sedimentation and associated features, that there's analogues, modern analogues of what these marshes looked like back in 12-20,000 years ago.

One aspect I'd like to point out is that these extents of groundwater discharge are many times greater than present groundwater discharge distribution, suggesting that the flux was much greater at the maximum pluvial, or plenipluvial climate, so that, in other words, the hydrologic systems, at least as measured by groundwater discharge, was much more vigorous and, of course, each one of these places represents for saturation, is reaching land surface, so there was a change in the position saturation.

In looking around, comparing modern, undisturbed water levels, you get a change in saturation in the basins in southern Nevada, south of Yucca Mountain, that ranges from like 20 or 30 meters to up to around 100 meters difference in position of saturation. That doesn't have too much meaning for a site-specific position of a Paleo water table in Yucca Mountain because of the differences in configurations of groundwater flow systems, transmissivities of the materials, et cetera; structural control and water level. So it gives an idea of the range which you might expect to see changes in saturation, but it doesn't allow you to project that to the repository block area with any level of confidence.

I throw this in. This is work from Jeff Spaulding, Jay Quade, and G.I. Smith and others. This is showing a history of lake level with time. This is showing these Paleo discharge deposits, what type of conditions, marsh, extensive marsh, here you have more restricted marsh, and you get over here someplace around 8,000, why, it slipped over into drying conditions. This is from Las Vegas Valley, and exactly the same records show up in Pahrump Valley and some of the other areas that have been studied in fair detail.

I want to mention something very specific that John mentioned, the pack rat middens offer up opportunity for site-specific evaluations at Yucca Mountain, but he failed to mention one aspect that they have the potential to indicate, and that's localized Paleohydrology. In a few middens, there have been found plants that are essentially phreatophytic-type of plants, where the roots sit down close to saturation, and a very careful search of the Yucca Mountain vicinity would give opportunity to see if there's any seeps and perches, perched water on the flanks or, say, in Fortymile Wash, et cetera, by making a very thorough pack rat midden collection and analysis. It has not been done.

However, under the state oversight program, we did a little scoping work up in Fortymile Canyon, and found, in one midden well above the level of Fortymile Wash some of these indicator plants that indicated wet root conditions.

This is a list of the site-specific type of evidence, and there's probably more. John went over most of it. One of the things I think is rather important that I don't think John did mention, is that there's glass alteration well above the present water table, and well above what people postulate is a plausible Paleo water table. That should be looked at fairly carefully as, is this evidence of perched water?

And, of course, the secondary mineral fillings of the fractures, understanding what those minerals represent both in time and process is very, very critical in terms of what is the hydrology of the vadose zone now, and what should be anticipated in the future.

And again, the macro-fossil type of evidence that is quite site-specific and, fortunately, widely-preserved; has the opportunity to show, did Yucca Mountain have little wet spots and seep areas or not? Some of the indication that we have from some quantitative evidence that I'll get into in just a moment would suggest that there may well have been little wet spots and seeps, and perhaps there were some phreatophytes going in Fortymile Wash, that type of thing.

Now, we have good regional evidence that allows us to put some quantitative, or at least have some quantitative targets to test at Yucca Mountain from a site-specific sense. The fact that all those pluvial lakes existed, there's ways to actually quantitatively evaluate them in terms of hydrology, and I'm not going to try to go into this in great detail, but this is just kind of a little sketch of, say, a hydrographic basin that's closed.

This, for example, is a modern lake that sits in there and is fed by the Catchment Basin, and this, then, represents the preserved shorelines, as you saw in the other photo, mapped out as to giving an area, and then we have an area of catchment, and then we have a ratio between the area of catchment, which is measurable, subtracting out the lake, and the size of the lake. That establishes what I, years ago, called a hydrologic index for the basin, a pluvial climate hydrologic index, and it's based on measurable physical parameters in the geologic evidence.

And that can be related to the process of evaporation, on the basis of a continuity equation, precipitation, runoff, and evaporation. It's all a closed system, assuming that you go to steady state, and the shorelines are well-developed at the high stages, so, apparently, there was a period of hundreds, or perhaps thousands of years where it basically at steady state.

Down at the bottom here, we see that this is a hydrologic index. This related to a lake area and a tributary area, as defined, and we see you can, in looking at a continuity equation, precipitation minus evapotranspiration, divided by the difference between the lake evaporation and direct precipitation on the lake is also equal to runoff divided by the same denominator, and so you have these quasi climatic or hydrologic parameters that you can relate back to precipitation, temperature, and runoff.

You can also do the same thing, modifying the equation a little bit, for modern hydrology in these basins, but it's not very satisfying because you don't have very many basins where you have lakes, and you have to deal with groundwater infiltration and discharge, and surface water runoff. The point I would like you to look at if you look over this is that one can assign numbers to the wettest types of basins that are reasonable, and determine what a hydrologic index is in the modern climate.

When you do that, and compare it with the pluvial climate hydrologic indexes, you get about an order of magnitude in the value of the hydrologic index; in other words, you have ten times more moisture that turned into effective moisture and reached the bolson basin during the maximum pluvial climate. This type of analysis could be done for the basins just north of Yucca Mountain, for example.

In the analysis that I and Margaret Wheat made many years ago, we took the climatic data, long-term climatic data within Nevada, and we re-characterized the climate based on these hydrologic indices as they changed throughout the region. They get very small. The minimum measurable ones are about .02 right there north of Yucca Mountain, and they get up to over one in some parts of the Great Basin in actual numerical magnitude.

These are different climatic zones within Nevada, pretty much as the precip data was broken out by the climatologists back in the old days, in the thirties and the sixties, and what these values here--this is old data. It's in Fahrenheit, but this southern central Oregon, for example, and extreme northwestern Nevada was, in annual temperatures, 42°F.

You come down, this is Yucca Mountain climate. This is the reconstituted pluvial climate. This is the existing mean annual temperature, for example. This is the mean, this is the high, and this is the low of what occurs in the record. This is the precip for high, mean, and low in the existing record. This is what was reconstituted as a plausible type of pluvial climate that would replenish the average pluvial lake in that region.

There is not any unique solution there. There is a number of different climates that can give the same hydrologic response, so these are not unique numbers. Those were numbers based on a fundamental assumption that the pluvial climates were very similar to modern Great Basin climates, so that we had precip and temperature relationships that were similar.

One last point, and I'm going to stop. Along those same lines, these, kind of the starburst here, are showing these pluvial hydrologic indices, or basin hydrologic indices, the values, and on one axis we see we have a value of lake evaporation minus lake precipitation, and on another axis we see we have, what is it, runoff; precipitation minus evapotranspiration equals runoff.

I just mentioned that it doesn't give you a unique pluvial climate, but it gives you a unique hydrologic response, and that's what we're after at Yucca Mountain. We don't what the paleoclimate is or the future climate is, we want to know what the hydrologic response is in terms of what the climate produces in a given terrain, and this has kind of short-circuited the scenario that Dwight Hoxie gave, where he started out with the climatic models, came down then to a watershed model.

And this, then, is the result of a watershed model, where you're looking at the hydrologic response that you already have a record of, and, in this particular study, I tried to find out what the climate was and went the other way. But the evidence was actually a hydrologic response.

With that, I think I'll try to keep within my time frame.

DR. DOMENICO: Thank you, Marty.

Questions from Board members?

(No audible response.)

DR. DOMENICO: Staff, questions?

(No audible response.)

DR. DOMENICO: Marty, help me out here. Everybody agrees that the pluvial climate was wetter. Are you stating that they are underestimating that wetness? Is that the main part of your message?

DR. MIFFLIN: That they're being underestimated?

DR. DOMENICO: Yes.

DR. MIFFLIN: My own opinion is, is that the types of evaluations that are being made, and probably as far as climate is concerned, are probably pretty much on line. There's more and more evidence that's honing in on, say, temperature and certain types of hydrologic features.

What concerns me about the proposed Yucca Mountain site is--and the reason I took the trouble to go back into something that really wasn't the topic, or didn't seem to be the topic--is that fracture flow is forced by the very low hydraulic conductivity of the rock matrix, and we're right at the edge with the modern climate of either fracture flow dominating, or matrix flow, or no flow dominating, and we don't really know whether we can characterize that modern hydrology. Alan Flint's going to tell us how we're going to do it, but anyway, we don't know for sure how accurate we can characterize that.

It's very clear, based on the different order of magnitude of the full pluvial climate, based on that type of analysis that I just briefly went through, that those climates had a paleohydrologic result that was about one order of magnitude of more effective moisture; that is, runoff and infiltration.

So if right now, for example, the average flux was 1 mm/yr, that means we'd have 10 mm/yr, just, you know, kind of a direct comparison, and that's the type of order of magnitude of difference between the climates. If you go to the terrains that have that type of climate, you see seeps. You don't see very deep regional--well, you don't have that much data whether you have deep regional water table, but you see lots of evidence of shallow saturation in volcanic tuffs and terrains.

DR. DOMENICO: And you're saying that the 10 mm flux would be conveyed mostly by fractures?

DR. MIFFLIN: All by fractures.

DR. DOMENICO: All by fractures.

DR. MIFFLIN: Except in the Bennett-type unit.

DR. DOMENICO: Are you ready to suggest the possible submergence of a repository with that sort of flux?

DR. MIFFLIN: Say that again?

DR. DOMENICO: No, never mind.

(Laughter.)

DR. DOMENICO: We're still early.

John?

DR. CANTLON: Yeah. Cantlon; Board.

The fact that fracture flow is forced any time the effective infiltration exceeds the matrix flow, this is the point you're making, that doesn't presuppose, though, that fracture flow would necessarily be the same scale as runoff; in other words, if the fracture capacity were such to accommodate a much larger per cent, you may not have gotten the pluvial lakes. It would all have run down in the ground and fed out into springs somewhere, but the fact you got very large lakes suggests that the fracture capacity is restricted, and, therefore, cannot accommodate a great deal of that water, and, therefore, you've got lakes.

DR. MIFFLIN: If you read the supplemental discussion I attached on there, there's a rather convoluted and extensive discussion of that type of problem, of how you judge what the actual terrain characteristics do, and what the evidence is in the varying type of terrains where you have basic, fundamental differences in the transmissive capacity of one type of rock versus another.

And what is probably true for the welded tuffs that are highly fractured, it has a very, very high infiltration capacity. You don't really start to saturate them up, except maybe at the bedded zones. You can go well above ten times the present precip or infiltration; actually, infiltration has got a direct relationship to effective moisture and precipitation.

What is not known, and what would have to be determined by establishing processes in time and space at Yucca Mountain or someplace else, is whether runoff and infiltration behave just as you asked in a proportional way. In the very arid climate, actually, the infiltration is probably greater than the runoff, as measured in the bolson.

The groundwater discharge--and, you know, you get zero surface water runoff, or very close to zero in many of these basins, and you have very infrequent water standing on playas for a very short period of time, yet you have areas of phreatophytes that are pumping water out that probably equates to a greater number of acre feet per year, but that isn't necessarily true when you go farther up on the moisture scale.

DR. CANTLON: Well, not only that, the pluvial period was really related to a cold climate, and having grown up in this valley and watched it through the years, the sequence in which moisture comes into the system of these very deep snows, warm rains in the spring, and all the water goes charging off down into the lower reaches, and it doesn't get into the ground at the same rate that it runs off. In other words, runoff and infiltration are two very, very different things.

DR. MIFFLIN: Are two different things, but--

DR. CANTLON: Infiltration is a very, very restrictive thing.

DR. MIFFLIN: --they actually--I argue with you a little bit there. They actually are occurring pretty much under the same types of antecedent moisture conditions. You get the most runoff, but it's very steady, with snow melt, and you get the most infiltration with snow melt.

Now, certain types of terrain can accept the trickling snow melt and it goes into infiltration; others, it comes right out. So the rock type is very critical.

Yucca Mountain will accept snow melt, or lots of it, and Alan will probably have some comments on that. If you go up to the Ruby Mountains, it accepts snow melt, and then puts it right back out into the mountain stream channel.

DR. CANTLON: But you've got viscosity problems when the temperature is low. You've got frozen ground problems when the temperature is low, so there are some really tough variables that prevent those two things from being related nicely.

DR. MIFFLIN: Right. There's two--not all hydrogeologists that work in the Great Basin agree to this, but most seem to agree to it. There's two probable conditions that are conducive to recharge. One is the concentrated runoff in channel, and that can either occur at the mouth of mountain streams coming out on the heads of alluvial fans. There's good documentation there that recharge occurs in great amounts, and the other is, of course, in the more ephemeral wash. There's fairly good evidence that, under certain conditions--and there's some evidence at Yucca Mountain that you get recharge there.

The other, I think--and it's not as well-documented by hard data sets--is snow melt, and what I think happens is the melting process lasts long enough, provides enough water and occurs slow enough for fractures to accept it and get past the root zones.

DR. CANTLON: In some years.

DR. MIFFLIN: Yeah, or just some events.

DR. DOMENICO: Max has a question. Can I get Max to state his problem here?

MR. BLANCHARD: Thanks, Pat.

Marty, I have two questions. The first one is, several years ago you ran a field trip for GSA, and a few of us in this room were lucky enough to participate in that field trip, where you shared with everybody that came on the field trip, from north to south in Nevada, all of the conceptual ideas you had developed over the last couple of decades in studying the relationship between climate and hydrology as we went from basin to basin.

It seemed to me to be a very, very insightful thing for me, and I was wondering if you had plans for running another trip like that, because the relationship between climate and hydrology, as everybody is aware, is very important with respect to assessing the suitability of the site, and some of the ideas that you've expressed here are things that need a great deal of hearing in order to try to best understand how the processes work, and when they go in one direction and another direction.

Can you share with us some possibilities for another one of those field trips?

DR. MIFFLIN: Well, as you know, we had a good time. Yeah, I'll do it any time. As you also know, it takes some funding to do that, but--and enough time to do it. I think that, what was that, about a four or five day--four-day, I think, at least a four or five-day trip, and if I did that, I would make it a round trip, not try to start it in one place, because you can go two different routes more south, and half of the participants started in Las Vegas, and we had an informal day trip, or day and a half trip, and then started the formal one, and the first part was just as good as the second part. We had a swimming hole in the first part.

MR. BLANCHARD: In your first view graph, you shared with us some of your views about some aspects of the hydrology and climate program aren't quite as conservative as you would like to see them, and I'm wondering if you are prepared today to cite some examples where you think a little more conservatism would be more to your liking.

DR. MIFFLIN: Yeah. One of the things that--there's different ways of viewing very sparse data sets and uncertain processes, and one way is to favor some model that may fit the data because it's kind of weak and ambiguous, and the other way is to take the worst scenario that fits the data, and in the process of selecting a high-level waste repository, it seems like, to avoid surprises, you know, surprises that everybody talks about, and some will occur, that one should take the worst case scenario, and then try to prove that it's not a valid data point, you might say.

I think one of the weaknesses of the long-term site selection, site characterization program has been many issues, including the vadose zone hydrology, is that there's been preconceived ideas that it's a dry site and that there is no fracture flow, and yet, there hasn't been a data set that proves that. And so, one should take the other perspective. It gives you a better way to design your programs. You try to prove that the worst case scenario is not there, if it's not there, or if it is there, you find it earlier on.

I don't know whether that answers your question, but that's a philosophy, I feel, that the DOE program has not been strong on. It's really looking for a site, it needs a site. It doesn't want to hear the surprises, and, therefore, it doesn't take a conservative analysis to design the actual characterization program.

Does that answer your question, or was that what you were asking?

MR. BLANCHARD: Well, yeah, I think it does, and I was just wondering if you've had enough time to follow some of the conservative modeling that's being done by not the site characterization people, but the performance assessment people, because we hope that we can get those bounding scenarios in through the processes that Jerry Boak this morning talked about, where he showed at least the PA people are looking, at least, like a WEEPS model versus another model, and the WEEPS model is all, as I understand it, more or less fracture flow, and so maybe we don't have all of the necessary bounding limits to apply that, but I think the concept is there embodied in the performance assessment as a goal, and we would appreciate input from you wherever you're willing to make it.

DR. MIFFLIN: I'd be happy to.

DR. DOMENICO: We're right on time now that Max has set Marty up for a field trip, and Marty's set Max up for a few bucks.

Why don't we take a fifteen-minute break and be back here.

(Whereupon, a brief recess was taken.)

DR. DOMENICO: Thank you.

Our next speaker is Alan Flint from the USGS, giving us a scientific perspective on understanding unsaturated zone infiltration and climate change.

Alan?

DR. FLINT: Actually, what I've been asked to do today is fairly difficult. I'm going to try fairly hard to do it, but if you can just imagine for a moment a beautiful blue sky, big white clouds, on Augusta National, beautiful green out in front of you, brand new Wilson Staff golf clubs in your hand, a Titleist teed up just right, and what they asked me to do was explain how you hit the ball, just one thing, don't hit it. I have just one word of advice to the people out here: Fore.

(Laughter.)

DR. FLINT: What I'm going to try to do is talk about a scientific perspective, or my perspective on how we resolve this difficult issue. I'm not going to give you the DOE management line or the M&O management line, although I hope it's in here, since they're managing the project, but I'm going to try to give you my perspective. It may not be correct the way they see it, but it's sort of the way I see it, and I will try to keep the technical details down, but I will use them as examples.

I have an outline that I'm going to go through for this talk, and I'll be referring back to this outline several times to show you where we are. I'm going to go through the study objectives for the infiltration studies. Then I will go through some definitions and concepts, a little bit like what Dwight talked about. Then I'm going to show you the research framework in which we're working to get some of the information, what information we need, how we get that information, and then in what order do we get that information.

I will talk a little bit about the current work, what we're seeing in shallow infiltration processes, deep infiltration processes, and finally, the unsaturated zone scale. I'll talk a little bit on real time decision making, how things have changed in the studies, and how we know when these experiments end, and then I'll have a summary slide which I think will cover everything and sum it up fairly well.

I've talked before, and you've seen this information--I think it was in 1990, we went through in great detail the infiltration study, meteorology study, matrix studies. This comes from the infiltration study plan, and the overall objective for the unsaturated zone studies that the hydrologic investigations program is working on is to provide an understanding of the past, present, and future flow characteristics of the unsaturated zone.

The specific objective of the unsaturated zone infiltration study is to define the upper flux boundary conditions--that was upper 10 meters--for Yucca Mountain under both present day and simulated future wetter climatic conditions, which include evaluation of the spatial distribution of infiltration, particularly over the repository block.

Since this was written several years ago, the upper 10 meters is not deep enough for the process that we're looking at, so we'll look in some detail deeper than that.

For the purposes of this talk, I want to show one of my later slides early, and so I can assign some definitions, so we have a common understanding of what we're looking at.

By definition in the hydrology, infiltration is the movement of water across the air soil interface downward, and the associated flow away from that interface. So infiltration, in essence, can go from the surface all the way down to the saturated zone. We use the term percolation talking about flow processes that are fairly deep.

Exfiltration is the process or the movement of water across the air soil interface upward, and the associated flow up to that boundary. Now, exfiltration, for the most part, includes evapotranspiration. However, things like barometric pumping or barometric pressure changes that move water up through the surface are also a process of exfiltration, which is not defined by the classical term of evapotranspiration, and, as you've seen from some of the work that others have done, that may be a very important mechanism of moving water that is not part of the simple ET process we like to look at.

Net infiltration is water that we've defined that gets below the root zone, water that's not readily moved upward by evapotranspiration processes, but that does in no way guarantee that it will become recharged. A lot of people say net infiltration will be eventually recharged. That may not be the case.

Recharge is the movement of water from the unsaturated zone to the saturated zone, and the associated flow away from that boundary, and discharge if the movement of water from the saturated zone out to the unsaturated zone of the land surface, and the associated flow towards or away from that boundary.

In this case, I want to show one thing in particular, that flux is not necessarily always down. In the case of Yucca Mountain, there may be a case where we can make an argument that flux is upward in part of the system, where it may be down in another part of the system. This profile that we see here that says that flux is negative or outward, exfiltration going on, and not necessarily all by evapotranspiration processes may be due to where we are in the current climatic regime, so we need to keep that in mind.

I'm going to talk a little bit about the research framework now, the model development, what information is needed, and then I'm going to talk about how we get that information.

We're developing a 3D watershed model which establishes the framework for the infiltration studies. This is still developmental, and we're looking what's available to us, but the parameters that we're going to need for this model are simple; geometry of the site, whether we're dealing with an alluvial channel, we have alluvium underneath, welded tuffs or nonwelded tuffs, the soil and rock properties, the boundary conditions, what we're going to put on top of the surface, the bottom boundary conditions, what our initial conditions are, and very important infiltration mechanisms and processes, whether we have fracture flow, matrix flow, run-on, runoff, that kind of process.

To get at those we have, in this case, three major studies, and we're not going to go in detail over this, but if you go through it on your own, you will see that this is surficial materials, part of the infiltration program, where it provides these properties; simple densities, porosities, water contents, some fractures, alluvial thickness, that kind of material, and we have a series of tests and analyses we use to get at those properties.

Characterization of natural infiltration, again, looking at things like mechanisms, the boundary conditions, initial conditions. We have a series of tests, our neutron logging, which was talked about a little earlier, water budget studies, and we can go after these properties; rainfall runoff, evapotranspiration, infiltration rates.

And then, artificial infiltration, again, more of the properties that we can get from artificial infiltration, the tests that we're going to run, infiltrometers, ponding, and I'm going to show you an example of a ponding experiment, and get some information on maximum infiltration rates, saturated conductivities, unsaturated conductivities.

The difference between artificial infiltration and surficial materials, to a large extent, is the artificial infiltration gives us a chance to calculate or identify properties for transient flow, unsaturated conductivities, dynamic flow processes, and these are more static properties; water retention curves, and things like that, and then nature puts it all together.

There are some supporting studies that we need to have to do this correctly. One of those is a regional meteorology program. A very important part of the infiltration program is this program. We're going to get into some information on the boundary conditions. We have our meteorological data collection, our characterization of precipitation, and other synoptic and regional skill data.

One thing that I want to mention, there was the earlier talk, two talks before mine, talked a lot about the

Great Basin. Yucca Mountain is not in the Great Basin. It's in the Northern Mojave, and I think we have to take that into consideration. Things are different in the Northern Mojave than they are in the Great Basin in terms of the distribution of precipitation, and the timing of the precipitation, and the processes that go on; quite different.

I want to just give you a little bit of information, also, on what's happening today. This is a map that comes from a report that was published in the Journal of Applied Meteorology, and if you'll look in the back of your handouts, you'll see a bibliography of some current work that's been done, and some older work that identifies where this information can be done and reports on Yucca Mountain.

What I wanted to point out here is that we're looking at a site in which our best estimates of precipitation around the repository area on Yucca Mountain is over 160 mm. When we talk about increasing rainfall rates, doubling that, you can double that if you just go north about ten miles, so there is a large distribution of rainfall. So when we talk about the Yucca Mountain site increasing rainfall, we have to realize where our sample sites are. If they're up here where we already have high precipitation rates, we need to be aware of that. So there's a large distribution within 20 miles of Yucca Mountain. You go from 80 mm a year to over 300 mm a year; so quite a range in a very small area.

And I want to show you what the current system looks like today. What I have here is precipitation. The red is the mean annual precipitation between 1967 and 1991 in Jackass Flats, about 15 miles from the Yucca Mountain site, and that was 170 mm/yr up to 1991. In 1992, we had 328 mm, quite an increase in rainfall, so that's seasonal variability that relates well to the spatial variability, and you can see that most of that came in the winter time, most likely due to the El Nino effects that were fairly lasting.

And then we go through October and November, still a high amount of rainfall, and then if we look at 1993 to date, you see a tremendous amount of rainfall, over 243 mm already in just three months, so we have an incredible amount of data. I brought a series of overheads that show you some very, very interesting, informative information about the site, but it is technical, and if you want to talk about it, we'll have to do that later.

The matrix property program also has a big contribution to make to the infiltration study. We're going to provide soil and rock properties, initial conditions, series of tests and analyses, core properties, 3D models, and as some of you know, we finished UZ-16. We're starting to process some of the core; very, very interesting information, also. I happen to have some overheads on that that we'll cover if anybody's interested later on. Again, it's technical in detail. We are going to develop a 3D matrix property model with all of this information at some point.

Okay, that's an overview of what information we need and how we're going to go about getting that information. Now I want to talk a little bit about setting priorities, in what order do we collect this information, and how do we go about getting the information?

In terms of setting the priority--and these are not necessarily in order of importance--the first is the issues hierarchy, the SCP and the study plans were defined to address certain large questions. So we set a series of studies up, we put that down on paper, and we said, here's what we need to do to do a site characterization study. So it's a fairly good way of confining your thinking in certain activities and certain areas.

The second way that we set the priorities is by peer review, professional interaction, and performance assessment. Peer review, as was mentioned earlier, the peer review team looked at what we were doing, and then helped us to set priorities based on their experience, incredibly valuable in some of the work that they had done in letting us find out what was first, second, third, fourth in importance. Professional interaction is very, very important, and that's going to be quite helpful in helping us to set priorities.

This year I'm chairing a session at the High-Level Waste Conference on the influence of climate change and infiltration, and we have several talks in there, and I've been involved in that and getting that session organized for some time, and I've learned a lot about the process and what other people view and how they view things, so that's going to be very useful to present information in those kind of forums.

And again, interaction with performance assessment, what Jerry Boak talked about earlier, I've had a lot of involvement with the performance assessment group at Sandia in trying to work what we see in real observations with what they're trying to model, and that's been real useful in helping me to set the priorities.

Budget is another way in which priorities are set, and at my level, the money is allocated to specific activities. They say, here's how much you have, here's what you can do with it, so I have to set my priorities by the amount of money that's in the project. If the project is well-funded, then I can do a lot of work in that area. If it's not, then I do less work in that area, and that helps to set the priorities, by where they set the money.

There is also the drilling and construction support for large-scale experiments. Drilling neutron holes costs money, and if the money's available, that process gets a priority, and it's been very, very lucky that they had the money. What Tom Statton said is quite true. We had an anomalously dry year for a number of years. The wetter years allowed us to make an argument that we wanted things to change, we needed to get these boreholes in, and DOE agreed with that and helped to come up with the money to get this job done, and again, incredibly valuable information came out of that.

Another important area is a balanced program. The activity has to be important to a balanced program. You can't spend all of your effort on one part of a program. You need a certain amount of information in certain pockets, and if one is overflowing, you're wasting your money if you're putting more into that if something else is limited. You need to go until you're limited in one area, and then go after that area when one area starts limiting you.

These data needs need to be met by an activity that has a specific model in mind, at least that's my perspective, is that I have something in mind, a model that I want to use, and I try to collect my data to feed that model and that helps me to set some of these priorities.

And the last one, a window of opportunity. When you're dealing with surface infiltration processes, we're dealing with a very dynamic system that require constant attention. This year, because of the tremendous amount of rain, I reorganized my priorities and we started going to the field measuring water potentials with tensiometers; very narrow range, very small window you have to measure those. In fact, for the four years, or four and a half years, we weren't able to measure those because there just wasn't a wet enough system. Now we have that, so we're putting a lot of effort into that part of the process. We had a little money left over, so we took some of that and spent it on procuring instruments to do some of those measurements, but because the system is so dynamic, that causes us to change our priorities to collect the best information we can get at the time when that occurs.

So, when we have these spontaneous events, with irretrievable data, we want to get that information right away, so this is very important to us. That's why we have to make real time decisions and we have to be paying attention to the natural system all the time, and that helps us to set our priorities.

Okay. Now I'm going to talk a little bit about the current work we're doing, and these are examples to show you how we put some of these processes together to come up with a big picture perspective. I don't want to spend too much time on the details, but I do want to show you them as an example of what we're learning and how we're trying to put that together to explain the big picture and how we're looking at it.

This is an artificial infiltration experiment, one of the ponding experiments that we ran. The first one, this was on the edge of Fortymile Wash. It's a neutron hole in the middle that we've been monitoring for a number of years, a large ring around it, three tanks for the three inner rings that we use, and then a large tank that will feed this system that's over here, off the screen, that will feed the outer ring. We have tensiometers, time domain reflectometry that we use to monitor infiltration at this particular site.

One of the advantages that we have here is that we have a fairly nice view of what the system looks like, so we can characterize all of these layers that we're going to be moving water down through. Just to give you an idea how far the water moved after we put about 13,000 gallons on a little ten-foot diameter ring, it made it all the way down to about here.

This is an example of the kind of data that we get from the system. We start with a water content profile versus depth. This is at one meter--I'm sorry--one hour, then at five hours, and we go on to 337 hours, and we're down to about six meters. There's a series of layers, boulder layers, carbonate layers, and they tend to pond on these for a long time, and then they'll break through and then they'll pond on another one, and we can use this information to estimate what large volumes of water would do to this system, huge amounts; increase in rainfall.

What you're seeing here in this profile alone is two and a half times the average annual precipitation, although we apply it about 20 times the average annual precipitation on these layers and move sideways.

We also used drainage experiments. This is the end of the experiment at one hour after we ended the experiment, then 25 hours, and at 150 hours, you can see that we're draining the surface. We use this information, instantaneous profiling techniques to estimate what the unsaturated zone properties are, but you can also see that while this is draining, water is also moving downward, so we're going to continue to monitor some of these pulses. It's about seven meters now.

That's in the artificially-controlled system, where we know how much water we applied, and we can monitor it. I'm going to talk a little bit about a natural system. We're trying to use the same techniques in a natural system, taking advantage of some past data.

This is the mouth of Pagany Wash. It's to the north. It's one of the areas that was talked about by Jerry Boak, where we've been working with performance assessment modelers to try to do some simple modeling up the channel and across the channel. We have a Bowen ratio station set up so we can do some evapotranspiration measurements, but I'm going to talk about some natural measurements we made in the channel.

This is the cross-section UZ-4 and N-7, which I'll talk about for just a couple minutes, in the channel. In 1990 to 1992, some climate records at UZ-7, we have rainfall rates, we have potential evapotranspiration estimated from some of the measurements we've taken, and we can do actual evaporation, also, but we have a database in which we also have soil/water content measurements in 7, so we can apply this rainfall rate to what's happening in a neutron hole, because we have a monitoring station right at N-7. In fact, we have a monitoring station at every borehole that we monitor on Yucca Mountain for rainfall.

We'll just look real quick. 1990, we didn't see anything, no changes in the profile. 1991, we started to pick up a little bit of moisture content flux from a few rainfall events, getting down about one meter, and then, in 1992, with all that rainfall we had, moved down quite a bit deeper, and in '93, it's going to go deeper than that, but we stopped at this point and we're going to try to run a model now that we have these changes in water content and then movement of the wetting front.

You can see the cumulative rainfall in this case. Normally, you'd see wetting front go down and then this would dry out, but we've been adding more and more water to it. So these are some of those same profiles looking at 1991, '92, dates in '92, again in '92, and the blue lines are the measured data, and the red are a calibrated hydrologic model, a simple, one-dimensional model, with ET, with a root function, with a flow, but no vapor flow under isothermal conditions, but we did a fairly good job of matching the data.

We had to estimate rock properties, soil properties, using Van Genuchten functions from inverse modeling, but we did a fairly good job, I believe.

And then we can look at specific depths. That's what this is. We're looking at the change in different layers; the top meter, one to two meters, two to five meters, and five to 13 meters; the blue, measured water contents, and the red are model. So we can actually do a fairly good job of looking at things. What are you seeing here? You're seeing a drying out at depth, drying out.

In this case, we happened to get a pulse at two meters because we had enough rain over two years, but in the long term, this system hasn't seen that change yet. The water hasn't gotten down that deep, and we've had a continual drying trend for the last couple of years, and that's starting at a zero point. If we look back from five years from there, it was drying down even before that.

So we went back and got some other data that we didn't have. This is Desert Rock. We have from 1978 to 1992. We used that as an input for a simple climate model, and tried that to see what's happening in the shallow infiltration. You can see these nice pulses up to one meter. It goes up, and then evaporation takes it out, and we just keep going through the cycle, and we can look at one to two meters in depth and we see, for the most part, we're running low.

The water doesn't make it down very deep in this system, so what we wanted to do is to increase the precipitation rate to see what if we really stressed the system like we will do when we do an experiment here, and we put two and a half times precipitation rate, so that's the model results, two and a half times, so we're getting huge storms, like in August, over 200 mm in our model.

Again, the surface, high evaporation rate still because the potential is there based on models and measurements; one to two meters, same thing. But finally, note down here, in this case, we have at 13 meters, five to 13 meters, we get a pulse. So now we're starting to get where we can push water through a channel at Yucca Mountain, and this might represent what would happen if we had a runoff event. In this case, we got no runoff in the model.

So now we have a model that we've calibrated and a model that we're testing out. We may not have a way to validate this, but we're going to use this in some of our artificial infiltration experiments to try it out, so we're trying to put this stuff together; observations, models, measurements, and trying to come up with a system that's internally consistent.

Now, here's Pagany Wash. We're just looking at the data. We go across this wash, and I'm going to talk about extrapolation now from 1D to 2D, because that's a very important part. We can take this system, which we have good measurement from. We can make a 2D grid of the boreholes from the neutron hole, N5, 6, 7, 8, all the way to Neutron Hole 9. There's the channel, and we can model some natural runoff events, which we did, and we had to put layers in the system in different places.

Some of the things we would observe is that the water balance would not go down vertically. We would have losses of water from the system, so we put these in the model, and it turned out water started popping out over here several months later, and we went back and looked at the neutron hole log data, and found that's where it had popped out. We had never noticed it before in looking at the logs until our model suggested that the water should be over there.

We're going to go back to this site this summer.

We're going to put an artificial infiltration experiment right here, and we're going to test this hypothesis. So we're going to do some hypothesis testing with old data that we have, and then running some new experiments.

Okay. I want to talk now a little bit about deep infiltration processes from 15 to 100 meters, and then I'm going to talk about the unsaturated zone scale processes and some of our current work.

This is a site from new holes that we've drilled this year. Going across the channel, this is Abandoned Wash, for those of you that have been up in that area. What these holes are supposed to look at is what's happening in the Topopah Spring. This is one of the only places where the Topopah Spring unit is directly exposed, other than Solitario Canyon, to regular channel activities, and we want to see how this fractured rock responds to natural conditions.

And one thing I don't think Marty mentioned, that I'm sure if Ike Winograd were here, he would have said, that the fractures in the Topopah Spring are not a bad thing, they're a good thing. That's one of the reasons he recommended we look at the unsaturated zone, because of those tremendous amount of fractures, so Marty made a good point, that it's a good thing for the site.

And now I'm going to look a little bit at what we're seeing here in two of these boreholes, after we look at this cross-section. In particular, I want to show what's happening in this one borehole, an inactive channel, and then an old channel borehole, to give you an idea of what current processes are that we see at Yucca Mountain now.

These are two graphs that show the water contents, and the interesting thing that I want to point out here is you see that this is fairly linear, and you see a fairly good slope in these two graphs. What we think is happening in this case is that water flow in the active channel, over a long time period, hundreds of years or longer, has been slowly going through the system and keeping the water content at these fairly uniform saturations.

In an old channel where, at one point, there was water flow in the channel, which is what keeps this saturation level, or made this saturation level, perhaps, equal to what we have over here, and then once a system changed and the channel moved, this system started drying out in response to the fact it was in a desert environment, and now we're starting to see this trend, which you saw earlier from a slide Jerry Boak showed, and I'll show one in a little bit.

So now we have some observations, we have some data, and now we want to do a little bit of modeling work. We want to try to take all of these small models and put them together into a larger model, and it's actually fairly easy when you take a series of small models that you find that you can verify, and put that into a large model. You get a fairly realistic hydrologic system. So if you look at any part of this hydrologic system, you can see that it's internally consistent in a small area around it. When you put it all together, it makes a fairly good, clear picture.

As anybody can see down here, this is Bo Bodvarsson. He's looking up at my hydrologic model, and he said it doesn't make sense to him, but that might be what virtual reality is, or at least what they mean with the movie and the book, and so, actually, Bo's been helping me to try to straighten this model out, and so I appreciate his efforts; and Ed Kwicklis, too, has been helping me to straighten this model out, and so now we have the waterfall going upward.

Okay. Now I want to talk a little bit about Neutron Hole N-55, which was the first borehole that we had drilled on the site since 1986 on the side of a ridge. UZ-16 is almost where you're standing, where this photograph is taken from. I'll show you some data from that.

This is the saturation profile Jerry Boak showed you earlier. We see it about 40 to 50 per cent saturated at the surface, increasing in saturation to almost 100 per cent at the top of the nonwelded tuff unit, it gets drier, wetter at the vitric caprock, and then drier again as we go into the

Topopah Spring unit, and I believe that Russ Dyer may have showed this a year or so ago, also.

Now we add some modeling work to that. What I have here is a series of different model results, based on some properties that I'll show you how we got to in a minute. Again, the measured data, the orange is a water content profile that you would get from the water table to the surface of Yucca Mountain under no flow conditions. So if there were no flow, no recharge, no movement through the saturated and a simple equilibrium with the water table, the Pgh kind of calculations, this is the water content you would get.

It seems to be in somewhat agreement with the nonwelded tuffs, but it doesn't agree with these high saturation zones, which is an indication that this system has had positive downward flux of water, which causes the saturations to be higher, and a negative flux at the surface, which causes the saturation to be lower. So we do have evidence for past high flux conditions.

If I put the system in equilibrium with this high saturation zone and assume that the whole thing is potential equilibrium, what do I get? Well, one is I get a much wetter surface boundary condition than what I see here. I also increase the saturation of the nonwelded unit, so that system didn't work, either, but I added, starting at this condition, I added a -.05 mm/yr flux for 3,000 years and got a profile that's starting to look sort of what we're seeing in the real data.

I can put higher fluxes on it, but when you do, you drive the system here a lot faster than you see this happening, so this gives us some controls. What this told me at this time was that I have to take into account the fact that there was varying flux in the system. At some point in the past, there were probably high fluxes. Today, there are probably negative fluxes, at least that's what I needed to make this system work.

To go after a little bit more information, we started on a project. Actually, this was a project that we started in about 1989, in some discussions I had with some performance assessment people, and also in a presentation I made some time ago, where we showed high tritium data in the UZ-6S borehole at around 550 feet below the Paintbrush nonwelded unit.

When I met with the performance assessment people, in particular, Maureen McGraw, we decided as a test case in 1990, that we would put together a two-dimensional model of the site, looking at the potential for lateral flow across the PTN and downward that may cause that high tritium level that we saw in the bottom of UZ-6, so several years ago we started this study, where we started collecting surface outcrop data to characterize some of the rock properties. There was no drilling at the time, and we needed rock properties.

So we put together a transect from the surface down to the Solitario Canyon Fault to get rock properties. This data actually worked fairly well, because we introduced this data to the INTRAVAL, International Model Validation Group, as a potential test case, and with everything else sort of falling flat with the NRC contribution at the Apache Leap site, this is the final test case we went with.

To get more data, we went to some other sites. We went to Busted Butte, took a transect of the Topopah Spring. We went up to the north, got a transect through the Calico Hills, and together, we put together a series of data from the entire unsaturated zone to get rock properties, with the point of using the N-55 data, which we had just collected, and that some of the interpretations we were trying to make to model UZ-16, model it before we actually drilled the hole, so we could put our hydrology on the line and say, here are some ideas we have.

Whether it's consistent or not is what we are going to test. Linda's going to talk a little bit tomorrow about the fact that you can get different answers with the same inputs, which I think is an important point, but we're going to take a stab at what's happening at UZ-16. So we went all over the site, collected a lot of information from rock core outcrops. We ran water retention curves on them, did saturated permeabilities on them.

This is just to show off that we can actually measure fairly decent curves now with the new technology that we've developed in the laboratory for these rock samples, and we're real pleased with this system because we get good information on even the Topopah Spring, which has been normally very hard to do, so we have some fairly good information, and there it is. So if you guys want to go home and model this tonight--if Buschek were here, he'd be done in a couple hours to test all this out, but I'll give you until tomorrow.

I just wanted to show you all the properties that we're working with, with the INTRAVAL data set. These are my interpretations of the Van Genuchten parameters. We provided INTRAVAL with the original data so they could put whatever interpretation, or use Brooks & Corey, Van Genuchten, or their own functions, if they wanted.

But you put this all together in the case of the modeling exercise that I ran for UZ-16, and we came up with this system as what our best guess as to what it would look like. The data points we have from the outcrop studies transposed to the units at about the appropriate location. The bars are element porosities that we assigned to keep our modeling domain small enough to stay within the tuff code, which is what we used to do the modeling.

So now we have a 1D model of the site, and we're going to try to do some modeling with that, but what we needed was some kind of climate change, because I didn't want to do steady state climate I showed in N-55. That didn't work. The hard part about N-55 was I didn't have a good lower boundary condition. I didn't know what it was, so in UZ-16, it offered the opportunity to put the water table as a boundary condition.

What we're looking at here are two different versions of pretty much the same information; that's Oxygen-18 data in trying to estimate what past climate was like, and as you've heard earlier, I don't think it would surprise anybody to know that it has been wetter in the past. It also has been drier, and wetter and drier, and we're looking at today's conditions. If we're down here, we're looking at dry conditions, warm conditions, and if we're up here, we're looking at colder, wetter conditions. This is data from Imbrie and others that goes back 700,000 years, so you can see that we're going through these cycles, and as was said earlier, it looks like we're probably in one of the driest times we've ever been.

I overlaid on top of that Ike Winograd's new data from Devil's Hole, fairly close to the site, and, for the most part, it's fairly reasonable agreement. Ike has some shifts in it that he can explain that don't fit in Malinkovich, but we need some kind of a climate system that we can run, and so we're going to use something like this as an input to our modeling effort, so whether I used Ike's data, which I chose not to, because he stops in the past when it was fairly wet, and I want to go on to a point where we are today.

And all I did, very simple, is I just assigned infiltration to fit this curve, so that's what I have here. These units are not written in stone. It's just what I used for this particular example. The first time through, this is a snapshot in time of the modeling work we're doing. I'm working with Ed to change things even today.

So what we're looking at, basically, is today's conditions. I'm suggesting that we use for our first modeling exercise -.01 net infiltration, a negative value, so upward flux or exfiltration, but not recently--these are 7,000-year points--not too recently in the past we had large flux conditions; albeit, they are small in this particular model run. We're just simply looking at the influence of climate change.

So now we have a climate change we're going to put in our 1D model, and we have some net positive climate change, so there is a net climate change, a net infiltration, there will be a net recharge, but it happens over a varying cycle, which seems to me to be very realistic of how we are going to have to model this site. I don't think we're going to be able to model this site as any kind of steady state system.

Well, what do we get from this? These are some of the kinds of data that we get, and very interesting things come out of these kind of simple models. The no flow is the blue condition, so that's what we get under no flow conditions, and again, this is from N-55. The cyclic climate change, the green is 25,000 years ago, using those inputs, and this is what I call present day, based on that particular climate model, and you can see that even though 25,000 years ago, when it was a lot wetter, versus today, the saturation profiles don't look very much different. That's because the flux is as small as it is.

Steady state condition, tenth of a millimeter. The flux gives us fairly high water contents. In fact, all it takes is a tenth of a millimeter a year flux to start building up a perched water body on top of the vitrophyre.

The argument that we make here is because of the low permeability of the vitrophyre, stopping water flow going downward, and this is where you would start to get into fracture flow. Since this was a matrix model, I stopped at this point so that I could just continue to look at the influence of the climate change.

When we talked a little bit about going into the Calico Hills a little bit earlier, I'd make the argument that you want to look at the Calico Hills, but, in particular, you want to look at the vitrophyre, because this may be a very important area in terms of perching. You can actually have a lot higher flux at Yucca Mountain if you go to a place like UZ-1, because you're going right directly into the PTn nonwelded unit, but in that model, if you look at an example of that, you're going to definitely have perched water building up in that case, and that's where they did stop the drilling because they encountered the drilling fluid, which may have been mixed with a perched water body, but this is a good location to find perched water. There's also another location up here where we might expect to see perched water.

But when we drill down to the Calico Hills, I think we should look for it closely at this level, because this may be high saturation zone, and that's something we want to look at.

One thing that Marty pointed out that I thought was real valuable was that--and this is an idea that we had, and this is in a high-level waste paper, too. This is going to be in next week--is that in the zone you're dealing with here, you're dealing with a system that is not fractured, so you can start to look at the matrix properties to kind of estimate what the permeabilities might have been like.

Okay. One little bit of information I wanted to point out, and this is in terms of permeability, saturated permeability versus relative permeability based on the current model view of things, just wanted to point out one thing: The saturated permeability of the welded tuff above the PTn is higher than the unsaturated permeability of the PTn, which is an indication that the PTn, under current conditions, is not a capillary barrier, because the permeability is so much higher than that of the overlying material. So you did get little bits of information like that, but you can see the little permeability zones; the vitric caprock, very important, and then the vitrophyre of the Topopah Spring.

How does that relate to flux? This is the kind of data we get from a flux profile. This is that mean flux that I said we had in the model, and you can see that we're coming fairly close to that mean flux, but what we look at is the flux getting higher as we get to those high periods, and we get it to be lower. And, actually, as we go across the PTn unit, which is up in here, we don't see much of a change.

Where we really see a sharp change is in this location. We have a definite change in the flux. That's where the vitric caprock of the Topopah Spring is. That's about a six to a twelve-inch thick layer. It covers the entire area. It has to be included in your models. You cannot block average that permeability out with a 10^{-16} and add a 10^{-20} to it and get a $10^{-16.001}$ because it's so thin. You have to incorporate that. That has a tendency to stop the flow under these huge climate cycle changes--in this case, actually, they were small, but that tends to stop the flow. It's got to be in your model, but also, because the PTn is so coarse, it allows water to start to build up in there, and you're going to induce lateral flow in the system, but it's an important layer when you do the modeling.

And the next step is, in this model, is to go in and start putting fractures in so we can increase these fluxes up and test some of these hypotheses that we have. An important thing, though, is the surface climate change doesn't penetrate very deep. In a small example, we'll find out how deep it does penetrate.

When you get down to this layer, especially if you're in the non-fractured part of the rock, you might find that a water potential profile, permeability profile may give you the flux, the average flux of the last 100,000 years or 700,000 years if you can do accurate measurements of this, if you have this kind of a process, because even though you have fracture flow, it goes through that zone as matrix flow.

A 2D view of the site. Again, this is where we started working with Maureen McGraw, Bo Bodvarsson, myself, and Laurie Flint trying to put together a 2D model to look at some of the issues. An example of the kind of data that comes out of that, these are water potential profiles, and some of the flow arrows to see that we can get a large amount of lateral flow if we put a large input into the system. But, again, we are working on a 2D model, and we'll add fractures to this at some point.

We also have a 3D model that we're going to work on, and this is what Bo is going to talk about tomorrow, the site boundary for our 3D model, as we build from our small N-55 model, UZ-16, 2D, 3D, and Bo will talk a little bit more about this, the repository area and how he has set up the grid to do some of this modeling.

Okay. Now I'm going to talk about real time decision making--we're almost finished--and see a little bit about how the study has changed, how experiments end, and, finally, a summary.

Has the study changed? Yes, it has. Based on recent data, we're doing deeper drilling in the neutron holes to examine the role of the PTn, very important unit, and DOE has been very cooperative and understands very quickly this kind of information, and have helped us out a tremendous amount.

What they would do is simply say, yes, that's a legitimate reason. Here's how much is in the drilling budget. If you want, we'll drill these holes deeper, but we may have to eliminate holes down the line if we don't have the money, and you make the decision, so they put it back in my lap to make the decision, and I make the technical decision because this becomes a very critical issue, and DOE has no problem with that, but they've been really nice and they let me drill the rest of the holes anyway, even though I drilled all these deeper.

There's more experimentation and model interaction, very important, and that came out of peer review. Peer review said--and this is true in professional interaction, too, that I've had. They said, you know, why collect data if you don't have a model you're going to use it in, or some reason to be collecting that data, someplace where it fits? So we've been doing a lot more work with the modelers, performance assessment, Los Alamos. I've actually, believe it or not, been working with Tom Buschek to a certain extent putting some of this stuff together.

We are working on an accelerated mapping program to feed some of the modeling efforts. We want to do more mapping quickly to get some baseline information, so we've accelerated that program.

Something I haven't shown any data on, but you'll find in the literature on some high-level waste is our initial stochastic view of Yucca Mountain is becoming increasingly deterministic. There are very well-known, deterministic processes that make this system simpler than what a lot of people thought, and we're putting a lot of this together now and trying to make some sense out of things, but deterministic processes means we can cut down some of our spatial scale measurements and we can cut down some of our statistical measurements, because we find the same thing in the same location. Drill through the PTn unit, N-55 profile, we see that in many, many locations.

An example of what's happened, some of the simple changes, artificial infiltration field testing program has been changed due to our increased knowledge and understanding, from a few simple tests. Originally, we were going to do 23 small plot experiments, 15 large plot, and 49 ponding. Now, we've scaled it back to where we think we can get away with seven small, three large, and seven ponding.

Now, we can work in between these two, but we're trying to reduce the cost because this information doesn't move our modeling ahead. It gives us enough information to get going, but we feel this is a realistic estimate of where we stand now, so we've reduced the cost based on a little knowledge, and this is very important. This is the way our system is designed to work. We put down what we thought would be the worst case, more or less, and we're going to be able to scale back by increased knowledge.

How do we execute these field changes? On the spot field changes, we say, "Drill it deeper." We're standing at the drill rig, and we get to a certain unit.

This is what the study plan says, this is what the work scope says, and we look at it and say, "It's not good enough. We want to go deeper." Now, although Max said that the scientists get to make the final decision, that's not quite true. DOE does make the final decision. We can just petition them very, very, very strongly in the field, and hopefully, someone that's more domineering than me is not out there, so I can get the hole drilled deeper if I need it.

Another important point is that our study plans were written to allow for experimentation and modification. We specifically stated in our study plans that this is the best guess as we stand now, and we designed the study plan to allow it to be changed, and if you're not, you know, people have trouble believing that, they need to look at the study plan number. There's a little R-0 at the end. That's Revision No. 0. That's just the first attempt at getting it right, and I think my study plan may actually be an R-1 now, because I had to change from a shaft to a ramp, and change all the words in it, and that process takes a little bit of time.

The process takes time. The science doesn't take time. The science can change almost immediately to go for the new kind of information you get, and all that other stuff, again, scientific perspective, I have all--I don't even care about that other stuff. Just someone go fix the documents and all that. I'm trying to get the science done and I have to concentrate on that system. So we can make the changes fairly easily. It's not that hard to do, based on new information.

I want to talk a little bit about, in this case, Searching for Certainty, what scientists can know about the future. John Casti has a chapter in here on climate change, and basically, what he says in terms of climate change, is that in terms of climate versus meteorology or the weather, in terms of predicting the weather, he said it's fairly easy. We can predict two or three days in advance.

The first time they tried to predict the weather, John Van Neumann of the Institute for Advanced Study, said that if you gave him the initial conditions on Monday, by Wednesday afternoon he could tell you what the weather was like the day before. So the process was kind of slow. They have sped things up quite a bit, but in terms of climate modeling, climate modeling, they give them a fairly low grade and say, "We don't understand the climate system, and we're not going to do a very good job of climate modeling."

Now, personally, I have my own theory about what's missing in the climate modeling, and since this is some kind of an official record, I'll say it now. I think that you have to take into account the earth's rotation, the relationships between the core, the liquid material around the inner solid core and the mantel, and that interaction in terms of how it affects magnetic fields and the wobble of the earth, and I think that needs to be in the climate models in order for that system to work, but that's not going to happen right away, so I kind of agree with what Marty says.

What I think we need to do is take a system like I showed, where we think we know what past climate change looks like, stress the system until it fails. That's easy to do. Just push the system until it fails, and then look at how far you had to push it. Is that a realistic scenario? You remember that climate change where we had all those cycles. We're at the bottom of a cycle. If you want, go back to the very beginning, 700,000 years ago, and go ahead and just run the system right up to as high as it went, and do it and stress the system, and see how realistic it is.

Ike Winograd says that we're in a frequency domain, not a time domain. We're not going into another Ice Age. He says we have another 5,000 to 10,000 years to go before we'll do that, based on his current information.

DR. LANGMUIR: That's comforting.

DR. FLINT: I know. I didn't bring my winter suit today, so...

So this is just some information. I think, from a hydrologic perspective, I'm going to stress the system, and then I'm going to wait to see what the climate modelers come up when they add this new idea of mine into their modeling.

How do experiments end? That's the next part of what I want to talk about, and there are two parts to this, how experiments end. The first part is really simple. Experiments end when the money runs out. That's what happens to most scientists, and the rest of them, when the time is up, so they run out of time. Another very simple one, though, is when the deliverables are complete. If your job as a scientist is to run an artificial infiltration experiment, you're going to run a ponding experiment, you go out there, you run it, you put all the water in, you let it drain, it's back to natural conditions, it's done. The experiment is over.

You turn in your project to DOE and say, "Here's my report." You're finished. G-Tunnel was a good example of wet versus dry drilling. We did the experiment. We didn't run out of money, we didn't run out of time, and we got the deliverables done. Very simple. That's when experiments end, when you've met your objective.

But, more importantly, not when do experiments end, but why do experiments end if you don't run out of money or time? I think they end when the science and the performance assessment, in this case, are satisfied that it can predict or explain the results of an experiment or an observation. I mean, you've simply developed adequate confidence in the models or the results.

I think the way we do this is what I've been proposing, and a lot of other people have, too, is that we take our most current information and we predict the results of the next borehole or of the unit contacts. That tests our 3D model. Are we going to find calcite? That tests some of our geochemistry models. What are the water saturations going to be? That tests our hydrology modeling.

The last thing you want to do is wait until you get to the last borehole before you want to do performance assessment modeling; otherwise, you don't have anything to test it against. I think that performance assessment modeling ought to be going full swing right now, and they ought to be predicting what they're going to see in UZ-14.

We tried UZ-16, and how well we did, we don't know all that yet, although I have some wonderful data in the overheads. We don't know all that yet, but I think that the performance assessment model and all the other modeling ought to be trying to predict what's at UZ-14, and the next hole, and the next hole, and the next hole, and the next hole. I think we ought to be doing that right now. I think PA ought to be doing that right now.

Another way is when the model results are insensitive to additional or more detailed data. You keep putting in more and more data and nothing changes in your model. Stop. Go do something else. That's why I think some experiments should end.

In summary, this kind of summarizes the entire idea that I've tried to get across, and what we're trying to do, and I want to bring up one point here. We don't go at, I don't think, this program to answer a specific question, like what are the influences of climate change on infiltration? We go after the issues to answer all of those questions, the next Board meeting, and the next one after that, and the next one after that. We hope that our study has incorporated all of the thinking so that we can answer any of the questions.

So we establish a well thought out, comprehensive plan for site characterization. We've done that. I think we've done a good job of it. We set the priorities for critical paths for understanding the site through measurements, observations, and modeling. So we've set the priorities through those various techniques. They're out there. We're ready.

Then the next one, and the hardest one to do in this program, is carry out the studies, analyze the data, and run the models. That's where we are now. Because we're just here now, this is not the right time to re-think everything and re-think what you're going to do and change thermal loads and all that all at one time. You need to get some of this information done. You need to get some of these studies carried out and do some of this data analysis. Here's where we are today, right here.

And then when you get some of that information, then you go to the last step: Redirect the research and the model activities to meet the objectives as redefined by the results. You get a chance to redirect. You get a chance to change some of your thinking and change your mind, to a certain extent.

Granted, I've made a few changes in here, but we're not ready to change and throw out all of our drilling program all of a sudden. We might. We have it in our plans to be able to do that, combine boreholes, do all that kind of thing, and it's easy to do, I believe, but we do that when we get down here, when we start to get some results, and then at some point in here when redirecting of our objectives change, we redirect and we don't have to do anymore. We're finished with the study.

Okay. That's it.

DR. DOMENICO: Thank you, Alan.

Any questions from Board members?

(No audible response.)

DR. DOMENICO: I have one, then, if I can. Your example model hierarchy, could you put that one up with all those little flow things? It's one of the early ones; first one, as a matter of fact.

DR. FLINT: The first one, example model hierarchy? Can somebody--Laurie, can you turn the slides? And is it--did I hit anybody with the tip of the pointer while I was talking? Well, there's another one here. I'll just use this one. Whose pointer? I'll just take the tip, actually.

First slide?

DR. DOMENICO: First slide.

(Pause.)

DR. DOMENICO: I'm trying to put this study in perspective of this program. It seems to me that, as I see this, this would be very valuable to give information on groundwater travel time, for one, under ambient conditions, which is a requirement.

I don't see how this study reflects on performance, how the repository will perform once we pump some heat into the system, or if it does pertain to that, or if it can be narrowed to pertain to that.

DR. FLINT: Oh, on. Whether you do or whether you do not pump heat into the system, there's a whole variety of ways you can do that. You still need some basic information on how the system operates; fracture flow, matrix flow interactions, properties. What I tried to do was put together a series of properties and a series of concepts, some modeling techniques to try to look at what's happening today, and the influence of climate change on a natural system.

That's only part of the model, and that doesn't incorporate that part of the system, but my modeling work, which, actually, I've been really lucky and I sort of get to jump around in all of this. That goes into the larger-scale models. A lot of this information that I showed--and some of those results were very, very important to Tom Buschek, and we spent a great deal of time going over some of these results so he could incorporate that thinking into his model and the important of what he, you know, the importance he puts on the high temperature concept, the extended dry concept. A lot of that came out of some of the results that we were getting.

So it, in itself, does not. It's trying to do the site characterization, to define the site, what is the character of the site. Because, to give you an example of what I think I'm trying to do and what I'm trying to get at, is for people that have done a lot of transport modeling, they simply assume that the hydrology is correct, and then they spend all this detailed time in transport when we can't make the water move in the right locations yet.

I think you have to have a good hydrologic model before you can do a transport model, and I think you need a good hydrologic model that has the correct physics and the correct geometry, and I think the geometry is one of the most important things that we have at Yucca Mountain site. You have to have that correct before you can start putting in your thermal loading. You have to know what the features are like. You have to know what the faults are like. Joe Rousseau will talk in some detail about that, and we're trying to make our system consistent with what observations we see at the site. You need that information before you do that, I think.

DR. DOMENICO: Okay. Any Board questions?

(No audible response.)

DR. DOMENICO: Staff?

DR. REITER: This is sort of a takeoff of an exchange between Marty and Max in the last session.

DR. FLINT: I already do field trips.

(Laughter.)

DR. REITER: Excuse me?

DR. FLINT: I already give field trips.

DR. REITER: The question between Marty and Max was with the conservatism of the models, and I think Max indicated that the WEEPS model was, indeed, a very conservative model with respect to reality, and I sort of heard that argument before, sometimes expressed in a more strong view, that essentially, the WEEPS model encompasses the worst that we can find at Yucca Mountain; that, essentially, the kind of work we're doing now is just backup to have in our pocket, because we've already demonstrated that the site is safe.

Could you comment on that? I mean, the assumption of the WEEPS model, which is--and this is not my words--which is conservative to reality, to any reality we can find, and you show--let's assume the criteria remained the same, you show that even with that worst WEEPS model, you don't exceed the CCDF; therefore, we've really demonstrated--we, DOE--have demonstrated that the site is really--groundwater is not a problem.

DR. FLINT: Okay, I think I can address that question, from my perspective, anyway, from what I see.

First off, a model of the site, when the first dry drill borehole to the water table core analysis, properties analysis is sitting in an overhead that no one else has seen before, is a model that I have some questions about, because they don't account for what we've seen.

I don't think that the modeling that's been done to date has been done on any kind of data set at all, a very good data set. I don't think it's realistic, I don't think it's right, I don't think it's accounted for the huge fracture network that we really have down there, not what we think we had from ten years ago. I don't think we're ready to make any kind of conclusion like that, because I don't think the modelers have worked with the real Yucca Mountain yet.

DR. REITER: Nobody's arguing whether it's realistic or not. The argument that I've heard, indirectly or directly, is that it's conservative.

DR. FLINT: Well, if it's not realistic, we don't know if it's conservative.

DR. REITER: So then you disagree with that idea?

DR. FLINT: Yeah. I guess I do; yeah. No, I don't think that it--I don't think they have the worst case in there. I don't think they have the failure mechanisms in there. I think they're missing those, and I think with site characterization, we can get at that.

MR. ROUSSEAU: Yes, Joe Rousseau.

I'm not at all familiar with the model you're referring to. Maybe you could add--

DR. REITER: The WEEPS model.

MR. ROUSSEAU: Yeah. I've heard the name. I heard it earlier today, and, personally, I'm not right into it. Maybe you can expand a little bit on it.

DR. REITER: This is a DOE model, the Sandia model for performance assessment. I'm sure somebody from DOE can explain it a lot better than I can.

DR. FLINT: Well, again, I'm familiar to a certain extent with it, and the other models that Sandia's been running, and Sandia, I think, and the modelers--in fact, the modelers are the last ones that'll get up and defend those models.

DR. REITER: This assumes fractures all over the place, running through everywhere, and essentially zero travel time to the--well, I don't--I'm sure somebody here can explain it a lot better than I can.

DR. DOMENICO: Jerry Boak's going to enlighten us all here.

DR. BOAK: What we have characterized as the WEEPS model is a model in which all water that lands on the mountain passes through fractures and goes at essentially an infinite velocity, or maybe limited by the speed of sound or something like that, through the repository, picks up water, picks up radionuclides, and carries them directly down to the water table.

We called it bounding in the sense that it was an ultimate limit on the degree of fracture matrix interaction, in that it said that there was none. We actually found that as the model was structured, and given the flux distribution that we had there, that, in fact, the radionuclide releases to the water table, and, therefore, to the accessible environment were actually less than in the composite porosity model.

Have we demonstrated safety on the basis of these two models? I wouldn't have ever suggested that we had actually demonstrated safety, but that we had, in some ways, bounded one aspect of the question of fracture matrix interaction; that we felt that given that this involved complete equilibrium between fractures and matrix, and that this involved absolutely zero equilibrium between fractures and matrix.

DR. DOMENICO: The WEEPS model, is that with less of a--so it's not a conservative model, if it resulted in less than the composite model.

DR. BOAK: Yes, that's right. We felt that we had bounded the degree of fracture matrix interaction with these two models. It turns out that, in fact, the releases are larger for this, which was not what we expected in the way of a result, because we thought if we had a lot of fractures with water running through them, and have it, essentially, move through instantaneously, we thought we would find ourselves with larger releases.

DR. FLINT: Well, I think what we're looking at in this is a very important point, and it has to do with issues like groundwater travel time, where you're looking at the speed at which the water can go through, and then you're also--in this case, we have some fairly fast times, but we have low volumes, small amounts of water, but high velocities.

In here, we're actually looking at large volumes of water at a slower rate, but the large volume, as Jerry said, can move more material through the system. Again, in this case, the fractures are a benefit. There are not as many--I mean, the pathways are faster, but there's less volume of water.

DR. REITER: I think the reason that the, if I remember correctly, the WEEPS model showed lesser release is because less packages were contacted, and I think that the argument was either of those models, with respect to groundwater, meant what you did with them did not show exceedence (sic) of the EPA criteria, and, therefore, the argument was that, essentially, you've satisfied the EPA criteria by these conservative and bounding models.

DR. FLINT: I don't think the bounding, in this case, where you have minimal contact in this case, because what you have, if you look at this very closely, you have six or seven pathways from the surface. If you were to extend all of these and put 100 or 200 or 300 of those in there--

DR. REITER: I think that's what they assumed. It's not six or seven.

DR. FLINT: Well, relatively speaking, you actually can have more contact, but I don't know.

DR. DOMENICO: Well, I think at this stage let's get on with the program. We have reserved some time for discussion.

If someone is interested in this, we can bring it up again.

And the last speaker on the program, I believe it's the last, Joe Rousseau, telling us a little bit about percolation in the unsaturated zone.

MR. ROUSSEAU: I'm going to be doing a multimedia here, and I have two assistants that'll be helping me in back, Linda and Helen, because I'll be using two slide projectors during the course of my presentation.

My name's Joe Rousseau, and I'm Project Chief for the deep unsaturated zone studies, USGS. We're dealing in the program that asked the program to drill a lot of expensive boreholes to get us to the water table so that we can study the deep percolation process at Yucca Mountain.

Could I have the first slide, Linda, please? I might spend a couple seconds here to get things adjusted.

I chose the title for this talk to be the "Features-Based Drilling Approach for Deep Percolation Studies at Yucca Mountain," simply because I wanted to bring focus to, perhaps, a very expensive component of the program, and emphasize why we picked certain areas to drill deep into Yucca Mountain, and bring, basically, the focus into the borehole environment, rather than getting into things that have to deal with maybe modeling studies and things that other people have done, because I feel I've been on the program now for almost seven and a half years, and we recently completed what I consider our first effort in studying the deep unsaturated zone.

When I first got on the program, two months later, we were in the "stop work" order, and we subsequently spent a number of years developing technology to do work that we felt was important, defending our program, going through an extensive amount of review, peer review, and that sort of thing.

Could I have the next slide on the right, please? Excuse me, I'm sorry. Helen, back that one up. Thank you.

Presentation outline. I'd like to go over the purpose and objectives of our program, do a quick overview of percolation studies, and in that overview I want to highlight work that's integral and part and parcel of our particular study plan, but also give credit to some other people, investigators, either within the USGS, or within Los Alamos or other groups who are also studying the deep UZ percolation problem.

I want to spend a little bit of time emphasizing our borehole siting strategy, which is outlined in our study plan, and it kind of compliments something that was said here earlier, which is basically, let's test the system to failure. Well, I suggest that maybe we test our understanding to failure, and that basically is the philosophy for our borehole siting strategy.

I'd like to go over the existing database, so that we get some appreciation of just which boreholes at Yucca Mountain provide us deep percolation information; how we set priorities within the program; what changes we have in the study to date; and that upper section there, I'd like to be able to get through this in about 15 minutes, spend about 20 minutes talking about some preliminary findings and possible interpretations of what we saw at UZ-16.

Now, the next slide, Linda, please.

The purpose and objectives of our program are to characterize present day flux in the unsaturated zone to Yucca Mountain. By and large, within the context of our study plan, we have three basic programs. There are other things that we'll be doing in these boreholes and that we've accommodated, and other people will be doing work within them, but, by and large, we're looking at a matrix hydrologic properties testing program, which is pretty much done by Alan Flint and his group; an in situ permeability testing program, which is primarily Gary Lecain's work; and an in situ fluid flow potential program, which is what I've been intimately involved in for the past four or five years.

We also have other programs. In particular, we have a vertical seismic profiling program. We have plans to do gas tracer diffusion testing. We have plans to do water flooding experiments in the deep boreholes.

If I were to characterize the objectives of the program, I could state simply that my emphasis in the program is concentrated flux. A lot of work has been done on dealing with the uniform flux-type problem, mostly because it's been easy to get your hands on the information, to either get core from outcrops, do laboratory determinations of those sorts of things, but we know we have a fracture system. We also know that we have a faulted system, and the possibility of concentrated flux, in my view, is very real.

Could I have the next slide? Bear with me a minute here. Helen and Linda both, please?

This next series of slides will be developed to try and give you an overview of the studies that are contained within this idea of percolation. What I want to do over here on the right is show you that we have various studies ongoing, the percolation, the infiltration, the discharge of the infiltration becomes the recharge of the percolation, if you want to view it that way. The discharge of the percolation becomes the recharge to the saturated zone, and the climate, of course, is going to affect the amount of flux in any of these sorts of systems, and then we have a series of ESF tests that are designed to feed this UZ site characterization of hydrology.

I'm going to expand on these blocks in here to show you what the scope of the program is, but, by and large, these are designed to define physical system, produce numerical model simulations and analyses, concede performance assessment directly, or, alternatively, I believe a lot of our information that we're going to be collecting will not necessarily be massaged through a model, but what it'll probably do is tell us a process is or isn't important, and if it isn't important, then somehow or another we have to "cartoonize" that process, if you will, and include it in numerical simulation.

We also have to be sensitive that what we see today may not exactly what will happen tomorrow. Flux today that might be predominantly a uniform-type flux problem, could very easily in the future be a concentrated-type flux problem.

Could I have the next slide, Linda, please?

Expanding on the blocks over here, the green and the brown, these studies that I show over here, the matrix hydrologic properties, what I'm going to do here is I'm going to show the data source as a borehole. We have a series of studies in here. We have some very definite objectives, and we have some scale features related to those things, and I want to trace you back from the scale features back to where the actual measurement's being taken, so consider matrix hydrologic properties. It's a core measurement.

You get porosity, relative permeability, moisture retention, so on and so forth. You've all seen this before, and it's a relatively small scale measurement and it's all right. Clearly, when you start to put lots of core information together in the vertical sense, then you can expand the scale of the measurement. If you take it two-dimensionally, aerially, then you're expanding again into a three-dimensional sort of thing. What I want to do is tie this back to a scale of actual physical measurement. Here it's a core.

Air permeability testing. The scale of the measurement is actually the borehole, and here we're trying to define fracture and matrix permeability combined, bring the two together and define fracture inter-connectedness. This I consider a medium to large scale-type test.

The reason I'm emphasizing scale here is because multiple boreholes are scale designed. Give us information over an area that's much larger than 12 inches. In this particular case, the multiple borehole size will give us information out 100 to 150, 200 feet.

Fluid flow potentials are borehole level measurements. It's pneumatic pressure, temperature, water potential. I'll bring this up a little bit later and describe why this is important. Flow directions and gradients in the multi-phase environment, system stability, diffusion and saturation permeability, that's what this set of measurements is all about. It's considered to be a very large scale thing.

Why is it large scale? The atmosphere perturbations are large scale, the geothermal gradient is a large scale feature. The geometry of the mountain is a large scale feature. Even though we measure this at very discrete points in a borehole, it's a very large scale measurement because it responds to perturbations that are very large.

The bottom slide, vertical seismic profile of the borehole, is designed to give us distinct geometry at two sites; 3D subsurface imaging, geologic structure, fault/fracture system continuity. It's a very large scale program, extending out to distances of about 2500 feet from the UZ-16 complex, or 2500 feet out from the UZ-6 when fully completed.

Could I have the next slide, Linda, please?

Here are some other studies that are not defined within the context of the study plan that I work to; Chlorine 36 studies that are being done by Los Alamos. It's objective is to date water, and it has a special spatial scale now, zero to 50 years. So now we see scale both--I mean, a temporal scale. We see scale defined either temporally or spatially.

An extensive amount of hydrochemistry work being done in both the core and the boreholes that are being drilled; dating of water and gas, pore-water chemistry, gas chemistry. To give you an idea, tritium is 0 to 100 times scale measurement; ^{14}Carbon, a 100 to 40,000 year scale measurement.

Another study that's very integrally involved with the borehole is the gaseous phase flow work. You've heard these presentations before by Ed Weeks. It's a convective gas-flow process is what it's trying to define, and it, again, is considered a very large type scale measurement.

Could I have the next slides, Linda and Helen, please?

Our siting strategy in the study plan was to target those areas of interest with the greatest potential to provide the evidence needed to assess the suitability of Yucca Mountain. When I wrote this, I wrote this in a positive mode. If you want to invert this a little bit, it says, go find the show-stoppers, go challenge our understanding of the mountain. If there's no problem, there's no problem, but I don't feel that we can afford to avoid it, and there is the emphasis of the deep percolation program.

Could I have the next slide, Linda and Helen, please?

We established certain siting criteria which are identified in the study plan, and these were basically large-scale structural features, surface drainage features, and topographic features, and probably as you've already seen, Alan has pretty much done the same thing in his neutron moisture program, the drainage features and the topographic features, and look at how those things varied.

How do we translate into where we sited boreholes at Yucca Mountain? Okay, let's start where we just finished a borehole, UZ-16. We actually have four boreholes sited at one location. It's primary target, if you will, was Imbricate Fault structure. This has been mapped, Scott & Bonk, if you find it. It's been renamed now. It's called the Broken Zone. This looked like an obvious place to go take a look at deep percolation processes at Yucca Mountain.

Ghost Dance Fault, two boreholes we've sited there, UZ-7 and UZ-8. Only until recently has, perhaps, the importance of the Ghost Dance Fault, if you will, come to the surface, but it is obvious, if you take a look at recent aerial photographs, you'll see this structure traverse the mountain from north to south.

UZ-6, 2, 3 and 15, four boreholes at the crest of Yucca Mountain. UZ-6 has already been drilled. It did not reach its total depth. I'll get to that in a minute, but here we have a very, very pronounced feature that we felt that we needed to build some scale to some measurement here, and try to understand what is really going on between the escarpment and percolation processes deeper in the mountain.

UZ-11 and 12 were sited to take a look at the Solitario Canyon Fault. If you look at that sequence of boreholes, we're basically developing a transect right now, and we're not asking for lots and lots of places to go take a look at things. Let's give it a one-time shot and see what it really looks like.

Moving up into the north end of Yucca Mountain, UZ-1 was already drilled and we learned something there that no one had expected. G-1 was drilled about three years prior to UZ-1, 55,000 barrels of drilling fluid were lost, and, lo and behold, they show up at UZ-1, so fast fracture flow is possible. Whether or not that's perched water, or whether it's water mixed into a high water table, we won't know, but we're back on UZ-1 now with UZ-14. We'll take another look at that. That particular borehole was sited in the Drill Hole Wash and "Fault"? These are not formally named right now, but I don't think any geologist would argue that this is probably a fault-controlled range.

Pagany Wash, UZ-4 and 5, lots of neutron holes in here, too, but here we go again; a very pronounced drainage feature with a fault structure. So I think if you take a look at the way we've distributed our locations, we've been very selective, very sensitive to the idea that we need to have scale built into our studies.

Could I have the next slides, Helen and Linda, please?

I'd like to spend a few moments going over the existing database at Yucca Mountain, and why I think it's important to recognize that we're really working with a very limited database in terms of understanding deep unsaturated zone percolation.

Could I have the next slide, Linda, please?

This is a map, if you will, of all the boreholes, existing boreholes in the vicinity of Yucca Mountain. There are some 122 boreholes in here, the P-holes, C-holes, G-holes, WT-holes, H series, N series, early UZ boreholes, et cetera. The whole idea here, we've got a lot of holes, but how much information do we have that--could I have the next slide, Linda, please?

This set of boreholes are the dry-drilled boreholes in the vicinity of Yucca Mountain. We see the density going way down. The predominance of this suite of boreholes belong to the neutron borehole program, depths of 50 to 250 feet.

Could I have the next slide, Linda?

What we'll see in the next two slides are probably the only place where we have a definitive base of information just to understand the deep UZ percolation.

Could I have the next slide, Linda?

Here we are. This is what's left over. Now, this is not a complete package. We have UZ-1, NRG-6, UZ-6s, 6, and UZ-16. These are all the existing dry-drilled boreholes deeper than 500 feet at Yucca Mountain. We're below the zone of the net infiltration zone that Alan Flint had defined earlier.

Linda, could I have the next slide, please?

Now, this is the set of boreholes that have been cored, dry-drilled, they're deeper than 500 feet in the vicinity of Yucca Mountain. Now, one of them goes to the water table; UZ-16, completed on March 12th. An attempt was made at UZ-6 to go to the water table. It terminated at 1800 feet. UZ-1 was supposed to go to the water table, ran into that water, and they stopped it.

Could I have the--I hope this gives you an idea that we're working with somewhat of a limited database at this stage in the game. Could I have the next slide, Linda?

I'd like to spend a few minutes going over drilling sequence and prioritization, and how we decided which boreholes to drill first and how to switch sequence of those.

Could I have the next slide, Linda and Helen, please?

We set these priorities; importance to early site-suitability assessments. There were some operational and technical constraints relative to the program; in particular, limited resources available. Many people want boreholes drilled. This set of bullets, if you will, are not necessarily mutually exclusive. We have test interference constraints. When we pick a borehole to work on, we have to get off the borehole so in-borehole testing can be done, so the rig has to be moved off site, taken someplace else. We also want to optimize information return. In order to do that, we have to see what that first set of information looks like, and it takes many months to get it all together.

Why do we want to do that? We want to start to develop some working hypotheses. These are expensive holes. Where should we put the next hole? How far away from that satellite hole should we do the work? And, of course, there are funding and resource limitations.

Could I have the next slide, Linda, please?

I have this slide here on the right so that you can see what we're trying to do here. This is one drilling sequence and prioritization plan for the deep UZ boreholes. We started with UZ-16. This was a seed borehole. It is a geophone-instrumented borehole, primary purpose from deep UZ percolation work is to try to develop some geometry in the system. Where do we site the next hole? Where do we site the borehole in the Ghost Dance Fault, UZ-7 and UZ-8? We hope to do it intelligently. We run the VSP surveys to try to give us that kind of information before we start the work.

After having completed 16, we have to move off site because we have a lot of in-hole testing to do, and the rig is now sitting over at UZ-14. UZ-14 will be a hydro-instrumented borehole. Hopefully, we'll be back and have completed our first recon survey and have selected a site for UZ-7. Now, how do we pick UZ-9? We'll have done that with VSP surveys, next to UZ-16, start to develop some very, very high detail resolution imaging between UZ-9 and UZ-16, between UZ-9a, UZ-16, 9b, UZ-16.

But, as you can see, this is a sequence-logical package in order to take the program from point to point in an intelligent manner, reduce the overall cost of the program, let's get information that's valuable to the program, and not just drill holes.

Could I have the next slide, Linda?

This is our drilling sequence and prioritization scheme II. Ideally, we'd have two rigs working, so we'd begin working this scheme the same time we're working the other one. The seed borehole here is UZ-6, geophone-instrumented VSP borehole. Move off site so we have time to site UZ-11, UZ-12 using VSP. These are the two holes in the Solitario Canyon system.

We can go over to UZ-4, drill UZ-5, take care of that business, and, again, try to integrate all the testing done at these local sites where we have multiple boreholes located.

Next slide, Linda?

Now, we had two boreholes in the program that don't show up here, UZ-10 and UZ-13. Originally, they were on the south end of the boundary for Yucca Mountain repository. They are non-structurally controlled boreholes. They were there to just give us some sort of a baseline in systems where we don't have strong structural influences, drainages, and that sort of thing. These can be placed anywhere.

When we first started our program, we were told to stay outside, to the extent practical, controlled perimeter drift boundary, but Sandia got their program in place, systematic drill holes, so we said, we can drop those. We'll go take care of a couple of theirs, and accomplish the same objective. So this is a change in our program right there.

Okay, could I have the next slide, Linda? I'm three minutes running behind, but I'll catch up.

Our changes in our study. To date, we've had limited changes. The program is in its infancy. We've drilled 15 per cent of the total footages we originally requested. We've included a select number of systematic boreholes, the Sandia boreholes, into the testing measurement program, a select number meaning six from the point of view of instrumentation, and all of them from the point of view of in-borehole testing, active testing.

There's a possibility to eliminate boreholes outside of the CPDB with inclusion of these systematic boreholes. I've already defined those as UZ-10 and 13. There's also a possibility that we could reduce the depths of some of these multiple boreholes at these sites if we're very comfortable with that kind of decision, we don't need additional information.

Alternatively, we may be able to reduce some of the coring requirements. So some of the holes may not be taken to total depth, if we're comfortable with that, and we may be able to drop some of the coring requirements and reduce costs here.

Okay. Now I'd like to do the thing that's closest to my heart, take a look at UZ-16. Could I have the next slide, Linda and Helen, please?

These are very preliminary findings. We completed the borehole March 12th. Alan has done a lot of work on the core. We've had geophysical logging done at the site. It took almost nine months to construct this hole. We started on May 27th, but I think it's important to see what sort of results we got out of this hole. I, personally, am very excited. I think it was a total success. Yes, a lot of years went into working this prototype rig, testing it, making sure things could work properly, but we got our objectives. We cored that hole, we got to the water table, we went into the water table, we managed to do that without damaging the hole.

Could I have the next slide, Linda and Helen, please?

What I want to show you here is a preliminary fracture map, neutron moisture log and the sort of thing that we've gotten out of the hole. This is the stratigraphic log here. Alan had already pointed out that the vitrophyre is a very, very important feature. We know the Paintbrush Tuff up here is a very important feature. We also know that this caprock unit sitting on top of the upper nonlithophysal Topopah Spring is also a very important feature.

We got excellent core recovery out of this hole. Many, many times, it was 100 per cent. The only places where we lost good core recovery, we went through a fault at the basal vitrophyre right here, and even though Alan had made the comment that we presumed perched water could form over the basal vitrophyre, when we went through that fault, we found that the whole thing had fallen apart, that there's really nothing that would hold water. I mean, it was intentionally fractured. It would easily flow water.

These traces that you see here are kind of exciting. Alan has a 5 curie moisture porosity tool that's basically designed for work in the saturated zone. We went ahead and ran it down a hole just to see what would happen. We got excellent correlation in the stratigraphic contacts.

We're not sure right now how these traces will correlate with moisture or water contents, but it's going to be looked into.

Look at this very, very sharp boundary here in the lower nonwelded Calico Hills, Topopah; very sharp break here between the Calico Hills and the Prow Pass. You can see some wave trends that are developed in here, and when you start to look over here at the fracture density, they make a lot of sense.

Up in here, we may not have correlated information with respect to moisture content simply because a big piece of the Paintbrush Tuff in here sloughed out of the hole, but excellent gauge down the hole, nominally running about 13 inches, big breakouts in the basal vitrophyre, Paintbrush Tuff here, and a section right below the caprock, but it's a testable hole. We can actually conduct a test in this borehole. We couldn't do that with the other holes that have been drilled.

What I'm showing over here on this trace is the fracture density of the various units, and you'll see that, invariably, the nonlithophysal units have the highest fracture density. When you get into the lithophysal units, that density falls off. I think there's a logical explanation for that, in that the lithophysae themselves are actually taking the strain of the rock, deforming, and not fracturing up.

You see huge, huge fracture density counts in here. These are counts per ten feet. It took one individual about two man months to map every one of those fractures in the core. I'll say a little bit more about this in a minute. Some of these fractures that we see down in the Calico Hills and Prow Pass may, in fact, be drilling-induced.

The interesting thing that happened with the neutron log here, Calico Hills was very nearly saturated, but when we got to the Prow Pass, we're dropping way off in saturation. There's a marked increase in the porosity of the Prow Pass, too. It's important, I think, to have seen that.

The exciting thing about the neutron log is we may be able to go in many of the existing holes, run the same log now, now that we have something to correlate it to, we've got the core section, and expand our information base a hundredfold, if it works; if it works.

Fracture density, moving over here, we see most of the fractures are nearly vertical. There are some places in here where there are no fractures, and very, very few places where there are just horizontal fractures. I should point out that fracture density alone is not necessarily an overwhelming feature one should be concerned about. Drainage has already been talked about as an important feature of the repository horizon, which is right here, but the thing that we're not trying to map here at all are apertures, and apertures are what actually controls fluid flow. We're not attempting to do that, at least on this presentation.

Over here on the far right, I do show some interesting things that occur when we did map the core, and that's the occurrence of vugs, and these look to be like residual depositional features in the fracture systems themselves, which indicates that the lower nonlithophysal unit here has a lot of vugs, and probably these vugs are interconnected and they're the residual anastomosing channels where water can run in the fracture planes.

Could I have the next slide, Linda and Helen, please?

I'd like to go over this in some detail, because by dry-drilling and coring, we were able to do something that we've never been able to do before. We certainly gained a lot of sensitivity about something that might have been passed up had there been fluid in the borehole. It was probably fortunate, coincidental, or whatever you want to call it that I was here on the day that we first encountered the water, and I would have missed that by about 30 minutes had I not caught an earlier flight out of Denver. I normally like to get to the airport about an hour early so I get my work put together, and the gate attendant says, "Hey, you can run right now. We'll get you on the flight." Thirty minutes later, we hit the water.

What I'm trying to show you here is that we went to two five-foot core runs and the core came up dry. Then we went through a five-foot core section in here in which the core surface came up wet, but when Alan has studied this stuff in his lab, we found out that the core was not actually saturated. There was only one place on that core section, and it was right here where we hit the water in a fracture that was saturated. Everything up above and below it was dry.

We went another five feet, and we bailed at that point, and we got some water for Rad safe testing and some water for hydrochemistry testing. Then we pulled out of the hole, we pulled back, said, "Okay, we want to do some work."

One of the things we were concerned about is, can we continue to drill in what we thought was a saturated zone, without damaging the unsaturated zone. So we pulled out of the hole, and Alan came in with his neutron moisture tool. We attempted to get a water level, we couldn't. This is estimated. This is my best guess having been there and seeing the core.

We ran that neutron moisture profile that you saw earlier. Then we went back in the hole and started reaming. We had a lot of backfall, we had to clean it up; started reaming, and when we went through this zone again, this whole section, reamed section came up wet returns, so I put the water level--probably that water level's right here. Interestingly, when we finally see stabilized water levels, we're very, very close to where we first encountered water.

The next section of reaming, all the stuff came up dry, all the cuttings came up dry, the bailer runs came up dry. A couple days later, we did get a water level measurement. It would appear that the water was draining into the hole, so we picked that up, continued to ream, bailed, came up dry, cored, came out dry, cored some more. We got another water level measurement here, right here from water draining higher up in the section. We bailed, got some more sample at that point, drilled some more, came up dry.

Finally, we hit saturated zone. To me--and then what happened, the hole came back up to piezometric level. This whole section in here is basically a center-confined or confined aquifer, and this has been suggested by many of the previous investigators for various reasons, but now here's prima facie evidence that this is what we actually have going on.

I have some interpretations which I will go into in a minute. What we subsequently did is continue to core down at five-foot increments, bailing periodically as we're going down, and then we circulated water out of the hole, some 200 gallons, thinking we might need it to continue to drill the hole, so we dropped water levels during that period, finally finished the hole out at 1686, and water levels just recovered, and now they're pretty stable, 1605. They're beginning to look like the water conditions at H-4 or other places where they're monitoring water levels.

This is the information base, but now I have some interpretations. Let me go through some of the other findings. Could I have the next slide, Linda, please?

Here are some of the findings. We found that the Imbricate Fault, at least the one that we drilled, the borehole's about 2½ to 3° deviation, is almost a vertical fault. The rake angles that came in off the core indicated 88°. If you correct two degrees, if you will, the borehole deviation and presume that the core was running at two degrees, you have almost a vertical fault.

We also noted the fracture density in the Topopah Spring was much greater than earlier estimates, probably because close proximity to a fault. The range is 50 to 250 per cubic meter, and compare the average--I just did an eyeball average on that: 125 versus 50 used as an average value from Montazer & Wilson. This very well may be a true average value when you're outside of structurally-controlled zones. Clearly, I think, if you're close to structurally-controlled zones, we're going to site some very, very high densities of fractures.

Water that we encountered in the fractures in the

Prow Pass is in an non-saturated matrix environment, and this is interesting in terms of trying to flow some hypotheses now about what might be going on.

Could I have the next slide, Linda, please?

Okay. I'd like to discuss some possible interpretations, and you need to remember the water level data that I showed you earlier.

Could I have the next slide, Linda?

Okay. To remind you, here's the setting. We picked UZ-16 to be at the confluence of three drainages, draining west to east. This is the Imbricate Fault structure. These are some fault trends or patterns or traces that people have traced on various maps. We just put it together here. We didn't necessarily try to site this borehole in the active channel; in fact, it's outside of the active channel, but it is near the confluence of three drainages.

The other thing to factor in here is that if these faults, in fact, are conducting water, all the saturated zone studies would indicate that, laterally, these things have very, very high permeabilities. The water table's almost flat. If they're, in fact, measuring in the fault zones, the fault zones are telling us they're very, very conductive. What we don't know right now, because we intersected the section at the base of the basal vitrophyre, whether or not the Calico Hills unit zeolitized can actually maintain an opening. We missed it the first time. We have plans to drill three more holes very near to this location, using VSP and other techniques to make those very intelligent locations. Maybe it should be over here, one over here, one up here; don't know.

I only put this slide up to say, hey, here's some possibilities. Here's some potentials for, if you will concentrated recharge into the unsaturated zone. I also show the Ghost Dance Fault structure in here, a lot of work, lot of activity going on in this area now. We eventually hope to put a borehole over here. I hope to use VSP to site that borehole from UZ-16.

Could I have the next slide, Linda, please? Okay, I need the next slide, Helen.

What I'm going to do, in looking at these interpretations, is just basically broad-sweep two, and possibly three different ways to interpret the water level information.

This is a slide Larry didn't want me to show. Do you have it, Larry?

(Laughter.)

MR. ROUSSEAU: Let me start back over here, because I think I need to explain some of the background here. This is a fairly sanitized version in which to work from. I basically don't do anything here that's any different from the conceptual models of Parviz, Montazer, and Bill Wilson, or other people. We'll let these arrows here kind of represent precipitation or stream flow or whatever you want.

There has to be some sort of input into the system, and there is an infiltration, if you will, through the alluvial cover into the fractures in the Tiva Canyon. I allow for exfiltration, if you will, for gaseous-phase flow of vapor going out of the system, allow for lateral flow to occur in the Paintbrush. I think most investigators would agree that some scheme like this is probably fairly realistic in terms of the matrix system.

I reduce some of the flux, if you will, of the Topopah Spring, but I want to still allow some gas circulation to occur in here. We don't know for sure if this is a fact or not. We haven't gotten down and measured this sort of thing. We have seen flow in the Topopah Spring at UZ-6. To what extent it occurs, where you don't have the topographic relief, no one knows, but I just want to include this as part of the discussion right now.

Again, some reduced flux, if you will, to the basal vitrophyre, and here allowing for lateral movement across the top of the basal vitrophyre, assuming this is an ideal candidate in which to perch water, which we didn't see when we went through the section; a Calico Hills unit here, some more flux in there, some reduced flux to the Prow Pass.

I guess the key point to make from here is how and what does this water level represent down here. Do we vertically accrete water to the saturated matrix that is confined, or do we bring water into the system via some sort of little fault mechanism? And is the "water" that we saw at this point in here actually telling us that maybe we're seeing water in a fault zone that would be equivalent to a piezometer--an open two piezometer if you were sitting in here--is that water trying to get into the system, or is that residual water sitting up here maintaining a level because it's a confined aquifer? Clearly, it could be either/or. We don't know for sure right now.

If you look at it from a perspective of percolation and you know you've got a confined system, it has some serious implications in my mind. Why? Because there's very little saturation available in a saturated matrix. It's confined already, and perhaps along the fault system itself--and this is why I included gas in here--we could have a condition system where we could accrete flow into the system deep by passing through the Paintbrush nonwelded unit, we could accrete flow to the system by perching on top of the basal vitrophyre with downward fault flow into the unit, we can bring flow into the system through direct infiltration up here.

It may very well turn out that this is climate insensitive. Why would it be climate insensitive? Because if you can maintain some sort of a gas conduction in here during the dry periods, you can keep the matrix blocks in here well-conditioned to allow flow to travel to the saturated matrix.

Alternatively, if you wanted to turn the problem around a little bit, say, okay, what would it take to raise water levels? You may not have to completely saturate the Prow Pass/Calico Hills/Topopah, you can bring it in locally right through the fault structure. If that fault structure trends north to south all the way across the mountain, and it's several hundreds of feet wide, then, locally, you can actually raise the water level without needing lots of water, and this is why I asked some questions about this other model study that had been done, the WEEPS model. What is it really doing? Is it allowing water to come in through the fault and just disappear into the saturated zone and it's gone? Because that may not be what happens at all.

These are a couple of ideas. They're pretty much contained within these definitions, or you could take a combination of these two. The concentrated flux problem, as I pointed out, brings flux in from the Paintbrush to the top of the basal vitrophyre, and direct infiltration, if you will, as a concentrated flux-type mechanism; whereas, the other one just allows for water to stand in here because it's connected to the saturated zone.

Well, this is the first time we've actually been able to do it with the core, with the measurements. We're going to get some great geophysical logs out of this program, and I believe we can take this kind of information and go back to some work that's been done years ago, and maybe we can get a better focus on that.

Could I have the next slide, Linda?

Okay. There's one other alternative. It could be perched water, residual water from some higher piezometric and/or standing water levels in the Prow Pass. I tend to discount that as an option. Why? We came back almost to the same level that we did when we first encountered the water, the very first encounter, the piezometric level, came almost back to that position. We did not have an absolute static water level measurement. This is true. I think it would be extremely coincidental to be piezometric if, in fact, you went through a perched water zone.

Okay, I'm just about done. I've gone through what I think I needed to do about UZ-16. Could I have the next slide, Linda and Helen, please?

Well, in my view, this is how we answer the question, short of running short of money, if you will, is to try and convince ourselves, some realism here, is concentrated flux something to consider in the program? If it's not, throw it away; fine, but let's go out there and give it a chance. Let's go out there and challenge our understanding, or is it a combination? Should we be dealing with a concentrated flux problem as something when climates change and it's wetter and becomes more important than uniform flux, or which way is it? Which way does the bucket tip when climates change? Or, perhaps, let's keep it all wide open. Let's not discard anything; perhaps none of the above.

Could I have the last slide? And I'd like this to be a parting shot. This is the drill rig at UZ-16. I think we've got a very, very successful program there. It took a long time to bring together, and I would like to see it continued. Let's keep on drilling.

Thank you.

Questions, please?

DR. DOMENICO: Thank you, Joe.

Does the Board have any questions here? Don?

DR. LANGMUIR: Joe, I share with you the excitement of finally having some data from that hole there.

My first question, how far away are you from getting some data? I know you just got the samples, so the data's going to help you, of course, determine what the source of the water is, whether it's truly water rising from the Artesian system, or that it's coming down and out.

MR. ROUSSEAU: Well, I don't know--one comment on that. I thought about that for awhile, and if it goes one way, it may not be conclusive; if it goes the other way, it may lean you towards an interpretation. But to answer your other question, both Bill Steinkamp and Al Yang have samples of the water now and are working with it. I believe that Al has an accelerated program to get some core squeezing done and figure out what's going on from the core, getting that water out of it. You have to remember, too, that the samples came up pretty dirty, so isotopes are probably about the only thing that they'll be able to use the original samples for.

We can clearly go back into this hole and clean it up when we're all done with the testing and get some cleaner water out of it, because the instrumentation design will let us go back into the borehole through a central support tube, and we can get water out, clean water, but we won't have the advantage of the water above the--the first encountered water anymore. I mean, that's gone, at least for this hole, but at least for the next hole that we plan out, there are two, if it turns out to be really important to do it, we'll know what to work with. I think we did a pretty good job of not letting things slip through our hands on this cut.

DR. LANGMUIR: I have one more; Langmuir.

I'm going to be corrected by a hydrologist in the audience here, but--probably. You've got Artesian heads there rising up above the--into the Prow Pass. Maybe you could comment on this. What you're suggesting to me is that you could potentially have water rise towards the repository horizon if you had an opening.

MR. ROUSSEAU: This is correct.

DR. LANGMUIR: It would not be a function of recharge here, it would be a function of recharge at a recharge zone to the saturated zone somewhere else in the system. It could be miles away. What does that mean to us?

MR. ROUSSEAU: You're partially correct there. I think we have to go back to some of the early work done in the saturated zone, where we found out, where investigators have found out that most of the flow was on the upper section saturated, so that's, at least, the most permeable. So you may be able to flow laterally.

But you're absolutely correct, there's no storage capability in the saturated zone. There's none, and there's very little in some of the unsaturated section, and yes, if you have a conductive path, that's one mechanism to bring water up. It's a possibility.

Eighty-meter head rise is what John has talked about already. You don't necessarily have to bring the entire matrix block section up to saturation to see water ponding up higher.

Any more questions?

DR. DOMENICO: Any more questions from the Board?

(No audible response.)

DR. DOMENICO: Staff?

(No audible response.)

DR. DOMENICO: Well, we can open it up to the audience, then, out there.

Well, let me get you started on the wrong foot, and this doesn't pertain to any presenters or to anything in particular, it pertains to something I saw there that's a little disturbing; things like the WEEPS model that tells us what doesn't cause failure. We know that's not the damn question. You have 800 fractures, you can move water down there, and it doesn't drive the system to failure.

The question is, how many fractures do you need to drive it to failure, and then work backwards; or what should the flux be through there to drive it to failure, then work backwards; or what percentage of the fuel rods must come in contact with the flux to drive it to failure and then start backwards? That is information content. You have no information content in either of those models, and there are no fools on this project, so I presume you do know those answers. I don't know why you think we're fools and not sharing with us, that's all.

(Laughter.)

DR. DOMENICO: I'd like a comment to that from anybody.

DR. BOAK: Jerry Boak, DOE.

I would prefer to have the people that constructed those models here to respond to that. To some extent, the total system performance assessment was a search for a variety of things, a plausible estimate of performance, as well as an examination of certain critical failure mechanisms, and it does seem that one of the things that happens when you do a performance assessment is that you, in designing a performance assessment, you search for things that you think are really important, and you listen to a lot of people saying, "Well, you haven't done this, so this is clearly something that ought to be in there. This is probably going to drive you to failure."

And you do the best you can to try and approach that, and what you usually find is that if it doesn't lead to a failure, then someone says, "Well, you forgot this, which was actually the thing you should have done instead."

DR. DOMENICO: I'll accept that.

DR. BOAK: The WEEPS model was a surprise. We expected, when we put that thing together, it would lead to a failure, and it didn't turn out to.

DR. DOMENICO: Why didn't you keep putting fracture in until it did? I'm sure you did. Maybe you didn't like the answer. If the flux is equal to the saturated hydraulic conductivity, you may figure we have a little problem here, you know, so, we're not fools, neither are you guys.

DR. BOAK: I would urge you to read the description of the model. There are reasons for it having the form that it does. In fact, what we did was to take all of the flux that we were assigning to a given run, all of the flux that came onto that entire block, and fill a number of fractures with it. The number of fractures was dictated by a chosen fracture aperture, and the amount of flux that you had. You can't add any more fractures than you've got water, so if you take the entire flux and distribute it among a number of fractures, and put it through there, you get a certain amount of release. You contact a certain number of waste packages.

DR. DOMENICO: No, I accept that, Jerry. I understand this is not your model, but I also understand that it's zero information, in essence, and I think everybody knows that. It's not a question of whether you have the water or not, just what is the flux that is required to drive it to failure, get that inputted, and then work backwards and see what you've got, but that's somebody else's model. But, thank you, anyway.

Do we have any--someone has certainly something to say. Suresh?

MR. PAHWA: I have a question for Alan Flint; Suresh Pahwa.

This has to do with the solutions. I know you looked at the history and you're looking that one explanation there is that the system is in transient conditions, but you have not included things like evaporation in the surface and you are doing 1D modeling. What is the rationale for looking at the solution that you did as opposed to looking at other explanations for coming to that answer?

DR. FLINT: In the model that I showed, we did use evaporation, evapotranspiration. If you remember, there were several models I was trying to present.

One was a 1D surficial material model which has root functions, plant root distributions, evapotranspiration based on real measurements, and some models using real data, and we went to a 2D model of infiltration with evapotranspiration, root functions again, lateral flow in the alluvium, and then we went to a bigger scale model, and, granted, we went to 1D, but we used a negative flux, which was an evaporation and exfiltration model.

We went to a 2D model, and eventually, we're going to go to a 3D model with fractures, with evapotranspiration, but as you go to the bigger scale models, then some of your processes have to be simplified. You can't go to the great detail, but we include all of those, evapotranspiration in the system.

DR. DOMENICO: Anybody else from the Board or the staff or the audience?

MR. TAYLOR: I'm Ed Taylor. I'm somewhat confused, too. I'm wondering what I learned today, and what I thought I learned is that the flux is very, very low, and that suggests to me there's nothing to worry about, unless heat does something.

DR. FLINT: No, actually, you weren't supposed to learn anything today.

(Laughter.)

DR. FLINT: Today's session was to talk about how our management program is structured to do the work. We weren't supposed to talk about the real science.

What I showed was just an example of an approach that we would use. I kept all the real stuff hidden away.

MR. TAYLOR: Well, you did show data, you did show models--

DR. FLINT: That was an accident.

MR. TAYLOR: --and I didn't see anything that contradicted the notion if the flux is so low, that unless heat does something bad, there's nothing to worry about.

DR. FLINT: No, no, no. No, that's not right. What I did is I defined the flux. I set the flux low. The reason I set the flux low, for one reason, I'm not a very good modeler, and I kept getting perched water develop and I didn't have any fractures, so I had to set it low so I could get the climatic changes through the system.

What Ed Kwicklis and I are doing now is we're putting a fracture network, a realistic fracture network into the system and we're going to greatly increase the flux and look at the response to the system. There are some ideas we have, when you look at these variable permeabilities and matrix flow. You do the same thing with fracture flow.

If you have, as Joe showed, a zone of lithophysal cavities which have a lot fewer fractures, the bulk permeability goes down, and if you keep those fractures fully saturated under fracture flow conditions above it, then when you--or even in that zone, when you go to the higher fracture density zone below, into the Topopah Spring, into the lower nonlithophysal, you can't maintain full fracture flow in all the fractures.

You just don't have the volume going through, because you're changing, and if you think about it, high fracture densities, low fracture densities, then back to high fracture densities, you can't sustain high flow through the fractures if you had to go through a low fracture density zone above it, and that's an important thing we're going to put into the model.

We're going to force the flux to go much higher than that. We're going to do what Joe said and what Marty said we should do. We're going to push the system to failure, and then we're going to see how realistic that is, because it's not the fracture density, as Joe said, it's the fracture properties. I've got a whole program in the matrix property program now in performance assessment modeling where we're doing a lot of work on characterizing the fracture properties, fracture fill material.

We've seen good evidence for fracture flow in the unsaturated zone. In the near-surface stuff, we've seen it down to 15 meters just because of these last rains in fractured rock, and two meters in alluvium. Now, the fractured rock passes a lot of water through, but we need some more characteristics, and we need to incorporate that in the water, and we might--incorporate that in the model, and we might find higher fluxes.

The Calico Hills unit is wetter than I thought it was, and we haven't processed all the core yet, but from the core we have seen, it's nearly saturated. Very important, below that, it's only 40 per cent saturated in the Prow Pass; lots of things like that, very important to include in our thinking, and in terms of all these models we're talking about, we're to the point now where we need to start working those models.

Any modeling--and it may be apparent, Joe and I are fairly unfamiliar with these models because we ignore all of that stuff until we have some hard data to work with, and I think we're to the point now where we can start to get serious about some of this modeling.

DR. DOMENICO: The whole story has not been told yet, and we've got Bo tomorrow giving us--where you left off is where he takes up with the unsaturated flow model, so the story's, you know, there's still more to be told there.

Do we have any other questions, comments?

DR. MIFFLIN: I'm Marty Mifflin. I've got a question for Joe.

I thought I heard you say something about large-scale pneumatic testing, and the question is, how do you view that with respect to the surface-based program and timing of the tunneling?

MR. ROUSSEAU: Well, I can tell you what I saw in G-Tunnel. We instrumented two boreholes in G-Tunnel; one, 150 feet deep, and one 15 feet deep, and there was never a pneumatic pressure grade reversal from the open drift into the hole. A flow of air was always from the rock in the tunnel towards the drift. There was another feature--I mean, that was exclusive. That was all the time. It didn't matter what pressure variants were occurring. There was no reversal there, so, obviously, the vacuum system in the drift was controlling the pneumatic pressure in the system.

Another thing we found out, too, is the rock actually dries from the inside out. If you go to the face of a drift wall, at least to 150 feet, we were consistently getting less-saturated core. Our water potentials were always higher, a more negative, and so what that's telling us is we basically have a heat pipe system set up, and that's what happens.

At the open face of the drift itself, what we saw was a seasonal variant of condensation occurring, which always kept that face a little bit wetter in the rock deeper in.

I can't answer the question precisely. I can tell you what effects we've seen, you know, trying to lead to what we might see with an underground tunnel. I don't have the answer to that one.

DR. DOMENICO: Anything further?

DR. CORDING: Joe, on your fracture densities, how do you think proximity to faults have influenced that as, you know, a lot of it ties in quite well, of course, with the various lithologies you're working with, but do you see some areas in there where the fracture density is really being controlled in one part of the borehole more by faults or proximity to faults and not in another?

MR. ROUSSEAU: Well, we, and like I said, the fault was almost vertical, that one fault that we transected. The borehole had about a two and a half to three degree deviation. We were very close to the fault the whole time.

In fact, when we were at the bottom of the hole, we're probably 15 feet off of the plunge of the fault. It was vertical here; about 15 feet off, so--and we intersected about 1150 feet, which would have meant on either side we were within a 15-foot offset of a fault.

There's some other indication right now--we've got to back and look at the core--that we might have run through another fault higher up in the section, the Topopah. For some reason, there was a couple sections in there where the core recovery just deteriorated to almost nothing, and everything was bagged up into baggies, and rather than break it all out, we had no indication that we had a problem there, we just presumed it was a mechanical problem with the drilling. I'm going to have to go back and look at that and see if there's any indication on the core faces there, whether we actually had other fault.

But we are very close, and I really believe that proximity to the fault will have a lot to do with fracture intensity.

DR. CORDING: In the Calico Hills, you were close to the fault, but you did see a quite reduced density there.

MR. ROUSSEAU: Calico Hills notoriously has very, very few open fractures in the zeolitized section. In the vitric, there's reported lots of fractures. The only question that we don't have an answer to yet is whether or not, in a local fault environment, the Calico Hill can actually remain open, because, presumably, the Calico Hill will seal itself. In the vitric phases, there's lots of fractures. The hydraulic conductivity goes way up.

In the zeolitized phase, there's very, very few fractures, but what if there's only one or two, something that holds the section open to drain the fault? I don't know. We didn't get that one yet. I think we'll pick it up. I think that'll be--well, we'll have to reassess the data, but that may be an important target for Hole No. 2 at that site.

DR. DOMENICO: Is there a test plan for the pneumatic testing?

MR. ROUSSEAU: Yes, there is.

DR. DOMENICO: Is there a test plan in place?

MR. ROUSSEAU: It's part of the surface-based borehole investigation study plan.

DR. DOMENICO: That's been in place for a long time?

MR. ROUSSEAU: Yes, it has.

DR. DOMENICO: I guess it's time to head for the barn.

Does the Chairman have any final words?

DR. CANTLON: No. I think we'll recess until tomorrow morning.

(Whereupon, the meeting was recessed, to resume at 8:00 a.m. on April 22, 1993.)