UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

January 29, 1997

Bob Ruud Community Center
150 N. Highway 160
Pahrump, Nevada 89048

<u>BOARD MEMBERS PRESENT</u>

Jared Cohon, Chairman, NWTRB
Jeffrey J. Wong
John W. Arendt
Clarence R. Allen

<u>INCOMING MEMBERS</u>

Alberto Sagues
Priscilla Nelson
Debra Knopman
Norman Christensen
Florie Caporuscio
Daniel Bullen

<u>OUTGOING MEMBERS</u>

Edward J. Cording, Afternoon Session Chair
Donald Langmuir
John J. McKetta
Ellis D. Verink
Patrick Domenico

SENIOR PROFESSIONAL STAFF

Daniel Fehringer
Russell McFarland
Leon Reiter
Daniel Metlay
Victor Palciauskas
Sherwood Chu
Carl Di Bella

NWTRB STAFF

William Barnard, Executive Director
Michael Carroll, Director of Administration
Paula Alford, Director of External Affairs
Frank Randal, Assistant, External Affairs
Helen Einersen, Executive Assistant
Linda Hiatt, Management Assistant

I N D E X

I N D E X
(Continued)

PAGE NO.

P R O C E E D I N G S

(8:00 a.m.)

COHON: Good morning. I am pleased to open the second day of our public meeting here in Pahrump. Let me again start by thanking the people of Pahrump and Nye County for their wonderful welcome and for the excellent preparations and arrangements for this meeting.

Let me also remind those interested that there will be a public comment and session currently tentatively scheduled for 4:35, or thereabouts, at the end of the day. We encourage you and ask that you sign up with one of the women staffing us in the back there near the door, and we look forward to getting your comments.

Today, we will turn to a series of presentations from DOE, the State of Nevada, and Nye County focusing on some specific activities related to some of the scientific and technical work at Yucca Mountain. The DOE will first present an overview of developments in the program. That will be followed by observations from the State of Nevada, and then we'll turn to specific reports of site investigations being conducted by and having been conducted by DOE at Yucca Mountain. After lunch, we'll be hearing about some of the independent scientific work carried out by Nye County.

It's my pleasure now to introduce to you Wes

Barnes, the Yucca Mountain Project Manager, and Russ Dyer, the Yucca Mountain Project Site Characterization Office director. Wes?

BARNES: Thank you, Mr. Chairman.

I'm not one to read to you. I assume everybody here can do that. We put this first chart up just so you can get a general feel for what the office looks like, but specifically to point out that Dr. Dreyfus who is no longer with us is Deputy; Lake Barrett is now Acting and that's official. The Acting Secretary signed that paper just in the last three or four working days. Our Government is in a nice peaceful transition. So, there's going to be a new Secretary of Energy, a new Director of this program, et cetera, within the Department of Energy. I don't foresee any problems as far as the Yucca Mountain Project goes in that we have a plan. The plan was approved and funded by Congress. So, that's what we're marching to these days is our project program plan.

Since the last meeting of this Board, this chart is new. Where the project is going these days with its organization is towards the products it is supposed to produce. I didn't make up this organizational chart in a vacuum. I went to our people at the mountain, to Washington, and asked if we were organized properly to get us to our goals, and basically that's license application. The one answer that was static from everybody polled was, no, that we did need to have a different organization to get us to our goals. What was not uniform was what should the organization look like, and we spent a lot of time talking about that. This is what we finally settled on, and we did that for these reasons.

For the new members of TRB, you're going to be bored. We saw this yesterday; I apologize. Basically, we are designing a repository. If the relative motion is up, going to the year 2002 and a license application, we're designing a repository. There's a lot of steps in between. We must tell the Congress how much that's going to cost; we must tell the world how it's going to perform. Which tells you that everything I do here, everything all our people do here is reflected here and here.

In 1998, there is a thing called a VA. This chart lets me in my own mind figure out exactly what a VA is. The Viability Assessment is no more than a snapshot of those three things in 1998; what we think the design will look like at that time, how much it will cost, how it will perform. Exactly what it is we send to Washington, D.C. will probably depend on the Secretary and the Director at that time. Will it be three pages? It could be, with 9,000 arrows pointing to all the documents we have to back up those three pages. Will it be the 9,000 documents? It could be. Whatever they'll want in Washington, we'll deliver it because we'll have it. But, I put that up there so that you know and see clearly what I see up here. This is what the VA is.

We are designing this repository to four reference wastes; industry spent fuel, DOE spent fuel, Navy spent fuel, DOE high-level waste. Four wastes. Are we taking into consideration the other wastes that DOE has? Yes, at a different level. We are looking at them for design ramifications, cost ramifications, performance ramifications, but we are designing to a reference design. Four wastes. The other thing that we have to do is come up with an EIS. In 1999, you'll have the Draft EIS which will feed in, of course, to the license application.

So, when you look at what we have to accomplish according to the plan that Congress is funding us to do, what the President of the United States wants us to do if he was here in Nevada and said "let that program run", that's how we came up with this organization. The line portion of this organization focuses on this. So that everybody that works at Yucca Mountain knows where they fit in; they know what they're doing.

To the best of my knowledge, S-130, the 1997 version of S-1936, is exactly the same. Senator Murkowski, the Chairman of Senate Energy, put a very passionate three-page letter about this proposal which is a public document.

The confirmation hearing is still on target for January 30. I'm very proud of that last bullet. We are on schedule, we are on budget, we're meeting our goals. As you can imagine, it's very difficult today with all the questions that are pouring in from the "new administration", but we remain on schedule and on budget. I feel that, sooner or later, the spotlight will come back to the project and that's how I want us found; on schedule, on budget, doing what we said we'd do.

DYER: I am Russ Dyer. I'm Wes' Deputy at Yucca Mountain. What I'd like to do is take the Board through some of the highlights of achievements since the last Board meeting, both at the project and also at the program.

Certainly, one of the things that's been a focus of our activities has been the Tunnel Boring Machine operations, the ESF activities. This is the current status as of about two days ago. This is the north portal of the ESF. Here's the run to the northwest. This is the main north/south. Here's the Ghost Dance Fault. This would be the south portal area here.

TBM currently here--this was our projected advance for the first time in a long time--we're slightly behind our schedule. We've run into some pretty challenging ground in here. It's been hard slugging for about the last month. The head of the TBM passed through a fault about a week ago, and we hope to be able to make up advance here coming out. Still looking at projecting a late March daylighting time for bringing the TBM out of the ground.

Let me point out while we're on this slide, a couple of other things that are going on and put them in context. I'll talk a little bit about them. You'll hear more about them from some of the follow-on speakers this morning.

This is Alcove 5. This is the thermal testing alcove. We have on test underway in this alcove right now. This would be the single-element heater test which was turned on in August. It's located in here. This is the heated room test, the large test. Bill Boyle will tell you considerably more about that. Construction of the top part of that testing drift is complete right now. I think we're going in and completing the bottom part of the drift.

This is Alcove 6. This is the Northern Ghost Dance Fault testing alcove. We drove a drift to approach the Ghost Dance Fault, drove a horizontal borehole across the fault, have been testing the fault of its pneumatic, hydrologic properties for quite some time. We are currently advancing Alcove 7. That will be the Southern Ghost Dance Fault testing alcove, and we are about halfway through the construction on that.

As I said, in Alcove 5, testing is underway in the single-element heater test. We turned that on in August.

Bill will tell you a little bit about what we've found and what we have left to do. We intend to complete construction by February of the drift-scale, the large room-scale heater test. And, as part of that test, there will be about 150 instrumentation observation holes that will be drilled. I'm not sure whether Bill brought the porcupine diagram to give you a three-dimensional view of the monitoring system that we have envisioned for this large-scale test. But, it's quite a comprehensive test. Still looking at turn on for the drift-scale test in December of '97.

The large block test at the southern part of Fran Ridge, we're completing the preparatory phases for that and looking at turning on that test in February. That looks to be on schedule.

Unsaturated zone hydrology, I talked about the North Ghost Dance Fault alcove, actually showed it on the first diagram. These are some of the actions that we completed already. As I said, we excavated up to the fault very cautiously, penetrated across the fault, did some initial tests in there to try to understand something about whether we're looking at a C-Well or a pathway here, and since then, have packed off the fault zone, have been monitoring the fault zone.

After that monitoring is completed in approximately the March time frame, we'll go ahead and complete the alcove across the fault, complete the drift across the fault, fishhook back, and then run a series of boreholes parallel and perpendicular to the fault for a more extensive testing program of the Ghost Dance. And, we'll follow the same scheme of testing in the southern Ghost Dance Fault. But, we're only about 50% of the way to the Ghost Dance in Alcove 7.

The saturated zone hydrology, we've got some very, I think, exciting results to share with you today. You'll be hearing from H.J. Turin from Los Alamos and M.J. Umari and Bruce Robinson--M.J. is from USGS and Bruce is also from Los Alamos--talking about some of the testing that we've completed, both conservative and reactive tracer testing and some of the hydrologic testing at the C-Well complex. We've got a tremendous amount of information about the characteristics and behavior of the saturated zone here. As Dennis Williams will tell you later, this is an area where we feel we have information needs. We are going to be adding more saturated zone testing into the program in fiscal year '97.

Some of the other programmatic activities. 10 CFR 960 siting guidelines, there was a proposed revision published in the Federal Register in December. We conducted hearings last week in Las Vegas on January 23 in response to many of the comments that we received. The comment period has been extended to March 17; extended, I believe, 30 days. We're targeting the end of fiscal year '97 for finalization of the final rule, and I would remind the audience and the Board that any comments on the proposed rule must be submitted in writing to DOE by March 17, the closing date for comments, in order to be considered. Our point of contact at the project is April Gil. If you're looking for information, either contact April, myself; anybody from DOE can give you information on how to get your comments considered.

Waste containment and isolation strategy, something that we've tried to keep the Board informed about, an evolving concept. Just to reiterate, these are the five essential elements of the waste containment and isolation strategy. Normally, they're couched as testable hypotheses. A debate going on in the project is how exactly you--what test you use to test these hypotheses, but we still have these five elements of the waste containment and isolation strategy. The rate of water seepage in the repository, waste package lifetime built around these concepts, release rate of radionuclides, transport through the engineered and natural system, and dilution in the saturated zone.

We gave a summary, a short summary, of that in July of '96. We're working on a much more comprehensive version now. That comprehensive version will be available in late fiscal year '97. We've got a draft that we are in the process of updating right now.

EIS activities, Environmental Impact Statement, if you'll remember in--we put the Environmental Impact Study activities essentially on hold for a year. We have now resumed those. There was an independent EIS contractor brought on board. That's Jason Associates. The contract has been awarded. They're fully staffed up. They are involved in the project now. The public scoping period ended over a year ago, December of '95. The comments were kind of shelved until the EIS was resumed. Those comments are being evaluated now, and we're looking at producing a comment summary document in May of '97, looking at a Draft EIS in July of 1999.

Now, let me step from project activities to programmatic level activities. These are mostly things going on by the part of OCRWM on the east coast, the waste acceptance, storage, and transportation activities.

The first one, of course, Dwight Shelor yesterday gave you much more detail about this. I'd just call your notice to a couple of dates on here. This is regarding the regional service agents, a couple of dates. The due date for comments on the Draft RFP, March 31 of 1997. What Dwight mentioned yesterday was a pre-solicitation conference scheduled for February 25, '97.

Other things that are going on involve Topical

Safety Analysis Reports, TSARs. There's two TSARs that are currently in the works. And, I'm very happy to see Chris show up; thank you, Chris. These are generic in nature and they have to do primarily--well, at least immediately, with looking at supporting the waste acceptance part of the system considerations. The TSAR, Topical Safety Analysis Report, for dry transfer system is at the NRC and has been since September of '96. We're waiting for comments. We're expecting a safety evaluation report by April of '98. The second TSAR is for an interim storage facility; Phase 1, a generic interim storage facility. Here's some dates on here. Chris is going to give you a status report on that a little later this morning.

Actinide Burnup Credit Topical Report, this is a dialogue that's going on between the DOE and the NRC to establish a methodology by which we can take credit for burnup for criticality considerations. There was a topical report submitted to the NRC in May of '95. We've had an ongoing dialogue with the NRC on that. Their second round of questions, NRC's second round of questions is currently being addressed. The response to this revised topical report is scheduled to be submitted in March of this year, in about two months.

In summary, not just the project, but the program is focused on implementing the '96 draft program plan. '96 draft program plan, we came out with the program plan and there was an addendum that was put together just recently to reflect the reality of the funding--Appropriations Bill that we received in fiscal year '97, and how that will impact the outyear parts of the program.

As Mr. Barnes said, we are on schedule for the '98 Viability Assessment at Yucca Mountain, and also as Dwight said yesterday, the implementation of the market driven initiative for waste containment, storage, and transportation.

That's all I have. Are there any questions for either myself or Mr. Barnes?

COHON: Thank you. Before we get into questions, I have a brief announcement. There is a vehicle with its lights on in the parking lot. It's a Ford Ranger, Nevada tag 28874.

Do we have questions for Wes Barnes or Russ Dyer?

DOMENICO: Russ, can you put your slide, Page 13, on please, your system attributes.

DYER: Okay.

DOMENICO: When are we going to be hear more about--we certainly won't be hearing about these at this meeting. These aren't on the agenda. But, when do you think we can arrange to hear more about the kinds of testing and the information that's being gathered on those five items? Do you think that's a proper topic for our next Board meeting?

Do you think there will be enough information?

DYER: You will hear some about that today. In the hydrology part of the talks, I know that they will--I'm looking at Dennis hoping that he's going to shake his head--but tying those tests back into the waste isolation strategy.

DOMENICO: We heard about them in the July--

DYER: He's shaking his head the right way. Okay.

DOMENICO: Up and down or--

DYER: Yes.

DOMENICO: He's always been a yes man, that's right. I'm just joking.

We heard something about them in July, and in July, they were mere hypotheses without any really sufficient backing.

DYER: Right, right.

DOMENICO: So, I presume there's a whole slate of testing going on to substantiate those favorable hypotheses.

DYER: Correct, trying to truly test the hypotheses, yes.

DOMENICO: Okay, thank you.

COHON: Other questions? Dan?

BULLEN: Just a quick question about your Actinide Burnup Credit Topical Report. Is the purpose of that report just for transport and storage and not extrapolation to burnup credit for long-term criticality control in the repository?

DYER: No, it would also apply to a repository.

BULLEN: Okay. So, this revised topical report is going to address issues related to long-term repository disposal and--

DYER: Well, eventually. Let me defer here.

KOUTS: You were correct in your assumption. It's only for storage and transportation. The topical safety analyses report we have in front of the NRC right now is only for storage and transportation. There will be additional work done by the project to deal with the long-term criticality analyses that will be needed for burnup credit. That's going on right now, but that doesn't address the--

BULLEN: Okay. But, the burnup credit for this is just for transport and storage?

KOUTS: That's correct.

BULLEN: Okay, thanks.

COHON: Other questions? Leon?

REITER: Russ, I just want to ask you something about 960. The last time, I think, the schedule was presented as showing that you're going to assess compliance with 960 in mid-1999. That was at least the last thing we saw. Is that still the target?

DYER: Steve is saying, yes, that sounds correct.

REITER: So, am I correct in my interpretation, since

960 is described as the siting guidelines and also for determining site suitability, that you will make a finding on the technical aspects of site suitability in 1999?

DYER: That's--do you want to take that?

BROCOUM: The 1999 date is contingent on having EPA standard because the draft guidelines that are out now point to the EPA standard. So, we have to have a standard in place. And, we would make an evaluation against the guidelines in 960 in 1999 to our current program plan.

COHON: Without even turning around, I can guess that was Steve Brocoum talking for the record.

Let me ask a question first and then we'll call on the audience. For Wes Barnes, could you say something about the pending legislation in the Senate on prospects--if you feel like talking about; I can understand why you might not want to--but, also and more importantly, what you see as the potential impact on the program of that Bill?

BARNES: No comment on whether it will pass or not. That's crystal-balling it. I don't believe that the industry--I believe the Bill is structured the way it is because there's enough momentum in Congress not to move away from the repository concept. I think the industry understands that. So, this work that you're reviewing today, project, will continue and will build, if it passes, an interim storage facility in front of the repository. You need to do something like that. The country needs you to do something like that anyway. I mean, if we just built the repository, I couldn't function without having a surface facility. You can't just take the waste off some transportation vehicle and stick it in the mountain. One of the concepts that they're working on with transportation in Washington that you heard about yesterday is to accept whatever is licensed. DOE is not the licensing body. If that becomes reality, we're going to have a more robust surface facility at the repository because you wouldn't take what's necessarily licensed to travel across the highway for the safety of the country and stick it in the ground.

COHON: Okay, thanks.

Let me remind questioners and commenters to identify themselves before they speak.

TREICHEL: Can you give me any idea what that EIS scoping comment document is going to look like that you're putting out? Is it just a record of what people said or is there any interpretation or mashing of the data or how does that look?

DYER: Let me if I can get some help here. Is there anybody who is familiar with what the project is going to be? Judy, I don't know. I'll have to get back to you on that.

TREICHEL: Okay. That's fine. Thanks.

COHON: Any other questions or comments? Please, wait until you're called on.

DEVLIN: Oh, I'm sorry.

COHON: Thank you. Ed?

CORDING: Russ, the diagram shows parallel tracks, and from what we've seen with the underground work, it's obvious that there's a tremendous amount of information that's being obtained and a lot still planned. But, there's a tremendous amount of access that's been gained and a lot of information that is pretty fresh and coming in, and at the same time, you're in the process of design where you'd like to close off issues and not have to go back and revisit design issues here. When we get to this point in design, it's obviously a situation where you'd like to be able to wrap it up and making any changes in the design is difficult. So, you've got two processes going on here that are continuing right to licensing. At the VA, you'll have a design, but there will be some issues that I assume would remain open or would be adjusted depending on what your finding as you continue the exploration because there is much more being discovered and to be discovered, certainly.

So, do you have a feel for how that design might change or how flexible one can be in that as you approach from 1998, say, to the 2002 date? How you're kind of putting that together? This is kind of an overview of what your perspective is.

DYER: Yeah, it's going to be a great challenge there trying to come up with something that is fixed enough to really go forward with a design; yet flexible enough to accommodate some information and some knowledge that might be developed. We have prioritized the design process so that the focus for VA is in those things most important to safety. There may be some flexibility left in those design elements, but as much as possible, we'll try to find something that will work in the broad range of operating conditions that we expect we might obtain there.

LANGMUIR: Russ, I appreciate that what I'm going to ask you probably will be answered sometime later in the day, but to me, it's a more--it's a broader issue that perhaps at your level would be worth inquiring. Specifically, a lot of us are concerned that the nature of the tunnel design as it exists with the ventilation system that's in place precludes ever seeing any water coming out of the walls the way it would have before construction. That is the bottom line in terms of characterizing hydrology is where is the water moving through the system? With that in mind as a goal, I understand that some of the alcoves will be instrumented for such measurements to establish flow through fractures, perhaps maybe through matrix if you could get at it on the can. But, I didn't see much discussion in your summary, at least, of where that might take place and, if so, how much of it would go on? I have a sense it's going to go on perhaps in Alcove 6. Can you clarify where it might happen? What might be done? The sense is we'd have to close it off. I think, we'd have to seal any place that such measurements would be made.

DYER: Put a bulkhead up, seal it up. I don't want to steal Dennis' thunder. He's going to--

LANGMUIR: If this is going to be covered, I can certainly wait.

DYER: Yes, Dennis will cover that a little later.

LANGMUIR: Okay.

DYER: And, this is part of the new work that we're bringing in here in '97.

LANGMUIR: My guess is once isn't going to be good enough. You've going to have to have several locations to do this to get any sense of--

DYER: I think, it's new alcoves, isn't it? Yeah. Some new alcoves not even on the map.

LANGMUIR: Oh, good. Then, we'll hear about those.

COHON: Ms. Devlin, I was--I'm very sorry to have cut you off before. Now is a good time.

DEVLIN: I'm used to getting cut off. That's all right. When Russ and I met, everything--Sally Devlin, stakeholder from Pahrump. When Russ an I met, everything was 1300 degrees Centigrade. In the newspapers when they talked about the heat tests, they said at 100 degrees Centigrade. Now, to me, that seems awfully low. I think everything is about 360 degrees Centigrade. I just wonder how hot they're going to do the heat tests. And, the other thing is while they're doing that with--you know, we all know how fractured and fissured and ponded and all the rest Yucca Mountain is, it leaks like a sieve from my friends that work up there. And, you're going to have a terrible time with the water. And, of course, all this diversion and so on, I thoroughly disagree with. But, what bothers me the most, are they doing any microbiotic corrosion studies up there along with the heat and anything else going on?

DYER: I think I heard about three questions there.

DEVLIN: Yes.

DYER: Let's see, microbiotic testing, I know we've done laboratory tests. Penny Amy at UNLV has been involved. I don't know if there's anything that--let's see, Bill is shaking his head yes. This is part of the large block test?

BOYLE: Large block.

DYER: Okay. Temperature--well, let's see, yes, it is a fractured system, presumably free-draining. Water moves through the system. That's what we're trying to understand now is the processes by which--ways in which it moves through the system. Temperature of the test, Bill maybe you can help me here. As I recall, large block test is about 140

Centigrade. What's the heated room test, the heater temperature? --temperature is less than 200?

BOYLE: Bill Boyle, DOE. In the heated room test, right next to the wing heaters, we'll go up over 200 degrees C. And, in the single-heater test, oh, I would say probably up to 160 and the rock is the hottest yet, but we've still got a few more months to heat.

DYER: Considerably below 300 degrees Centigrade anywhere in the system.

COHON: Thank you very much.

As you heard yesterday, Bob Loux had to cancel his participation in this meeting. Representing the Nevada Agency for Nuclear Projects in Mr. Loux's place is Steve Frishman, Technical Policy Coordinator, for that agency. He'll be presenting the perspectives of the State of Nevada, especially with regard to the proposed new siting guidelines and viability assessment. Steve?

FRISHMAN: Well, Bob is sorry that he can't be here. He and I had discussed what his presentation would be. After hearing the very end of the public comment period yesterday, I've decided I want to revise the approach a little bit. For those of you who might remember, I recall about five or six years ago making a presentation--one of the very few where I ever used viewgraphs, by the way--making a presentation on the concept of suitability as it's developed in the Nuclear

Waste Policy Act and also how suitability and licensibility are two separate issues. Your staff may want to go back and --I remember the room we were in. I think it might have been in Denver and you might want to go back and get that out because, to date, nothing has changed that I recall. The Act is still the same, the guidelines are still the same, 10 CFR 60 is still the same. The only thing that has changed is the Department's perspective on how it wants to go through this decision process. So, I think, it might for those of you who were there refresh your memory on that, and those who are new have the staff go back and put out that package. Things may change in the future, but as it stands right now, I think that presentation was one that I recall was somewhat revealing to some of the members of the Board at the time because they had not thought very much about the decision and regulatory side of the program. But, I think it's worth understanding now.

The reason I bring that up and have somewhat revised what Bob and I thought we would--or the direction we thought we would go in a presentation to you is hearing Jared's response yesterday to Rick Nielson's question about how a site may--the note in your report about how a site may be found suitable, but may not be developed as a repository and your explanation of what that meant. Well, just as almost everything else in this program, it's not really as it appears. The word "suitability" is also not really as it appears. Jared, your explanation, I think, was eloquent and intuitively correct, but at the same time, doesn't match up with what goes on in the Nuclear Waste Policy Act and the guidelines. The word "suitability" is never defined. The word "suitability" is used in the program and has been from the very beginning as essentially shorthand for the decision that the Secretary makes to recommend the site to the President. So, it's not a continuum; it's a decision point. And, if the word is shorthand, it means what the Act says it means in terms of how you can pull out the elements of the Secretary's decision. But, it's not, at all, a continuum. I think the continuum will be something that I'll discuss in a little more detail later. The continuum will be probably TSPA. TSPA is essentially independent of regulation or decision. TSPA is the tool that tells other people how the person that ran the TSPA thinks the system might work.

So, let me show you--this may be interesting to some people and maybe redundant, but probably will be new to many people and that's the primary use of the word "suitability" in the Waste Policy Act and how it does attach itself. And, also, you'll see how it is directly related to guidelines. In Section 113-B, the requirements for a site characterization plan are laid out. One requirement for the contents of that is "criteria to be used to determine the suitability of such candidate site for the location of a repository developed pursuant to this section." This criteria can only be the guidelines because there are no other criteria required under the Act. And, it was interpreted to be that because that's what was put into the site characterization plan. So, the word "suitability" here is the basis of a Secretary's decision.

Now, the criteria that define that suitability are the guidelines, 960. The Secretary has other things to include in the recommendation decision, but the guidelines represent the criteria for one portion of the recommendation decision. And, we've all been somewhat sloppy in our thinking about the sure hand use of the word "suitability". I think it's important to understand. Part of the reason that it's important to understand is that, despite what was just told you again this morning about viability assessment, as late as yesterday a comment from one of the members here on the Board indicated that there's some connection between the viability assessment and a finding of site suitability. Well, there is none. But, at the same time, the viability assessment is set up in such a way that if people want to believe that, they can believe it. I've spoken to, I think, members of this group and others about the mistake that that is and the Department did not design the viability assessment specifically for people to make that misinterpretation, but it's going to happen. So, it's very important to, I think, understand that suitability is tied directly to the guidelines. And, suitability is a determination that the Secretary is to make according to the current schedule well after the viability assessment and based on the analysis that was just--that Pat just asked about and that's a documented analysis of whether the site actually complies with the guidelines.

We've had sort of an interesting succession on what happens to the guidelines. If you recall back in '95, we were having a discussion of a different approach that the Department was taking towards providing or developing the information towards a suitability determination meaning a guideline's compliance determination. And, at that time, one of the major issues was should the guidelines be changed? And, the Department's determination was that the guidelines did not need to be changed, and they published a statement of that decision in the Federal Register on September 14 stating that they did not see any need to change the guidelines and the parts of the guidelines that are devised for the comparison of sites would just be set aside and not used because there is only one site; so, therefore, there's no comparison necessary to be made.

We now are in a situation where, as you know, a new section to the guidelines has been proposed in a rulemaking.

That new section goes off in a direction that our Attorney General has already advised the Department of Energy does not comply with the Nuclear Waste Policy Act. And, the reason it does not comply is that it does not meet the requirements of the Nuclear Waste Policy Act, Section 112-A to provide factors which qualify an disqualify a site. It relies entirely on a total system performance assessment. The Act has not changed. The guidelines as proposed also eliminate any considerations of environmental aspects, eliminates any consideration of transportation, and eliminates any consideration of socioeconomic effects. All of these are required to be included in the guidelines under Section 112-A of the Nuclear Waste Policy Act which has not been changed. So, just from a legal standpoint, our Attorney General has a large problem.

There are other problems and I'm not going to go into a discussion of the value of TSPA in decision making because I just don't think that it's productive at this point to do that. I think it's more important to understand that the Department is essentially intentionally making a move that satisfies their view of how the program should go and is outside the requirements of the Nuclear Waste Policy Act. So, what that does is it changes the whole view of suitability of the site. I think if you look at the guidelines, at all, and you see the statement of factors which qualify and disqualify a site and compare that to the discussion that went on yesterday about total system performance assessment, I think that you can see that there is a great gap in terms of what would be available for a defensible decision regarding site suitability and where those milestones are and how it could be defended if the Secretary's decision about suitability, meaning guideline compliance, is challenged. So, it creates a bigger problem to eliminate these specific factors. Well, we know why the specific factors are there in the guidelines the way they are and that's not to say that we agree with the 960 as it stands as being a rigorous compliance with the Waste Policy Act because we don't. We don't think that it fully complies with the requirements of Section 112-A, but it certainly makes an effort to comply compared to the proposal that's out there right now.

Now, if you look at the value of considering other things, things other than performance assessment or as the guidelines are now, elements that must be considered individually in a deterministic way, and then ultimately the guidelines do get rolled up and that's a reasonable progression. They do get rolled up into a performance assessment and ultimately get compared to the NRC and EPA regulations. But, if you look at some of the other factors, we're dealing with a repository system. The repository system overall is the spent nuclear fuel leaving the reactor to the closure of a repository. That's the system. And, that system includes considerations other than just total system performance of the repository itself.

If you recall, the guidelines were used as required under the Act in the screening process of sites from the beginning. Now, the guidelines we use to screen from non-sites down to five for nomination, then to screen from five sites down to three, for candid eight sites for characterization. Well, in that process, there was one site that went out based on guidelines--on socioeconomic guidelines and that was Davis Canyon in Utah. That was the factor that the Department used to say that we're no longer even going to consider this site. So, the other parts have been useful in the past and have had, in part, resulted to why we're where we are today.

There's one other element that I think you need to be at least cognizant of and that's in order for these guidelines, the new guidelines to be promulgated. They have to be concurred in by the Nuclear Regulatory Commission. There also is to be some type of consultation with CEQ and EPA and others. I don't know whether that's gone on or not. But, the concurrence issue is an interesting one and it was very interesting the last time around when we sat with the Commission through a couple of meetings while the Commission was trying to figure out, first, what basis to use for a concurrence decision and then whether to concur. Well, the only basis that the Commission has for concurrence with the guidelines is their Rule 10 CFR 60. The test is whether the guidelines are consistent or not with 10 CFR 60 because there's nothing else they can test it against without being arbitrary. After some modifications, they determined that the 960 guidelines were consistent with 10 CFR 60. Now, when the change as proposed hits the NRC, I don't know how they're going to make their judgment. And, if they're not consistent, then the NRC is going to have to say that they're not consistent and, therefore, they won't become the new guidelines. I don't understand--and I think there might be some reason for this Board to look into it at some point. I don't understand the rationale for having created this morass. I can make some cynical guesses about it which I'm not going to burden you with, but I don't understand the real rationale because it seems to be putting an awful lot of the system to an unnecessary test. But, it is worth, I think, watching the guideline procedure because it's going to have an awful lot to do with the mistaken interpretation of viability assessment which is that the site is suitable and decisions that are made based on that mistaken interpretation. So, I think this new guideline proposal is reflective of where the Department would like their program to go and is not reflective of the legal requirements of their program.

I think it's going to have some major ramifications on the future of the program because of the opportunity for misinterpreting what is going on right now. And, this is as fouled up as I have seen this program procedurally as it's ever been, and I've been here since before the Nuclear Waste Policy Act was passed. So, I'm sending it out as a warning that I certainly think that it's worth this Board's understanding and especially important that the Board not get caught up in the misinterpretation of viability assessment, suitability, and guidelines that don't conform with the Nuclear Waste Policy Act.

COHON: Thank you, Mr. Frishman.

FRISHMAN: It probably doesn't require any question.

COHON: Oh, okay. Would anybody from DOE like to respond or talk about any of this before I invite questions?

(No response.)

COHON: No, thank you.

Questions despite the fact that Mr. Frishman believes it doesn't require any?

(No response.)

FRISHMAN: Thank you. I saw you taking notes.

COHON: Yeah. And, I appreciated what you had to say. I learned something from it. Putting aside the legal aspects, the procedural complications, start thinking about what's right in a, let's say, technical sense. What's the argument for objecting to the proposed change in the siting guidelines? That is, performance assessment, as the name implies and as we've learned to understand what it is as DOE's practice again, is a sincere attempt to answer the question how will this repository perform if it's designed in this way in this site using information that we have about the site? It seems to me that to arrive at a suitability determination or, better yet instead of using that word, for the Secretary to recommend to the President, the Secretary would want to know exactly that. How will this system perform? So, putting aside all the procedural and legal stuff which we certainly have to pay attention to, putting that aside, I'm asking you with your years of experience and knowledge about this to say something about that?

FRISHMAN: Well, I'll tell you at the core of my concern about essentially changing the standard of judgment from individual factors that are investigation in site characterization and from making some level of judgment about how the site works essentially in undisturbed condition, now changing from that to sort of a rolled up performance judgment, that transition was anticipated to take place. But, what's happened is where the removal of sites for comparison went away was the necessity to essentially compare sites based on these factors where you've looking for favorable aspects, you're looking and trying to find whatever adverse factors there might be, that process went away because of the lack of sites to compare. So, Yucca Mountain has never been put through a rigorous review of what we know about the undisturbed natural system based on requirements that there are--that there's some pass/fails involved. We're stepping over making judgments about those individual factors and the extent to which they really might be favorable or adverse and moving on to how the whole system works that's driving everything towards a higher and higher reliance on engineering to take care of factors that may be very unfavorable. And, one of the interesting examples, I think, is the groundwater travel time. That's the obvious one to all of us. The thinking has changed through time and right now the Department is struggling with how to deal with what seems to be maybe a better understanding of flow in the unsaturated zone.

The site has never been put to the test of how is it performing right now before we start messing with it? And, are there aspects of it that create such great uncertainty, and had the rules been applied the way they were, probably with what we know now would have disqualified the site? Are we in a position now where what we're trying to do because we only have one site is trying to dream up engineering concepts to take care of factors that under a system where sites were compared could well have resulted in this site being disqualified for good technical reason? The set has been missed and has been--we're leapfrogging over the step of looking at those factors in a context of what we thought was reasonable just a very few years ago.

COHON: The Chairman should probably be the last person who puts this meeting off schedule, but that's what I'm doing here. I just want to continue this one moment longer because there's an opportunity here, I think, to increase understanding.

Groundwater travel time is an excellent example of a factor that probably taken by itself is not very meaningful. I mean, so what if water travels through the mountain in a second or 100,000 years? That number by itself really is not very helpful. It only makes sense, I would claim--but it's a personal view--when understood in a system context. That is, a design and what it might mean for the system.

FRISHMAN: Well, it's a surrogate for understanding how fast and ostensibly how much of the radionuclide inventory can arrive in a place where it is accessible away from the repository. It's a reasonably good surrogate, but that--if you start looking at the consequences of saying groundwater travel time doesn't matter, we have in front of us in the program today that consequence. First, what it has done is it has driven the Department to greater and greater reliance on the waste package as a means of trying to get a long delay in the release of radionuclides. We hear statements that it is not out of the question that we may have a 10,000 year waste package. Well, if you say you have a 10,000 year waste package, that's as good as saying that the Titanic is unsinkable. It's an arrogant statement. It's one which you can never demonstrate.

But, you've also done another thing. You have moved over into the area where if you are going to get releases, you have this high groundwater travel time, and you have to change the whole philosophy of protection against pollution. Now, the system, all of a sudden, is relying on dilution where when the EPA wrote its rule originally and with much agreement from the Department of Energy at the time, no, dilution should not be considered part of the barrier system. Dilution is only an encouragement to allow greater releases. Philosophically, dilution, the EPA determined, was the wrong approach to regulation.

So, now, because we have faster groundwater travel time than expected and it looks like it's a real problem for Yucca Mountain without doing something, well, what's being done? Reliance on an engineered barrier when the primary barrier is supposed to be the geologic barrier. You have the flaw in the geologic barrier; so, you start relying on engineered barrier. Plus, all of a sudden, we have to change a widely held and widely respected philosophy of regulation which is don't rely on dilution.

COHON: Thank you.

We hear now from Christopher Kouts on generic analyses of interim storage facilities. As you know, the issue or the possibility of interim storage facility is a very significant one for the program now. So, this is a timely topic and we look forward to hearing from Mr. Kouts who is from the Office of Waste Acceptance, Storage & Transportation at OCRWM. Mr. Kouts?

KOUTS: Thank you.

I will, first of all, say it's been a while since I've been in front of the Board. It's good to see some familiar faces. Dr. Verink has been around since my days in transportation and members of the staff and certainly people in the audience.

I appreciate the opportunity to give the Board an update as to what the Department is doing in developing a Topical Safety Analysis Report for a Phase 1 Interim Storage Facility. I'd like to explain to you a little bit about what our Topical Safety Analysis Report is and why we are doing it. It's essentially being done, as you know. There is a lot of pressure in the system that if interim storage is authorized, something will have to be done very quickly by the Department. What we've done is develop a generic design that we will submit to the NRC for them to review all the safety aspects of it. This has been done under 10 CFR 50 with advanced light water reactor designs. It's also been done by the vendors who have dry cask storage systems under Part 72. It's a way of submitting a non-site specific technology or design to the NRC for them to review. Then, when you come in with your specific design, they will tick off that they've already reviewed aspects of the design and it will speed the technical review of the NRC.

Now, it's also important to note that this is different from what the Department has done in the past with MRS designs and ISF designs. This is the first time we've taken a design like an MRS and an ISF to the NRC to be analyzed for its safety considerations. So, although we've done a lot of design work in the past, this is the first time we've taken it to the Commission staff for their review.

I'd like to talk a little bit about the rationale for what we're doing. As I said, it's to resolve generic technical issues prior to the submission of a license application. It's based in part on the proposed legislation. And, if you're saying to yourself, well, based in part, what does that mean? It's essentially that this facility is not designed to go into Area 25 of the Nevada Test Site. This is a generic facility. There will probably be changes that would need to be made anywhere that it would go. What we've tried to do is envelop the environmental parameters and the design parameters of the site, so that when we do get a specific site, we will modify the design to fit that specific site. So, it's not being designed for any specific area of the United States. It also establishes the DOE and NRC interface. It's being done also to integrate what we're doing with the RSA concept that you heard about yesterday from Dwight Shelor. And, it's consistent with our current program approach.

Design requirements, first and foremost, we're to minimize the time and the cost of the facility, the time to develop and construct it, and to minimize the cost. One of the things we're doing in order to accomplish that is to only accept canistered fuel from existing certified systems by the NRC. I will talk about those in the next slide. We're assuming bounding site characteristics. We're also assuming a ramp-up rate of 1200 metric tons in the first two years, going to 2,000 tons, then going to 3,000 tons, until the total storage capacity of 40,000 metric tons is reached that would have about 6500 storage casks on site.

The approved safety analysis reports and the technologies that we're looking at are the VECTA systems. Some of you may be familiar with it as the NUHOMS. That's a horizontally emplaced storage technology. The other three are vertical storage technologies. That's the NAC storage transportation cask, the Holtec, and the Sierra Nuclear TranStor.

The source criteria in developing the design criteria for the facility came from a variety of places; first and foremost, 10 CFR 72 which is the NRC regulation on storage. With the Regulatory Commission Reg Guide 3.48, NUREG-1567, NUREG-0800. 0800 is for the design of nuclear power plants. 1567 has been issued by the Commission in draft and we're using that as a basis for--that's their standard review plan. We're also using that as a basis for our Topical Safety Analysis Report.

We're also taken information from the advanced light water reactor certification documents that are already approved by the Commission. We've looked at the vendor size. We've looked at ANSI/ANS standards. And, we've also looked at basically industry experience designing nuclear facilities. This effort is Duke Engineering who, as you know, has a great deal of experience with nuclear power plant design and activities.

I'd like to talk a little bit about generic site criteria. Again, they provide a basis for the design of the Phase 1 facility. The values are intended to reasonably bound the United States and I'll show you the areas that are blacked off, but that doesn't mean that we can't site the facility there; it just means we'd have to do some modifications to the design. The site criteria are based on NRC accepted codes and standards that has been shared with industry and cask designers.

A listing of some of the criteria or the major criteria here. I'll be talking about a few of them in more detail. Some of the things, we cannot address in this TSAR. For instance, we will not address aircraft impacts because we don't know where the site it located. If it's not located anywhere reasonable where there is an overflight by an aircraft, then you don't really have to worry that much about aircraft impacts. The same thing would be true for volcanic eruptions. You don't really design this thing to deal with volcanic eruptions. What you do is you don't site it near a volcano, hopefully. Meteorology, we try to look and bound those things; seismic surface design. We are looking at--and I'll get to that in a moment as to what our seismic requirements are.

Since most of you have seen *Twister* in the movie theater, I'd like to explain to you on our design basis tornados. Basically, we're assuming that this tornado, assuming that it would impact this facility, would be traveling at a speed of about 70 miles per hour, and its maximal rotational speed would be 290 miles an hour. Now, when you add those two vectors for the engineers in the audience together, you get a maximum wind speed of about 360 miles an hour which is what we would use to basically evaluate the impact of that storm on the facility. Rotational speed occurs at 150 feet from the middle of the tornado; the pressure drop is 2psi which is very substantial at 1.2psi per second; the gust factor is an NRC required analysis. And, this all comes from Regulatory Guide 1.76 which is the criteria for nuclear power plants.

Now, besides looking at just wind speeds, we have to look at massive missiles and the types of missiles that would impact this facility. We look at, for instance, an automobile impacting it to see whether or not the structure would buckle. We look at a penetrating missile which is not an artillery shell, but the size of an artillery shell. We basically look at that because if that projectile hits the facility and penetrates, what secondary missiles would be moving around within the facility? Also, looking at small missiles and its impact on various aspects of the facility. This all comes from the standard review plan for nuclear plants, NUREG-0800.

Seismic ground motion, our assumptions are .75g acceleration in a lateral direction which is a fairly substantial seismic and that's again taken from Reg Guide 1.6.

In your viewgraphs, you'll probably see some maps we're going to be looking at now. On snowloading, the assumptions that we have in this facility--this is in the eastern U.S.--are that it's 50 psf which means the snow and ice loading would be 50 pounds per square foot. Now, if this facility was sited in one of those darkened areas, what we would have to do when we submitted the license application to the NRC would be to reinforce the roof in the areas of the facility that would have to have a higher snowloading capability. But, the concept of the TSAR is that if it's not located in one of these areas, the NRC would not review it any further. They would say I've just reviewed it. I reviewed that in the TSAR and they would reference it and move on to the next aspect of the design. So, that's how this Topical Safety Analysis Report speeds the review process.

Here is the western U.S. snowloading. You can see certainly across the Continental Divide and areas of California and the Pacific Northwest snowloading is much heavier. Again, if a site is identified in those areas, we would have to modify this design in the license application. I'd like to make that point very clear that that doesn't mean that this facility couldn't be built in these areas; it simply would have to be modified prior to the time of license application.

The same thing is true for precipitation. You can see that the area of the Gulf area and Florida area, they have a high frequency of hurricanes and so forth. We're not really designing this facility to deal with that kind of storm in terms of flooding and the rain that would come from it. If indeed it was sited in that area, we would have to design the site specifically to deal with high water. But, this analysis would not take that into account.

To give you a sense of what the facility looks like, I also have some color visuals that came out of the computer in a design program at Duke that I got last Friday when we were doing a status of this. To give you a sense of the size of the facility, this is about 6400 feet by about 5800 feet. It's about 1300 acres. The distance from the storage field--the closest distance from the storage field to the site boundary would probably be somewhere between 700 and 800 meters to make sure that the public does is acceptable to NRC standards. The facility that we're designing here is the Phase 1 facility which is the little number 1 on your map. The Phase 2 facility would be a follow-on facility that would handle bare spent fuel. The other facility is basically a-- and security areas within the fence.

The actual transfer facility itself as it's presently designed--and this is still evolving as we speak-- would have three basic entrance bays here where the cask would be brought in. Perhaps, I can use this. These are the entrance bays. There would be three of them taking basically large canistered fuel. There would be a 225 ton crane that would upend the transportation cask, but first the impact limiters would be removed. Then, it would be carried into this part of the facility. These are tornado shield walls right here for missiles that might come in from a tornado impact. And, these are the various transfer belts that would take the canister out of the transportation cask, put it into a storage technology, and the storage technology then would be taken out of the facility here and gone off to the storage field. This area is the change-out room for the staff of the facility.

Right now and using ALARA methods, we're looking at trying to keep the dose rates down to about .04 to .1 person-rem per person per cask. With about 10 people in that facility, that would mean an average of about 4 to 10 millirem per person. That should be a 10 instead of a 20. One of the challenges of this facility is although they have done these types of transfers on site at nuclear plants, they only do about four of these a year at most at the maximum receipt rate. If this facility was built at a 3,000 ton per year acceptance rate, that would be about 200 casks per year. The annual exposure would then be somewhere between .8 and 2 rem per person per year. Now, that complies with Part 20.

The NRC requirements for this, as many of the Board would know, is the NRC limit is 5 rem per person per year. Most nuclear facilities try to keep an administrative limit of about 2 rem per person per year, but most of the actual dosages to the workers at nuclear facilities is about 350 millirem per year is my understanding.

Let me talk for a moment about what we're finding in our evaluation. What we're finding is that we can't handle these casks on a manual basis in this facility. There are too many of them. But, if we use the traditional cask handling requirements that these are designed for, that we would not be within the limits that we would like. So, we're looking at remote and automated techniques. What we're looking at is standard robots that have been used in the industrial environment for many years. It's not in a high radiation field. It's essentially in a low radiation field. What we would like is basically when you have to take these casks apart, the transportation casks, someone is up there with a crank taking these bolts off. That's the way they do it at nuclear plants. But, given the amount that we would have to be processing through this facility, we can't do it. We'll get too high exposures and we'd have to have such a large staff that that wouldn't be economically feasible. So, we are looking at the application of industrial robots, automated techniques to get that down. We feel that with that we can probably get down to less than 1 rem per year for the operators of this facility.

We may have to look at additional factors depending on the mix of technologies that we get in one year because the dosages from each of the different technologies are different and we have to accommodate that. If we get a lot of, let's say, a high dosage technology, we may have to do special things in the facility to keep it again under a level that we would feel comfortable with. Basically, our design basis accidents and our recovery from those. What we would do if we dropped canisters, things like that, inside the facility and we would recover. Also, whether or not our site criteria is broad enough.

As the design proceeds, issues are arising and we're trying to deal with them. We've had a variety of interactions with the NRC which I'll talk about in a moment. Our feeling is that our design can deal with a lot of the issues associated with the handling of this amount of canisters per year, but there may be some things that the vendors will have to do to go back and reanalyze their technologies. Ambient temperature is one. The ambient temperature range of the vendors is not consistent with our facility. So, assuming that we're in an area where the ambient temperature is going to be outside the range of their SARs or their Safety Analysis Reports, they may have to go back and reanalyze their casks to deal with different temperature extremes depending on where the facility is. Also, as I mentioned earlier, the standard cask handling procedures, the manual handling may have to be modified to deal with the rate of casks that we'll see into this facility, assuming it's build.

We've had two meetings to date with the NRC. One in August which was an introductory meeting; we talked a little bit about the project and a little bit about the generic design criteria. Our second meeting was a little bit more focused on design basis events. We talked about our quality assurance program and some of our ALARA analysis. We are planning another meeting with them in mid-February which will be our last meeting before we intend to submit this topical safety analysis report on May 1. That meeting will cover any odds and ends. We still haven't focused on a final agenda for that meeting as of this date.

We have gotten some fairly positive feedback from the NRC. They feel the preapplication meetings have been important and productive. They're interested in how we're handling with the different systems since we are handling four different technologies. Also, an issue with them is the amount of casks and canisters that we'll be handling in any one year. The schedule is the schedule that you've seen in our draft program plan that was issued last year. We're planning on submitting on May 1. We're looking at about an 18 month review time by the NRC. And then, whatever comes out of the policy process, hopefully, we'll be ready to deal with.

And, that's the end of my presentation. I'd be happy to entertain any questions.

COHON: Thank you very much, Mr. Kouts.

Questions?

ARENDT: Arendt, Board. What interaction are you having with the Navy at INEL? They're, as I understand, providing a dry cask storage facility. So, are you getting any information that might be useful in designing this facility? And, I assume you have contacted utilities to get the information from there.

KOUTS: This facility would certainly be capable of handling the Navy fuel assuming that it met NRC requirements. We do have a regular interaction process with the Navy, and we do work with them. So, yes. The answer to your question is yes.

BULLEN: Bullen, Board designee. Could you put Viewgraph #19 back up there which was your Phase 1 site plan?

KOUTS: Sure.

BULLEN: I just have a couple of quick questions for you.

KOUTS: Perhaps, this would be more helpful. I don't know if that shows up very well. Is that better for your question or--

BULLEN: Well, actually, the question that I had was in regard to horizontal storage pads. Are you going to use the NUHOMS technology and take those canisters out of their current horizontal storage pads, transport them, and put them back in in the same can?

KOUTS: No, what we would do is probably a field transfer of the NUHOMS can out of the transportation cask in the field. We would not have to bring the NUHOMS into the building.

BULLEN: No, but you're going to basically take that NUHOMS can which is on rails and slide it out, gouging the daylights out of the side of the can, and then put that same can back in another one. Is that going to meet the Safety Analysis Report necessary? Are they designed to do that is my question.

KOUTS: Well, they were designed to be basically slipped in and out of a storage--

BULLEN: Once.

KOUTS: Once. And, what we would have to do is look at realistically what would happen to that in transit to see whether or not there would be any problems. There might be some inspection that we would have to do of the canister before we would put it into a vault on site.

BULLEN: My concern is not what's happened in transit; it's what's happened during storage during the past six, eight, 10 years that it's been sitting in there. Essentially you've got potential for other degradation. It's just a 304 stainless can.

KOUTS: That's correct.

BULLEN: That if you've already gouged it putting it in, you've put in great crevices already. And, if it's in a moist, humid environment with radiation around, you've got potential for radialysis and nice decomposition products, not the least of which is nitric acid which is not your friend. So, I'm very concerned about reusing those cans.

KOUTS: Okay.

BULLEN: I know they were licensed for once in and once out, but it you take them and you use them again, I think you're going to have to do some significant analysis to prove to the NRC that they can do that.

KOUTS: In our preapplication meetings, they have not raised that issue with us.

BULLEN: Well, then, be warned that they will.

KOUTS: Okay. That's a good point. I think that there might be a requirement for some kind of inspection of it prior to the time it leaves the site itself to make it transport capable. In addition to that, then they have to do some kind of inspection when it reaches the site.

BULLEN: Well--

KOUTS: So, I think your point is well-taken. I think that's--

BULLEN: At the worst case, you might have to repackage or adapt another technology or maybe not take NUHOMS right away.

KOUTS: Well, that's a good point because we really don't have the capability to re-can these on site. In fact, that's probably a subject that we'll deal with in the NRC in our next meeting and get their thinking about that. But, this was to be a fairly simplistic facility; basically, a very clean facility dealing with canisters. If there are issues such as you're suggesting, then we may have to rethink it.

BULLEN: Well, the other concern that you have were problems with the Palisade Plant where they had faulty welds and the difficulty that you might have in justifying using the can again after you've transported it. I mean, it's one thing to load it at a site and take it out to a pad, but to put it into an overpack, transport it in, and come back and say that you have to reinspect the welds which may be a problem or may not be a problem depending on what kind of facility that you build. I guess, I just want to caution you that it's a great plan, and I want to say that, you know, you're going to have to have some way to deal with the fact that there's stuff that's already canisterized or already stored. But, I'm not sure that the original design for dry cask storage let's you use it again. I think that's the concern that you're going to have.

KOUTS: Well, unless it's certified for transport and--

BULLEN: I know--

KOUTS: --it will never leave the facility that it's at. It will have to be repackaged at the facility itself.

BULLEN: Well, I know. It will be in an overpack, but I'm still concerned that you're going to take something that was designed to be used at a dry cask storage environment and then has been used and then try to reuse it for a second time. I think that's something that you're going to have worry about.

KOUTS: That's a good point.

METLAY: Dan Metlay, Board staff. Chris, I want to ask you a little bit about your schedules. I'm referring to the overheads that Dwight distributed yesterday and I don't know whether you have seen those or not.

KOUTS: I've seen them previously, yes.

METLAY: There are just a couple of things which I wanted to talk to you about. You've just said there was an 18 month anticipated time from submission of the TSAR to the NRC to some resolution or some response. Given the previous interactions that DOE has had, for example, on the burnup credit report, do you think 18 months is a realistic time to resolve these things?

KOUTS: I think it is and I'll tell you why, Dan. We've had very good feedback from the NRC on our dry transfer system which was designed in a very similar method. In fact, it passed acceptance review within a month which is kind of an all time record. I think with burnup credit--and, again, all three of those topicals are under my area of management. That is a little bit more challenging area that was a ground breaking area for the NRC and we're seeing that they're taking their regulatory time in dealing with it. In fact, we had a meeting with them last week, a technical exchange with them where I feel we've made a lot of progress, but there's still clear concerns on the part of the NRC as to how you would implement burnup credit. Measurements of the assemblies before they go in and so forth. With these facilities, we're not looking at those kinds of same issues. We're looking more--especially, seeing the litany of NRC documents and references that we have, we've done that essentially to try to expedite the review process. So, my expectation is that we will be--we will not be in a burnup credit kind of mode; we will be in a review that moves quickly along with the NRC and I do have some confidence based on what we did with the dry transfer system that this will move forward smartly.

So, in answer to you question, I do have more confidence than we had with burnup credit simply because again we're using the little bit more known areas that the NRC is comfortable with.

METLAY: Let me ask you a second question having to do with schedules. In the milestone chart that Dwight presented yesterday, both Phase B of the transportation contract and the submission of Phase 1 licensing application to the NRC for an ISF, is scheduled to occur sometime in the spring of the year 2000; 4-1000 is the marks. And, coincidentally, both the beginning of Phase C of the transport contract and the operational start of the Phase 1 ISF is scheduled to occur in September of 2002. The logic being that you want the facility to be ready to accept material that you're prepared to transport. So, you have roughly, essentially, identical time slots for the transportation and the ISF. Given again the fact that DOE's schedules have in the past not always followed what was planned, which of these two routes do you think are more likely to be subject to delays?

KOUTS: Well, very simply here, we need both. WE need the transportation capability and we need a facility. So, the critical path will be whichever one takes the longest.

METLAY: And, I guess what I'm asking is in your judgment--I know that currently they're planned to be identical, but in your judgment which is likely to actually turn out to be the longest.

KOUTS: Well, you're asking me to speculate. All I want to say is this, Dan. Assuming some kind of authorization is passed by Congress to do this, the Department will be under incredible pressure to move the RSA and this facility forward. And, my sense is that there will be equal management pressure to make sure that this thing will happen, and we will look for methods to make sure that the schedules mesh. In terms of which one will be the winner and which one will be available before the other, it really doesn't matter because again the critical path is whichever one is the last one ready and we'll be determining when we begin to accept fuel. But, in terms of which is the more challenging route, if as that schedule indicates all the things go down and Congress passes bills and so forth, I think they will be neck and neck. I think there will be a challenge to do 1200 tons the first year, but nonetheless, I think that both of them have a good chance of happening assuming that the pressure that would accompany any authorization is there and I have high confidence that it will be.

METLAY: Thanks, Chris.

CHU: Mr. Kouts, what is the period of license for the Phase 1 facility? Is it still 100 years that you're trying to pursue?

KOUTS: After we submit a license application, we are looking at a--and this fits into the assumptions that you take. If indeed the Department is not doing an EIS and the NRC is doing an EIS, we're looking at about a 32 month review time for the NRC.

CHU: No, I don't mean--I mean the actual period for the life of the license for the facility?

KOUTS: That will be determined by the NRC. If it's standard 72 technology, it will be 20 years license renewal.

CHU: At one time, as I understand it, you were pursuing a 100 year license for the facility?

KOUTS: We could look at that. That's something that--a lot of it has to do with the timing of the repository and when the repository is available. If the repository is available earlier, then we would empty this facility out and it would be a shorter time duration. If the repository isn't available, then this facility would have to be around longer. I'd have to go back and check our assumptions. I don't believe we're looking at a 100 year license, but we'll maybe have to go back and look at that, Woody.

CHU: My question would have been somewhat similar to Dan Bullen's or along the same lines and that is the canister technologies you're considering are certified for 20 years at a time. Some of the stuff that you may be moving in are in canisters that's been already lying around for, say, a number of years.

KOUTS: That's correct, but what--

CHU: Are you envisioning transfer of stuff within the facility after it gets moved?

KOUTS: Well, let's talk about what the 20 year licensing period is. I've been in public meetings where the NRC has had to explain this, and it's essentially at the end of the 20 years, the applicant has to go back and explain to the NRC why this fuel should stay in the same storage technology that is then and they have to prove analytically or with testing or inspection, whatever the NRC may require, as to whether or not the license can be renewed for that cask or for that technology. So, if there are problems with it, then clearly it would be incumbent upon the applicant to go back and repackage in something new. And, that's all dependent again on the reanalysis of the storage technology at the end of the 20 year period and whatever testing is done during that period.

CHU: Okay.

BULLEN: Just along those lines, it's another question that I raised yesterday. If we do have these questions about the technology and if the utilities are indeed interested in maybe moving stuff that's not canisterized, why don't you just load new stuff and have new containers that you can store and transport with greater assurance and without having to do the retro-tests?

KOUTS: Ultimately, that may be something we'll have to look at. I think the desire would be to use the cans if they are still usable, to use the cans that the fuel are in now and transport them and store them just to save the additional expense. But, if indeed there are technical issues associated with it and they are insurmountable, then we'll have no other choice but to re-can them. But, they'd have to be re-canned at the facility site.

BULLEN: What I'm looking at is you envision 6,500 containers. So, if you wanted to make an impact initially or right away, would it make more sense to just go ahead and start building new containers and loading those at the reactor, transporting them to your interim storage facility, and putting them into whatever technology you decided to use without having to re-evaluate five, 10, or 15 year old casks that have been sitting around? I mean, it would also make the utilities a little bit more happier because I'm sure they want to empty out their spent fuel pools. The stuff that's in dry cask storage right now isn't really of a major concern. It's what's uncanisterized already in their spent fuel pools that they'd like to move.

KOUTS: That's correct. What you're suggesting may be the way we go. Again, it will come down to a technical issue as to what we have to prove and somewhat of a monetary issue as to whether or not--you know, what the impacts would be of re-canning all the fuel. And, again, all these operations would have be done at utility sites. My sense is from interacting with utilities, I don't think that they would just volunteer to re-can all these things unless they absolutely had to to get--

BULLEN: No, no, I agree with that. But, I'm sure they'd volunteer to let you can new stuff.

KOUTS: Well, can new stuff at our facility is different. First of all--capable--capability to do that--

BULLEN: No, no, I understand that. Right now, you want to take canisterized fuel.

KOUTS: That's correct.

BULLEN: But, if you are going to walk in with a can and say we'll pay for the can and we'll take your spent fuel out of your pool, I think utilities would be very happy, and I think it will preclude a hurdle of re-evaluating used cans.

KOUTS: I think it's a good subject for debate.

BULLEN: I agree.

KOUTS: And, as we go forward, I hope we get that far and--

BULLEN: I'd like you to take it into consideration.

KOUTS: We certainly will.

BULLEN: Thank you.

KOUTS: Thank you.

COHON: I'd like to ask a question that's sort of related to Dan's line of questioning. If you could choose the spent fuel that you would store--that is, suppose, of all the spent fuel now in existence at utilities, you had the right and power to sequence the order in which you would accept it--would that have or could that have a significantly positive effect in terms of Phase 1 design?

KOUTS: I don't think it would have that much of an effect simply because these systems are basically designed to take fuel that has been pooled for a short period or a long period and they're fairly broadly designed systems. So, really, when you're moving cans, it doesn't make that much difference. The heat load isn't going to be a problem because they're out in the field. Who cares? The bottom line is I don't think it would really affect it and I don't know that we'd really need to. As you know, the way the standard contract is set up, the utilities really have the call as to what they put in the cans except for in terms of the cooling of the fuel and the type of fuel. Except for the fact of whether or not it's failed, we can basically say, no, we don't want to take failed fuel until later. So, I think we've designed this facility to be as flexible as possible, and my sense is it really wouldn't make that much difference.

ARENDT: I assume that you will be furnishing a specification to the RSA so that the material that arrives at IFM will meet your specifications?

KOUTS: Our specifications are NRC approved packages. And, if they bring us one of those, we'll take it.

KNOPMAN: Knopman, Board designee. I didn't hear in your discussion of the design any considerations that were going into the design relative to decommissioning that storage facility at some point in time. Can you comment on that?

KOUTS: That's something we will have to address when we submit the license application. We haven't spent a great deal looking at that. Most of the materials that would be contaminated on this site would have to be basically taken to some low-level burial ground and dealt with. We're not addressing that specifically. Again, we've got a generic site, but we could evaluate the amount of materials we would expect over 40 years of operation or whatever of 40,000 tons on site. But, that is something we will have to address and it's something that we're mindful of. That's a good question.

COHON: Thank you very much, Mr. Kouts.

We will now take a break and reconvene at 10:00 o'clock.

(Whereupon, a brief recess was taken.)

COHON: Before we start the second half of the morning schedule, I want to point out something so that there's no confusion on anybody's part. Not surprisingly, the revised guidelines under 10 CFR 9960 is a topic that comes up repeatedly. It has come up repeatedly since yesterday, as it should because it's a very important topic. We've not heard official response to these comments and won't from DOE because, as we know, they are in the formal comment period when they are receiving comments from the public. It would be inappropriate, therefore, for DOE to respond in any formal way to comments received at this meeting. I think that was clear to most people, but if it wasn't, hopefully it is now clear.

The remainder of this morning's session will focus on a very important topic. I'm tempted to say hot topic, but we avoid that phraseology in this program. It's certainly very current and we expect from the little tidbits we've heard by way of preview very interesting because we're going to hear some new results that go directly to some of the central issues related to the suitability of the Yucca Mountain site.

Dennis Williams will introduce the remainder of this mornings session. We're pleased to welcome him back to our Board meeting. His title now, it wasn't last time, Deputy Assistant Manager for Licensing. He's got a new title. That's a very nice title. Congratulations.

WILLIAMS: For this presentation, I will use visuals and I will try to make things clearer so we won't have to use that T word.

This is intended to be just a few opening comments to introduce the saturated zone flow and UZ saturated zone transport studies, some of the things that we've been doing with the C-Well complex, and with the modeling effort associated with that. Three of the people that will talk about these hydrologic and modeling processes, of course, are M.J. Umari of the GS, Jake Turin of LANL, and then Bruce Robinson of LANL. I'll leave possibly more formal introductions to the Chairman later. I might add at the end of this session, we will get a bit of a microburst of an overview of the ongoing ESF activities from Bill Boyle.

Some of these overheads are pretty wordy. I've gone through and just highlighted a couple of high points on them. This one, saturated zone flow and transport, why study the SZ, all jump over to the waste containment and isolation strategy. It must have been two years ago when the Board was last in Nye County. We were out at Beattie. One of the items of discussion was the waste containment and isolation strategy. We went through quite a discussion of how all of the testing tied into that waste containment and isolation strategy.

So, you saw these attributes that Russ put up. What I've done is highlighted some of the things that I feel are important to the site characterization part of it. Of course, the rate of seepage into the repository and, of course, down at the bottom, we have dilution in the groundwater below the repository. In this middle zone, we've got the radionuclide transport through engineered and I've got also loaded in here the natural barriers. Now, what we've been trying to do over the last few sessions with the Board is talk about what we're doing as we move down through these attributes. Of course, last time in October back in Vienna, we talked about percolation flux. That has to do with the seepage into the repository. In Bill's discussion today, we'll talk more about the rate of seepage into the repository and some of the testing associated with that and basically how things are going as far as our testing down to the area of the repository horizon. Okay. From that point in then, we get into the transport area which is below the repository and goes on down to the groundwater. So, I'll leave this up for reference as we move along because some of the points I make, we can readily go back to that waste isolation strategy.

Why do we study it? Because radio--

LANGMUIR: Dennis, would you do me a favor and move your microphone on your tie a little bit?

WILLIAMS: Okay. I'm blowing you away? How's that; better? Okay.

Why the saturated zone? Well, any radionuclides released will be transported to the accessible environment. There's no--you know that's what's going to happen. The saturated zone, it's greatest importance would be for radionuclide dilution. When we get down there, we're going to have mixing and dilution of the radionuclides in that volcanic aquifer and, of course, this is the downgrade in area. This is where the plumes is going to go. And, over here, we can look at the canisters. It says "in breached canisters". Canisters are going to breach. Those things are not forever. I can't think of anything that's forever except maybe teenagers. That's what comes to mind today.

The C-Well complex, that's where we're doing most of the field studies associated with the saturated zone. Now, these holes, as M.J. will tell you, were put in back in the mid-'80s, did some initial tests in that, and then like a lot of other things on this project, they just kind of went into a little bit of a hiatus for a time being. I think I recall Russ Dyer saying that when he came here in 1988, the big thing was to get the C-Wells going. Well, when I came here in '91, the big thing was to get the C-Wells going. Well, we got the C-Wells going in '94 and I think we've been pumping on those holes continuously since then. So, later on today, we will also see that it is our intent to continue to do hydraulic and transport tests in those C-Wells because we want to get everything possible out of that complex. Testing objectives from the standpoint of the C-Wells, basically the hydraulic properties through the pumping test and transport parameters from tracer tests.

A lot of this stuff, to me, is about as exciting as watching paint dry. But, when I got a presentation on what they're using as far as the tracer soup from the Los Alamos folks last week, I mean, I was pretty excited about the results that are coming out of this. I think that you fellows will be excited, too, when you see how these next presentations develop.

Where does the information go? Into the TSPA/VA transport calculations. And, ultimately, we'll address the radionuclides moving from the repository horizon to the accessible environment via the groundwater right back to our waste containment and isolation strategy. Coming out of here, moving through the groundwater into the accessible environment.

There's two parts to the C-Wells test. Basically, the hydraulic testing and get your typical hydrologic units. The point I wanted to make was where the testing has been conducted. Basically, in the Bull Frog and the Upper Tram intervals, those are the more transmissive units down there at the water table. For you folks that don't remember your stratigraphic package and for the new designees, I just throw up a predictive stratigraphic package from the SD-6. It was a handy item here. But, basically, we're coming down through the stratigraphic package from younger to older in the geologic and hydrologic units. The Tiva, what we call thermomechanically the PTn basically, the bedded units above the repository horizon, moving down through the Topopah. Here's the middle non-lithophysal; that's what we're doing the excavation in. If you get down in the vicinity of the Calico Hills, you start running into the water table. Then, there's the Prow Pass Tuff, the Bull Frog, the Tram, and eventually you'll get down into the Paleozoics. Where the tests have been conducted is in the more transmissive units in this vicinity to make sure that the testing would work. One of the things we'll talk about later on this afternoon is going back up into the Prow Pass and getting into some of the less transmissive units, those units right below the water table, to see how they respond.

Same thing with the reactive tracers going for what we've called that tracer soup. Again, the tests, so far, have been conducted at the Bull Frog and the Upper Tram, the more transmissive unit. The indications are that it's a dual-porosity flow and transport system. The radionuclides travel in the fractures and also in the rock matrix. Estimates coming out on dispersivity, that was a real doozy of an issue yesterday. I'll let the technical guys get into that.

ALLEN: Dennis, excuse me, just a point of clarification. What is a C-Well and where are they?

WILLIAMS: C-Wells, oh, seeking clarity.

ALLEN: C is for?

WILLIAMS: C is for conservative tracer. That's why it was designated that way in the beginning. When you go out to the ESF--I think everyone has been out to the ESF--you're traveling, oh, let's see--okay, M.J., you gave me a doozy here. ESF is up here, okay? You're coming out the road, coming out of Forty Mile Wash, you're going up to the ESF. There's a road that turns off and you go to the south and you go to the C-Hole complex which sets in here. Those of you who have been out to UZ-16, one of the first LM-300 drillholes, that's sitting up here. So, you come around here and go in that direction. I'm sorry, one of these lack of clarity assumptions. I assumed most of the Board members have been to the C-Well complex. But, anyway, that's where it's at. M.J. will give us a couple of more.

ALLEN: Does C have any significance other than just a code for those locations or--

WILLIAMS: Conservative tracer. Yeah, it was a code for conservative tracers. And, there's three of them. M.J. has a diagram that shows you what the pattern looks like and we'll roll on that.

What's the transport modeling starting to tell us?

Of course, you remember these numbers from percolation flux we talked about in October. Everyone reminded me that I must have said about 14 times that there's no direct measurement for percolation flux. So, there's no direct measurement for percolation flux. Those are the ranges that we're dealing with, although they indicate they may go higher than that. We're largely looking at neptunium and technetium from a peak dose perspective. We've got thoughts on fast pathways, and also how the weakly sorping radionuclides, how fast they will reach the water table.

The big points on this whole thing with regard to saturated zone transport, what are we looking at? It gives us the indication that we can significantly reduce the peak concentration and delay the arrival. I realize that that doesn't fit into some of the regulatory concepts, but we're looking at what kind of processes are going on out there at the mountain, the physical processes, understanding them, and what's going to happen to the environment when we do this, release the radionuclides. Because we know that when we go out of the canisters and come down through that remaining portion of the unsaturated zone, the only thing that is left is the saturated zone. That's your last line of defense against the rapid transport of any fracturing of the inventory that gets down there. So, if you're looking at this from a multiple barrier/multiple defense standpoint, I feel that you have to consider that. If the regulatory arena does not allow you to consider that, you still know what's going to go on down there.

So, that's kind of where I stand as a bottom line on that and, I think, at that point probably we'll jump to M.J. and get him into the details of some of the hydrolic testing.

COHON: Thank you, Dennis.

Yes, please?

WILLIAMS: Rather than invite questions now, unless there are burning issues that Dennis has to deal with, we'll just continue right on. But, we will pause for questions after this presentation by Dr. Umari who is from the U.S. Geological Survey.

UMARI: My name is M.J. Umari and the principal investigator for conducting hydrolic and conservative tracer testing in the C-Wells complex.

The location, now that you've asked Dennis about the location, I've pulled one out here that I wasn't going to use. Maybe, this will clarify it further here. You can see Yucca Mountain here and, basically, the C-Holes are on the east flank of Yucca Mountain, on the west side of Fran Ridge, and this gives you the overall location of the complex. In terms of the particular location, we need to look at this because we need to look at it in the context of surrounding faults. At the C-Wells complex, we have a fault zone that has intersected the complex at the bottom. The complex has three wells in it. They were, as Dennis said, constructed in the 1982-84 time frame. Hydrolic tests were conducted at that time, and then for a period of time, there was a hiatus. At the bottom of the complex, there's a fault zone that intersects it. It had been initially interpreted to be the Paintbrush Canyon Fault intersecting the bottom of the C-Holes, but there have been some recent faults traced here. The Midway Valley one may turn out to the one that is the fault that intersects the bottom of the C-Holes. But, in any case, I think this is a significant point because we are going to propose doing some tests in the fault zone.

If you look at the surface trace of the complex, you'll notice that it's in the shape of a triangle. C-2 and C-3 are aligned in a northwest/southeast direction and C-1 and C-3 are aligned in a northeast/southwest direction. This helps in terms of alignment with the overall principal directions of the transmissivity tracer that was perceived at the time that the complex was constructed. And, it's also interesting because this particular direction here lines up with a fault that had been mapped recently and that I'll talk about in a little bit.

You can see quite a bit of deviation in these boreholes and the distances that are actually used in terms of interpreting the test are the distances at the particular hydrogeologic unit that's being tested and not the surface distances. So, we actually go back to the deviation logs and use those.

A geohydrologic cross-section that is specific to the C-Holes, as Dennis said, the water table starts being encountered in the Calico Hills. Then, below that, we have the Prow Pass Tuff, then we have the Upper Bull Frog, and then a Bull Frog-Tram combination that we have from a geohydrologic standpoint divided into a Lower Bull Frog and an Upper Tram, and then there's the Lower Tram. You can see here the intersection of the fault zone at the bottom of the complex. In fact, we think that there is another--we think that there are two faults that intersect the bottom of the C-Holes; the one that would be the Paintbrush Canyon Fault or Midway Valley and, in addition to that, there's another fault that actually offsets the first one because the intersection of the first fault at C-3 is higher than it should be. All the testing that has been done, so far, has been either in the combined Lower Bull Frog-Upper Tram or in the Lower Bull Frog. And, these are the most transmissive zones and that's also significant in terms of proposing work that would be in the low-flow zones in the future because we feel that performance assessment modelers should really have values of the hydrolic and transport parameters in the saturated zone not only at high-flow zones, but at low-flow zones to have a complete picture.

At any rate, since the testing that was done in 1984 time frame, there was a long period of time in which no actual pumping took place at the C-Holes because of various reasons. At any rate, eventually, a discharge pipeline was constructed to carry the water all the way from the C-Holes to Forty Mile Wash and various other hurdles were gone through until we were able to actually--it was in May '95, not in 1994, that we were able to start testing again at the C-Holes complex. So, in May of 1995, we started an open-hole test and the objective of that was to look at the whole picture, to look at the total thickness of the saturated zone intersected at the C-Holes without first looking at the specific zones. That was the first one. Then, following quickly after that, we kept the pumping well which was C-3 in open-hole conditions, but we backed off observation Well C-1 and C-2 and started looking at the components or the geohydrologic units that are the components of the total section of the C-Holes.

Then, in February of '96, we actually started the first tracer test of the C-Holes, but it was preceded by a week of a hydrolic test in order to establish steady-state conditions. That's why I'm listing it under hydrolic test and that was specifically in the Bull Frog-Tram combination.

Then, in May of '96, things were configured again. The packages were reconfigured and we started another tracer test soon after that. Actually, Los Alamos National Lab did that particular tracer test, but it was preceded by a week of hydrolic testing. Again, I'm putting that under hydrolic test. And then, since we started this particular pumping phase in May of '96, we have not shut the pump yet. It's been pumping at 150 gallons per minute. So, we have a very long term pumping test going on as a background and a backdrop to conducting a sequence of tracer tests. So, I'd like to point out that that's an efficient way of doing it because the NRC study plan has said that we needed to do a long-term test and, of course, it's a good idea. But, we're doing it without waiting for it to be done because we're superposing on a sequence of tracer tests.

The results here from the hydrolic testing. This is the cone of depression that is the result of the May '95 hydrolic test of the C-Holes and that is the test whose results are--the results of which actually reached ONC-1 and H-4. And, the interesting thing here that I'd like you to notice is the fact that the cone of depression is elongated in the northwest/southeast direction. That direction happens to be again aligned with this newly mapped fault that--probably, I should put this one up to highlight. As you can see here, there is a new fault that has been mapped recently by Warren Day and it's the Antler Wash. And, that, if continued, lines up with the elongated direction of that cone of depression that I just showed you there.

Another result from the hydrolic testing is this long-term hydrolic test that I'm saying is the background to tracer testing. About here is when we started with the May '95 pumping. This is the beginning of the Los Alamos--benzoic acid tracer test, and down here Los Alamos started another test, the lithium bromide test. The test that I'm going to tell you about, the conservative tracer test that the USGS conducted, was prior to that. At any rate, the point of this is just to show you the water level fluctuations. A lot of these are the results of atmospheric pressure and--changes, but you can also see the effects of injection at particular beginnings of tracer tests. And, what's interesting is that not only does the pressure go up, but it's actually followed by a decrease in pressure. We feel that maybe what happens when we start an injection for a tracer test that that is propping up some fractures and so it's followed by a period of decrease in pressure. Anyway, the line here through it is basically a projected line that would be according to the dye solution running throughout here. And, you know, we're looking at analyzing that long-term test by various methods; confined aquifer solution and also by fissure block solutions.

The final results summarized for hydrolic testing at the C-Holes, if you look down here, you see that the composite transmissivity for the whole section is an order of 18,000 to 32,000 feet squared per day, and you can see that the majority of that is taken up by the Lower Bull Frog and the Upper Tram. The other units are much less transmissive. However, we are proposing that we would go into the Prow Pass and do hydrolic and tracer testing to determine the hydrolic and transport parameters of low-flow zones.

As a transition slide here to tracer testing, this is a little picture of the C-Holes complex. Some of you went there yesterday. The salient feature here are these pipes that are, in fact, ones that convey the tracers to the injection well for, you know, the one that's being used for a tracer test. So, for example, this particular pipe here is conveying the tracer to this particular well which I think is C-2. This pipe here is taking the tracer down to C-1; such that if tracer testing is done in C-1, it would have the tracer injected through that pipe.

I'm only going to talk about conservative tracer testing of the C-Holes which is the USGS's responsibility and then the reactive tracer testing would be represented by Los Alamos. Essentially we started on February 13 of '96 pumping C-3 and injecting iodide as the tracer into C-2 and that background flow field was a convergent flow field. We just pumped C-3 and at the time we started at 139 gallons per minute. And, over that convergent flow field, we inject from one or more locations. In this particular case, at one location.

The other conservative tracer test that we're conducting, we just initiated recently, January 10 of '97. This is a similar flow field to the previous one except in this case the pumping rate is 150 gallons per minute and we have two tracers being injected; Pyridone from C-1 and a 2,6 di-fluoro benzoic acid from C-2. The flow rate on the second one is higher. Also, the zone isolated in this particular one is the combined Bull Frog-Tram whereas in this one it's just the Lower Bull Frog.

We have decided to look at a simple solution of the advection dispersion equation in order to be able to analyze the results of the tracer test. So, we took an analytic solution of the advection dispersion equation that had been published by Alan Moench. And, the basic concept of it is that you look at cylindrical area that is bounded by the injection well and centered around the pumping well. The salient parameters involved are the Peclet number which is the ratio of the inner borehole distance to the longitudinal dispersivity, the advective travel time which is basically how quick the breakthrough curve gets to the pumped well, and then there are two dimensionless parameters, gamma and sigma

--this is a dimensionless porosity parameter which is the ratio of matrix to fracture porosity. So, this solution is a dual-porosity solution, okay? It's an analytic solution. So, it only assumes a homogeneous and isotropic situation, but it doesn't assume only grains of sand. You know, it assumes a dual-porosity medium and you take it to one extreme and go to a single-porosity medium with it, but it allows you to experiment with a dual-porosity environment. And, this gamma is a dimensionless molecular diffusion coefficient that would determine how much of the tracer goes into the matrix blocks as opposed to continuing in the fractures.

I think at this point I want to show this particular diagram that just conceptually tells you how we're thinking about this medium. We're thinking about this medium in one of two ways. We think that it's a dual-porosity medium, but we have two different conceptualizations. One is that we have a continuous network of fractures where the fractures are all connected and that we have matrix blocks that are isolated there and that the tracer actually goes into small boundary layers around these blocks. The main solution does not allow for actual transport flow or advection into the blocks. The blocks are only used like sponges, as storage areas. So, the main transport occurs in the fractures, the blocks are used as storage locations, and only a portion of the block is really used.

Another conceptualization is that the fractures are discontinuous. So, you have a fracture here, a fracture here, but they're not continued. In order to be able to move through, actually the tracer will have to actually be jumped through a portion here which is the matrix portion. So, you can't really discontinue in the fractures. You have to go through the matrix a little bit. Both concepts are useful at this point for us because they explain two kinds of extreme results that we can get by fitting the curves. In this particular case here, the matrix--the effective matrix porosity that you get if you just consider the small boundary as effective, becomes much less than the actual total matrix porosity. So, we have certain fits in which the matrix porosity is a lot less than what would have been obtained from geophysical logging, for example. That conceptual model explains that.

In other cases, we have fracture porosities that we can fit the results with fracture porosities that are higher than what is considered to be the typical one of .1% fracture porosity. In that case, we think that this concept might be taking place and that the tracer actually accesses not just the fractures, but a little bit more from the matrix in terms of its primary flow field.

So, with that in mind, I'll show you two results here. These are the data points from our February 13 iodide test. The line is a theoretical curve for the main solution. We have written programs to automate that solution and this is just the front panel of that program, but it shows you the parameters that were chosen to make the run. So, for this particular one here, the Peclet number of 4.68 which translates because of the inter borehole distance to a dispersivity of 20.7 feet. This solution has a small matrix porosity, only 3.2% and a fracture porosity that's .68%. The fracture porosity is consistent with what researchers think fracture frequency should be, in the .1% range. The matrix porosity is low compared to what we think it is there from laboratory tests which is like 20 to 30%. So, that first diagram that I showed you would come into play to explain the low matrix porosity.

And then, I'll show you another solution where again for the same data we have a case of 18.95 matrix porosity here which is more consistent with what you see from your physical logs and laboratory tests. But, the fracture porosity is 8.6% which is very high if you were to assume that normally it's .1%. So, the second diagram, the second conceptual diagram there, would come into play explaining that. So, we're in the process of attempting to figure all that out.

Also, the results for a tracer test between C-2 and C-3, it's a short distance. If you do another tracer test between C-1 and C-3, it's a longer distance. If you get different results for the dispersivity, for example, do you attribute it to the fact that you looked in a different direction and that that might be a directional result or could it be a scale result because C-2 is only 100 feet away; whereas C-1 is 200 feet away from the pumped well. This particular diagram would show you that we might be looking at a scale effect in that if we looked at the dispersivities that we have calculated by one of those two approaches that if you look at C-2 which is 96 feet away--so, you look at a distance of about here and you plot the dispersivity of eight meters, you're about here. Whereas, if you look at C-1 which is 85.6 meters here and you're looking at somewhere here, then your dispersivity that you get which is seven meters, you know, plots against a result from Gelhar that indicates that the longitudinal dispersivity is a function of scale. So, that's one result that might be indicative of something.

This is an overall view of the tracer testing, so far. This is our test that was started on February 13. We stopped here because the pump had been degraded and we stopped at 98 gpm, although we started at 139. So, we had a problem with the pump. The pump was shut off. Later on, the pump was changed and the pumping well was reconfigured on May 8 of '96. Although it says 2nd here, it's actually the 8th. We started the pump again and this is iodide results at C-3 that are actually a continuation of our injection on February 13. Then, on June 18, Los Alamos injected iodide into C-1. So, from that point onwards, the iodide in C-3 were a result of both C-1 and C-2 and we had to look at antecedent concentrations of iodide from the first test to kind of look at the results of the iodide test from C-1.

Anyway, to summarize here, the results of conservative tracer tests indicate for us fracture porosity range of .68% all the way to 8.6% and matrix porosity range of 3.2 all the way to 18.95% and a longitudinal dispersivity range of 8.68 to 20.75 feet. We do feel that the dual-porosity medium seems to be indicated by the data and that the transport parameters overall are less firm at this point than the hydraulic parameters. That, I would say, is the overall conclusion from our tracer tests from the USGS standpoint.

So, overall conclusions for hydrolic and tracer testing is to say that high-flow zones of the C-Wells complex have been successively characterized for both hydrolic and transport properties. We feel that way, although with a weakness of the transport movement. Results from the hydrolic testing have provided information on the hydraulic parameters not only at the C-Well scale, but at a scale larger than that which is interesting. And then, we feel that the success of testing at the complex indicates that it should be used for additional testing, both for low-flow zones and for the fault zone that intersects the borehole at the bottom.

Then, the last slide here is to indicate that there is planned future work to conduct the hydrolic and conservative tracer testing in low-flow zone at the C-Wells. In fact, the money is for FY97, and it's going to be initiated towards the end of FY97. And then, also, there are future plans to conduct hydraulic and conservative tracer testing--actually both conservative and reactive tracer testing for both at sites other than the C-Holes, at other locations in Yucca Mountain.

Thank you.

COHON: Thank you, Dr. Umari. A little bit like taking a drink from a groundwater fire hose. It's a meaningful metaphor.

We have a time problem, but on the other hand, this is very important. We'll entertain a couple of questions. Don't go away, Dr. Umari, please. We might need your overheads.

Pat?

DOMENICO: I have two; I'll take the two.

COHON: Well, no, you get one.

DOMENICO: No, the first one is very easy.

COHON: Okay.

DOMENICO: How did you pick the Peclet number off the breakthrough?

UMARI: How did we pick it?

DOMENICO: Yeah, how do you pick a Peclet number off the breakthrough for that?

UMARI: Okay. Basically, we first run the Moench code with a single-porosity version. The implication there is that at the beginning that at any--single-porosity elements--

DOMENICO: Okay, I know what you do.

UMARI: --happen later. So, we just look at the beginning of the curve and then move it back and forth based on the fracture porosity and then keep on trying different Peclet numbers for the shape of the curve and that's how we get the Peclet number and then fix it to go to the other parameter.

DOMENICO: Okay. Then, my second question is what was the percent recovery of the conservative tracer?

UMARI: The percent recovery was in the 20 to 30% range.

DOMENICO: You lost 70% of the tracer?

UMARI: I don't know whether lost is the right way to say it, but it hasn't been recovered at C-3.

DOMENICO: What does that mean about the calculations, especially for the longitudinal dispersivity?

UMARI: What we do is basically look at the actual--maybe, I should put just one of those back just for a background here. What we do is look at the actual mass recovered. In fact, we look at the dimensionless concentration. We normalize the curve based on the maximum concentration, and then when we look at the analytic solution, just stay at the dimensionless level. And so, that implicitly assumes that the mass--that we're working with the recovered mass.

DOMENICO: Okay, thank you.

KNOPMAN: Knopman, Board designee. M.J., we spoke about this a little bit before, but I would like to elaborate on the question of using these tracer tests not just for parameter estimation, but for model discrimination. You've got still competing models or various models still in play. Could you show on that plot, for example, what you would expect if you had only fracture flow? What sort of breakthrough you'd get without a--not a dual-porosity model or what would happen if you had just matrix flow and no fractures?

UMARI: I have an actual solution, if you'd like, of a single-porosity solution. It may not be very easy to find. So, maybe I should just talk about it. Let's see, essentially, what would happen is if you're assuming a single-porosity medium, you wind up with a fracture porosity that's higher than that. And, that kind of leads to that second conceptual picture that maybe if you only rely on the fractures and do the fit, that you seem to be needing a fracture porosity higher than what is normally considered a fracture porosity. So, that seems to be indicative of that second concept there where we're having discontinuance fractures.

I was thinking after you were talking to me yesterday and I was thinking, yeah, you're right, we are trying to identify parameters here and it appears that we've kind of accepted a particular model that we're going with and so the focus is on parameter estimation. But, I think, as you can see from my effort here to explain extremes, that we're still not sure what the actual conceptual model is. So, there is an effort maybe not very structured at this point in terms of trying to address which conceptual model, you know, truly is the one. And, we're going to be going through this data in a very systematic way to kind of rule some models out.

KNOPMAN: I mean, the point is to figure out which time period and under what sort of pumping conditions you need to start seeing the differences between these various models and to be focusing in on that. It's always a range for any of these conservative tracer tests where you can fit 10 models to the data. So, that was the point.

UMARI: Yeah, and I think you'll see that probably from the Los Alamos presentation when they have several tracers, that some of these conceptual ideas are starting to come out from that; not in terms of different flow rates, but in terms of using different tracers and they have different molecular diffusion characteristics and so on. So, we're attempting to sort these conceptual models through and we're not like stuck with one yet.

COHON: Don had his hand up first and then we'll go to Ed and then that's it. We'll move on.

Don?

LANGMUIR: Yeah, I'm learning as I listen; obviously, we all are. It looks as if the Lower Bull Frog is the likely avenue for accessible environment travel. Is that one implication of this? That's where the high transmissivities are; so, therefore, you're going to get your contaminants down to the sat zone and then to the Lower Bull Frog? If they're going to get there first, they'll go through the Lower Bull Frog?

UMARI: I think some of this--and, the emphasis on it should be based on what particular criteria is going to be used. If it's going to be a dose based standard, then what you're interested in is the concentration as opposed to the mass that would have been delivered. If you're interested in the mass delivered of tracer which is our current standard, i would assume, then the high transmissivity zones are important because they're going to conduct more mass of the tracer--of the radionuclide. But, if it is an issue of what the concentration is at a point, say, that's being measured outside, then I would think that low-flow zones should be investigated, too, because they would tend to have concentrations that are higher. So, I think part of it depends on what criteria is being used to judge whether the site is acceptable or not. At this point from our perspective, we think, well, then, really all the range of parameters should be given to the modelers who are going to do the big picture.

LANGMUIR: A related short question. Usually, you have different permeabilities in the vertical sense, lower values than you have in the lateral sense for matrix like these. How significant is this? In other words, what are the travel times in the sat zone from the water table down to the Bull Frog? What would you estimate those to be? Of course, you're talking about dispersion and a lot of other processes that would get you there, but is that a barrier to flow to the environment?

UMARI: I think, you know, one of the things that we're seeing is that there is enough fracturing, such that those zones are really not as isolated as one might want them to be. You know, like for example, when I say we're conducting this test in the Bull Frog-Tram, you know, we have packers at the top and bottom and the predominant amount of flow happens in those; specifically, since they are highly transmissive. But, if you do in the middle of those zones and put packers, you're not going to be able to isolate anything. You know, this test that we're going to be doing in the Prow Pass may be able to give us some idea, a little bit more about vertical flow. So far, every test that we've done, even though that we'd have packers isolating those different zones and we pump from a particular zone, all other zones respond. So, at some level, there is a pressure being transmitted and, you know, that's got to be through the fracturing that's taking place. But, as far as how much mass of a material would be transported, we haven't done tests to try to identify that. And, maybe, when we do the Prow Pass test, that could be one of the things we can try to address. I'm not sure.

COHON: Ed?

CORDING: Just a comment on the influence of a fault zone. Are you in the fault zone in all of the tracer--in all the wells? Are you encountering the fault zone or is it at all lithologic units in that test area?

UMARI: It's in the Tram at the bottom of the borehole in all of them. In all three boreholes except the actual location of it in C-3--the actual location of it in C-3 is higher than what is projected to be from the first fault zone. So, all of them, anyway, are in the Tram.

CORDING: I guess, the question is to what extent are the parameters you're obtaining being controlled by the fault? See, that would be a model perhaps that is more continuous and normal to the fault itself in the horizontal direction. You're seeing different behavior certainly. Are you able to pick out characteristics that relate to the fault as compared to areas away from the fault? Is that part of what you are going to be able to pick out of those?

UMARI: That's what we'd like to do. We haven't done that. We have only looked at the Lower Bull Frog and isolated zones and all three wells at that horizon and we're proposing to do that at the Prow Pass for a low-flow zone determination. Then, we're proposing that we would do something that would answer those kinds of questions and try to position packers and transducers in a way that we could do cross-fault testing. We haven't done that. So far, all we know is that this definitely is a very high-flow zone. Inter borehole surveys indicate that and we know that it's there from geologic logging. But, we have not focused on it to study it. We're just thinking that it would be something worthwhile to do given that the complex is constructed, it's there, we know there's a fault zone, and--

CORDING: But, is it a lithologic feature that's controlled by the stratigraphy or the fault in terms of the various permeability of the transmissivity that you're getting?

UMARI: It's mainly in all of the zones. We feel that it's not strati-bound, that it's not related--that the flow is not controlled by what lithologic units and it's more by the fracturing.

COHON: Thank you, Dr. Umari.

Our next speaker is Dr. H.J. Turin who is an Associate Investigator for the C-Well complex and Chlorine-36 programs at Los Alamos National Laboratory. Dr. Turin, while you're getting wired up there, let me just tell you I will be strict about keeping you to your 15 minute time limit. Thank you. And, I'll do that by motioning frantically to you around 14 minutes.

TURIN: My name is Jake Turin and I'm from Los Alamos. I'm going to be talking a little bit about what we've done at C-Wells, but I think before I get into that, I'd like to point out that our whole C-Wells activity is part of a longer ongoing Los Alamos effort to understand and be able to predict radionuclide transport through the saturated zone. As has come up a few times earlier today and we'll be hearing more about it as the day progresses, as we find out more about the unsaturated and the possible existence of some fast paths, it's becoming more important to understand how the saturated zone may act to retard or decrease radionuclide sorption.

Now, the best way to determine how radionuclides would move through the saturated zone for 10,000 years, the ideal way would be to dump them in there, wait 10,000 years, and see what happens. Now, we don't have the time to do that and there's obviously environmental and regulatory problems with injecting radionuclides into the aquifer. We don't want to do that. So, instead what we're doing is we're using actual radionuclides in the laboratory at Los Alamos. We've been doing laboratory studies of radionuclide/rock interactions. Inex Triay has spoken to many of the Board members, I know, and that work is continuing. At the same time, we're looking at reactive tracer transport in the field which I'm going to be talking about today which is to look at how something we can inject into the ground moves. We're trying to use this as a sort of analog. And, finally, we're integrating all of this work together in predictive modeling which Bruce Robinson will be talking about shortly.

As the Chairman mentioned, I'm an associate investigator on this project. The principal investigator, Paul Reimus, is sorry to not be able to be here today. For your information, I've given you his name and telephone number in the handouts. So, you'll be able to get in touch with him if you need to.

We've got three main objectives for our reactive work at the C-Wells complex. First of all, this will get to some of the questions that came up after M.J.'s talk. We want to validate our existing saturated zone transport model, our conceptual model which consists of envisioning a dual-porosity system which again M.J. talked about a little bit in which matrix diffusion and sorption are important processes affecting reactive tracer transport. Secondly, all the work that Inex's group at Los Alamos has been doing in the laboratory looking at radionuclides, we wanted to be able to increase the defensibility of using those numbers in our long-term field-scale predictive models. So, we're going to try and look and determine the field-scale applicability of laboratory sorption data. And, finally, to obtain some field-scale transport parameters.

This concept of our double-porosity conceptual model is important. I'm going to spend just a second here on this. Let's start off without a double-porosity system. In a single-porosity system which can be either a totally fracture dominated system or a standard porous medium where you've just got matrix and bare fractures and you've got transport of tracer moving down here with a constant input. The only processes, the only effects that are going to change that is we've got advection along the fracture and dispersion, hydrodynamic dispersion. This is a physical process which is only dependent on the flow and the medium. So, we get some spreading, blurring of the front here, and we have motion of the tracer.

In a double-porosity system in which we have matrix diffusion, the tracer in addition to moving down the fracture can diffuse out into the matrix. There's no flow in the matrix, just the opportunity for diffusion into it. Because we are losing tracer from the active flow path from the fracture into the matrix, there is a net loss from the fracture which means that in a given period of time the material will have moved less far down the main flow path. This is a diffusion process. So, we are dealing with the diffusivity of the solute. For the first time, the actual chemical diffusivity, the properties of that solute, are going to be important. Now, if in addition to diffusion, we have sorption onto the matrix or into the fracture, that will further retard movement downstream here.

So, simple single-porosity system, advection dispersion where all we can do is take some credit for dilution caused by dispersion. We introduce a double-porosity system. We have less movement because of diffusion. Finally, if we introduce sorption, we have yet again less movement.

How does this look? How would we see this in a field tracer test? Let' me show you what we call a breakthrough curve from a pulse injection of a tracer. This is made up. This is sitting back in our offices coming up with very conceptual predictions. This is not real data at this point. What we're plotting here is concentration of a tracer versus time. In a single-porosity system, a conservative non sorping solute would see a breakthrough curve which rises and comes down. That spreading is due to dispersion, hydrodynamic dispersion. It doesn't matter what the diffusivity of that material is because there's no diffusion taking place in this model. In a sorping situation because of absorption onto a material, that peak is delayed and it's lower in magnitude because of the retardation of the material.

Now, what if we go to a double porosity system? We're changing our conceptual model here to one in which we have flow along the fracture and diffusion into the matrix. What this does is here is a conservative tracer with a low diffusivity, a conservative tracer with a high diffusivity. For the first time, we're separating out these two tracers on the basis of their diffusivity. Again, in a single-porosity system, they would be perfectly superimposed; this new conceptual system, we're seeing a separation. The low-diffusivity material does not diffuse into the matrix as much. So, we see a higher peak. Eventually, at long times, these two cross for balance. If we introduce sorption in the matrix, just like sorption on the last slide, the sorption will decrease the peak and retard the peak. Finally, if we have sorption in the fracture also in the active flow path, that results in major amounts of retardation of the peak--this is eventually going to peak way out here somewhere--and decrease in concentration. So, this is the conceptual model we're looking at and this is sort of an idea of how we're going to get at it.

So, what's our strategy? The first part of our strategy is the laboratory sorption studies which have been going on at Los Alamos for a number of years and are continuing both with radionuclides and with reactive tracers that are permissible for us to use at the C-Wells. Our first step out in the field was to conduct a conservative tracer pilot test; a single conservative tracer similar to what the USGS did. This was to give us some idea of how the field site was operating, how and when to expect breakthrough, enable us to plan, enable us to determine what concentrations we needed to inject.

Then, for our major test, we introduced multiple tracers; a soup of tracers mixed together. We've got PFBA which is a conservative tracer similar to the fluorinated benzoic acid that M.J. talked about. This is dissolved, low diffusivity, no sorption. Lithium bromide, we introduced as a salt; they dissolve in water and disassociate. So, we actually have two tracers in that lithium bromide. The bromide is also non sorping, a conservative tracer, but has a higher diffusivity than PFBA. Lithium has a low degree of sorption. It does sorp onto the rocks. And, finally, we introduced microspheres which are colloidal particles fluorescently dyed to give us some idea of how colloids would move through the system and also to see how very, very low diffusivity materials work. We introduced this whole soup simultaneously so that they are moving through, they see the same system, they see the same conditions, and then by comparing the breakthrough curves of these different tracers, that's how we're going to do our interpretation.

First, let me show you the results of our pilot test here. Here, these dots represent the observations. Again, we're looking at a breakthrough curve of concentration versus time. We see our peak arrival here at about 240 hours, about 10 days after injection. One thing that's important here and M.J. was talking about this, too, in his presentation, is that with a single tracer, a single breakthrough curve like this, it's very difficult or impossible to uniquely solve the transport parameters and tell you what exactly is going on. As an example here, we've tried to fit different models to this with varying amounts of matrix diffusion from no matrix diffusion whatsoever with this purple curve to a very high degree of matrix diffusion in the red curve and all of them fit pretty darn well. With this one tracer, this one test, it's impossible for us to say, yes, matrix diffusion is occurring; no, it isn't; and, if so, what degree.

We then moved on to our reactive tracer test with our soup of tracers and here is breakthrough curve for that soup of tracers. The blue dots are PFBA, the low diffusivity conservative tracer; bromide, a higher diffusivity conservative tracer; lithium, the sorping tracer; and, the microspheres. We do see a peak here at 240 hours corresponding to our pilot test and that's the peak we expected to see. But, we see a very large peak early-on.

This is a little difficult to look at. There's a lot going on on this graph. So, just to make it a little easier to see, I'm going to switch to a log/log axis. This is the exact same data as we just looked at on a log/log scale. A couple of things we can see here. First of all, here's that 240 hour peak which we expected from the pilot test. Here's an earlier peak. We'll get to that in a few seconds here. I'll explain why we see that. We see the PFBA and the bromide which are both conservative tracers. In a single-porosity simple system, those curves would lay on top of each other perfectly. Instead, we see exactly what the double porosity model predicts. The low diffusivity tracer, the PFBA, has a higher concentration and eventually at long time intervals it crosses the breakthrough curve for the higher diffusivity bromide material. Lithium sorping material, we see lower concentrations again just as our conceptual pictures predicted.

Microspheres, we're not going to talk about a lot because we're still analyzing this data, but a couple of things we want to point out here is we see the same two peak behavior indicating that we do have successful breakthrough. We could see it. We could do a colloid transport test over a 30 meter scale. However, the concentrations that we observe are much, much lower than of the conservative tracers in the solutes suggesting that there is some sort of trapping or filtering mechanism that's preventing most of the spheres from coming through.

The pilot injection was a very small volume injection. The injected slug of tracer was relatively dense and here in the injection borehole, it sunk down to the bottom and it flowed out through these pathways, fracture pathways, down near the bottom of the borehole. In October, we injected a much larger volume of tracer solution, filled up the entire borehole, filled it up three times over actually, which would have activated pathways up and down the entire length including some very large important pathways that we knew about, some large fractures up near the top which we knew about from flow logs and televiewers. So, we have activated more pathways with the larger volume injection.

In fact, although we see two humps on those breakthrough curves, when we went back to model the data and fit the data, we found that the most reasonable way of fitting it, the simplest way of fitting it well involves three pathways. What we're looking at here is these red triangles are observed data. It's not all of the data. We picked out enough to define the curve and make the graph look nice here. And, we're fitting it with a combination of three pathways which are shown here and the sum of those three pathways is what we're seeing at the pumping well, C-3. The sum of our model pathways is this heavy orange line and, as you can see, we can fit the observed data very well here for the PFBA. We did the same thing with bromide, a three pathway fit and lithium, the sorped tracer. So, we can fit all three of the solute tracers with this three pathway fit.

DOMENICO: What's a three pathway fit? What does that mean?

TURIN: What we're looking at is we're looking at a sum of three pathways where we've got three pathways from the injection well to the pumping well with somewhat different transport parameters, somewhat different travel times. When we saw the double-pump, it was obvious that there is no simple single pathway breakthrough that's going to match that. We have to have a sum of two breakthrough curves, and it turns out to fit the data, it actually takes three here.

In just a second here, I'll get to what those three pathways are telling us.

COHON: You'll have to do it very quickly, though.

TURIN: Okay. So, we've got these three pathways now. We're not randomly changing parameters all over the place to match different pathways from different solutes. It's important for you to realize that by having the three solutes in combination, that adds a lot of constraint to the system and we are solving for flow and transport parameters for these three solutes simultaneously. The same three pathways are giving us those fits for the three different solutes.

This is not real important as far as the numbers here. Again, this is somewhat preliminary analysis. I wouldn't be surprised if some of these numbers changed very slightly as we refine our work here. But, there's a couple of things. We see the three pathways. Just to give you an example to how to read this; 68% of the injected mass is going down Pathway 3 while only 8% is going down Pathway 1. They have different travel times. We have very past pathway which gave us that early breakthrough on the long travel time, Pathway 3 which gave us that second hump that we saw in all breakthrough curves. Different dispersivities, this is the Peclet number that M.J. talked about. This is important. All three pathways show a positive alpha which is our matrix diffusion term. So, we are seeing matrix diffusion in all three pathways. We're seeing sorption. This area in the green, this is the sorption patterns which only apply to the lithium breakthrough. So, we are seeing matrix sorption in all of the pathways and we're seeing fracture sorption in two of the pathways. The matrix sorption comes out about the same in all of them which is reasonable considering a somewhat homogenous matrix while different fractures are going to have different fracture characteristics in terms of apertures, mineral coatings, things like that. So, seeing varying fracture sorption parameters doesn't bother me too much.

I mentioned that we are trying to establish whether our laboratory sorption measurements can be applied to the field work. We have looked at sorption of lithium onto the matrix of this material and actually a core taken from the wells in the laboratory. Our laboratory measurements came up with a retardation of the matrix of 3 to 6. In the field, we came up with an sorption of 5. So, it falls right in the laboratory estimates which gives us a good feeling about the applicability of the laboratory measurements.

Just to summarize here, this is the three solute breakthrough curves with the three fit model in black lines, and you can see we've done a good job here of fitting the black line corresponding to the PFBA. It actually lies below the dots the whole way and that's why you don't see it.

So, what have we got, so far? Preliminary results from the field work indicate that we see multiple pathways with these different characteristics, but all of them demonstrate matrix diffusion. We also see sorption of lithium to an extent that it agrees very well our laboratory predictions based on laboratory measurements. And, we have gotten some dispersivity estimates which will come in very important for long distance transport modeling which Bruce will talk about next.

So, what are the conclusions of our work, so far? Well, we believe we've demonstrated that our saturated zone conceptual model, this dual-porosity matrix diffusion sorption model, is valid at least for the Bull Frog Tuff at this location at C-Wells. Furthermore, we've demonstrated that our laboratory measurements sorption of lithium can be used to predict field transport of lithium. And, finally, most importantly, that in this experience at this site we've demonstrated that matrix diffusion and sorption are effective retardation and dilution mechanisms.

Where are we going to go from here? As I said, we've done this for the lower Bull Frog at the C-Wells. We need to look at some of the other formations to see how universal these results are. As M.J. said, we hope to move up to the Prow Pass at the C-Wells and possibly down to the Tram. Also, we obviously would like to look at other locations to get more confidence in the universality of these results. Our laboratory work is continuing. We're looking to find other environmentally and regulatorily acceptable reactive tracers that we can use in the field. Lithium has one particular sorption mechanism. There are other relevant sorption mechanisms and we're working on that. And, finally, we're integrating these results into our ongoing modeling and predictive effort which Bruce Robinson will be talking about shortly.

Thank you very much.

COHON: Thank you.

Time for just a couple of brief questions?

SAGUES: Sagues, Board designee. In this presentation and in the previous one, there is an--process on the models you use to interpret the results from the measurements. But, the question that I have is how do these numbers compare with what you would have expected from this particular site and whatever previously that you evidenced existed? Is this at least the transport mechanism--are the transport phenomena faster than what you would have expected for this kind of geographic location, slower?

TURIN: Basically, our way of predicting the transport of the sorping material such as lithium is based on our laboratory experiments, and the retardation relative to the conservative tracers is just about exactly in the range of what we expected. So, our observed lithium transport was just about what we would have predicted based on previous studies.

As far as the actual transport time, that is--especially, on a relatively small scale like this in a fractured medium, that's going to depend to some extent on what actual fractures your boreholes tend to intercept and what they miss. As you saw based on the pilot test, we got a 240 hour peak time. When we filled up the borehole more, we got an initial peak that we didn't see before which came through at about 20 hours. So, there are going to be some surprises on this small scale, and that's why we feel that we need to go out to the field then and look at these things.

LANGMUIR: Jake, you touched on this that additional work is going to be done on other kinds of associated mechanisms. I'm concerned because of the fact that we're not allowed to use radioactive elements down there which are the things we're really concerned about. I think, for example, the lithium versus neptunium, lithium presumably is going to be adsorbed chiefly by a--

TURIN: Exactly.

LANGMUIR: --perhaps by zeolites. Neptunium may be specifically adsorbed by trace sites on other minerals. So, that by using a reactive tracer like lithium, you have not really addressed what's going to happen to something important to us like neptunium. Without using neptunium, you probably can. So, you've got a constraint here, inherent constraint, and how well you can understand neptunium moving in the saturated zone; I guess, is my concern.

TURIN: I think there will certainly always be some uncertainty because we can't--I mean, we wouldn't want to inject neptunium down into the groundwater. Lithium does sorp primarily by ion exchange. In this particular interval, there are not zeolites to speak of. Some of the radionuclides of potential concern, strontium and cesium, are going to have a similar mechanism and perhaps the lithium data is more directly applicable to that. Neptunium, I think, Bruce is going to talk about this a little bit, but depending on the EH/PH conditions in the neptunyl species, we believe that that's primarily an ion exchange sorption mechanism. Some of the other radionuclides that do sorp by other mechanisms--for instance, surface complexation, that's a different mechanism. We are working on trying to find tracers that also undergo surface complexation adsorption and would like to find some that we can use in the field. We're looking at some of the rare earths, for instance. It's tricky because we need to find a tracer that sorps, but not too much because if it sorps at all strongly, the travel time through even the 30 meters at this C-Well site might be five or 10 or 15 years, which while very useful scientifically, I think it would be very difficult administratively to get a test like that.

LANGMUIR: I just want to point out your own work at the lab is showing adsorption of neptunium which is somewhat irreversible and increases as a function of time when you have things like carbonates involved. So, it's a very complicated business which you may not be able to really get a handle on too well with these tests.

TURIN: Yes, it is.

DOMENICO: You got a parameter, alpha, with units of 1 over a centimeter that's related to matrix diffusion. What is that?

TURIN: That is actually equal to the matrix porosity divided by a fracture aperture.

DOMENICO: Alpha is the matrix porosity?

TURIN: We made that up for this slide just in order to keep the slide simple. But, that's matrix porosity divided by actually a fracture half aperture.

DOMENICO: And, that's a measure of--it's a measure of diffusion?

TURIN: It is a measure of the availability of a matrix --if you've got something moving through the fracture--if your fracture is 20 feet wide, what's going on out there at the edges is just not that important. Well, in a very, very narrow fracture, you're very aware of the matrix throughout your flow system. So, it's inverse to the aperture and the matrix porosity, of course, is the available space that you're going to diffuse into.

DOMENICO: I would use a different symbol if I were you because I thought you were making up something analogous to the dispersivity for diffusion process which--

TURIN: Oh, no, that's a very--alpha was not a good choice.

DOMENICO: Yeah, it's not a good choice.

TURIN: Okay. That's a point well-taken.

KNOPMAN: Very quickly, did you construct confidence intervals about these parameter estimates? How well are they really--you're giving us single point estimates.

TURIN: Sure. Let's see, Paul is the one who actually is running the modeling work. So, I'd hate to answer something incorrectly for him.

KNOPMAN: Just order of magnitude?

TURIN: My feeling at this time is that these are--you can see the variation between the different pathways and those give very different curves. You can see it in your handout there. So, clearly, if you change them that much, you're going to be moving far away. I don't think that at this point he's done a formal sensitivity analysis or something like that. Although, I'm sure he can and will.

KNOPMAN: But, just the variance in the estimates?

TURIN: I wouldn't want to guess without Paul.

COHON: Thank you, Dr. Turin.

Our next speaker is Bruce Robinson. Dr. Robinson is the Principal Investigator of the Retardation and Sensitivity Analysis Program at Los Alamos. We're pleased to have you here, Dr. Robinson. Your new time target by that clock is 10 of. If you can aim for that please, I'd appreciate it. We want to leave as much time as possible for questions and answers with the presentations especially.

ROBINSON: Sure.

The ultimate goal of any site characterization activity at Yucca Mountain is to make predictions of the possible flow and transport of radionuclides from the repository to the accessible environment. This includes many aspects of the system; two of which I will talk about briefly in this presentation. I'll talk about flow and transport of radionuclides in the unsaturated zone and flow and transport in the saturated zone. So, there's unsaturated zone and saturated zone aspects of the talk that I'll be giving today.

I'm going to touch very briefly on some of the data sources that are used in order to essentially apply the parameter values and boundary conditions that one uses in large scale flow and transport models. One of the principal unsaturated zone data sets are the ambient system characterization that's been going on in the unsaturated zone for several years now. I'll briefly touch on the sort of data that we've put into these models. Realize, however, that there's much more to transport of radionuclides than simply the flow of water, although it is a very important component of a prediction of transport. I'll present some unsaturated zone models of radionuclide migration from the repository to the water table. Then, I'll move to the saturated zone and show how we're attempting to incorporate data such as the data that you've seen this morning at the C-Wells site. How we incorporate these into models of transport in the saturated zone.

I'll put the two together because, in reality, at the accessible environment presumably in the saturated zone some distance from the repository, one needs to consider every step along the pathway from the repository to the accessible environment. So, what I'm going to do is take unsaturated zone transport results, feed them to a saturated zone model, and show how the predictions of radionuclide concentrations appear at the accessible environment in the saturated zone.

The first step of making predictions of radionuclide migration are to characterize the ambient system. It is the first step. It's not the only step that's required. But, the sort of data that the project has been collecting for on the order of 10 to 15 years now are both hydrologic and chemically based data sets. We have fluid saturation and moisture tension data in wells. The USGS has been collecting these sorts of data. Modeling has gone on of the ambient hydraulic system in order to explain these sorts of data. I'm not going to get into that sort of modeling result. I'm going to move quite quickly into the transport of radionuclides. But, I'm trying to set the stage and show how different aspects of the data that are collected at the site are used in these models.

Infiltration and deep percolation of fluid obviously is a very important for radionuclide migration. There are surface-based infiltration estimates. There are estimates of percolation in the deep subsurface and also proposals that we'll talk about later in the day, I believe, on how to get a more direct measurement of percolation rate at the repository horizon. We also have chemically based data sets that in my mind are just as relevant as hydrological based measurements for the simple reason that it is a solute now that you are attempting to explain in the form of, say, an environmental isotope measurement or the water chemistry of the unsaturated zone fluid. These are solute transport, natural analog experiments. Our models need to explain those data in order to be credible models.

So, in the unsaturated zone hydrologic studies, those sorts of studies define the system in terms of its percolation flux, hydrologic property values, permeabilities of fractures and matrix interaction between the fractures and matrix, stratigraphy, and faulting. These enter into this sort of building of a numerical model for transport. This information is then used in radionuclide transport studies which I will move on to in the next slide.

After making measurements in the ambient system, we have to realize we are putting radionuclides which give off decayed heat and impact the hydrology of the system in a way that we are attempting to measure experimentally. You'll hear some of that experimental thermal testing that's going on within the project. There's also elements of the program that are specific to radionuclides themselves; sorption, solubility that enter into predictions of unsaturated zone transport.

What we do is basically the first step is to determine which radionuclides are expected to be the important ones from the standpoint of performance. Radionuclides with short half lives, highly sorptive radionuclides, almost get pushed to the side right at the start because of their lack of ability to migrate in the Yucca Mountain system. And, what you're left with are those radionuclides that either are low sorping or even exhibit very little or no sorption in the Yucca Mountain fluids and don the Yucca Mountain rocks. But, an important point here is that the sorption and solubility of these radionuclides in the Yucca Mountain fluids and on the Yucca Mountain rocks allows you to winnow down to a few key radionuclides and the screening that's done to determine those radionuclides is an important part of making performance predictions. When you get to radionuclides that are low sorping or have no sorption, it's important to measure for the low sorping species sorption coefficients and also solubility data because the solubility will govern the rate at which radionuclides can escape the near field environment.

This is laboratory data. We also have transport tests at various scales including laboratory and in the case of the saturated zone at the field-scale. We have both diffusion and column transport tests that we try to provide a level of defensibility beyond just saying I have a batched sorption measurement. This can be plugged right into a transport model. This is proven at least in the laboratory scale for the key radionuclides. Finally, the flow and transport model predictions that come out of the large-scale simulations are the result of pulling that information all together into performance models.

The rest of my talk is going to focus primarily on one radionuclide, neptunium 237. The sort of data that's collected for the key radionuclides in terms of sorption is summarized on this slide. This is a distribution of Kd or distribution coefficients for sorption of neptunium on zeolitic tuffs. The average is about 2-1/2 and the range is from essentially zero to about 5 or 6 on zeolitic tuffs. Neptunium itself does not seem to sorp strongly to other strata that don't contain significant zeolite abundances; although, there are individual mineral studies that show some high sorption of neptunium when you put it in contact with the Yucca Mountain tuffs. The general rule seems to be that neptunium sorps to zeolites and very little else.

This is an unsaturated zone model prediction. What you're looking at is an east/west cross-section, a small part of the total model, but it's a part of the model that contains the repository. These colors are related to concentrations predicted for neptunium in the aqueous phase. So, initially, because of thermohydrologic effects, the drifts near the edges of the repository are predicted to re-wet after the thermohydrology has done its thing in the first thousand or so years. The edges re-wet first. We predict radionuclides to begin escaping from the edges of the repository first. By 10,000 years, there is a prediction of rather rapid movement through the Topopah Springs Tuff at this sort of infiltration rate to the Calico Hills. There's sorption at 50,000 years. The plume has basically reached the Calico Hills and underlying units, and the sorption of neptunium is highly constrained to those rock types that exhibit significant sorption. This is a adsorbed concentration at the same time as this aqueous concentration. These are the zeolitic units that are present within the model at this location. So, performance predictions of this sort are made in order to include many of the different processes that I've described already. Bottom line analyses are also important in this sort of an exercise.

This is an unsaturated zone result. What I'm plotting is the rate of neptunium arrival at the water table versus the time since waste emplacement for two different percolation fluxes. The first thing to notice on either of these curves--and, let's look at the 4 millimeter a year case--is the bimodal nature of these curves. Our models that we are currently using for flow and transport have flow fractured continuum, a matrix continuum, and an interchange parameter in between them. The early arriving radionuclides are due to flow primarily through the fractured continuum, although there are units in which matrix flow is predicted in either of these infiltration rates. There is a prediction of early arriving neptunium at the water table at either of these infiltration rates. The importance of this to peak dose depends on essentially the height of either one of these curves after it's been fed into a saturated zone model. That's the topic I'm going to get to next. But, these are essentially input radionuclide breakthrough curves, input conditions to a saturated zone transport model, and taking these results, convoluting them, and putting them into a saturated zone model is the next step in making a prediction at the accessible environment.

We've looked at all the different data sources that are available to constrain unsaturated zones flow and transport. One can do a similar thing for the saturated zone. There's water potential data in the saturated zone, as well as water chemistry and isotopic measurements of saturated zone fluids and major ion chemistry would set the initial condition for whatever sorption processes might occur in the saturated zone. We've heard a lot about the C-Wells transport testing. The way that these data are used in models and how they result in model predictions are basically in two ways. One way to think of them is that you're estimating transport parameters. I think a more important goal of field-scale studies is really to validate conceptual models; in this case, a conceptual model of transport that includes flow fractures and also interchange between the fractures and matrix. I think we saw quite strongly that using tracers of different chemical characteristics and diffusion characteristics allowed us to validate a transport conceptual model.

This is a model result that I'm going to use kind of as a conceptual guide in describing the processes that occur in the saturated zone. This is a saturated zone model of radionuclide movement from the repository area which sits up in this location of the model at the upper surface of this box. This is transport with the prevailing groundwater flow system. Several things occur when the radionuclides hit the water table. You've got essentially what would be an instantaneous dilution of a more concentrated radionuclide fluid when it hits the saturated zone. However, just the movement of radionuclide with the prevailing flow field will result in longitudinal dispersion which is the sort of parameter that we get out of testing at the C-Wells; albeit at a somewhat smaller scale. Three's also transverse dispersion both in the horizontal and vertical direction that would be predicted by almost any theory of transport in saturated systems.

What parameter values you use really impact the predictions. And so, one of the goals, aside from validating conceptual models, is to simply try to determine what parameter values to stick into these models. M.J. presented a plot that had essentially the same information that's contained here. What we're plotting is the longitudinal dispersivity, the mechanism of spreading, and hydrodynamic spreading of a solute as it travels with a mean flow field versus the length-scale. Length-scale is the flow path distance in this case. So, at C-Wells, it's 30 meters. The accessible environment from Yucca Mountain to the accessible environment might be 5 kilometers. It might be 25 kilometers. Therein lies a problem with taking values at one scale and using them at another scale. However, the field testing is a start toward being able to set parameters that we know already are important to the overall performance of the system; in this case, dispersivity.

The C-Wells tests are identified with these dots. The main point is that although the experience in both fractured and unfractured materials follows a general trend here and there's a lot of scatter, although that's the case, the C-Wells do fall in the range of dispersivity values that have been measured when presented in a correlation such as this as a function of scale.

We're up here, say, at 25 kilometers. We need to predict or select a dispersion coefficient that's suitable for a 25 kilometer calculation, and this is the band of dispersivity values that we've selected in saturated zone transport tests based on correlations like this. The nice part is that the C-Wells experiments which is the actual site or something very close to the site as opposed to--I don't know--Timbuktu somewhere for some of these data points, the nice thing is that the C-Wells tests do fall in the range, and so it gives us increased confidence in being able to use correlations like this to set our dispersivity values.

The other aspect of taking field results and using them in models is to validate what conceptual model is the best to use in the field-scale results. What I'm showing here is the result of taking different conceptual models of the saturated zone flow and transport system and what effect it might have on performance. Let me explain the plot, first of all. This is neptunium concentration at a 25 kilometer distance with the exception of one of the curves that I'll get to in a minute. It's the concentration of neptunium versus time. What we're doing is we're taking results from the unsaturated zone, feeding them into a saturated zone transport model, and making predictions of concentration and the time variability of that concentration. What I've got are different conceptual models for how the saturated zone behaves in terms of transport and the predicted concentration of neptunium that results from that.

Dilution only says that if the radionuclides come down to the water table, they immediately get mixed in the saturated zone and the concentration right there under the repository is what's plotted with the blue curve. Only dilution; there are no dispersion effects because the mass of tracer has not--of radionuclide, excuse me, has not transported any distance, at all. It's right under the repository. Adding dispersion and taking it out to a 25 kilometer distance results in a predicted concentration versus time at that 25 kilometer distance that's lowered. And, this is essentially a measure of how much the dispersion alone, the dispersion of the system, might impact the concentration. So, dilution only, dilution and dispersion. If you have dilution, dispersion and matrix diffusion, what ends up happening is that the saturated zone itself instead of having a travel time of, say, a few years to the accessible environment which is what you get if you assume pure fracture flow and transport, you get travel times on the order of 5,000 years. Is this important in the grand scheme of things? Well, it might depend and certainly will depend on what regulatory time frame that you're looking at. If you're talking about a million year criterion, 5,000 years of travel in the saturated zone is not going to make any difference. But, what it does do, if you assume the matrix diffusion--and I think we have good evidence to do that at this point based on the C-Wells data--it tends to negate any very rapid travel times that might be predicted from these models. The unsaturated system, even if it transmits some portion of the radionuclides very quickly to the water table, will still--the saturated system will still result in predictions of breakthrough times in excess of, say, 1,000 years. If the peak dose is what's important to this calculation, then we've shown that the addition of matrix diffusion alone will have very little impact on the peak dose.

When you include sorption in the saturated zone, the situation changes. This is a prediction that includes all of the processes. So, the main comparison to make here is a no sorption versus sorption with a Kd of 1 which is in the range of values that we predict for neptunium. Travel times are increased when you include sorption in the saturated zone. And, again, if we're talking about a short regulatory time period, travel times might increase enough to where you would predict very little or no radionuclide, in this case neptunium, reaching the accessible environment.

The other point is that sorption tends to lower the peak concentration of sorption in the saturated zone, especially if the peak is due to rapid movement in the unsaturated zone. What I'm saying is that for the part of the system in the unsaturated zone that we have great uncertainty about the nature of fracture flow versus matrix flow, the saturated zone itself might provide a hedge against rapid movement through the unsaturated zone. So, it would be an additional barrier that would become important if models start to predict that the early moving radionuclide reaches the saturated zone within, say, 1,000 to 10,000 years. Sorption in the saturated zone becomes important in these calculations.

This is another plot in your packet. It just shows in the unsaturated zone we predicted the flux of neptunium that reached the water table versus time. When you feed that to a saturated zone calculation, you get results that are highly dependent on the assumed value for the percolation flux in the unsaturated zone. So, that result translates even if you have a saturated zone. Percolation flux again is quite important to these calculations.

COHON: Excuse me, that last one was including dilution, dispersion, and matrix diffusion?

ROBINSON: Yes.

COHON: Is that right, no sorption?

ROBINSON: No.

COHON: Okay.

ROBINSON: That's right.

COHON: Okay, sorry.

ROBINSON: Just to make that crystal clear, it would be the third--it would be actually the green curve here.

No matter how many experiments we do in the saturated zone, we're still going to have uncertainty on certain parameters, percolation flux. Well, the same goes in the saturated zone in terms of important parameters such as the dispersivity. This is a calculation, set of calculations that predict neptunium concentration at the accessible environment--a 25 kilometer distance assumed in this case--versus time for different dispersivities. Higher dispersivity means more mixing both longitudinally and transverse. It results in lower predicted concentrations at the accessible environment. The same is true for sorption. I want to remake the point that you have similar sorts of effects occurring via sorption in the saturated zone as occur for dispersion. The magnitude of these effects are similar in size even for sorption coefficients that are quite low. Kd of 1 were used for this calculation.

This is a slide that tries to get at what sorts of testing is required to further constrain these models. We saw saturated zone transport tests played a key role in building saturated zone models that have valid parameters and valid conceptual models underlying them. We feel that in the unsaturated zone, a validation of tests of that kind would be quite important, as well. And, in fact, we're designing tests that attempt to get at certain parameters of the system. One is the fracture matrix interaction parameters for transport. We know that they are important for flow. They're also important for transport. Whether or not radionuclide stays in fractures versus if it goes into the matrix and sorps is a very key part of the predictions that I've rolled up in the simulations.

This is interaction parameters that we want to get a handle of experimentally in the unsaturated zone. We'd also like to have increased confidence in the validity of sorption data for radionuclides similar to what we did in the saturated zone. We feel that the same process could go on in unsaturated zone and we're planning tests to do that.

There are saturated zone parameters, as well, that we could test. With further tests, we could narrow the bounds on those parameters, as well. The scale dependence of dispersivity in the saturated zone is one parameter and also further validation of dual-porosity models of flow and transport in the sorts of tests that both M.J. and Jake described to you for the saturated zone.

So, to conclude, I didn't talk much about dose. I talked in terms of concentrations. But, concentrations should be related to doses. And so, therefore, the peak dose down the line should be controlled--may be controlled by that portion of the radionuclide inventory that travels through fractures in the unsaturated zone.

A point that I didn't bring out is that in all these calculations, we're only predicting as a small fraction of the total radionuclide inventory actually traveling through fractures. But, since the travel times are so fast, that gives rise to high concentrations. And so, even a small fraction of the radionuclide traveling through fractures has important performance implications down the line. The saturated zone modeling showed that dispersion and sorption could mitigate the negative impact of that fracture flow in the unsaturated zone that I just described.

The saturated zone field testing that you've heard about this morning has provided us with important information to constrain conceptual models and also to actually set parameter values in the large-scale saturated zone radionuclide predictions.

Thank you.

COHON: Thank you. Thank you for hitting your target.

Questions?

CORDING: In the unsaturated zone model, you're talking about a validation test. How would you do that?

ROBINSON: There are proposals for how those tests would be performed that could be provided to you to give you the detail. But, in general, that sort of test needs to be performed by essentially injecting at a low flow rate in a series of boreholes, wetting up the rock mass which is unsaturated not to the point where each is saturated conditions, but conditions that perhaps are high enough to induce fracture flow, for example. At the bottom of this test block, one would apply a suction basically to extract fluid or there are other techniques that are coming on line for actually make those measurements at the bottom. But, basically, running a vertical flow and transport test while taking care to keep the conditions as unsaturated as you can and still be able to pull off a test in a reasonable time period.

CORDING: I think also that looking at the distribution of faults or fracture zones, particularly as one gets down into the nonwelded materials, they would tend to be areas which are much--you know, the fractures are much less frequent than at the repository level. Now, I'm interested in how you're bounding the fracture matrix--relative amount of flow in fracture matrix in that type of situation. You're saying a relatively small amount is going through the fractures, and I could see in different parts of the repository in fracture zones or fault zones a much different picture, particularly in that lower level of Calico Hills than it may be in other parts.

ROBINSON: That is true. And, furthermore, although we have a fairly good handle on what's occurring from the surface to the ESF because we do have this wonderful hole in the ground that allows us to collect data that's relevant to the surface hydrologic conditions, we don't have similar information in the units below the repository. So, that is a problem. The way we try to bound this is by essentially sensitivity analyses, but I think that those really need to be backed up with transport testing to try to narrow the bounds because with the degree of uncertainty in the fracture and matrix coupling in unsaturated flow that we have right now, that has important implications for performance. The direction in which that goes is rather obvious. More fracture flow means worse performance, in general terms, though I don't want to make sweeping statements. That's a general statement. Getting a better handle on those parameters through field testing would be a very important thing to do.

COHON: Don and Pat both have questions. But, if you don't mind, colleagues, I'd like to interject a question now because it fits so well and Dennis Williams would be disappointed if we didn't ask this and this goes to the east/west crossing. It's clear that the information you're talking about is crucial; the information you were just talking about with Dr. Cording. And, the results of your models and their sensitivity to these parameters underscore the importance of that information. Don't we want this information in that piece of rock which is designated to be the repository block rather than from some other piece of rock? Dennis, do you want to respond to that? And, if not, why not? I mean, what's the rationale?

WILLIAMS: Dennis Williams, DOE. The area that I believe that we are talking about is the rock mass below the repository horizon. A lot of this pairs on what goes on down in the Calico Hills. At one point in time, there was plans for significant excavations in the Calico Hills. That didn't come to pass. One of the things that we've talked about in the past and have it on plan is what we consider to be a surrogate to the Calico Hills testing which falls under the category--and I always have trouble with this term--demonstration of applicable--what is it? Gilles, help me out. Anyway, there's a specific test that is described and what we have done is we put in our long-range plan--we've got in the '97 plan. We basically have said to the PI on this which is Gilles Bussod to plan the best place that you can field this test that is not in that area that we do not have access to. So, one of the things that we have looked at in surrogates is the PTn. We've also considered--which is over on the test site and we've also considered Calico Hills outcrops in the vicinity of Yucca Mountain. Again, not below the repository, not--you could ask questions about its representativeness, but as a surrogate for--you know, as a surrogate location for this kind of testing.

BUSSOD: Gilles Bussod, PI for the demonstration of applicability of laboratory tests to field. Basically, that's the long title. There are two levels we have to look at this. Obviously, the most favorable condition would be to be in the Calico Hills to describe specifically the heterogeneities that are there, et cetera. However, we can actually do the validation testing without necessarily going into the Calico Hills recognizing from our descriptions and characterizations that the important attributes here are both the mineralogy--that is, you need tuffs that contain zeolites; that's what we are interested in--and also the heterogeneities. And, we have several areas that in terms of origin, volcanic origin, and heterogeneity, mineralogy, and structure represent layered volcanics that are very similar to the Calico Hills. From these units, we could actually test what we are calling our conceptual model in an unsaturated zone made of tuffs, similar mineralogy porosity as the Calico Hills units, interbedded, and with heterogeneity such as fractures. That would represent a major step in validating and bounding transport parameters that Bruce was talking about. And, those are our options right now.

WILLIAMS: Again, for where it sits in the plan, we are planning those tests in the '97 program. And, we've got several million dollars in the outyears dedicated to this testing in the long range plan. But, just to make a comment on the east/west drift with regard to this, the way the east/west drift is laid out and where it's largely in the repository horizon of the middle lithophysal units, basically wouldn't do a whole lot to satisfy this question.

COHON: For this specific question?

WILLIAMS: For this specific question, yes.

COHON: But, let me just--just to try to bring closure to this phase of the issue of the east/west crossing, it seems to me that given what was just said that the best one could hope for in terms of current plans is some kind of contingent recommendation on the site. That is, we believe--I'm putting words in your mouth, not the Board--DOE believes that we've collected sufficient data buttressed by other forms of work to say that our understanding of this overall site, but not the repository block, is valid and that our models are valid. And, here's the contingent part. If the site itself, the repository block itself, has certain features or is within certain bounds, then this site is--to use your own word--viable, suitable, whatever for a repository. But, without actual data about the repository block itself or what's under the repository block itself, I don't know how you could do anything more than that kind of contingent recommendation.

You don't have to respond. I just wanted to make that point.

WILLIAMS: We'll probably get into a lot more on this this afternoon as we talk about what additional funding we've provided into the program and I'm sure the east/west drift will come up in those discussions, as well. My point on the east/west drift from the beginning and to this day has been what are the data needs? What can we expect to satisfy from a collection of data standpoint to reduce these uncertainties that we're talking about. That's the context that I would like to have the discussion in.

COHON: Don and then Pat?

LANGMUIR: Bruce, I know it's tough. Your overheads are not in numerical order. They are numbered, but not in order. Looking at #15 and 18, if you can find them, I was surprised--and, I think it's important that we focus on what you already discussed a little bit, the issue that I'm concerned about. Specifically, I think those of us who hadn't thought about the modeling implications of early arrivals looked at the Chlorine-36 data which I'm assuming this is based in large part on the idea that you've got early arrivals--is presumably based on the information coming from Chlorine-36 in the ESF. That we have some early Chlorine-36 stuff, some very young stuff.

ROBINSON: That gives rise to the sort of conceptual model that's embodied in these calculations, the dual permeability fracture matrix model, yes.

LANGMUIR: Okay. I'm guessing that the math is simple, but it isn't obvious. And, I'm not sure you can get a number. Maybe you can help me with a number. What percentage of the water in 1,000 years is going to go down those fast paths and what percentage is going through matrix? This kind of stuff. Maybe that's too specific a question. But, I'm surprised to see that if it's a small percentage of Chlorine-36 of fast stuff that, say, on the Overhead 15 which is arrival at water table--

ROBINSON: Right.

LANGMUIR: Your largest peak of all for an actinide is at 5,000 years. Which, by the way, I would assume you have to couch that; that's implying its failure of waste packages in 5,000 years which a lot of it is debate--

ROBINSON: That's an important part of this. That's why you don't take curves like this and directly do a back of the envelope calculation and do a dose because we're looking at the transport of a far-field system under an assumption that waste packages fail essentially. The role of the TSPA is to put together realistic models for that process and these processes to come up with a real prediction.

DOMENICO: Well, why do you label it since emplacement? It's not since emplacement; it's since breakdown of the waste package.

ROBINSON: Well, since emplacement, let me tell you what the assumptions are regarding the waste packages. It is that when drifts--the rock around drifts re-wets, then waste packages begin to fail. That occurs in these models on the order of 1,000 years or so.

LANGMUIR: Then, is it complete failure of all packages in 1,000 years? How are you--what assumptions underlie this?

ROBINSON: That is time dependent based on where you are in the repository. But, I'm throwing out a ball park type number.

LANGMUIR: The other assumption that's inherent in here that hasn't been mentioned, if you look at 18 versus 15, is that the first arrival of neptunium at the water table in 5,000 years is at the same time as the first arrival of neptunium at the accessible environment, 25 kilometers away. It's 5,000 years. The plot shows the same time as if there's no delay, whatsoever, in the saturated zone.

ROBINSON: That was the point that when I brought in the different mechanisms that we have studied in the saturated zone and now appear to hold--that's the difference between a immediate breakthrough--in other words, there is no delay in the saturated zone--versus delay in the saturated zone which draws it out several thousand years.

LANGMUIR: Okay. But, why do we have two peaks? Why do we have a 5,000 year peak and then a 100,000 year peak; two separate peaks?

ROBINSON: Very simply, the models have fracture flow in which we assume no sorption occurs and travel times are fast. If neptunium gets into the matrix of the zeolitic units, it has a Kd of about 2-1/2 in these calculations. That's an order of magnitude delay time. So, you're seeing on the order of an order of magnitude time difference between the first peak and the second peak in this calculation.

LANGMUIR: It's like a chromatograph in a way?

ROBINSON: Yes, uh-huh.

TURIN: Just one thing for Dr. Langmuir. You asked about the biggest peak arriving early-on and I think it's important to keep in mind that there's--we're putting this on log to log paper in order to be able to see it all, but if we--that first peak which looks as big as the second peak, in actual mass is very, very small and that's perhaps a perceptual distortion.

LANGMUIR: Okay. And, that's an important point--

ROBINSON: --5 to 10% in the first peak.

LANGMUIR: All right.

ROBINSON: The peak looks as big, but you have to integrate in log space, and if you do that, you'll convince yourself that most of the mass is coming out in this later peak. But, the peak dose down the line may, in fact, be controlled by the portion that's traveling in the UZ through fractures, the first peak. Okay? So, that's the distinction I'm making.

DOMENICO: Bruce, does this model include transverse dispersion? Do you have transverse dispersion in the model?

ROBINSON: In the saturated zone calculation that I showed you, there's horizontal and transverse dispersions. And, to anticipate your next question, the longitudinal dispersion I showed the numbers for. I chose 1/10 the longitudinal dispersion as the transverse.

DOMENICO: So, when you scaled up the sensitivity analysis, you scaled up the transverse dispersion?

ROBINSON: That's right.

DOMENICO: You know, the point here, this is fractured rock and there's been studies by the USGS at Idaho and these are not fractures; they're actually unconformities. They extend over the whole Columbia Plateau. People call them fractures, but they're not. And, the transverse dispersion is high. They figured it out to be--I don't believe it was this high, but they figured it out to be close to 100 meters. We did a very careful--one of my students did a very careful study in a chloride plume there that was 20 kilometers, 15 kilometers in length, and in a very careful study, inverse method, we figured it out to be 30 meters. We did the same thing at Hanford. We had the flow tops, a very careful inverse study, and it turned out to be 40 to 50 meters. That gives you a lot in terms of spreading. And, I was just wondering if--if we have horizontal fractures in this medium--vertical fractures aren't going to help you, at all, with that. But, if you have horizontal fractures, the transverse dispersion can be considerable. I don't think we're ever going to find that out because I don't know any way to test it. But, I think the models are very sensitive to a transverse dispersivity.

ROBINSON: They are.

DOMENICO: Because you're spreading the mass. You're not--

ROBINSON: That's right.

DOMENICO: Actually, they're more effective than longitudinal if you look at it.

ROBINSON: Yes, yes. I plot these in terms of the longitudinal dispersivity with a constant ratio. So, you can look at them. It's moving the decimal place over one and that's the transverse dispersivity. And, you're absolutely right. That is actually a more sensitive parameter than longitudinal.

DOMENICO: So, if you have horizontal fractures--so it can spread just like spreading, let's say, along those unconformities or flow tops, the transverse dispersivity should be higher than what is has been assumed. Now, if you had--if it's all vertical fractures, then horizontal spreading is nil. It's practically nil.

ROBINSON: That's a tough one. I mean, I think we're going to have to select values that bound things. The bounding in this case is going to have to be to assume lower dispersivities than might actually be the case given the mechanisms of the sort that you're describing here.

NELSON: Nelson, Board designee. I have a question relating to the--what are the most important features of the ambient system that you use as a starting point for the model and the uncertainties in that model that really affect your conclusions on unsaturated zone and flow and transport?

ROBINSON: Let's look at this one. Percolation flux, it's critical. One way to look at that is to say that mass reaches the water table more quickly. The correct way to look at it in my opinion is to say a higher percolation flux is in the unsaturated zone. The first peak is higher, basically. And so, it's not so much the delay time or the difference in travel time in one case and another; it's the fact that more mass of radionuclide would be predicted to go directly through fractures at higher percolation fluxes. So, percolation flux is one of the key parameters.

NELSON: Okay. Can you tell me some others? I'm concerned about the modification that's going to happen. You have a mountain that's there right now. You're going to put a repository in there. It's going to take a period of time to excavate. There may be some changes that occur in conjunction with it being opened for whatever period of time that is. Are there any of those condition changes that would have a strong effect on the starting point for your overall model relating to the unsaturated zone model?

ROBINSON: Yes, and in fact, some of those--I won't say it's exclusive, but some of those processes are included in model calculations like this. In other words, the thermohydrology that one predicts to occur as a function of the radioactive decay heat is included in models to the extent that we know those processes. One thing that--a lot of the bottom line performance of the unsaturated zone is governed by processes that are occurring some distance away from the repository up to 100 meters or more in the Calico Hills, as opposed to right where the most vigorous changes due to the waste heat will be occurring and whatever mechanical changes might occur just from emplacing drifts. Since the Topopah Springs is fractured and has a high fracture permeability and relatively low matrix permeability, transport times through that unit are quite short anyway. The primary barrier to radionuclide movement, what slows it down the most, is flow through the Calico Hills, both in the vitric and in the zeolitic Calico Hills and underlying zeolitized horizons. And, that's some distance away from the repository. So, that's sort of good news from the standpoint of your question.

LANGMUIR: Looking back at the--I realize that you did the log plots to help us out, but they're kind of scary if you forget that their log plots. We're talking about 5 or 10% early arrivals. Educate me a little bit, if you can, Bruce. I know it's not your expertise necessarily. If we're looking at--per liter neptunium at 5,000 years to the accessible environment, how does that relate to at least past doses that were a health hazard, were considered violations of the EPA standards?

ROBINSON: Given the caveat that I said about five minutes ago that one shouldn't take curves like this and do dose--

LANGMUIR: People are going to do it.

ROBINSON: Anybody got a calculator? No--

LANGMUIR: Has anybody thought about that in the group?

ROBINSON: I've determined a number for this peak right here. It's about 2 millirem per year.

LANGMUIR: The lower peak?

ROBINSON: Yeah. I believe that if one starts to consider the processes in totality including non-instantaneous failure of waste packages, throwing those processes in to the mix, also has a preferential impact on particularly this first peak. So, you've really got to put this into a total system performance context before really drawing the conclusions. I'm showing you, I hope, how the unsaturated zone system works for one set of releases from the near-field.

LANGMUIR: I guess another question for you. Is this plot the way that technetium would look? This is a non-adsorbing--is this basically the technetium plot if you were to draw it up?

ROBINSON: Technetium plot would have the second peak displaced in this direction which would tend to make it somewhat higher in relation to the first peak.

LANGMUIR: Okay. This is a no adsorption plot though, isn't it?

ROBINSON: This peak is no adsorption. This peak has fairly strong sorption of neptunium in the zeolitic rock. This peak is bypassing of those sorptive minerals in the unsaturated zone. So, technetium, first peak is the same, the second peak is in here more.

COHON: Thank you very much, Dr. Robinson.

Our next speaker and last one of the morning session is William Boyle. Dr. Boyle is team leader for Performance Confirmation in OCRWM, and he's speaking today on thermal and underground testing update. Dr. Boyle, your new target is 12:40 by the wall clock. I apologize for shaving a bit off your allotted time, but you realize, of course, you're standing between all these people and lunch.

BOYLE: Myself, too.

COHON: Yes, and yourself, right. Thank you.

BOYLE: I had a whole lot to begin with and I knew I had to go fast. So, that's just reinforced. If Ms. Devlin is here, the 1300 C--I'm not a metallurgist, but I don't know where that number came from. I think there are metallurgists in this audience and you ought to talk to them. We'd have a tough time getting our tests up to 1300 C. But, I'll come back to the temperatures.

You just heard from a series of PIs. I'm not a PI and I'm going to represent the work of many PIs. So, I'm going to go quickly and at a higher level than the talks you just heard.

One topic is, I think, why are we stuck, so to speak, with the TBM right now? We're right about here right now. All these lines on here, dashed or solid, these represent faults. We're having some difficult ground conditions right now. An interesting thing to see is we knew we had them in that ramp, we knew we had them in this ramp, and in the repository block life should be better. The repository block was partly chosen for that reason. And, here briefly, is a diagram of what went on; a sketch of a possible fault. It really doesn't matter if there's a fault there or not; it's whether it's a fracture zone or whatever.

Here's a vertical slice through it where we were in the middle mile lithophysal unit which has elsewhere been one of the more difficult rocks to tunnel in. This is a better rock to tunnel in, the upper lithophysal unit. They had some fallout, and if there's nothing for the grippers to react against, the machine doesn't go anywhere and they've got to do an awful lot of work to fill in these voids. This is a plan view of looking at it. They had some very substantial voids that they had to fill in with timber and shotcrete. The good news is the machine is largely--the grippers are in the upper lithophysal units. So, things should pick up.

There are some more faults to go through in the south ramp. As I showed, it's not just the fault that's a problem; it's the fault and the rock type that you're in. The--has been a tough rock to tunnel in elsewhere in the ESF. Faults that we're going to encounter, the rock types that are going to be there are better. So, perhaps, they won't be as much problem. We have experience with that even in the south ramp. They went straight through the Dune Wash Fault very easily because the rocks there were in better condition.

This is a historical diagram just for--I know there's some designees. This is from 1995. This alcove was never built and this one's now in #5 and I'm going to talk about #5, 6, and 7. There were once many alcoves planned, but leave a note on deferral. They may not get in.

This is something I hope you bring to the next couple meetings in October and the one in the summer. It's just a listing of schedule of the things we hope to accomplish in the ESF this year. And, also, the large block test is on here. We should turn on the heaters next month. Dr. Dyer mentioned this morning, we're going to finish up the excavation of the heated drift very soon. They're already to the maximum length. They just have to take out some of the floor and a bit of a bench. We're going to finish the first phase of testing in the North Ghost Dance Fault alcove.

There's so many phases in the North and South Ghost Dance Fault alcove. I hope to straighten some of that out.

So, I'll jump right into the purpose of testing in those two alcoves, 6 and 7. They were to determine the flow properties of the Ghost Dance Fault through pneumatic monitoring, pressure tests, gas chemistry, temperature. I have slides on all of these except gas chemistry. So, I'll tell you that result now. We have a single borehole through the fault right now in Alcove 6. They were able to get gas samples out of there. The air outside has a certain signal in terms of Carbon-14 and soil gas has another signal and what they see in the fault zone is a mixture of the two indicating that there is perhaps mixing in the fault; that air moves up and down the fault.

Here is a plan view looking down at the ESF. We're stuck right here now by these faults or we're getting ready to make good progress again. The Ghost Dance Fault, here it is. It actually stops right there. You know, depending on how you want to define things, you might argue that it stops right here. But, it's been connected with this strand of a fault and so it continues on and actually crosses the ESF here, but doesn't cross it up here. And, you can see these numbers here, 20 feet, 40 feet, 90 feet, 40 feet. That's the amount of displacement on the fault. You can see that it varies as you walk along the fault and what also varies, 78 degrees dip, 90 degrees dip, that's how steep the fault is. So, it's not a uniform structure that we have out there. This alcove is already a partly constructed and we have a single borehole in it that we've done tests in. More construction will take pace after we finish the test in the single borehole. This alcove is presently under construction and we don't have the single borehole in there.

Here's a view of the Ghost Dance Fault at the ground surface. This at the UZ-78 drill pad. This, if you will, is a fault zone and it's a poor quality slide, but you can actually tell, particularly if you're there in person, that the rocks on either side of the fault responded differently to the--I'll get back to that.

This is where that strand of the Ghost Dance Fault crosses in the ESF. If you blink, you'll miss it if you've ever been there. So, this is just to give you an idea that over not a very long length, this fault has not a very long length and not a very great depth. The fault is variable. Although we talk about the Ghost Dance Fault a lot, for people from California, it's not quite in the same league as the San Andreas Fault or faults like that.

There's an awful lot of information on this diagram. It actually tells a history of the excavation of the North Ghost Dance alcove, Alcove #6. Here's the ESF Main. The main things to get out of it are that the excavation is right here right now. There's the single borehole through the fault. Here's the fault from the ground surface. This is a blowup showing you, in general, the cartoon description of what the fault looks like. And, again, the deformation on either side of the fault, the hanging wall, if you will, a geologic term for--we'll just say the left side, this was more fractured than the right side.

And, this is not to scale. Right now, the excavation is only right up to about here. The single borehole pierces through the fault, and I'll show you some of the results from it. This is for Alcove 6. I'll essentially show you the same diagram because it's not to scale. It's for Alcove 7. It shows the borehole yet to be drilled. We'll do the same thing. We'll drill through the fault first with a single borehole. We're back here. As Dr. Dyer mentioned this morning, we're about halfway of this first phase. In both alcoves after we're done with the single borehole, we'll come back in and excavate out the rest of the road and then drill holes parallel and across the fault and do more tests.

Now, I'll actually show you some of the results from John Sass, I believe, of the United States Geological Survey. This is from Alcove 6, a single borehole. At first, they logged it on the 7th of November when they had a 60 meter hole. This is temperature in degrees C marked by tenths of a degree. There was this dip in temperature which at that point they thought that might be an indication of flow in the fault of some fluid that was, you know, carrying away the heat. When they came back roughly a month later, December after they had done some more excavation and now they only had a 30 meter hole, that pronounced dip was no longer there. So, now, one possible interpretation by the Geological Survey is that perhaps this dip just represented evaporation as part of the drilling process. But, they will continue to think about that and eventually come up with an answer.

They also mentioned they just monitored the pressure. You know, they had barometers in the hole, if you will, in different zones. For those of you who have color, how does the pressure front--you know, as the weather system goes by, how does it get into the fault zone? If the pressure front traveled through the ESF down the alcove and then horizontally right through the fault zone that way, these responses would be numbered 1, 2, 3, 4, 5, sequentially like that. Well, then, there's any number of explanations that might explain the response that they got that the pressure front goes down the fault or perhaps it goes partly through the ESF and then through a fracture zone into the fault and over. They are working on sorting this out. This is the work of Gary LeCain and, I think, Gary Patterson of the GS.

Here's some more information from their tests which this will relate to flows. The way I think of their measurements is they are getting at permeability, you know, that eventually might be able to give an idea of how much water goes by. This is their barometer in Alcove 6. SD-7 is near the southern end of the ESF Main, NRG-5 is near the north end. These are boreholes drilled from the surface with pressure gauges at depth at roughly the same depth as Alcove 6. And, essentially, the responses are the same which means that pressure front goes very quickly through that part of the mountain which covers the whole north/south stretch of the ESF Main. So, it appears to be very transmissive.

Now, I'll try and get to how much water goes through. This is the work of Zel Peterman and Jim Paces of the USGS. They're looking at opal and calcite. What they try and do is shave off very thin layers of these crystals that were deposited by water, date them, and from that, back out how much water had to be doing by to deposit that much opal or calcite in that period of time.

Here are some of their results. Given that Yucca Mountain has been there a long time, they have to use different age dating techniques. They use Carbon-14 for the younger, the most recent layers. They use thorium and uranium. They also use uranium and lead. But, their results indicate a protracted history of deposition extending to relatively recent time, but at very slow growth rates. They also state the fact that there are no major gaps in here indicates that the mountain at no time plugged up, so to speak. You know, that the water carried down minerals, plugged up fractures, and there was no more deposition. They just had this continuing through time, this very slow deposition.

I'll jump right to the bottom line. They're very honest in stating that whatever they estimate or, you know, base their conclusions on, are dependent on the hydrochemical evolution models. But, once you have those models and make some assumptions, you can convert the amount of secondary minerals that were deposited by water to an amount of water. So, in some ways, these are just secondary mineral abundance, but if you wanted through some manipulation you could convert this axis to amount of water that went by per unit time. Their results indicated point-wise maybe up to 20 millimeters a year, but spatially averaged it's a much smaller number. The interesting thing to see here is that in this area there was apparently more water flowing in the paths and much less along the ESF Main. There are any number of possible explanations. One is water is diverted above this or water never gets in in the first place. It doesn't rain there. I think that you may get some discussion about it this afternoon.

--what the Strontium-87 measurement is, but the main point is is this unit, the PTn, has a pronounced effect on Strontium-87 indicating has an effect on flow of water. This plot of the uranium, it's not as pronounced, but again the flow of water is affected by the geology. It's not just the USGS looking at this problem. These are results from June Fabryka-Martin from Los Alamos National Lab in the ESF. There are Chlorine-36 measurements. They did some systematic samples. I think, they're every 200 meters. They just go along and get a specimen and make their measurement of this ratio.

They also made measurements of these gray squares at specific features; fractures, whatever. At a first cut, a person could state that the highest readings are all associated with features. But, you could also say not all features have high readings. So, again, there's any possible number of explanations. These features might actually not be--in the sense that they have intrinsically low permeability. On the other hand, they actually might have intrinsically high permeability, but for some reason water never came in, infiltration at the surface is insufficient, or it's diverted.

Joe Wang of LBL has some measurements of what happens with water in the ESF. In the ESF, I think Nick Stellavato will--his talk this afternoon relates to this. This has to do with how much water the present ESF ventilation system can yank out in terms of flux. You can see, whatever numbers Bruce Robinson showed or whatever, the ventilation system can actually take it out. Whether it's at 100 millimeters a year, 200 millimeters a year, or 300 millimeters a year.

Also, what's plotted on here is the amount of water usage used which controls how much water they used in the ESF which controls how much water you can suck out of the rock. If the ventilation system is busy evaporating the construction water, it actually takes less out of the rock. And, Alan Flint also makes measurements with respect to this. I'll call it the movement of the drying front due to the ventilation into the rock, and I don't know if it moves fast or slow, but it takes months to move in a meter or so. I think, Alan is here and if people need a more precise number, he could give it.

Now, I'm switching over to the thermal tests; the single-heater test, large block tests, and we also have a drift-scale test. Large block test is at the ground surface. It's not in the ESF. Actually, all three tests, we're doing all of them to examine this; thermal, mechanical, hydrological, and chemical processes. The big advantage of the large block test is we have such good control over the boundary conditions. It's just an isolated block of rock. We know what the stresses are.

I believe that Ms. Devlin this morning brought up the bugs, if you will, in my terminology. We have added microbes of some sort to the large block test such that we know that there are bugs, that there's life out there underground in the large block test, but Livermore added some bugs that we know are ours and we also added coupons in the waste package material. I believe she brought that up. So, we'll be able to see what happens to both the bugs and the waste package material.

This is what the large block test looked like, I guess, a week or two ago. It's now completely wrapped all the way to the top and it's actually ready to go. We could turn the heaters on, but we're going to wait and get some more ambient temperature results of permeability first.

We also have the two heater tests in the ESF itself. The drift-scale test is still under construction. The single-heater test is already started.

This is just to give you an idea of where they're at. This is the ESF. You come out of the first turn and here's the test location. This is a plan view. Here's a cross-section. We stayed beneath this geological contact. Heated drift excavation has now reached its final ending point. We just have to remove a little bit of the floor and also a little bit of the bench.

I'll show you a cartoon-like illustration just to give you an idea. Here is the single-heater test. This is actually producing results. Finishing up excavation of this. This test is scheduled to start in December of this year. And, you can bring this to our next meeting. You see it has roughly a two-year heating period, a two-year cool down period, final report in 2002. It's a planned two-year hearing period, but we'll evaluate at the end of two years, have we accomplished what we wanted?

This is an old diagram, but it will work just to give you an idea. The heated drift will be, I think, 47-1/2 meters long. It's roughly five meters wide. All these colored holes are either heater holes or instrumentation holes for making measurements. There will be a bulkhead right here. So, before we turn the heaters on and the bulkhead has been up for a while, this will serve the purposes of sealed alcove and we will get a brief snapshot of what happens when you seal off that alcove. I think, you'll hear more this afternoon where specific alcoves will be excavated and sealed off. Roughly, 12,000 feet of instrumentation hole. That was a plan view; here's a cross-section to give you an idea of all the measurements.

Why the drift-scale test? This actually applies to all the tests, but this is the biggest test we're going to run. In some ways, it gets at the idea of an appropriate scale. To give you an idea, the single-heater test and the large block test will heat tens of cubic meters to 100 degrees C or more. This test may be 25 or 50,000 cubic meters to 100 degrees C or more. The repository, Tom Buscheck did a back of the envelope calculation for me last week, and he says 500 million cubic meters for the repository. So, it's tough to do a test at that scale.

Here's the single-heater test before it got wrapped in insulation. This is more what it looks like now. It looks like a big marshmallow. When you go down there it's like watching a marshmallow; nothing much happens, actually. You have to look at the results. Why the single-heater test? Again, the same technical reasons, but all these tests were part of our thermal test strategy at starting simpler, smaller, and shorter duration tests and then gradually getting more complex and bigger, longer. And, it also served a very useful purpose of shakedown of the measurements and also the organizations and individuals involved. This in some ways looks like a cartoon, but it's an accurate representation of the--if you will, the heater in red and the various instrumentation holes. These blue-purple ones are mainly for looking for water and the green ones are temperatures and the orange ones are either chemistry or mechanical.

Here's a plan view of a horizontal slice. Heater starts about two meters in and it's five meters long. It's a four kilowatt heater. We have instrumentation coming in from the sides and parallel to it. I have a vertical cross-section of it. In this case, the heater is running right there, Hole #1, in and out of the plane. Here's the various instrumentation. And, I'll show you some results from these four holes. Also, 16 and 18 and 15 and 17. This one was started on time through a very dedicated effort of a lot of groups on the project. It's going to heat for roughly nine months, cool for roughly nine months, and we'll get our final report planned in June of '98.

Results to date at a high level. Test is largely proceeding as planned. I'll actually show you some measurements. The measurements are the dots. We had some predictions beforehand. This flat spot for the high permeability model, you get the flat spot at the boiling point because there's a convective mechanism happening. As you can see in our measurement, we don't have that flat spot. We've got a little glitch here where the power went out. This is from a hole that's parallel to the heater, a foot away, and this is one of the 10 thermometers, if you will, that's in that hole roughly at the heater mid-length. So, what this shows is we don't--and, this is what all the PIs expected; for this scale-test and volume and time, we would not have convective response. That conduction only would be a good approximation for the temperatures.

There's another way. That was temperatures through time. Here's temperatures through space. Let's get back to Ms. Devlin's questions from this morning. These, we took some of the thermometers, if you will, that were near the heater mid-length whether they were above it, to the left, to the right, below, and just plotted them up as a function of radial distance. So, these are from throughout the rock mass and they define a reasonably good curve. I think, these are actually all from the same hole. So, either the material is different or the gauges are different or something are different.

But, what are the temperatures in the drift-scale test going to be? Russ Dyer asked that and I said that over 200, but I didn't say on the high side. Next to the wing heaters, the rock may get actually over 300 degrees. But, as you can see here, there's a very steep decline as a function of distance away from the wing heaters or any heater. As you get away from it, the temperature drops quickly. I've mentioned that we may have as much as 25 to 50,000 cubic meters and 100 degrees C or more. The volume of rock that will be 300 degrees C or more is trivial in comparison. We are heating our test to temperatures higher than the repository will see, but it's a very small amount of rock. The reason we're heating it up at greater temperature is to get the test done in a reasonable amount of time.

These are locations of the 300 plus or so temperature measurements in there. I'm going to show you some contours. Those are actually the only locations where we have knowledge. Contours can play tricks on you at times, but this is contours of the measurements on November 30, 1996. Heaters along here. For those of you who have readable copies, the interesting thing to do is just flip back and forth between the measurements and the predicted and you'll see that they're, in general--they look the same which is heartening.

We also with our contouring program can calculate the volume within a contour. And, on this date, the volume within the 100 degree isotherm was 16.6 cubic meters which is 16,000 plus liters of rock, if you will. And, if it's 10% porosity, that means we have 1600 liters of void space; and, if it's 90% saturated, we have 1500 liters of water that were just within the 100 degree isotherm and some of that water went somewhere.

COHON: Could you try to wrap up?

BOYLE: Yes.

These are ERT measurements that show--this shows where the water has gone. It's not radially symmetric indicating that there may be convective flow of water. This is a more detailed view of that. The legs again indicate we may have condensate shedding, perhaps; that's one explanation. We also use ground penetrating radar. The heaters here--and, the blue means drier; this color indicates that it's wetter indicating that water has moved. Our calculations showed that--these are from LBL--that ambient and as a result of heating that water would move away from the heater and start to fill up the fractures and raise the saturation.

And, we had one--we also had other evidence of water moving. Hole 16 produced 5-1/2 liters of water on a day when LBL went to open it and do a permeability test and the water drained out and the chemical results to date indicate that it was condensate. It's very clean. It's almost a distilled water. And, that also meets the expectations of the modelers that the heat would drive some of that 1500 liter water away and it would condense somewhere where it was cooler.

So, I wrapped it up quickly.

COHON: Thank you.

Questions?

LANGMUIR: I appreciate this is not your specialty, any particular question we might ask. But, I'm concerned and always have been that it will be extremely difficult, if not impossible, to do a heater test, even a drift-scale test, that will capture the most important processes that might affect repository performance in any kind of a time frame you could deal with. And, you've got a two-year heater test here for the drift-scale and the single block test, I guess, is a similar kind of thing in terms of the heating time scale.

BOYLE: Five months. It's even shorter.

LANGMUIR: Five months, okay; even shorter. And, you're talking about thermally influenced processes that you're trying to capture. And, these are evaporation, movement of fluids, condensation, and if you've got coupled processes that might affect performance, you're talking about precipitation of solids in a thermal gradient which can affect permeabilities and may seriously influence long-term isolation of the waste in one way or another. I don't see how we're going to capture this sort of thing, at all. You'll get rock mechanical properties, certainly. You'll get some bulk property changes. But, in terms of coupled processes, I can't see how we're going to find these out.

And, I also would question while I'm at it that we know how to measure them, that we have the instrumentation, and we could know how to operate it to evaluate these changes in rock properties.

BOYLE: Now, my way of describing what you just said is it's a very tough problem, and I would say that the thermal tests of PIs, that they're doing the best that they can under the circumstances. And, I agree with you that the real answer--I think, Bruce Robinson or Jake had brought it up--if you really want to know, you've got to go out and dump the radionuclides in and come back in 10,000 years or so. The same sort of thing applies at the repository scale. So, they're doing the best they can. They're looking at trying to get a handle on what phenomena do occur.

LANGMUIR: I think what realistically is likely to happen and would be very constructive would be to carry these tests on through the retrievability period so they're not two-year tests; they're 10, 20, 30, 50 year tests. I know this is not in anybody's plans yet, but this is the logical thing to do.

BOYLE: Oh, no, I've talked to Leon about this. The last fiscal year the first attempt at defining the performance confirmation program, that's the measurements that we would make starting now, but also continuing throughout the life of the repository; measurements of temperature, water, that sort of thing. The first report has been written that looks at, well, what are we going to need and there's an ongoing effort this year to flesh it out some more. So, there will be measurements throughout the life of the repository. And, that's where--I think we would agree on this--you're going to get a whole lot more information out of that because you've got a whole lot more time and you've got a whole lot more volume, and if you really want to know what the mountain is doing, that's the place to do it.

COHON: Thank you, Dr. Boyle. I'm sorry, one more moment. Ed?

CORDING: The estimate of the amount of water that the ventilation system is taking out is an average 300 millimeters?

BOYLE: Right. And--

CORDING: Or 200 millimeters or 100 millimeters. I guess, the question is has anybody produced some numbers that say what would the concentrated flow along a single fracture, for example, have to be in order for it to overwhelm the ventilation system? How much--

BOYLE: Right. How much water would have to be coming through a fracture for us to see--

CORDING: We know it's less than that because we don't see any water.

BOYLE: Yeah, right. Yeah, and the wettest fracture we did see in the ESF at 6,720 meters in, it did eventually dry out. So, whatever it had, that wasn't enough to produce, you know, a stead drip.

CORDING: Right.

BOYLE: I don't know if somebody has done that calculation. They would have--maybe Alan Flint or Joe Wang or somebody has, but I don't know of it.

CORDING: Certainly, as you seal off drifts, you're going to start seeing--you'll be able to pick up the effects of smaller flows, I would assume.

BOYLE: Well, you don't even have to seal off the drift. You know, going in the heated drift now, they have part of the fan line down because they're blasting. And, just with not getting the air circulating in, it's hot and humid. So, you don't even have to wait to seal it off. You know that water is coming in. The minute you turn the ventilation on.

COHON: We have a question from the audience. Please, identify yourself and maybe spell your name again?

SZYMANSKI: I just have a comment. My name is Szymanski, S-Z-Y-M-A-N-A-S-K-I.

COHON: Thank you.

SZYMANSKI: And, I would like to make a comment and I would like the presenter to put, if he can, two slides; #16 and 17 on the screen.

BOYLE: I didn't number them. So, tell me what they are and I'll--

SZYMANSKI: One pertains percolation--

BOYLE: How about that? Is that one of them?

SZYMANSKI: No.

BOYLE: This one?

SZYMANSKI: No.

BOYLE: This one, opal and calcite.

SZYMANSKI: Right. And, the following slide regarding the ages.

BOYLE: This one.

SZYMANSKI: Well, we probably all will agree that the entire--well, I would like to get back to this issue of suitability that you have mentioned which I would imagine is of paramount importance here. Entire syllogism which will pertain to the definition is related to the truthfulness of the statement--percolating water. Now, let's us imagine that that statement is not true. We have upward moving water, geothermal water. --suitability looks in that regard. In other words, it seems to me that the business of downward/ upward is a crossroad. In this regard, I would like to call the Board's attention to the work done in Russia using sand crystals. It was done by Nina Shugarova, Vedim Reutsky, and Yuri Dublyanski. The studies pertain to fluid inclusions. They consisted of homogenization temperatures which everybody would agree measure temperature of deposition of the mineral in question. We have examined chemical content of liquids incorporated in the inclusions. We have examined the gases which are contained in the inclusions. And, we also measure the pressures under which the inclusions are kept. Four tests. To repeat them, it would cost somewhere on the order of $100,000. The results: the gas consists of carbon dioxide--nitrogen, nil oxygen. It's not oxygen. Now, how the hell downward percolating water can bring gases and eliminating oxygen? It's very possible is that someone can come up with something.

Number two, the fluids which are contained in the inclusions have a content of total dissolved solids ranging from 100,000ppm to about 10,000ppm. Now, salts, magnesium chloride, sodium chloride, and so on, we have done pressure estimates. The gas and the fluid inclusions which they contain were entrapped by pressures which were significantly higher relative to being able to discriminate the atmospheric pressure. It can only be known in view of these ages under a column of water. Finally, we have measured homogenization temperatures. The results: 50% to 150% higher than ambient temperatures.

Therefore, it seems to me that since there is no doubt, scientific doubt, that this statement "downward percolating water" is at best an error. Therefore, the reason--question the entire DOE program strategy and so on. It is important. Why is it important? Well, we have to advise the President and we have to advise the Congress, we do have the data.

I thought I would make that comment. Thank you very much for the opportunity.

COHON: Thank you.

The last question, once again, belongs to Dan Langmuir.

LANGMUIR: This is such a privilege to have this opportunity to be the last question again. I was reminded of my own notes and where you quickly went over early-on the point that 13 alcoves will not be constructed that were originally planned. This makes me wary as an erstwhile scientist that a lot of scientific work may not be going to get done that was thought at one time to be important. I would ask you if you think that the alcoves that will be opened or that are in process of being constructed or whatever suffice? Is there a consensus that the science to be done in those alcoves will be sufficient to satisfy the needs of the program?

BOYLE: I would say no. Dennis will talk about that this afternoon. There are plans for more alcoves already. So, those that I showed, I think the answer is obviously no. But, whether the ones that Dennis is going to talk about, adding those in makes it sufficient, I think then you can ask your question again.

COHON: Well, that's the perfect way to end. It interjects an element of suspense. We'll bring everybody back.

I want to thank Dennis Williams and his colleagues and contractors for the good morning presentation.

We will reconvene at 2:05.

(Whereupon, a luncheon recess was taken.)

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

COHON: This afternoon, we're going to start with a presentation and discussion on scientific investigations in Nye County by Nye County. Then, we'll turn to an over-arching, a very important topic, of reducing hydrologic uncertainty in our understanding at Yucca Mountain. Ed Cording, our colleague on the Board, will chair that session when we get to it. But, for now, I'll continue to act as Chair while we take up the first order of business.

We're joined today by two people who have been working on scientific investigations in Nye County; Nick Stellavato and Parvis Montazer. Mr. Montazer will give the presentation after which we'll have questions and discussions.

Mr. Montazer?

MONTAZER: Good afternoon. I hope you enjoyed the lunch as much as I did.

Nye County began drilling a borehole called ONC-1 for the on-site Nye County investigations in Yucca Mountain in late '94. We've been monitoring and collecting data from this borehole for a period of time, and we have also instrumented one of the boreholes that was drilled by DOE, NRG-4, that are shown in this slide here. NRG-4 is on the north ramp, and ONC-1, the south ramp.

I've been asked to talk about our activities in the tunnel. We started last year some instrumentation in the tunnel and we've been making and monitoring observation. And, we have come up with some findings that we'd like to share with you.

The purpose of the instrumentation of the tunnels was basically to evaluate the potential for the removal of moisture and heat in the tunnel, but along with several other purposes that you can see on the left slide. On the right slide, I'm showing the places that we've instrumented. We're looking out away from the tunnel. In this case, I guess, we're about in 1,000 meters that the TBM had bored, and we hooked three probes at the basically tail end of the TBM and these are monitoring temperature and pressure. Well, #1 is monitoring pressure only; all three of them are monitoring temperature and humidity. The distribution of the instruments are shown on the left hand side and this is an example. We have continuous data for almost a year or a little bit longer than a year.

Here, we're just showing you an example of what we see in response to the ventilation. As you see, these data indicate there's some air flow in the tunnel and, generally, just during where the activities are underway. And, when we have these relatively smooth recovery type curves and both the humidity on the top and the temperature on the bottom, it indicates the weekend when there's no ventilation. Basically, both the temperature and the humidity try to calibrate with the tunnel.

One significant thing that we see in this if you look at the position of the Probe 2, this is farther away from the wall as compared with Probe 3 and 1. When the ventilation is going on, Probe 2 shows a different response and basically more reduction in humidity and temperature. The reason for that is a relatively simple principle which we refer to as the eddy diffusivity. It's old chemical engineering terminology and atmospheric modelers use that a lot. It's basically a creation of eddys as a result of change in the velocity of the air flow across a profile. These eddys create a situation that there's a transfer of heat and moisture perpendicular to the system, in addition to the transfer of heat and moisture that is occurring along the tunnel.

Now, we thought about this from our experience in the tunnel. Some in the mining industry recognize that if we have a system that is ventilating--now, this shaft does not exist in the site, but if--we've seen in a lot of tunnels that there's ventilation occurring basically by natural conditions as a result of both the temperature and pressure differences in the atmosphere as opposed to the tunnel. Actually, in the old days before they had fans, they used to put fire down at the bottom of the shaft to create air current moving through. So, if we can use that concept, as I'm showing on the right hand side, can we use this to see what effect this is going to have if we are ventilating a repository and whether that's really going to affect the safety of the repository which is our primary concern.

Here, I'm just showing a simplified conceptualization which I built a numerical model, a simplified, very simplified numerical model for strictly demonstration purposes. Basically, we have these heaters which I have placed it in this mesh on the left hand side. You'll be looking basically as a cross-section--the horizontal cross-section of this cylinder, basically half of it. And, the heater would be in this middle spot on the right hand side.

We made some assumptions. We took some data from the TSPA-95 as far as the heat load was concerned. And, this is heat load for about 10 canisters placed at 40 meter intervals. And, basically, we doubled up the amount of heat load that we're putting on the canisters and basically doubled the TSPA-95 using one of their examples. The interesting that we saw about the temperature was during all the time that we were monitoring and we were doing the simulation, the maximum temperature rose to 33 degrees. And, this particular simulation, with a--of .1 which we calibrated based on our actual data collection. We have a number of other--there are 11 different simulations since doing the analysis that we've done. They're all documented in a two volume report which I have a copy of this if you want to come look at it at a break.

But, anyway, we see this as the center of the--the temperature all the time remains below that threshold and the rock and everything stays cool. Well, another concern we have was the saturation, basically moisture condition, in the rock. On your right hand side, we see the saturation after about 10 years stuffed at near the--in the tunnel basically goes bone dry and the rest of the rock about down to 8 meters, I'm showing here into the rock, came down to about 50%. What does that mean in terms of flow conditions? This diagram just basically shows the capillary pressure, that distribution that you would see along the mesh that I showed you earlier.

We see that, after about 100 years, would evolve a very strong gradient from an originally uniform condition, assumed condition, that basically promotes flow towards the tunnel at all times. So, what this tells us in this kind of scenario, with this kind of a design, there's a potential for increased safety. And, because of the lower temperature, there's a potential for a reduction in the area because the waste can be packed more closely. And, we have come up with basically another thinking in that area. That you can possibly--because the amount of flow that we generate are tremendous, there's a possibility of generating electricity by focusing this flow into some specially designed turbines.

Okay. I'd like to just tell you a little bit about what we have done in ONC-1 and some of the interesting things that we have observed and the simulations that we've done. On the right hand side, I'm showing you the instrumentation for both of the holes; the NRG-4 which is on the north ramp and ONC-1 which is shown in this diagram to your left. And, we've been observing pressure in this--I'm going to have to skip some of the questions.

One of the interesting things that we saw was that in about November or so, we started--we looked at the data--actually, in February, we started--this is before the tunnel had progressed enough. If you look at the down probes, the deeper probes, you see that there are more or less increments. All the probes that are below PTn, Probe 2 down from this yellow line that I've dashed to make it clear, they're all, more or less, synchronous. And, later on in February when the tunnel crossed some structure in the--basically what's the extension of Sundance Fault, we started seeing some perturbations and we suspected that that may be the tunnel and tunnel effect. Later on, this perturbation relatively increased in the magnitude and the wave length, but the important thing that we saw the difference is that the response in all the probes deeper than Probe 2 started coming earlier than the Probe 2. Basically, it was saying that somehow the pressure response is getting to the lower probes through some fault or some system that we--that that had changed the response of the whole.

We tried the simulation of one-dimensional without any of the fault system and we couldn't. Despite the fact that earlier on, about a year before this incident, we were able to simulate and perfectly match everything. When we did the three-dimensional simulation by putting the response from the tunnel, we had a prefect match basically. We put all of these tunnel nodes--this is just an extension of the Ghost Dance Fault and really doesn't play a major role in this particular simulation. Basically, the pressure through the tunnel is rapidly transferred to Sundance Fault or some system in that area and is affecting the fault that goes through in the borehole, those intersected by borehole--

I have three more slides. One of the interesting things that we had seen in NRG-4, we have another report that we--Nye County's report that we made last year specifically more about the NRG-4 response of the tunnel and the calculation of the permeability of a lot of the units there. But, one of the things that we have been observing lately on NRG-4 and we compared it with ONC-1--on the left hand side, I have the ONC-1 temperature gradient data, and on the right hand side, I have the NRG-4 data. After April '96, we changed the instrumentation. Before then, we had some problem with stability of the temperature probes and we have been calibrating and this calibration kind of gives us a relatively good representation of what was going on before, but these fluctuations are really the instrumentation. We've realized that they are instrumentation. After we changed the instrumentation, we have been getting consistent results. Basically, all of these points are plotting on top of each other. We cannot see the difference in ONC-1.

However, we tried to look, make that comparison, on NRG-4. It appeared that we really have several basically kinks in the temperature profile that has basically changed in time. Specifically, December '96, we have a lower temperature profile than earlier on in between April to December. And, we suspect that this is because of the cooling air that goes through the tunnel. We've done some simulations to calculate large-scale thermal conductivity by making the assumption that these responses are from the tunnel. And, because the NRG-4 is about 50 feet away from the ESF, that gives us a really--probably the largest scale thermal conduct, too, that has been measured on-site.

In conclusion, it's our thinking that the--as is proposed right now in the regulations, the backfilling of the repository may not necessarily be the safest way to go. An upper repository concept needs to be looked at because we think that ventilation may prove to be--may prove to really take a lot of the uncertainty out of the subsurface data that has been collected. The generational--is kind of a windfall. It's a secondary thing. It really doesn't have anything to do with the repository, but it can provide--we've done calculations of maybe it will generate about 10 to maybe even over 100 megawatts of electricity using just the air flow system. And, we'd like to see more construction activity, the construction related testing activity, basically taking advantage of the conditions that are in progress and getting as much data as possible by making slight changes in the program.

That's all I have, and I'm open for questions.

COHON: Thank you very much. We appreciate your willingness and ability to compress so much into such a short period of time.

Questions, comments?

LANGMUIR: Parvis, your study suggests that there's some substantial avenues for gas flow in the mountain. We've known that, but would your perception of where they are and their effectiveness be in agreement with what the GS has come up with over the years?

MONTAZER: Well, basically, we've been coming up with this stuff, more or less, synchronously. This pneumatic pathway was not being a priority at the time that we were really pushing this. Ever since then, it's become a relative--I mean, there's been a lot of effort done in analyzing data and we've been, more or less, seeing the same kind of results as far as in a vertical sense is concerned. There is some--I think we still haven't really gotten together and hashed out all of the differences, but there are some differences as far as this long distance response from the tunnel for the ONC-1. We've just talked in the hallway with Bo that he didn't think that was happening. I'm assuming that his model doesn't predict that. So, those are some of the minor things that we have to work out. But, generally, yes, we are in agreement as far as the PTn is a barrier to the--basically, in dampening the pressure in a vertical flow. But, as far as these really long distance travel of pressure along some of these faults as we're proposing and hypothesizing, that I think we're still trying to hash out, the difference.

LANGMUIR: Another implication and I know the reason for this, at least one of the major reasons for doing this kind of work, was to get a sense of where gases, radioactive gases, might move from a repository. But, equally as important--and, presumably, performance will be the avenues that gases, water vapor, moisture would go if you evaporate with a heated repository from waters around the waste packages. And, therefore, these avenues might be expected to be preferred directions for fluids to go and condense and give us this hypothetical refluction process that we're proposing is likely to happen there. Any thoughts on that?

MONTAZER: We're concerned with that process and that's one of the reasons we're promoting or encouraging this alternative be looked at. The open repository, even if it is for a shorter period of time--you know, just for 100 years or 200 years, however long we can engineer to keep it open--is going to, more or less, guarantee the flow into the tunnel and into the situation. It's under the control of whoever has the institution of control over the repository. Everything can be measured that comes out of the tunnel, the air and et cetera, and you're, more or less, guaranteed an effluent from the rock and not into the rock. So you, more or less, minimize any chances of outward movement of the radionuclides in an oil and gas use or in the aqueous phase. By the way, we have been making some measurement. We have taken some gas samples, Carbon-14 and tritium, from ONC-1, and we've been analyzing that. We're seeing some interesting results on that.

COHON: We have a question from Russ.

MCFARLAND: Russ McFarland, Board staff. Parvis, have you looked at any conceptual designs of a ventilation system for a repository? If so, what would it take to move air through 160 some emplacement drifts in terms of shafts?

MONTAZER: Well, I've been really looking at it in a lot of different aspects. And, it all depends on whether you want to incorporate the electrical generation or not. So, for a minute, forget about that. The number of shafts and they don't necessarily all have to come from the top of the mountain. There are many different ways we can design to get away from--I shouldn't say we. By we, I mean the project, Yucca Mountain Project; not necessarily Nye County. It can be designed to basically minimize or eliminate any potential movement of water into the--any infiltration water into the tunnel. We've considered some of those. But, this is, more or less, a new idea and we haven't really had a lot of time or resources to hash out all the different areas. We're not really in the process of thinking about the specific design. But, preliminary just to answer your question, anywhere from 5 to 10 shafts may be required; anywhere from 6 foot diameter to 10 feet diameter shaft. We may be required to carry out this. As far as the distribution of the waste is concerned, with this type of design the distribution becomes much, much denser. It does not have to be spread out as much as it is right now. So, the number of tunnels will be decreased considerably. The--requirement will be much less with this concept.

COHON: Any other questions?

REITER: Leon Reiter, staff. Parvis, I wonder if you could comment on what we heard this morning from M.J. Umari. He talked about the determination that there's some sort of a northwest trending fault in the C-Well complex which I gather is not too far from where you are. Could here be any correlation between your suggested projection in the Sundance and what they discovered? And, maybe, Umari could comment on that also.

MONTAZER: Nick has got some ideas on that about what we have seen in and, once more, I'll let him--

STELLAVATO: Yeah, just a couple of comments because I talked to M.J. and I was real excited when I saw Warren Day's new work on that proposing that fault. We did hit the Bow Ridge Fault and Bow Ridge and that well is right about 600 to 700 feet of downdrop. However, we also saw some missing section in the Calico below the Bow Ridge. We hit the water at 1420, the fault was at 1100, around that; I can't remember exactly. But, there's some sections missing in the Calico and then with the responses were seeing and then the gas chemistry work that Parvis has alluded to and then our saturated probes--because we have two probes below the water table packed off at two different zones--within two hours of pumping of C-Well, our well faults. You know, we can follow every time they pump 2600 feet away within two hours. So, we've gotten a real good connection on northwest/southeast whether it's at the Sundance or the Broken Limb or whatever it is--I don't really care--but I think there's two faults in ONC-1; I think the Bow Ridge is one and I think we have one northwest/southeast.

COHON: Thank you. Thank you, Mr. Montazer.

MONTAZER: The one with the geochemistry that Nick was talking about. I took it out and this is what he's talking about. We're seeing a major change in the Delta C-13 and this is all gas samples. This really hasn't--from my discussions with USGS, apparently this heavy of Delta C section has not been seen in the unsaturated zone. So, there's something going on across this fault that we're in the process of figuring out what.

REITER: Is there anybody from DOE who could comment on this?

COHON: Is anybody moving? I can't tell.

SULLIVAN: Tim Sullivan, DOE.

REITER: The suggestion has been made by work by Parvis that based their ONC-1 that there's a northwest trending fault. It may or may not be the Sundance. Now, we also saw this morning some work done by Umari which also--the C-Well complex was right nearby--that there also may exist a northwest trending fault there. I just wanted to know if there's any correlation between these things? I guess, Nick thinks there is a correlation, there is a connection. I wonder if somebody from the DOE organization would comment on that? It's kind of significant.

SULLIVAN: Mark and I have talked with Warren about this. Both of these boreholes are collared in alluvium. However, by matching some beds in the Tiva across the gap in some north trending ridges, Warren has inferred that possibly a northwest trending fault with a few tens of feet of offset in the Tiva may be present in that locality. I guess, Nick, that's what you were referring to earlier. So, there may be a concealed fault there. I can't address Umari's comments.

UMARI: M.J. Umari with the USGS. All I can say really is to reiterate what I said earlier and that is simply from the testing at the C-Wells complex, we have noticed that if you were to include the drawdown and add-on in C-1 and at H-4, then you would see a prolonged cone of depression in the direction that is aligned northwest/southeast. And, basically, that correlates with the leaning that seems to be being identified in that direction. So, from a hydraulic standpoint, we're seeing something anomalous that's indicating alignment with something. That's from our end the end of it at this point. But, we're kind of excited about the fact that the geology team is getting to the same conclusion. So, it's a situation where the hydraulics and the geology seem to be indicating the same thing.

COHON: Thank you, Mr. Montazer. Thank you, Mr. Stellavato.

STUCKLESS: John Stuckless, USGS. Leon, we've tried very hard to walk that Sundance in the direction of this lineament and it just plain doesn't exist. You have complete outcrop in between the last place where we know there's movement in the Sundance and then where they pick up this other fault. There may be a fracture zone in there, but it really is misleading to try to call the whole thing the Sundance Fault. Sundance stops and this other thing picks up.

DOMENICO: I don't know if you're relating the fault to the pressure drop you saw, but that formation, that is one of the highest hydraulic diffusivities that you'll ever measure. That's the ratio between the hydraulic conductivity and this specific storage. You have a very high hydraulic conductivity, extremely low specific storage, probably 10^{-4} or 10^{-5}. And, with that sort of a hydraulic diffusivity, that pressure wave can move at speeds approaching the speed of sound. That's a fact.

MONTAZER: That's what actually we've calculated. In your handout--I didn't show the slides. In the handout, there's tables that show calculation. There's one particular one, if I can just find it. It's one of these yellow background tables. I haven't paged these. The one that says for ONC-1. Yeah, this one. At the bottom of the figure, we have--

COHON: Can you get a little closer to the microphone? Thanks.

MONTAZER: At the bottom of the figure, I've calculated the permeability or basically the porosity that we suspect to be prevailing to be able to transfer this pressure from the tunnel to ONC-1.

DOMENICO: The porosity is not involved in the hydraulic diffusivity. This is a pressure wave. The thing is to be controlled by the specific storage which in this case would be totally controlled by the expansion of water when you lower the water level.

MONTAZER: You're talking about the saturated zone. I'm talking about--

DOMENICO: Oh, you're talking about the unsaturated.

MONTAZER: These are for the air.

DOMENICO: I was talking about the response that Nick was talking about in the saturated zone. Yeah.

STELLAVATO: Yeah, and I didn't--what I was looking at is geology and looking at the geophysical logs and looking at our cuttings and during drilling. That paid a lot, in part, into what I thought something was going on below the Bow Ridge, and then with what M.J. is saying and Warren Day, it fits. I didn't base it on that calculation there.

COHON: Thank you very much.

Ed Cording will both Chair and introduce the next session. Ed? With a target ending time, break time, of 4:00 o'clock.

CORDING: Okay. That's the time for the break.

COHON: Right.

CORDING: And, we would have a 15 minute break.

This afternoon's session is one that we've been looking forward to. We have over the past year seen much progress in the exploration and testing, the underground work at Yucca Mountain; drillhole work, also. We're on a really steep running curve, I think, and there's been a lot of new data that's become available. We've appreciated DOE's efforts to in real time provide us with an update on what they're learning about the mountain; not just waiting to have final reports come out. So, that's been very helpful to us and I think we saw some of that this morning. We got a good overview of the effort that's underway and what has been accomplished regarding principally the hydrogeologic issues.

This afternoon now we have the opportunity to hear more about the plans for the immediate future, the next year or the next two years perhaps. These plans are those which are principally related to reducing hydrogeologic uncertainty. In a few moments Dennis Williams will be making a presentation.

Then, following our break, the members of a panel or a group of--principally, a group of people that are very close to this work, principal investigators from Los Alamos, from Lawrence Berkley Lab, from the USGS will be joining us at the head table, and with Dennis Williams and others from DOE, we'll be discussing some of these issues about reducing hydrogeologic uncertainty with the Board and then with the audience's participation, as well. I think, we'll have opportunity for that.

Last year at this time, it was understood that we were dealing with a relatively no-flux. The flux that was actually approaching the repository level, the estimates were in the range of .1 to .5 millimeters per year. Also, a year ago, there was a well-conserved preliminary waste containment and isolation strategy that was emerging. One of the assumptions of that strategy was that the flux was low. Today, there's evidence, as we've been hearing this morning, that the percolation flux may be higher in the range or order of magnitude range, say, of 1 to 10 millimeters per year. Most of the evidence for this is indirect. We do know that there are fast pathways. We know that the tunnels themselves are dry and that local seepage flows are no high enough, they are not concentrated enough to overpower the ventilation system. We know the ventilation is high capacity. It can take out almost an order of magnitude more water than we think is percolating into the mountain, as Bill Boyle noted this morning.

What dose a higher flux mean? I think that you've been seeing some information today of some of the model studies that are trying to take these little higher fluxes into account. It may change conclusions as to how and for how long the waste heat from the spent fuel will mobilize or be able to mobilize and drive moisture away from the emplacement drifts. It may certainly influence the amount of water contacting waste packages and ultimately transporting radionuclides toward the saturated zone.

As I noted before, the evidence for seepage flux is indirect. There is still large uncertainty in what percolation flux actually exists within the block where the repository is proposed. And, certainly, there is uncertainty on a very important issue of the distribution of that flux; distribution over the entire Yucca Mountain footprint, also distributions locally. Are they concentrated on a few fractures? Is it concentrated in a way that it's just going to be distributed perhaps on a lot of different fractures, as well as seeping through the matrix? Is some of the flow really focused in a way that it's a Ghost Dance Fault zone or some fracture zone that allows it to be avoided in the placement of the emplacement drifts and the waste in those drifts?

Now, that bring us to the session today because we've asked the DOE to offer us their ideas and approaches, principally and specifically, how hydrogeologic uncertainties can be reduced. What exploration and testing can be done between now and 1998; between now and the date of potential license application, 2001? What tests can be performed? What specific data should be sought? I certainly look forward to this session and I hope that we can reason together and perhaps at least better understand some of the thoughts, the arguments, the ideas that are being produced by a large group of people in the program. So, we think that this effort today really points out--is really focused on really a very hard priority issue; the issue of reducing our uncertainties regarding the flux, the water flow in the mountain.

So, we'll start now with Dennis' presentation and it's appropriately entitled "DOE Plans for Reducing Hydrogeologic Uncertainty".

WILLIAMS: With the permission of the Board, I'd like to experiment a little bit in this presentation and possibly make it a little bit more interactive. I've got it kind of broken up into modules where we can talk about a key uncertainty. Then, we can talk about the proposals that DOE received, some of the latest things that have been coming out of some of the extractions that we've been doing, and how that fits into the testing so I can bring up our cast of characters. And, as we get to that point, would give you the opportunity to interact on that particular item. Then, we possibly could get some of our questions answered a lot earlier in this whole process.

CORDING: That's excellent. Let's do that. So, if the PI people participating would join us at the head table, we'd appreciate that.

WILLIAMS: Yeah, and you know, it's some of the usual suspects. So, I'm sure you'll recognize--

CORDING: You'll be introducing them, I presume?

WILLIAMS: Yes, I will.

CORDING: We'll look forward to that.

WILLIAMS: Again, this is a little bit of an experiment, but in talking to the staff, they said the Board would like to have more of an opportunity to talk with the PIs that are involved in some of the most recent developments and some of the testing. So, we're going to see how it goes.

CORDING: It's helpful to us, I think, to see a slice of the program from top to bottom. I'm not saying this is necessary bottom. But, I was disappointed you didn't have a couple of chucktenders here or the drillers in here to participate, as well.

WILLIAMS: They would have loved it, as well.

Okay. While everybody is getting settled again, it's DOE plans for reducing hydrologic uncertainty. Again, I'm Deputy Assistant Manager for Licensing. Here we go with the suspects; kind of a mix of USGS, LBL, and you'll see mix of saturated zone, unsaturated zone. If I went back to the waste containment and isolation strategy, you would kind of see us arrayed here from unsaturated down through dilution. Of course, they kind of mixed up on me a little bit here, but that's integration.

We have Russ Patterson from DOE. He's our technical lead in hydrology modeling. Eric Smistad from DOE, sitting in and representing PA. Of course, we have our guru of PA in the audience to really bail us out in case we get into problems, but Eric is perfectly capable in those areas. Ed Kwickles from the USGS, principal investigator of UZ hydrology. Ed's been involved in some of the elicitations and abstractions going on. Abstractions is what we've been dealing with lately, largely with regard to the UZ modeling. Bo from Berkley, principal investigator in UZ flow model, but also delves in testing, sampling. I think he's probably even been on the drill rig occasionally out in the tunnel. Alan Flint, UZ flow, but we know him largely from infiltration, USGS. M.J. Umari, you heard him earlier this morning, again USGS, hydraulic testing in the C-Wells. Gilles Bussod from Los Alamos, principal investigator, the transport field test; that's a little better terminology than what I tried this morning. And, Bruce Robinson, also from LANL, principal investigator on UZ and saturated zone transport modeling.

So, again, we've got the unsaturated. We've got the saturated. We've got flow. We've got transport. And, we've got PA representation and, of course, the DOE bureaucrats to kind of keep it rolling along.

If we don't get to the end, I kind of planned something here which you can--executive summary at the front. My boss, Mr. Barnes, wanted me to mention this first bullet. In the second quarter of fiscal year 1997, we will have supplemented the FY97 program with an additional $13.1 million. Now, you probably think about the total $300 million that's in this program, but it's not really relevant whenever we're talking about percentages of increase because this is in the areas of basically repository design, the site investigations, waste package design which right now roll up to about $85 million in total. So, 13 on to $85 million is 12 or a 15% increase. So, that's a substantial increase this late in the year.

The work includes 39 individual tasks. They cover all the four major product areas of the VA, but the majority of them contribute to the design and the TSPA products. Remember, of course, that the site investigations program is not a sperate entity anymore; it's really rolled up into that TSPA part of it.

The work is intended to provide more confidence in those four major products of the VA and then ultimately, of course, this work contributes to the license application.

Because a great deal of scientific uncertainty is associated with the hydrologic program, this presentation and discussion will focus on a set of key hydrologic uncertainties. I mean if we wanted to talk about the uncertainties on this program across the board in areas of thermal, climate, modeling, everything else, you know, we could spend a couple of weeks here in quite a workshop on that. So, today, we've selected four that I think cover the range of some very important items and we'll talk about those, some hopefully in detail.

A little bit of the outline. I know you wouldn't get by without a review of the waste containment and isolation strategy. I hit that every opportunity I get. I'll also mention a little bit of review of the progress report. We'll talk about process modeling ties and linkages; give you a little bit of an idea how all these models fit together. Then, we'll get into that discussion of selective key uncertainties in hydrology; there's four of them. And then, the testing strategies that are being implemented to address each of these selected key uncertainties. We'll deal with these individually. So, when we get through the first one, whenever I give a little bit of a background on it, that's when we can start getting interactive and talking to the PIs, the testers, whatever, and see how that goes.

Now, I need a major time-consuming faux pas here because I used this overhead out of this presentation earlier this morning, but I'll get to it real quickly again. The key attributes of the waste containment and isolation strategy; the seepage into the repository, the integrity of the waste package, the release from the canisters; radionuclide transport through engineered and natural barriers; and, dilution.

What are we talking about here in reducing these key uncertainties? I mean, we're talking about the site characterization part of this. The seepage coming into the system, going through the UZ, coming down through here interacting with the waste packages, interacting with the repository, and rolling on out the downside to the unsaturated zone, through the saturated zone of the natural barriers, and dilution in the groundwater below that. That's just a fact. That's the way it sets up.

The progress report, just briefly. This morning, we talked about the C-Well complex. We looked at the hydraulic and transport characteristics. Bill Boyle took us through a whirlwind of the underground testing. Unsaturated zone flow, of course, last October in Vienna we talked about the percolation flux and what it's doing to us, what kind of measurements we're making on it, and of course, we had some very good discussion this morning of unsaturated and saturated zone transport. So, we keep all those things in the back of our minds as we roll on through here.

All these models, they confuse me until I get a diagram like this, and then I think I have a pretty good idea of what I'm talking about. But, most of our process models, the little lines tell you how they fit together. The piece that isn't a process model, but was really fundamental to the whole characterization of the site is the 3-D geologic framework model. That sets the basis. That sets the geology basis for the entire site. All these other models use that framework model. We have tried to get this model well-identified, very robust, and force the rest of this modeling effort to consistently use that framework model. So, whenever the geologists tell us what's out there, everybody uses that.

Okay. From that, we flow to the UZ flow model. We've got an infiltration model that feeds into that. We've got climate models in here. We've got near-field environmental models. Waste package; arrows going back and forth here because with regard to the source term on the waste package, you have to look at an iteration and then feed it back to what your near-field is doing. So, this thing, as you are with it for a while, it will start to make sense, and if it doesn't by the end of the day, Russ Patterson will explain it to us.

Another thing that's somewhat confusing in the terminology we use is how process models go through abstractions, go through elicitations, and then get to a TSPA. You can see over here in the small terms that at one point in time we had this all tied together very nicely, but we could easily see that we were trapped in a loop. So, we had to get out to VA and LA eventually on it. What I showed you before was largely the process level models. We basically abstract from those models to get in a usable form of TSPA. In some cases, we will call in expert elicitations to help us develop some of the values for the process level models. Of course, they take into consideration process level models. They will also take into consideration the abstractions that we are doing. And, they will also take into consideration new data that is developing, but hasn't been hard wired into the process models yet. All of that eventually feeds into TSPA in an iterative process. You get a TSPA that you like in the end and then you feed that out to VA and LA depending on the timing.

I speak of hard wiring and soft wiring data into our systems. This came into play whenever we started asking for additional funding for FY97 because it was obvious that if we were going to go out there and reduce some uncertainties on key things like percolation flux and we're going to put a test in for the remainder of '97 and we have a UZ flow model that's basically being put together in June of '97, then you ask yourself, well, how are you going to use this new information that you get? Well, on the top line, I'm showing basically what I would consider to be a hard wired situation where you have the data collection activity. That goes directly into data analysis and modeling, the process models for VA, runs through an abstraction process, on down to TSPA, and then goes wherever you need that TSPA product. So, that is finish/start type of a process. You series up all of your data feeds and your modeling processes, hard wiring.

If you to a soft wired approach like we've done with some of these additional tests that we've funded, we basically go through the preparation of our test bed, we start out data collection, our measurements and observations, but what do we do with that? We can't hard wire it in here because the model developed back here. However, we do have that data that we can look at in our abstraction process. We can have that data for consideration during our elicitations. We basically can use that data to confirm what's happening up here to increase our confidence in the VA product. Now, if that data starts diverting from what we have assumed up here, you know, then we can say, hey, you know, we may have a problem developing here and hopefully we will see that early enough that we can remedy the situation and get back on track.

But, anyway, that's very important to why we were able to convince our management that it was valuable to fund some of these additional--some of this additional work throughout FY97, even though it could not be hard wired into that modeling process.

The selected key uncertainties. Percolation flux, fast paths, dilution, transport parameters and mixing depth, and saturated zone hydrochemistry. I thought it was interesting yesterday John Austin of the NRC was talking about what they had looked at in the '95 TSPA and percolation flux, dilution. Some of these things that we're talking about today, some of these things that we're considering additional funding on are the same kinds of things that they are looking at to a great deal of detail. That's another indicator that these are very important things to know.

To get the kind of a star that we will use here today, I'll give a little bit of a brief on percolation flux, the background, and what we are doing, what we intend to do, and then talk a little bit about the testing in alcoves that we're going to put into play. That's the point where we'll try the experiment and see how it goes with our panel members here making comments on it. They don't have written scripts. So, it's just going to be from the gut. They said they were going to fight over dollars, though. We'll see how it goes.

Percolation flux, we'll do this in a little bit of a definition form. It's another one of these wordy graphs that I have trouble with. Percolation flux is the portion of the infiltrating water that passes through the first few meters of bedrock and penetrates to deeper levels. It affects four of the five attributes of the waste containment and isolation strategy and probably indirectly it would affect all of them. We've talked about the percolation flux that we reported on in October, the range somewhere in the area of 1 to 10 millimeter per year. Of course, TSPA-95 assumed 0.03 to 1.25. So, things have changed.

Some of the uncertainties associated with that, the distribution at the repository horizon in time and space. The role of the PTn; what's that barrier doing up there for us or against us? The degree of fracture-matrix interaction. Of course, all your spatial variabilities. The roles of your faults and fast pathways. And, again, as I said 14 times in October, there's no direct measurement of it. Bo really contributed to that 14, but we'll just leave it at that.

Again, reviewing a little bit, the various components of the present UZ site characterization program that contribute to the determination of percolation flux. Bill talked about some of the things in the ESF that were being worked on, but basically we have infiltration measurements and modelings; saturation and moisture potential; pneumatic; environmental isotopes; this is the area of the Chlorine-36 fracture coatings, the work that Zel does; temperature data; and, perched water data.

And, the enhanced program. What we've got here first, I've got it marked off in rainbow. It reminded me that this is the piece that's associated with the large-scale percolation flux test. We'll talk about that a little bit because it bears on the '97 program. But, basically, the things that we're going to be working on today with regard to this is we do have some percolation testing in the south ramp excavating some small holes, drill some instrumentation holes, some bulkheads, and monitoring.

Lateral diversion on the PTn. We'll talk a little bit about that going along in the north ramp between Alcove 3 and Alcove 4. Of course, we've got the whole section of the PTn exposed there. We want to do more work on that. I think, Alan is a real champion of this and he'll probably give us a lot of good thoughts on that. Environmental isotopes, we continue to want to deal with the Chlorine-36 and the confirmatory isotopes of technetium. We're putting some more money into the tritium analysis, and we're also looking at getting C-14 ages from better extractions on TSw2 pore water. The role of faults; again, continuing with a geothermal boreholes and basically continuing to roll on that Ghost Dance Fault program.

Now, we've got a couple of--these are some alcoves, some alcove diagrams, plan and section associated with some of the testing that we want to do. This bears on the question that Bill brought up. When we had the multiple levels on the ESF out of the ESF Alternative Study down in the Calico Hills, we had the main level. There was dozens of alcoves planned to cover probably every eventuality as far as looking at a contact, looking at a fault, looking at everything that I think the SCP could conceive of. As we've got into that program, what have we done? First off, we went to a hierarchial system of saying, hey, some alcoves can be deferred; some alcoves can't be deferred. We said alcoves on major faults can't be deferred. So, we're not going to get very far past those particular intercepts or those faults before we start an alcove on those.

That's basically what we have done. We went through the Bow Ridge Fault. We immediately started an alcove. When we got to the right spot to get over to the Ghost Dance, we started an alcove. When we got to the contacts of the PTn, we knew those contacts were going to be very important. We put in Alcove 3 and Alcove 4. Original Alcove 5 was supposed to be the Drill Hole Wash Fault. WE thought that might be a major structure. Those of you who have gone in the ESF, you have a hard time finding that particular structure. It was probably only prudent on our part to say, hey, maybe we don't need an alcove here. Let's don't stop this whole excavation progress to build an alcove that we may not need. So, we rolled on past it, and then we go on to alcoves that we know we need. This is the plan to put some more alcoves in that ESF. We want to make maximum advantage of the ESF in that underground opening where we want to do it right. We want to do an alcove for the right reason, not just because it happened to set in a plan that had 48 odd alcoves in it. So, we're very careful. We try to be very mindful of the ratepayers' and the taxpayers' dollars, do the right thing that gives us the most bang for the buck.

So, at that point, the key uncertainty associated with percolation flux, I'd like to let this roll a little bit. How do we want to start? Who wants to talk about uncertainties on percolation flux, the latest findings? Ed, you look ready to--

KWICKLES: Okay.

WILLIAMS: Put him on the spot.

KWICKLES: In terms of resolving uncertainties in percolation flux and spatial distribution, I still have some hope that the chloride mass balance method may be the best indicator of long-term percolation rates. Those of you who have heard some recent presentations of mine may be surprised that I feel that way given my criticisms of the method as it's been applied, thus far.

The method basically says that along the vertical column of rock, the chloride flux is constant. And, that by looking at the concentration of pore salts--to what was arrived through wet and dry fallout at the surface, you can relate that to the average precipitation rate to determine what fraction of that precipitation answer does--infiltration. And, the problems that I've seen with that method as it's been applied is that we know we have run-on and runoff, that the colloid application rate isn't uniform on the ground surface of the mountain. We know that we have lateral diversion. Probably, that lateral diversion is more pronounced at the Calico Hills than elsewhere. We know we have some fraction of flux bypass the PTn and possibly bypass the Calico Hills, as well.

But, in spite of all those limitations, I think it's a method that potentially can--if we sample the PTn and the Calico Hills in enough places that we account for these variations in spatial variability and colloid application patterns and account for the fact that we're going to have lateral flow from one area to another, I think we can get a good sense of what the long-term percolation flux has been.

Do you want me to go through my whole list of ways to reduce uncertainty or do you want to just keep it--

WILLIAMS: No, just a couple ideas--

CORDING: And, I'd like the Board to participate in asking questions as we go, also. But, do you feel at this point that you have some data from that method that would tell you what the flux is?

KWICKLES: Well, we had a lot of data from some of the shallow UZ holes and--I mean, the deeper UZ holes and some of the neutron holes. You know, we know that the pore water chemistry that we obtained wasn't the whole story because we know that there was some fracture of water with a different water chemistry that had not been reflected--had not been captured by the PTn and so it was not being reflected in the pore water chemistry. But, I mean, it's basically--conceptually, it's a very simple method to apply. I don't think we, as a project, have really gone after it and applied enough resources to surmount the complications that arise from dealing in a system with structured flow pathways. I think, LANL has proposed some additional sampling from the tunnel for the remainder of the year and actually I would question if that's the optimal place to do it because probably the matrix pore water is not going to be reflecting the fracture flow to the same extent that samples from the Calico Hills or PTn would.

CORDING: You need some sense of flow path to be looking at this.

MR. KWICKLES: Well, another thing I think is important to reducing the hydrologic uncertainty is identifying better than we have the chemistry of the waters in the infiltration zones that Alan has identified and potentially it's--and I've seen Zel Peterman apply this to a limited extent of characterizing the strontium isotope ratios of waters in different zones of either exposed bedrock or alluvium and then looking at that strontium signature deeper in the rock and being able to use the chemical signatures of water in those source areas as a basis for arguments as to where that water originated and, hence, what flow paths that water must have followed to get to where we've sampled it.

WILLIAMS: For the benefit of the group here, I want them to know that I have no problem with living dangerously during this discussion. And, I mean, we've put some testing things in the program, but you know, do we have testing things going into the program that's addressing these kinds of issues? You guys have said in numerous proposals, you know, tell me how those tests address these issues or if they don't, then that tells us I need to go back to the drawing board perhaps and see what we can do as far as addressing these issues.

CORDING: One question from Don Langmuir.

LANGMUIR: I have a suggestion to making it more dangerous, Dennis, and that is that a year or so ago, Bo Bodvarsson presented us rapidly--it was an impressive blizzard of information difficult to follow--with a dozen or so approaches to getting at the infiltration and percolation flux, various techniques that were available to us that haven't been used within the program, including the thermal gradient measured from the surface to the subsurface, the chloride mass balance, mineral precipitation rate information. I can't remember them all. The radioactive isotopes that were available to us.

I would propose to make this more of a challenging discussion by having that list on the board and having our presenters over here who are expert in some aspects of infiltration flux or one part or another, one or more of those different approaches, vote on them. What are the best of those approaches? Which have the least uncertainty? Which should be emphasized or prioritized in future work?

Is that too much to ask? It would focus the discussions certainly here. Bo, can you remind us of what they were?

BODVARSSON: Yeah. Let me talk real fast.

LANGMUIR: Put them on the board over here perhaps if you're willing to do that.

BODVARSSON: Okay.

(Pause.)

CORDING: Is it on the previous overhead that you had, Dennis? Why not start with that? That might be a little easier for us.

WILLIAMS: Now about that?

CORDING: Is that what we're talking about?

SPEAKER: No, no.

CORDING: Well, we can work with this and then, if need be, come back after the break and get more detail. But, I think, we're interested hearing some of the other approaches as to how people are--on these approaches, what you feel are the best ways we can get at the flux issue. Some of it is a matter of continuing what we have begun on. Some of it is some new things, perhaps. Or, perhaps, some comment from the panel on this.

BODVARSSON: Let me make one comment about this list to Don before we start here. All of these methods are indirect methods and all of them are very uncertain methods. Therefore, I'm not sure a vote of confidence of one or more of them is very useful. But, combined together, they indicate similar things; therefore, they become much more credible as a joint thing rather than individual things.

What I wanted to mention just briefly here before I let some of the other panelists talk, I want to explain very briefly this drift-scale test we talked about. The issues that Dennis mentioned is the following. The average percolation flux is extremely important to know because that controls basically how much water flows to the saturated zone. But, really, a key thing is the flow into drifts because that controls the humidity conditions around the canisters, corrosion procedure, various mobilization procedures, and all of that. It so happens and I think most of the panelists will agree with me that we don't have a very sound theoretical background to determine if you have an open fracture, how much of that water is going to flow into the drift and how much of it is going to flow around the drift.

So, what DOE is trying to do with these tests is to do active and passive tests. The active test is actually to introduce liquid water and dye on top of the drift and see how much flows in and how much flows around it. The passive test is actually to close the drift and just monitor what happens in those drifts. This is one approach that they are doing with this additional funding.

LANGMUIR: So, nothing additional is planned in terms of trying to reduce uncertainties among the individual techniques that are being wrapped up together to give you an average infiltration here?

FLINT: There are actually a lot of things on that list of, however many, seven items that you saw earlier. There are quite a few things that we're doing in the tunnel that relate to individual parts of those. For example, one of the things that we're doing is looking in the north ramp and in the south ramp at exposures of the PTn. This year, we're going to go into the south ramp and take about, I think, 40 core holes, two meter long core holes, to collect data for chloride mass balance, for other geochemistry for things like tritium or chloride-36, for detailed water potential measurements and water content measurements, details that we could not get any other way than to be in the tunnel itself. We're doing 20 core holes in the north ramp also in the PTn, looking at the same kinds of things there.

One of the questions was about whether or not we have any related chloride mass balance calculations. If you look in the conceptual model of infiltration report, you'll see a table that shows the infiltration unit, overlying infiltration unit, over where June Martin had made calculations of chloride mass balance, and the relation was very, very good between those two.

What we've never had in any of those measurements was an area of high flux. Some of the areas we talk about from the infiltration map are the high flux zones. We now have the ability to go into the south ramp and go directly under a high infiltration zone. Even though it may be on a hillside where we couldn't get a drill rig in before, we can now go in and collect evidence such as these kinds of samples. But, we put all that together and that's where a lot of this new testing for chloride mass balance is going to come in is from a lot of this work we're trying to do; but, to get detailed measurements.

We're able now underground to measure water potentials which we're measuring, which is extraordinary to me, on the order of a tenth of a bar to two or three-tenths of a bar. We get in right after the TBM goes in. If some of you have been there recently, you've noticed a whole bunch more sheets of plastic protecting the rock for geochemical samples. So, we're taking an active role in getting the best measurements we can as soon as we can before the effects of evaporation takes its toll on the near surface where we can get access to.

ALLEN: This is Clarence Allen. Can I make a comment here? Within the next 26 hours, this Board has to formally decide whether or not it wants to continue to push for an east/west drift. Anything you people could offer, pro or con, would certainly help us. We'd like to have all the information that we need and we would like your input--I realize there's an--we'd also like your individual opinions. It might help us.

FLINT: It wouldn't get you fired?

WILLIAMS: No, it won't. They're welcome to their own opinions. Like I said, we live dangerously here.

BUSSOD: I'll take a chance. My name is Gilles Bussod, Los Alamos. There's an issue here. The program given its real context has to make prioritization decisions. What we're weary of, a lot of us in terms of the science, is that when you decide to continue a massive drilling program, such as the TBM today, it has seriously impacted the science programs that are supposed to go behind it. They have not always been able to go behind it because those are highly costly. The east/west drift is a mixed bag. There are many things, I think, that we could do with the east/west drift, but we do have some realities to face. I, personally, would say that the east/west drift, at the expense of other things that we need to do scientifically and we can do in other ways, might not be the favored way to go. Being interested in transport, Los Alamos particularly very much--how do you say--concerned about the issue of radionuclide transport beneath the repository. There are several very expensive solutions that we would be interested in to go to Calico Hills. However, we would rather do something, for example, to validate our tests rather than push for something that will essentially allow us to drill, but give us no money to do the test that was the driver for that. That's my personal opinion.

CORDING: We've talked to people in the last years. Is it going to affect my program and to what--you know, will I be able to do the things that I see as important for evaluating Yucca Mountain. So, I see it as a very real concern that others have expressed, as well, regarding various aspects of the program and things that do take more resources.

I guess, the other question is there's still a question there as--because at the management level, ultimately, they have to determine these things, but at what other--do we see benefit in the east/west crossing and can you separate yourself from saying it's going to affect my program. If it did not affect your program negatively, you know, that's another way of looking at this and say what benefit could it have?

WILLIAMS: One of the things I'd like to interject here is we didn't come here to specifically enter into the dialogue of the validity of having an east/west drift. And, I will say that this particular presentation is not set up that way. However, if we want to do that, I welcome the opportunity and we have supplemental visuals that we can use to discuss some of these points.

CORDING: Well, I think our first focus here is what hydrologic uncertainties are we dealing with? How do we deal with those? I think, part of the aspect of this has to do with how one gets access to evaluate those uncertainties. And, I think that's one of the things that Bo's been talking about and what you're showing here. And, I think we'd like to try to work from that perspective, and I don't think it hurts to discuss the east/west drift. I think our intent has been to discuss it in terms of--or discuss all these issues in terms of what we can do to reduce hydrologic uncertainty.

LANGMUIR: Can I try to make a connection here? I've been trying to do this. Let me do it, will you please?

The point is, I think, an issue we are concerned about. Why don't we roll it into the infiltration issue? The point was made by Alan that a lot could be done and should be done and it sounds right to me. The bottom line on infiltration is what's going to get into the repository? So, if you can make your measurements of moisture entering the repository horizons, you really--that's the bottom line. Now, you're doing that now and you'll have more resources to do more of that apparently coming up. Do you think you'll know enough about infiltration as measured at repository horizon levels from the existing ESF to be comfortable that you know enough about it to describe it for the whole repository block which you're not going to be in or would you want to measure those things from an east/west crossing to get a better handle on it before you were comfortable with it?

DOMENICO: Before you answer that, I don't think it's fair to put DOE in this position. They came here to this session to discuss hydrologic uncertainties, not the east/west crossing. And, if we want to talk about the east/west crossing, we should have set up a panel to do it. So, I don't think it's really fair to lay this on them when they came for one other specific thing and got all these guys together specifically to discuss the hydrologic uncertainties, not the east/west crossing.

LANGMUIR: Why don't you sit on that side of the table?

ALLEN: Well, all I did was ask for comments. It would help us in the next 26 hours.

FLINT: I'd be glad to make a comment on the east/west drift when I answer the other question. When I look at how to--one of the things that I want to do and one of the reasons that I'm real interested in working underground right now is that I have the ability to go under and see what the effects of what I think are high infiltration rates at the surface. I can go to parts of the PTn and get samples that I couldn't get any other way than from there because I couldn't get a drill rig above it to drill that particular hole. One of the things that we're trying to address right now is the question of where the perched water came from. Chloride somewhere on the order of 8 milligrams per liter in the PTn above it, under a thick alluvial valley, we get more like 80 milligrams per liter. So, now, I have the ability to collect a sample from an area underneath a ridge and put the test to that data to say will it say 8 milligrams indicating that the perched water actually came through the PTn under high infiltration zones or under these high infiltration zones will it say 80? A real definitive test, I think, in some ways to address one of the big questions we have. But, when I start to look at how I'm going to go after percolation flux, the first place I want to go is in the PTn. All the water that infiltrates in goes through the Tiva, goes through fairly fast. That's what the bomb pulse signatures tell us. In the PTn, it's in matrix flow; more in matrix flow, more of our methods are able to capture the information that we need to capture from the PTn. So, I'm more inclined to look at the PTn. Because once it gets through the Tiva, it all has to go through there eventually, and if there's lateral diversion, I'm working on that right now.

And, I think I said this last time when we asked about the east/west drift. I said I wouldn't mind one as long as it was vertical in the center of the repository and we collected core. That's what I thought; an east/west drift would give us more information. My personal belief is the highest flux zones at Yucca Mountain, at least in the repository area, are in areas that we haven't looked at yet. They're in the center of the repository where the high permeable Tiva Canyon overlies that area under thin soils. And, I think, right in the center of a repository is probably the highest flux. We have to investigate that area. No question in my mind that we have to investigate that scientifically. The first approach I would take would be a vertical borehole where we collect core all the way through the PTn, all the way down to the Calico. I think that would give us more information about the high flux, if there is, in the repository.

LANGMUIR: Can you reach something with a vertical hole that you're confident you've--

FLINT: Yeah, the PTn.

WILLIAMS: One of the questions I asked Alan because he gives that explanation to me. I asked him, I said, well, will you see very much of the PTn in an east/west drift? And, I think if you can envision the cross-section of the mountain, you will see as we go west with an east/west drift, we get further and further away from the PTn. Things are dipping to the east. The drift is going out basically flat. We're getting further and further away from that particular unit that is of most interest to him.

FLINT: There are certainly some things that I think that we can attack with where we are now. One of the, I think, most exciting things we're going to be doing are these two niche studies that we have that are going to be starting this year. One is going to be a monitoring study and we've located one of them at around Station 3566 which is near the Sundance Fault and near another major cooling joint which has bomb pulse chloride-36 all over the place.

So, one of the first things we have to look at in terms of an uncertainty is a fast pathway which we haven't gotten to yet. A fast pathway, a high flux pathway. When we go in there and we set up this monitoring station, we might find that this zone which we know has fast flow may have the same signature or water potentials and saturations that another area that we go into has where we know that there is no bomb pulse. You know, my view of this is that once you get below the PTn, the water travel time is the same. The difference is whether or not right below the PTn you have bomb pulse signature in the water or you don't. But, I don't think there's any difference in the flux. That's something that we can test with this new set of data that we're going after. In these higher flux zones, are we going to see high flux zones and are they wetter? That's something that when we go through and work together on taking these systematic sampling that June wants to do for chloride mass balance, if I'm right in there behind putting instruments in, am I going to find high saturation zones and low saturation zones? Over long-term, if these are fast flux pathways, those zones are going to exist. They're going to be wetter. That's just the way it's going to happen. An,d if we can identify those, we'll be doing real well.

In the last couple of weeks, we've gathered some really interesting data on the main drift. WE have gone through and systematically sampled the rock matrix properties and found that there are huge differences from, I think, 7 or 8% porosity to 14% porosity over 40 meters. And, we can see a very interesting zone that has real changing properties. It correlates very well with this fractured zone. So, there's lots of things that we can gain from what we have now and we should be taking advantage of all that we can do in the main drift and in the north and south ramps. I think there's a lot of effort that needs to go in there before we go on to somewhere else.

CORDING: I think the idea of getting out and sealing drifts to me is really very key to looking at the flux. One should start to see along some of the fractures--should start to see some infiltration if we have some fast paths. Now, the fast path that may have put bomb pulse chlorine somewhere in the last, what, 50 years, 40 years or whatever, it may not be the one that's feeding all the water this year or over the last five years perhaps that's causing water to come in.

I think one of the concerns I had and we've had some informal discussions at break time is will a short drift give you enough so that you can start to get a feel for the distribution on fractures? You could go down 20 meters of drift, 10 meters a drift, 100 meters a drift, and you can see--you know, you may or may not see some of those features. And, I think the matrix characteristics are one thing, but what is the drips or the leaks, whatever we see? It seems to me if you have a sealed up drift, it's going to start to show flows. I mean, if 5 millimeters of flow is being concentrated on fractures every 20 meters or so, you're going to see, what, a liter a minute coming in or something like that. So, one should start to see those features and I think it takes a fairly large amount of drift to be able to differentiate among features that are obviously widely spaced.

So, that's one of the issues that I think that I'd like to hear your thoughts on. I'm very much in favor of sealing off as many drifts as we can.

BUSSOD: Can I clarify? Last year or thereabouts, we knew absolutely nearly nothing about this system. Now that we've matured our studies in terms of environmental isotopes, combining that with mineralization studies and pneumatic data measurements and all that, what we actually have found out are that all of these studies are converging to give us not only a notion of what the pathways are through the system above the repository, i.e. the fast paths through fractures/faults, but also matrix. The models using laboratory data on matrix hydrologic properties are telling us that the volume going through the fast paths is minor, but that it's getting there. The mineralization study that Zel Peterman and Jim Paces are leading from the GS are telling us that where we do not have bomb pulse chlorine-36, we have evidence of mineralizations that are 10 million years old and that have been there for a long time, i.e. a continuous type deposition. That means that these pathways and this is a GS interpretation right now, these pathways are stable pathways through time. They have not been clogged up and restarted. If you match these two things, what you have is a very much bounded system on how the mountain is behaving.

Now, in many of the questions we're asking and taking for granted, in a way, we couldn't even dream of asking last year. So, I'm just saying this because I think we need to recognize the enormous progress that we've made here. And, if we were simply to continue along this pathway alone and then combine some of the other tests that are being proposed by the program to add to the confidence of those interpretations, I think we are well on our way to actually bounding flow paths through geologic time in the mountain system. The next inference is climate change and how that might affect it. But, again, if it is true that the mineralization zones that the GS is seeing are 10 million years old, they are already telling us part of that story.

KWICKLES: I'd like to make a comment on what Gilles said. I don't think we know at this time whether the zones with a lot of secondary minerals in the fractures represent zones of high flux or low flux. They may represent areas where the flux was low enough and the water is more concentrated that it led to mineral precipitation, and it may be that the barren areas, the areas devoid of calcite, represent higher flux areas where the waters were more dilute and moved so fast through the mountain that they left a smaller accumulation of secondary minerals. So, we really don't know in terms of the implications for flux what the mineral record means at this point.

But, as Gilles mentioned, whatever it means in terms of fluxes, we know that the basic pattern has been relatively stable over time or we would expect a much more uniform distribution of calcite as the high and low flux areas swept back and forth across the mountain. So, we don't know what it means in terms of flux, but we do know it means the flux pattern has been stable.

DOMENICO: Is it flux that controls the mineralization or lack of mineralization or is it the velocity? I was under the opinion it was the velocity.

KWICKLES: I don't think we know what's controlling the deposition. You know, the two competing hypotheses right now are release of CO_2 by the percolating waters is causing the carbonates to be precipitated and possibly a minor amount of evaporation due to vapor diffusion along the geothermal gradient is leading to an additional component of the opal being deposited.

DOMENICO: You know, the--number incorporates a velocity and that's a tendency for reaction to a tendency for transport. In of the models I've seen--the mathematical models indicate that the higher velocity intrudes upon the kinetics and the distance to saturation gets longer which means you don't precipitate.

KWICKLES: Right.

DOMENICO: I've never seen it related to flux, but I have seen it related to velocities.

KWICKLES: You're right. It could be that--well, you'd also think that the velocity is somehow proportional to the flux.

DOMENICO; I don't know about that. You know, it's like Alan said. Those fast pathways may not be large flux pathways. There's a large velocity, but not necessarily a large flux.

LANGMUIR: I can't imagine kinetics come into precipitation of the carbonates and silicates if they're millions of years old. The rates are so much faster than that. There isn't much of a--

KWICKLES: No, I think the idea here is that the water gets from the top of the mountain to the Calico Hills, you know, weeks or months following a rainstorm and that the water has moved fast enough that the release of CO_2 doesn't keep pace with the geothermal gradient.

LANGMUIR: Yeah, I guess, I was thinking about the precipitates being dated for uranium isotopes, the older matrix flows where the kinetics can't be an issue, I wouldn't think.

KWICKLES: One of the observations they made in the drilling of some of the early gas sampling is that the initial samples taken from the Calico Hills had CO_2 concentrations that were the highest of any up there at Yucca Mountain. And, to me, one scenario that might explain this is that that water that hadn't moved through the soil zone and acquired its CO_2 content moved so fast through the Topopah that it never released that CO_2 and it arrived at the Calico Hills and only after arriving at the Calico Hills did the CO_2 begin to--from the solution. So, when we sampled those holes, we encountered this anomalously large concentration of CO_2 gas which was many times more what we sampled from some of the instrument boreholes. So, I think it's related somehow to exolution (phonetic) of CO_2, and in fast flow paths, it just can't do it fast enough to allow carbonates to precipitate.

FLINT: I wanted to add one thing there and some interesting information that came out from June's work. When they looked at all of the places where they sampled for chloride-36, they found calcite in half of them. Of all the places they found bomb pulse, they found chloride in 95% of them. So, statistically, there's a good indication that where you have bomb pulse, you have the calcites.

BODVARSSON: Yeah, I wanted to address a little bit your question about the length of the niches. To get at that question, a lot of us have been thinking about how to go after the percolation flux in the flow into drift because it makes a huge amount of difference. PA tells us if the flux is 1 millimeter per year or 10 millimeters per year. It really depends a lot on if we have to do a lot of engineered barrier work or not. So, this is a really a--to start with.

Now, if we take a look at where we are at, we have infiltration estimates at the surface that are estimates that are very difficult to quantify. And, I would be the first one to agree with that. So, my thinking is a little different perhaps. I am no very much in favor of a lot of PTn studies myself. Why is that? Because we can only access the PTn away from the repository. I'm interested in where the repository is supposed to be; where the infiltration is supposed to be at the repository rock, not close to the Bow Ridge Fault. So, therefore, I have always been in favor of niche studies in the repository. And, I certainly agree with you; if possible, I would like to have it 10 meters or 20 meters or 30 meters. We have it now as 5 meters because of cost. We want to get some information before the Viability Assessment and, therefore, it cannot be very long.

Personal opinion--and these gentlemen may disagree and that's only healthy--is that we need to go into the repository block and I see a lot of advantages of an east/west drift in terms of hydrology. The question of if it's going to take resources away from other things, that's for those which are much higher up than me to decide. But, there is certainly a lot of interesting things to go across the mountain because suddenly--thinks the highest infiltration rates are at the surface. So, I mean, we would be fooling ourselves if we didn't say that that would be an interesting thing to do.

LANGMUIR: If you can encourage the tunnel boring machine folks to get out of there 20 days sooner, you'd have your money for an east/west crossing.

CORDING: Let's not get into what the money is that we have available because I think we need to keep it as a technical discussion at this point. I think one of the points is that we--as much as possible what you'd like to do underground looking at the site is things that are not strongly model-dependent. We can fit different models to the same data very often and it may lead us as we extrapolate to conclusions that aren't appropriate. You've all been working very hard. Many of you have been working very hard on looking and trying to find what are the appropriate models.

It seems to me that looking at the flux at the repository level--if one can look at a long drift, it's relatively model-independent. The only real model there at that point is, first of all, where is it located? Is it located in an appropriate location? Can we represent the entire site or other portions of the repository block with what we do? That's obviously always a question. And then, the second is is the drift itself affecting the way the flow occurs? Well, certainly, the drift itself becomes actually a full-scale model of the emplacement drift. And so, there are boundary conditions around the emplacement drift that will be very similar. It would be around an actual drift. And so, if there is flow that's being diverted so that the flux is not--what's coming into the tunnel is a seepage flux rather than the actual flux in the mountain in the free-field, then it's still--it's really giving you the information perhaps that you need. I think going forward and understanding a bit about what happens on the boundary value problem--the liquid release holes, for example, those are things that are trying to tie you back to that and that can be helpful obviously, so that you can tie yourself back to what's coming through overall.

I think that was kind of the thing that I've been thinking is that as much as possible try to get the information that limits your dependence on models and certainly the other information on temperature and things are only going to contribute to all this obviously.

FLINT: One of the things that's really, I think, pretty exciting about this first niche is that these liquid release holes, we're going to do these simple gravity flow experiments prior to the niche being installed. When we mine out the niche, we are going to map out the pathways that the water took going through this rock. So, we'll have an idea of how the water flowed. Then, when we put the niche in, we will know where the fractures are in the roof or in the walls that had flowed prior to that. Then, when we do the liquid flow experiment again, we will be able to monitor those zones and see if the major pathways that existed prior to the niche being dug exist there now. Which gives us an idea of the difference in how the hydrologic system works, how the flow pathways work with and without the emplacement drift being in place or without the niche being there. So, that gives us a lot of information about how the construction actually alters the flow pathways from what we would get otherwise.

CORDING: If we're going to get through four, we need to move on. But, I will defer to Jerry for a comment. He had his hand up.

COHON: Dennis, you were obviously wise to put up the executive summary before you launched through this.

This question is perhaps even more direct than Clarence's and I think it's appropriate. Obviously, you're not going to do any east/west drift or anything as ambitious as that before VA. It's impossible. It's impossible financially. If you were to do it, by the time you got it done, it would not produce data that would be useful to support VA. So, that's clear. But, now, the question posed to you, Dennis, as Deputy Assistant Manager for Licensing, could you imagine submitting an application for a license to NRC or NRC granting a license to go ahead with this facility if you've not done an east/west drift or something similar in scope in terms of actually getting into the repository block? Now, this is Dennis Williams, scientist/expert on this project and knowledgeable about the licensing process. You're not stating the official policy by DOE. I want to hear your professional opinion.

WILLIAMS: It is a term that I seldom use either referring to myself or to others and that's the term "expert". I personally believe that there are none. We are students of this kind of stuff; we all are, even some of us that are approaching the half century mark.

Would I advise my boss, Steve Brocoum, to go to license application without an east/west drift in the block?

I've thought about it a lot. Steve has only had me as his Deputy for a couple of months. But, there are ways that I could do that, that I could be comfortable with that. Pieces of this are in the rest of this presentation. I was trying to get us to the large-scale percolation flux test which is something we've got in planning right now, something we plan on implementing; planning stage in '97, implementing in '98. That's a piece that helps us. If we go to some other things--okay. I'm having a hard time finding it here, SD-6.

One of the things that Alan says is the best thing that he could have is a borehole right in the middle of the repository block that has full core from top to bottom down into the Calico Hills. One of the things that we've discussed on this program from the beginning of time, what gives us more bang for the buck; the surface-based program that goes vertically through a sub-horizontal package or whether we go in with a set of underground excavations that basically run along one horizon?

Now, that was one of the big problems with the shafts that we dealt with in the beginning. Two shafts basically got a good picture of the vertical, but it didn't get a whole lot of the horizontal. We went over to the ESF to get a big look at the horizontal, but really one horizon. SD-6 is what we're putting into the plan. We'll probably drill this in FY97. It's the old SD-6 location. It's been around with us for a long time, part of the systematic drilling program. It's up on the ridge on the west side of the block. 2500 foot drill dried hole, reverse circulation, LM 300 type hole, west side of the repository block; basically, use it to calibrate and validate the 3-D geologic framework model. My geologist, Mark Tynan, screams at me almost every other day. Dennis, we don't have anything on the west side of the block. How can I tell you we've got a good 3-D geologic framework model unless we get something over there?

We've had a lot of problems with coring the section because of the cost associated with it. We're going to rely a lot on geophysics. We're going to rely on sidewalk coring down in the Calico Hills, a formation that this is very effective in. We're doing a couple of little different things on this drilling program. I talked to the Board '92-'93 about LM 300 boreholes that were costing us $1000 a foot. WE were putting $4 million/$4.5 million into these boreholes and they were taking us all year long. We want to get this one knocked in in a matter of two to three months probably for under $1 million. If that works, if the technique works, then we'll expand out and possibly look at other places on the repository block.

If I can get these types of things coming together, the large-scale percolation flux test, some of these other niches that we're using in the existing ESF, a few boreholes like this, some with full core on the repository block, then I can go to my boss, Steve Brocoum, and say let's go to licensing. We can defend this thing. That's where I stand today, that's where I stood probably two months ago when I started working for him. I'm not a cabbage. I can change my mind. But, that's the way I feel about it.

COHON: Thanks.

WILLIAMS: You bet.

COHON: That's very useful.

CORDING: Why don't we move on to the--you had other parts to continue with, Dennis, and I think we can cover some of that. We may want to take a break in the middle of it, perhaps. What do you see as time at this point for your other parts of your presentation? What we're really doing, this is our discussion session, as well. So, we're not quite so far behind.

COHON: Dennis, just let us have a quick discussion about time. What if we just went without a break and finished this up?

CORDING: Let's just continue with your presentation, if you would, Dennis, and we'll try to go for as long as we can stand it.

WILLIAMS: Okay.

CORDING: Let's restate that. If you need to take a break, just take it.

WILLIAMS: What are doing? Are we continuing?

CORDING: We're continuing, Dennis.

WILLIAMS: All right. The second thing I was going to talk about was fast paths. I think I will just defer that because we've had a lot of fast path stuff come into this discussion. And, maybe for the benefit of the saturated zone and the transport, go on into the dilution transport parameters and mixing depth and I have a few thoughts about that.

You heard a lot of that this morning. Basically, what is it? Gilles and the guys have worked long and hard to get me up to speed on what dilution really means. I'll give it a try with the help of the visual here. Dispersion, matrix diffusion, and sorption. Those are the things we saw on the tracer tests outcomes earlier today. So, those are the key issues that they tell me are important in understanding this dilution process.

And, my favorite thing to go back to, of course, is the waste containment and isolation strategy. Dilution is sitting right down there at the bottom, #5, the last defense. And, basically, the uncertainties associated with that are the hydraulic connectivity between the individual hydrogeologic units. We talked about that this morning; the Prow Pass, below that you have the Bull Frog, the Tram, and then, of course, ultimately, you would have the Paleozoics. Velocity is in our saturated zone and the role of the heterogeneities, the faults and the contacts, and sorption and matrix diffusion.

TSPA estimates are conjectural. I'll take that with the nod from Eric.

SMISTAD: I'd like a kinder, softer word.

WILLIAMS: A kinder, softer word possibly, but maybe close.

The ongoing investigations, we had a water table monitoring program out there in the system for a long time. I think those again were some of the--or that's the WT system of boreholes all over the area that gave us the location of the water table. A variety of models we had on our little model diagram on the regional saturated zone flow model, the flow and transport models, different size models covering different sizes of the area, the site and the region. And, of course, we talked quite bit about the C-Well complex, the hydraulic, and the tracer tests this morning.

Okay. What are we going to do to deal with some of these uncertainties? And, again, this is just--the stuff that we've enhanced in '97, the C-Well complex. One of the things that I'm a believer in is if you've got a facility out there, use it to the maximum advantage. If we've been testing the high transmissivity zone in the C-Wells and we've got some concerns about the low transmissivity zones especially because they sit right next to the water table, let's go for it. So, that's one of the places that we're funding and we're telling our folks, hey, get out there and perform some more hydrologic and tracer tests in the uppermost zone right when we go into the water table. Likewise, with some aquifer testing and some of the WT holes, originally developed just to look at the water table. However, it is an open borehole. You can do some single hole tests in it. It's not as good as this, but it could provide us with quite a bit of data. It would provide us with some additional data to understand the system a little bit better. Not a great way to do things, but they're there and for a minimal expenditure, we have the potential of getting more bang for our buck.

So, this is what we're doing as far as supplementing the saturated zone portions of the program in the remainder of '97. So, that's the point to experiment again.

CORDING: Okay.

BUSSOD: Yeah, can I make a point here?

CORDING: Yes.

BUSSOD: I think that the reason the C-Wells testing is so important to the project is that again until that test came to fruition, all we were going on mostly was that the saturated zone was an area of dilution and mostly meaning mechanical dispersion. We now can take credit for the saturated zone being a full and efficient radionuclide barrier in the same way the unsaturated zone is; meaning it also involves retardation through sorption and matrix diffusion. This is not a minor gain for the program. It is something that now we can pursue aggressively. Future testing in the saturated zone can help confirm that beyond this point.

Again, when we address hydrologic uncertainty, this also magically has allowed us to now take credit for the unsaturated zone below the repository being handled. In other words, we recognize that we had at a minimum two pathways through the unsaturated zone; fast pathways associated with fractures or faults and matrix. Even though it's a small volume through the fractures, we recognized that in terms of dose, this may be a problem. Now, we can use the saturated zone to completely mitigate--or at least that has to be decided, but there is a possibility to completely mitigate any fast path problem.

The major hydrologic uncertainty left in terms of what we've done in the program is the unsaturated zone beneath the repository. That's my point of view, of course. Taking the fact that conservation of volume says that the flux that you'll get at the repository will also go below, I would say that if you're considering spending quite a bit of money on doing something, something that reduces the most uncertainty would be a program that tries to look at--for confirmatory purposes even, tries to look at the Calico Hills which we are strongly relying on because of its zeolites on retarding--providing enough retardation for the saturated zone barrier to be effective.

CORDING: More comment on that? Pat?

DOMENICO: First, I disagree with your dilution of lumping all those processes together. Those are all different processes. They all act to lower the concentration. So, macroscopically, they do the same thing; microscopically, it's a different process. Dilution is basically the mixing of contaminant with fresh water. And, you're not taking enough credit for it. I think what you have for the dilution underneath the repository is a correct one. But, what's going to happen when that contaminate gets out? It's going to follow some fractures. If there's a large interconnectivity, some of the contaminant will move along one fracture, water will be removed from this, say, bunch of fractures, otherwise fresh water will be coming in in order for that system to stay saturated. The more interconnectivity you have, the contaminant mass will move out of some fracture, move out into other fractures, and be replaced from underneath or sideways by fresh water. That is dilution and you're not--

BUSSOD: That's fine. I'm only trying to keep to the programmatic definition right now. I agree with you.

DOMENICO: But, the point is how can you possibly take care of the phenomena that I've just described because that's going to be more effective than the way you're doing it. And, incidentally, the slower you're moving the contaminant, the more effective that process is going to be. It's going to see more pore volumes of water by the time it gets to the --

BUSSOD: That's why matrix diffusion is effective; essentially, you have no flow.

DOMENICO: Well, that's a different process. So, if you have a retarded species, you're actually going to get it diluted more.

ROBINSON: These processes though, though distinct--

CORDING: Yeah, let's identify yourselves. We're not always sure who is talking. So, Bruce Robinson?

ROBINSON: This is Bruce Robinson, Los Alamos.

CORDING: Thanks.

ROBINSON: We're now onto the uncertainties that I feel have the most impact on performance and, therefore, I'm speaking up.

CORDING: Sure.

ROBINSON: These processes are, in fact, different.

However, when one occurs to a great enough extent, they tend to become lumped together. Let me elaborate on that. We know that the flow in the saturated zone will be primarily through fractures. However, matrix diffusion at the levels that we see it in the C-Wells experiment are such that in a groundwater system over kilometer distances, the porosity sampled by the radionuclides--which is what we care about, I insist; not water, but radionuclides--the amount of porosity seen by those radionuclides starts to become dominated by the matrix porosity, not the fracture porosity. There's several orders of magnitude difference. So, a matrix diffusion model begins to resemble a single porosity system at larger scales and that porosity, in fact, is the matrix porosity rather than the fracture porosity.

Furthermore, if this model is correct and is applicable not just at the C-Wells, contact of radionuclides with the matrix rock allows one to include sorption of radionuclides in the saturated zone rather than just assuming that the radionuclides are jetting through fractures in the saturated zone. This has a significant impact on performance.

You are correct; there's uncertainties there. We're trying to bound the problem with field data and also studies from other localities in such a way that it's conservative and defensible, but takes proper credit, if you will, for the saturated zone system.

DOMENICO: --fresh water which I consider dilution. I don't know how you would do it because it's a difficult problem.

ROBINSON: It is and the approach has to be one in which we try to bound it on the conservative side. Even bounding it in a conservative way, I believe a 100 meter dispersivity at these types of line scales is quite conservative given that we've seen values almost that large at the C-Wells within a factor of 2 or 3 of that. Even that conservative assumption, I show that the dilution caused by that is a significant factor. It serves to dilute the radionuclides in a manner similar to differences in, say, percolation flux in the unsaturated zone.

So, if you're assessing uncertainties, I believe that there are uncertainties in the saturated zone which when fully entered into this calculational process will result in better performance if some of these uncertainties can be tied down.

CORDING: Okay. Dennis, do you want to move on? Do you have some other things?

WILLIAMS: Yeah. The next one was on hydrochemistry. It's not really--it's an uncertainty, but it's not something that we're able to put a lot of additional effort in it. So, you've got the visuals associated with that.

Maybe to get this kind of closed down, I'll hit a conclusion slide. We'll call the experiment over.

We've enhanced our investigations in key areas through supplemental '97 funding to the tune of about $13.1 million. Again, the hydrologic issues are our main concern. We have chosen what we consider to be the best data collection and exploration techniques to utilize the existing ESF excavations to the fullest extent possible. It's there, it's big, it's got a lot of access, we get to the bowels of the mountain; let's do it. And, we are resuming our surface-based drilling operations to a limited extent.

CORDING: Thank you very much.

Now, we do have time for general comments and further discussions. About 4:30, we're going to be looking for public comments. So, we have some time at this point for general questions and comments from the Board.

LANGMUIR: I guess, this is for Bruce Robinson and coming back to his presentation. I think I already have an answer to the question, but maybe it's worth repeating it here, the answer for you. My sense is that you have a comfort zone and I can see how you've done this with assuming that radionuclides that reach the saturated zone will tend to be reduced in concentration by a series of physical and geochemical processes. And, the obvious ones I think we can agree to are going to be the dilution dispersion and the diffusion processes. I'm comfortable that the conservative tracers give us a bound on all of that, and what you observed in model with their behavior is a good way to bound the individual movement of radionuclides.

The difficulty comes in getting any cleaner than that or any more accurate than that when you come to specific radionuclides. We can discuss that. I think there's a lot more that could be said and done with it. Lithium is not a very good analog for most of the actinides. It's not good, at all, for the actinides, by and large. You might argue that neptunium is somewhat similar. Technetium certainly--forget it; I guess it's just your water basically going through. Technetium is not going to be affected by anything other than the physical processes in diffusion.

Would you disagree with that? I'm not aware that anybody has observed the adsorption of it that's significant in the materials that are in the saturated zone.

ROBINSON: Not with the form that is generally assumed to be retained through the unsaturated zone and oxidizing environment and the saturated zone.

One of the aspects that wasn't gotten to in this experiment was the last topic which was the hydrochemistry. There is an experimental effort that is being planned right now to try to get at the oxidation reduction potential within the saturated zone. Early measurements of this were imperfect, but it indicated that the EH in the fluids in the saturated zone ranged from essentially oxidizing to partially reducing some of the key radionuclides if they encounter reducing conditions such as neptunium which I'm sure you're in agreement with me on, would have a profound impact on performance if one could show that saturated zone radionuclides encounter fluids that are significantly reducing in such a way that neptunium goes from neptunium-5 to neptunium-4. Solubilities just drop to very low values, sorption goes way up; we know this. It would have a dramatic impact on the performance of, say, neptunium which definitely shows up in most PA calculations as being one of the key radionuclides.

So, I think an experimental program that in a sense takes another look at the sorts of geochemical processes that might occur with some of the key radionuclides that are showing up in PA analyses is an important next step in tying this down a little bit.

LANGMUIR: I'd be interested in how you propose to measure the redox state of that groundwater. I'd like to talk to you about that outside.

ROBINSON: It's a tough problem.

LANGMUIR: Yeah, it's very poorly poised. There's nothing much down there to maintain that redox state.

ROBINSON: There is a program. I would have to direct you for details to Aaron Meyer in order to get the details. But, it's a program of attempting to measure both EH and to look at redox couples that would tend to be in a sense confirmatory of the EH measurements and, hopefully, allow you to really hone in on what the proper oxidation reduction state of that fluid is.

BUSSOD: Gilles Bussod. By the way, the same experiment is also being looked at because of technetium which is right at the bounds of its reduction potential; meaning that its Kd which is now considered to be nearly 0 or .1 could be as high as 1000 if it crosses that boundary. We may be just within 100 millivolts of that. So, this is one of the things we didn't talk about that is a planned experiment for this year.

KNOPMAN: I believe that we need to be focusing on the modeling in the unsaturated zone because that's the greatest area of uncertainty, even more so perhaps than the saturated zone. But, I'm wondering if the saturated zone modeling can be used to kind of bootleg into a better unsaturated zone modeling primarily by trying to get a boundary condition for a saturated zone model. That is what the percolation rate or the infiltration rate, if you will, to the water table is.

What could you say now about a water budget in the area underlying the repository block? Can you say anything about what's flowing through; what's just flow and what's, in effect, recharge?

FLINT: I think Bo would have a good answer to that question. Actually, that's one of the big uncertainties that we have to deal with. When we look at flux, we have an infiltration map that goes through the PTn, maybe there's some amount of diversion. If it's a high enough flux, there may not be a lot of diversion. Once it gets into the Topopah, probably more or less vertically downward, maybe some channelization. But, once we get to the zeolitic vitric boundary of the Calico Hills, then we have a big problem because there is a good possibility that there is a tremendous amount of lateral flow, and that the distribution and flux that we would start at the top would be very, very different when we get to the subsurface.

I know Bo has done quite a few modeling calculations. I'll him answer what the actual flux is.

BODVARSSON: I don't know what to say. I thought your question was different from what Alan answered. I thought you said why can't you use the flux with the unsaturated zone as a source term to the saturated zone model, and then let the saturated zone model determine what the infiltration flux is. And, if that was your question, I think most of us believe that the flow in the saturated zone coming laterally is much, much more than what comes vertically from the unsaturated zone. So, certainly, some of us believe that the saturated zone and the unsaturated zone models, there's no need to couple them because they are so vaguely coupled. Because the saturated zone is basically a boundary condition to the unsaturated zone and the flow of water and chemicals as a source term distributed in time and space is a boundary condition to the saturated zone.

KNOPMAN: Yeah, I guess I was suggesting using the saturated zone model to get a better boundary condition, in effect, on your unsaturated zone model.

BODVARSSON: We think that the flow in the saturated zone laterally is much, much more than what comes vertically down through the unsaturated zone, at least some of us.

KWICKLES: One of the things we are trying to do is we are trying to use some of our techniques that we've developed around the Yucca Mountain scale of infiltration, and we're applying that to the saturated zone, regional saturated zone, modeling. We're using that as a boundary condition to see if the methods we've applied specific to Yucca Mountain do help us to understand the regional saturated zone picture. So, we're working on that to, at least, compare the technology at the point at Yucca Mountain to a much larger regional area.

KWICKLES: This is Ed Kwickles. One of the early estimates that was reported in Montazer & Wilson was based on exactly the technique that you described and that turned out to be 4-1/2 millimeters per year which is, I believe, what Alan's last infiltration map has estimated.

CORDING: Thank you.

Other Board comments?

NELSON: Priscilla Nelson. Just curious; there is some Calico Hills--the stratigraphy is not firm in my mind. So, pardon, if I'm not getting it right. The Calico Hills--was generally in the Calico Hills formation and there is generally some Calico Hills above the water table in some places in the repository block area. All of the water in that Calico Hills that's above the water table would generally have come from infiltration from above. Is that true?

SPEAKER: Yes.

NELSON: Could some of it have come up from the water table, maybe?

KWICKLES: The water table is estimated to have been as much as 100 meters high--80 to 100 meters higher in the past based on geochemical evidence from the fracture coatings. So, it's probable that large parts of the unsaturated zone were submerged in the last pluvial period. But, in general, the pore waters have given younger C-14 ages than the underlying groundwater. So, if the C-14 ages are to be believed, the water in the Calico Hills originated in most part from down percolation.

NELSON; So, the difference there between what's up in the Paintbrush--what's the--

SPEAKER: PTn, yeah.

NELSON: The Paintbrush up there and the Calico Hills down below. That's telling you what the net overall, combined matrix and fast path flow, is doing in between these two formations which is the percolation through the Topopah Springs. Is that true?

KWICKLES: The question was does the difference in the estimated fluxes between the PTn and the Calico Hills tell you what the fracture flow of Topopah Springs was?

NELSON: It's as if this material in between the Paintbrush--what is it, Paintbrush?

CORDING: Nonwelded Paintbrush, tin roof, with a few leaks. One of the problems is that we really don't know what the flux is in Calico Hills. That's unknown.

FLINT: Right. The easiest way to think about it is, if you really want to think about it, just think about four units; the welded Tiva on top, the nonwelded Paintbrush next, the welded Topopah, and the nonwelded Calico Hills, and then the water table. The Calico is mostly above the water table under the repository. The Calico is broken into two parts, a vitric part and then a total zeolitic part. The zeolitic has an extremely low permeability compared to the vitric; probably, four orders of magnitude. What goes through the Tiva at flux probably goes through the PTn. What goes from the PTn, if we don't have lateral diversion, goes through the

Topopah. Then, the pathway at the bottom of the Topopah, top of the Calico Hills, how that gets in the water table, where it gets in the water table is a very large uncertainty. Because of lateral diversion, there's likely very much lateral diversion on top of the Calico. So, when you look specifically in the Calico Hills, that water may not be a reflection of the total infiltration because it may have all gone sideways at that point. That's one of the big problem areas.

NELSON: Do you think that the perched water is left over from last pluvial or is it--

KWICKLES: No.

NELSON: No.

KWICKLES: Most thoughts is that it's from two different sources; one, infiltration near or right above it and the other is possibly from some of the faults nearby.

BUSSOD: We disagree. And, it's model dependent and it's a non-linear complex model. It is. It is a non-linear complex problem, and the only way to get at that question is try to see if we can with our coupled process models and using the entire database which includes isotopic ages, water chemistry, et cetera, and hydrologic systems if we can bound that question. But, there is disagreement on that.

CORDING: Thank you.

DOMENICO: Did you measure such a potential at that contact you said where you may get a lot of lateral diversion? Have you had any measurements down there of the potential?

FLINT: Right now, we have about, I think, 10 locations where we're measuring water potential. We have two locations specifically; one about the base of the Topopah and the other one near the top of the PTn. And those are pretty much the same; around a tenth of a bar, a third of a bar.

DOMENICO: So, you don't have any measurements where you believe that lateral diversion has taken place?

FLINT: Oh, okay. No, we don't--basically, what we know of that zone is that in the Calico Hills, that--the whole Calico Hills is, more or less, saturated on near-saturated. Although it's above the water table, the water potentials are probably on the order of a tenth of a bar or wetter.

DOMENICO: Okay.

WILLIAMS: We went to the truck and pulled out this overhead. It explains the previous discussion.

CORDING: This is the visual that Alan was referring to.

WILLIAMS: Right. Probably not one that he would choose, but probably one that we could work from.

CORDING: Visually, it might clarify it a little.

FLINT: That's a little too complex.

CORDING: But, of course, that's what we're looking at. I mean, in the past year, we've had some interesting theories or concepts about the way the mountain worked. And, we're really in the process of trying to evaluate that and explore that. And, it is a real pleasure to hear all the things that people are doing and all that's being learned about it and the things that DOE is considering here because I think we really are learning a lot about it. We see there's more to be done, and we appreciate your participation with us here today to help us understand where the program is and what the plans are.

At this point, I'm going to turn the meeting over to our Chairman, Mr. Cohon.

WILLIAMS: If I might add a credit on this. This is a takeoff of Montazer & Wilson of '84. And, Parvis who gave one of the earlier discussions was instrumental in this back in the early '80s. It still seems to be valid to some extent today. So, a lot of insight back then.

Thank you.

COHON: Thank you.

I am learning continually in my new role as Chairman. One of the new responsibilities that I just discovered before is Chief of Lost and Found. Earlier, it was taking care of automobiles in the parking lot. We have found a unique button. If the owner is still here and has lost it, I'm sure he or she will want to claim it. It says Hugo Boss on it. So, everybody please check your clothing and see if you lost a button. If you did and you want it back, Helen is the person to see; she's got it.

We'll turn now to the public comment period before we close. This is very important to the Board, and I'm glad so many people have stayed for it. This first commenter could easily have been included in the previous session, but I made an executive decision not to include him in the previous session because it would have probably prolonged that session for another hour. Instead, we've put it into the public comment period. That's Steve Hanauer who would like to set us straight on the east/west crossing.

Again, please, identify yourself again and your affiliation?

HANAUER: Thank you, Mr. Chairman. My name is Steve Hanauer. I'm a DOE employee on the program director's staff. What you're going to hear is my opinion.

I have not formulated an opinion of when the east/west drift should be accomplished, and I suggest to you an alternative way of thinking about it and that when is the only question to be asked. Let's remember that the viability assessment, the site suitability, and the license application we are talking about are the first steps in a much longer sequence of events. If the project gets a construction authorization, then we're going to go down and dig a bunch of stuff including a whole bunch of east/west drifts and a peripheral drift and all that other stuff you see in the design.

So, the issue is not one of safety. There won't be any waste emplaced before there are lots of east/west drifts. The issue is risks of time and money. We have to decide this first in the DOE. You will render your advice on this question. The NRC has to decide whether they need the information from an east/west drift to grant a construction authorization or whether there's enough information without it. But, there will be plenty of east/west drift information available before the decision has to be made maybe by our grandchildren whether to emplace waste in a repository if there is such a thing.

Now, what is this time and money risk? Well, it's a balance and this balance is very difficult. If you do too little site characterization, then the world--meaning the DOE, the Technical Review Board, and particularly the NRC--will decide there's not sufficient information for a construction authorization and maybe a satisfactory site will thus be rejected. On the other hand, if you do too much site characterization, you will spend so much time and money that everyone gets tired of the project, as very nearly happened a couple of years ago, and that's another way of rejecting what might otherwise be a satisfactory site for a repository.

The issue, I reiterate, is a question of risks, program risks, time risks, money risks, but not safety.

COHON: Thank you, Mr. Hanauer. Thank you.

Don Langmuir?

LANGMUIR: Speaking for myself, but on the Board, Langmuir. I think our concern is that, yes, wonderful, there will be some east/west drifts as part of repository construction, but they will not be in place to measure anything. That's our concern is the need to measure the properties of the system across the block, particularly the hydrologic properties, the fluids, the fluxes, this sort of thing. You will discover those things and they'll fall into the drift, but you won't be equipped to measure them or learn from them.

COHON: Thank you.

Hal Rogers?

ROGERS: Thank you. I'm Hal Rogers, Co-chairman of the study committee. I want to go back to yesterday, if I may, and talk about the Sandia crash tests. They were referred to yesterday as a propaganda effect and they were not; they ended up being used as, say, propaganda item, but they didn't start out that way.

The purpose of those crash tests was to demonstrate the validity of engineering analyses and scale model data and to gain quantitative data under extreme accident conditions. The early preliminary testing was done in 1975 when two obsolete casks were dropped about 2,000 feet to undisturbed soil. Number 1 impacted at 246 miles per hour which is about equal to a drop of 30 feet to a hard unyielding surface. There isn't any such thing, but if there was. Number 2 impacted at 230 miles per hour and in both of those there would have been no release. Of course, the angle could not be controlled. They were free fall. So, that test was considered just a preliminary.

I have a couple of pages that I will skip in the interest of time. The first of the real Sandia tests, the tests were configured as they would be for normal use; impact limiters, mounts, and so forth. The first truck cask was impacted--this is on 1-18-77--20-1/2 ton cask with normal transportation tie-downs, head forward, with balsa impact limiters. It contained a Savannah fuel assembly plus weights to simulate its normal load. The truck impacted at 60.8 miles per hour. The cask impact was 28 miles per hour, 20g. The fuel was undamaged and the cask was okay, also.

Number 2 was impacted on 3-16-77, the same as #1, the cask with fuel, water, and so on. The truck impacted at 83.8 miles per hour. The cask impact was 65 miles an hour. Now, that is the equivalent to a free fall from 140 feet. Cask damage was moderate. There was minor seepage from the cask head seal; about two drops per minute for a total of 100 cc's. The fuel was deformed, but there was no clad failure.

Number 3 test was on 4-24-77, a grade crossing test, a 25-1/4 ton cask. It was impacted by a 208 ton locomotive. It was a glancing frame impact. It had the same fuel load, but this was a dry cask. There may have been a small leak in the head seal, but it was not considered significant. Fuel, there was some rod bowing, but no clad failure. There was no breach of container and there would have been no public risk if it had contained radiated fuel. Incidentally, that locomotive hit at over 80 miles per hour. A regular broadside hit at 80 miles per hour.

A Yankee Roll rail cask about 70 tons with standard frame mount was impacted at 80 miles an hour and then submitted to an engulfing fire. That is the cask was actually suspended over JP-4 and it was set fire; 1475 degree fire radiating temperature. The test was terminated at 90 minutes. All the lead in the cask was molten, as predicted. And, this was scheduled to be the end point for the test. The fuel to the fire pit was turned off. The fire burned another 33 minutes at about 100 minutes of smog. A crack in the outer cask stainless steel shell occurred about six inches long by 4/1000 of an inch wide which is about the thickness of a dollar bill.

COHON: Mr. Rogers, excuse me, I'm sorry to interrupt, but I feel I must. If the remainder of your comment is additional details on crash tests, first of all, let me say I think you made your point. Second of all, if you do have more details, you could always submit those in writing. If you would have other points to make, I encourage you to make those.

ROGERS: All right. I do have more details, but we won't go away with those.

COHON: I am serious about submitting those, by the way.

ROGERS: Yes. My point is that some of the Board and especially maybe the new members aren't aware of those crash tests or what they were or why we had them. I think that if they haven't seen those films, they ought to before they get involved in some of these transportation of spent fuel.

Thank you very much.

COHON: Thank you, Mr. Rogers.

Jerry Szymanski? We now know how to spell your name. You don't have to spell it again.

SZYMANSKI: Well, thank you very much.

COHON: Thank you.

SZYMANSKI: What I actually would like to convey to the Board for comments which I registered over these two days, first of all, it was very gracious of you to allow us, the lay public, to speak. Thank you very much and I'm pretty sure I'm speaking on behalf of all observers here. So, thank you very much.

The second comment, it seems to me that our biggest uncertainty pertains to a site suitability. In order to address this, we have to get deeper with what is it? I was very perturbed yesterday what I kept hearing from the Chairman of the Board. Well, we don't know what that is.

To make my third comment, I would like to ask Dennis to put this last viewgraph which is Montazer & Wilson, and I might try to shed some light on what suitability might be. Although we have a model, it expresses certain concept of what we have about this site. But, there are two unspoken parameters inside this thing. The one is basically what is resisting the flow which is a conductivity structure, three dimensions. Now, another aspect which we know is important are the boundary fluxes. Again, I mean, boundary fluxes in three dimensions. My uncertainty pertains to essentially conductivity, how it is distributed first in space, and the second, in time. Will it remain what it is today or will it change? The same thing pertains to boundary conditions.

Now, we do know that reasonable inferences can be drawn that this site is underlined by two--instabilities. One sits inside Solitario Canyon Fault, another one in the Paintbrush. Now, there are instabilities. It's a non-equilibrium motion. Such a system cannot possibly by its very definition be stationary; it must be fluctuating. When it is fluctuating, it means that the boundary conditions are changing in time.

Now, there are two--uncertainties which I haven't heard addressed for the last 17 or 18 years. It seems to me that they directly feed into our business of suitability. My first comment is to recall once I've been at the Nevada Test Site in '89, I think, and BBC was filming our debate. What's this rock site all about, the actual evidence. Well, I wanted to finish this filming, and I would also want to finish my commenting with some light talk here. And, at that time, I described to John Stuckless a situation which I had observed in England.

Now, imagine a highway and a railroad crossing. We are traveling on the highway approaching railroad crossing. Along the road, there was this English gentleman walking with a Golden Retriever. Well--and the first car very suddenly stopped, the second car hit it, and we stopped a few yards behind. Well, the English gentleman with the dog wanted to assist people in the first car. Well, he tied the dog to the pole and he was assisting. Now, the train went by, the pole went up; you can imagine what happened to the dog.

That's our question pertaining to--conditions, conductivity structures. Where is this stupid range. Where did it come from? I wish Board would be cognizant of these uncertainties. Why? My understanding is that this will become a Court issue of Nevada notice of disapproval and we will investigate that, under oath, what are the facts.

Well, thank you very much once more for your being so gracious.

COHON: Thank you, Mr. Szymanski.

Are there other comments?

(No response.)

COHON: I understand the cookies are gone which can only mean one thing. It's time to end this meeting. I've also been informed that in the course of two days, we have consumed over 1,000 cookies, believe it or not, which I'm also told is the new indoor two day record for a meeting in Pahrump, Nevada.

I want to convey the thanks on behalf of the whole Board and our dieticians and weight loss counselors. Our great thanks to the people who provided the cookies and the coffee and the wonderful hospitality. Actually, it's a pity that the new Board members started off this way because you're not going to see anything as welcoming and hospitable as this. Our thanks to the people of Pahrump and Nye County.

I want to also thank the people who record this meeting. That's no mean fete. If you think it's hard to sit there for two days, think how hard it is for these two gentlemen. John Stout, who does the AV who works the microphones, and it went extremely smoothly, thank you; and, I have no idea how he does what he does and continues to stay coherent, Scott Ford, the court reporter. It's quite remarkable. Thank you.

And, to the three people on our Board staff who have most to do with arranging this meeting, Helen Einersen, Linda Hiatt, and Mike Carroll. This is not an easy meeting to arrange and to pull off and it went remarkably smoothly. We thank you very, very much.

Finally, most important of all, our thanks to all those who came so far to inform us and help us to understand better the challenges that present themselves. And, our thanks especially to the people who live in this area who gave up two days of their lives to be with us to help us to understand better their views of this. It's very important for the Board to do this. You confirm for us the value of doing that.

I declare this meeting adjourned. Thank you very much.

(Whereupon, the meeting was adjourned.)