UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

HG&G PANEL MEETING
GROUND-WATER TRAVEL TIME (GWTT)
and
FLUID-FLOW PATHWAYS

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Las Vegas, Nevada

BOARD MEMBERS PRESENT:

Dr. John Cantlon, Chairman, NWTRB
Dr. Edward Cording
Dr. Donald Langmuir
Dr. John McKetta

CONSULTANTS:

Dr. Patrick Domenico

NWTRB STAFF:

Dr. William Barnard, Executive Director, NWTRB
Dr. Carl DiBella, Senior Professional Staff
Dr. Victor Palciauskas, Senior Professional Staff
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

P R O C E E D I N G S

8:30 a.m.

DR. DOMENICO: Good morning. Welcome back.

I admit, I prefer one o'clock starts than eight-thirty. Eight-thirty is more appropriate for funerals than meetings, but, let me see, I have a script here someplace.

Let's come to order. We have two presentations concerning the ground-water travel time in this session. One is a commentary on the problems with the present criteria and the possible alternatives. The other presentation is State of Nevada comments concerning the present ground-water travel time criteria.

Following these presentations, and a short break, we will have a round-table discussion on ground-water travel time related issues. It really won't be a round table. We'll maintain our normal position here.

Our first presentation is by Paul Kaplan, from Sandia Labs. Yesterday, we saw some of the inherent difficulties of the ground-water travel time criterion. One difficult is, clearly, the inherent complexity and technical nature of the solution to the computations and interpretations, and one question is, is the original intent of the criterion lost due to this complexity.

Well, Paul is going to give us an overview of an approach to a possible alternative criteria--and, by the way, Paul, welcome back. I thought you'd demised. I haven't seen you for ten years.

DR. KAPLAN: I'm going to talk this morning to the concept of an alternative. We're going to go through five different parts in the talk. Very quickly, we're going to review the intent of the GWTT Criteria, although not in the legal sense, as was done yesterday; discuss some of the problems with the current criterion. We're going to set some rules, before we discuss what might be a possible new criterion, or how we might want to do that, and then summary and conclusions.

This is a personal view of what I think was intended when the criteria was set. There's nothing in the discussions that occurred yesterday that has changed my mind on this.

I think, first and foremost, it was intended to be a simple measure of performance, very clearly independent of any engineered barrier or anything we would do in terms of construction of the site; ambient system/natural system criteria, and this is important.

Over the intervening years, concepts of risk have become much better articulated with respect to setting regulatory criteria and establishing a framework as a basis for a decision. At the time this was written, my feeling is what people were trying to do is set an intuitive surrogate for risk. If we answer this question, which may not appear to directly address our concerns, human health and safety, others, it's going to do it implicitly.

But the real issue, I think, is the question of: Is it safe? That's the question that's on the table, and I've thought of many different ways over the years how one might want to ask the question: What is NRC asking us to do? What are they asking for proof of? And I think one of the ways of saying it is, imagine the NRC coming to DOE and saying, "Dear DOE, again, given that you are planning to dispose of radioactive waste at Yucca Mountain, what assurance can you offer the public that the natural system offers adequate protection?"

Now, I'm going to build the rest of the presentation on the assumption that this is the question that's on the table.

What are some of the problems with the current criterion? I think one of the most important problems we're facing is that multiple interpretations are possible, and what's even worse is, those interpretations are different depending on who the stakeholder is.

I'm sure Carl is going to give us a very different interpretation of ground-water travel time, our ability to meet it, than we had yesterday.

Current criterion is actually independent of quantity. We sat here yesterday morning and the NRC said, "Here are the things we're truly concerned with, in addition to rate; quantity, geochemical, environment, geometry, but since all these things are site-specific, we're not going to ask you the question we want to ask you." So they said, again, a surrogate, but our concern has always been on the part of technical staff is that you can phrase the question in such a way that you do not answer the question, some of the ones we've posed on the prior slide.

George Carlin, the comedian, basically described the situation we're in right now as "vuja de." We've all been here before and we know it. We have not discussed anything, to date, that wasn't being discussed my first week on the project eight years ago. The fact that the debate has gone on this long is a big hint that there's something fundamentally wrong.

Another problem, again, depending on how you interpret it, it's independent of consequence, and we heard yesterday, we heard the word "dose" come out. What we didn't hear was the word "effects," but somebody did say, "human health effects."

One of the problems with ground-water travel time is depending, again, on how you set up the calculation, the real answer you get is, "So what?" You haven't answered any of the fundamental issues that are on the table.

How might we go about establishing a new criterion? I guess the first argument I would make is that any criteria should explicitly address what can go wrong. Define the failure. Define what you're concerned about. It should be phrased in such a way that you end up with field-testable hypotheses, hypotheses that can be refused by data. What do I mean by that?

Basically, you go out, you set a criteria, a compliance criteria for performance. You go out, and prior to getting any data, you're just as likely to meet it as not. The purpose of spending $6 billion on characterization is to change the odds.

Stakeholder consensus. Stakeholder is a word that wasn't around much eight, nine, ten years ago. Up front, before you start the problem, before you get any data, before you do any project planning, get consensus as to what a failure is, how you're going to measure it, and what the acceptable risk is--and we're going to come back to the concept of risk over and over.

There is a certain obscenity to the fact that we're sitting here, after eight years, debating what was intended by the disturbed zone, and the people that asked the question, cannot tell us what they meant, or what they're asking.

In the world I live in now, you've got to define these things up front, and I think one of the issues is de-emphasis of complex models. As an example of that, think of the waste isolation pilot plant. Despite the regulatory criteria, there is an intuitive logic with respect to performance in the fact that the thing is emplaced in a salt formation. Salt dissolves in water. Therefore, there can't be any water there. I think that's actually the driver behind WIPP. That's the thing that the gentleman from Nye County goes home and can explain to his children.

We don't have a measure like that for Yucca Mountain, despite the intent of GWTT. It doesn't answer that question.

How might we set a criteria? First, we start by defining the failure we're concerned about in context, and I think the context here is fairly clear, with at least one failure scenario, and that's ground-water transport of contaminants. We're taking highly toxic waste. We're emplacing it in the mountain, and our concern is, is there any chance it's going to get from where we put it to where we don't want it? That's the problem.

Having done that, we ask ourselves, okay, now, what could go wrong? You need necessary conditions. You'd need to start working through this step-by-step. One of the first things you have to have if you're going to have this failure, is you have to have a source of water. Where might that water come from? Infiltration is a source. Perched water is a source. Start building up the problems with questions you can answer from site characterization.

Source of water is not enough. Having the water, then you have to have contact with the contaminant. What might that involve? Well, some quantity of water has to reach the contaminant. The contaminant has to dissolve. You have a residence time. These are issues that you can test.

Necessary conditions. We're going to continue. Source, contact with the waste. Okay, now, how do we get it from where it is to where we don't want it? You need a pathway. There's got to be some quantity of water that goes down the pathway, and the pathway has to exist.

Now, I want to point out something important. Those are necessary conditions. They are not sufficient for failure. To have a failure, you've got to have some probable combination of all the steps before you have a consequence.

And, hopefully, we're making this short and sweet; summary and conclusions. Ground-water travel time does not work. The major reason was well-illustrated yesterday in that we're still asking, what is it we're being asked to answer? It's the lack of specificity.

I also want to point out, this is not unique to ground-water travel time. I've spent the past year working in a broader regulatory environment, got the same problem.

As an analyst, I generate answers, lists and lists of answers. With a computer, I can generate as many answers as you want. We go to the regulatory. We say: "Are any of these answers the answers you want?", and they say, "We don't know, because we're not sure of the question yet," and we go back to go.

The economic consequences, now, are devastating. DOE is spending $6 billion annually on environmental restoration and waste management. DOD is spending the same. That's $12 billion, and that's only 5 per cent of the annual expenditures in this country. We're not doing it well, and part of the reason we're not doing it well is nobody knows what the question is.

I'm going to argue, from the basis of my brief presentation, that the conditions that would constitute a failure can be articulated. The likelihood that those conditions exist is a function of characterization. As I said, you can postulate the failure. You're just as likely to have it as not, until you start characterizing. Information changes the odds. That's the purpose of characterization.

The consequences of failure can be estimated, and it's the consequences that people are interested in. That's the risk, and the consequences are human health effects or ecological effects.

Basically, what I've laid out and what I'm recommending is a risk-based approach, which is remarkably similar to what we call performance assessment. Basically, we're trying to answer three questions:

We want to the answer to what can go wrong. We want to know how likely it is, and then we want to take a look at the consequences if there is a failure.

Now, word of warning to Carl and some of the others: In this approach, there are responsibilities. Now, we saw, yesterday, a very clear demonstration that most of the debate is perceived to be between DOE and NRC and their gets. Gentleman from Nye County, or somebody would say something, and it's kind of like, maybe his microphone wasn't on, but all the debate has been between, again, only two of the parties to the decision so far.

All the stakeholders have to agree to acceptable risk. That's the only way you can make a decision. Also, as part of the risk-based process, iterative reevaluation of the criteria as information is gained by all parties to the decision.

What we're hearing--and the NRC said it very clearly--is that in the intervening years, we have obtained information. We probably would have done it differently, but for some reason, we're stuck. For some reason, they carved their criteria into stone, and the fact that information and events have eclipsed that seems to be irrelevant to the process, and unless there are any questions, that's basically it.

DR. DOMENICO: Any questions from the Board? Staff? Audience?

MR. JOHNSON: Carl Johnson. Paul, your example of WIPP and the dissolution of SALT as being the area which the public can understand and can relate to, I think, through my years of working on the Yucca Mountain Program, I have seen that there is something almost equivalent for the Yucca Mountain Project, and it's a question that I get asked all the time, and it's very appropriate for this meeting, and the question I get asked by the citizens of Amargosa Valley, which is the population area downgradient from Yucca Mountain, is, "When is my ground-water going to be contaminated by this project?" It's not "if," it's "when," and it fits very nicely into what you were trying to say.

DR. KAPLAN: I appreciate that, and you've also raised, again, a fundamental issue with a risk assessment is that there is always a finite probability that that's going to happen. The question is, are the risks acceptable to all the parties to the decision?

DR. DOMENICO: Mal, do you have something?

MR. MURPHY: Thank you, Pat. Mal Murphy, Nye County.

First, Paul, I'd like to compliment you on the presentation. I don't interpret that as meaning that I necessarily agree that ground-water travel time should be expunged from the program, but it was a very--especially the summary at the beginning, I thought it was very well done.

Stakeholder responsibilities. Would you, if the stakeholders, under your scenario, are sort of required to agree to an acceptable risk, what role should the stakeholders have in the ultimate decision? In other words, if we have to--if, under your sort of perfect world, we agree at what level the risk becomes acceptable, what happens if stakeholders don't agree at a certain risk level? Would you envision a program in which stakeholders have a share of the ultimate decision as to whether or not the program should go forward?

DR. KAPLAN: Yes. The stakeholders should, and that should be decided up front. Basically, stakeholders should be involved at all stages, including the planning of the project, what information, and consistently during the review to make sure that, as information comes in, you've asked the questions that are truly going to satisfy you.

Now, you can delegate that responsibility to DOE, or to some other agency, if you want, but at least there should be some sort of advisory board that has the power to affect the program that involves the stakeholders.

MR. MURPHY: Well, more than advisory board, I guess I was referring to something akin to, and make it--I'm not suggesting, you know, I'm not pushing this idea, but something in the nature of the state's notice of disapproval right. I mean, would you go so far as to say that if they can't feel comfortable with the level of risk, if the level of risk is just not acceptable to a certain stakeholder, would that stakeholder have a veto over that part of the program, for example?

DR. KAPLAN: If you're talking about an individual stakeholder, one group, no. I think it needs to be a board or some sort of panel that is representative of the various stakeholder interests, but there are models for this, and some of the models are discussed, and I think there are starting to be some case histories that actually indicate that, at least in many situations, the wisest counsel is actually coming from the stakeholder groups.

MR. MURPHY: That's certainly true in this program.

(Laughter.)

MR. MURPHY: I include the TRB in that.

DR. DOMENICO: Steve?

MR. FRISHMAN: Paul, you know, it's fine to talk about ways to come to new criteria, and so on, but can you envision, even if what you're trying to describe as a way out of what I don't really think is a morass, I think it's been created as a morass, but even if your new process became the rule of the day, is it conceivable to you that any different or less, or different focused site characterization would have to go on to answer and meet a test of your criteria versus what we have out there right now?

DR. KAPLAN: You know, I'm reluctant to comment with that because, for the past year, I have not been keeping up with the project. I really don't know their characterization plans, or how intimately they're linked with the performance issues.

I've had the opportunity, now, to test out some of the things we recommended to DOE in Yucca Mountain, the performance assessment that these issues drive characterization, and I've had the opportunity to actually do that on a much smaller scale, and I'm very happy with the results. Whether or not that's occurring in Yucca Mountain, I do not know.

MR. FRISHMAN: Well, I guess what I'm getting at is, regardless of what the standard is or what the criteria are, you still have to have some very fundamental knowledge of the hydrology.

DR. KAPLAN: Yes.

MR. FRISHMAN: And until we have that fundamental knowledge, I think it's a red herring to go out there and talk about the criteria being wrong.

DR. KAPLAN: What I'm saying is that the criteria are not well-phrased, so that you can go out with testable hypotheses. It's impossible to measure ground-water travel time. What I'm saying is, you can build up a failure scenario with testable hypotheses; perched water. Does it exist, or doesn't it? You're never going to know for sure whether or not it's there if you don't find it, but as you drill more holes and don't find it, the odds that it's not there increases. You never have perfect confidence.

If you want "yes" or "no," the site is safe, I have no way of knowing how to provide that answer.

MR. FRISHMAN: Well, we're in that situation now, where the program says, "We will meet regulations." Well, if the program says the regulations are a little bit foggy, it uses that as the reason to not go out and test the system, and I guess what I'm getting at is, I think it's premature to argue whether, philosophically, we're even chasing the right criteria, when we're using the criteria as a reason to not go test the system.

I think, to me, anyway, the system is going to have to be tested regardless of what the standard is, and that's what we're lacking right now. We're not lacking understanding of criteria, we're lacking the data to understand the system, and then maybe we can talk about criteria.

DR. DOMENICO: With respect to data, Paul, you spent a good part of your life on ground-water travel time in Yucca Mountain, as I recall, a few years back. I don't know if that was your study plan or not, if you wrote the study plan. I won't ask you if you did that, but so that was one of your main jobs for several years, and you were going at it stochastically, and you gave it a lot of thought, like you give everything.

So, what are your thoughts about the problems that you were confronted with when you were doing that job of work?

DR. KAPLAN: A number of problems. One, of course, to come up with a number, or even a distribution. Fundamentally, it is essentially dependent on the model. You choose a different model, you get another number. You do this enough, and it becomes very unsatisfying as to which one of these numbers is the most appropriate under the circumstances, and then we have the NRC saying, "We can't actually tell you what's appropriate yet."

The complexities--

DR. DOMENICO: So it's model-dependent?

DR. KAPLAN: The answer is model-dependent. Ralph Peters, Elmer Klaviter, and I put up a small paper in the Waste Management '89, a little three-pager, where we show we took the same flow field, postulated two very rational interpretations of how to do ground-water travel time calculations, did them, and basically got six orders of magnitude in difference, and at that time, the paper was to the regulatory community, saying, "Until we get some more specification as to how you want this calculation done, is we can give you any number you want."

The other issue is as the modeling becomes more complex, we run into situations like we ran into yesterday, where there were some questions over what Rally was doing to calculate the GWTT, and by Rally's own admission, yes, the logic is somewhat contorted. And so, now we're completely divorced from a simple standard and an intuitively simple measure of risk that, again, can be used with respect to going home and explaining to your children as to why DOE is putting a high-level waste repository in your back yard.

I think the modeling in the program that Duguid laid out is important, because a lot is learned from there, and the important factors, hopefully, will pop out of that, but the modeling exercise as a whole, in generating a number as a basis for a regulatory decision is entirely bereft of meaning to me.

DR. DOMENICO: Thank you.

Was there a question?

DR. BROOKS: Yeah. David Brooks, NRC.

My comment has to do with, in general, Paul's presentation, and it picks up on some thoughts that Steve and

Carl just expressed, and what interested me--and could you put this up?--was what Paul described as he worked towards his alternative was actually the--it contained the logic that 10 CFR 60 works from, and that is, you really start at the bottom, with the mechanistic process model. You have to understand the system.

And that, if you're looking at 10 CFR 60, you're looking at the site description, which is then evaluated under the favorable and adverse conditions, and those are the pieces, like perched water, a rise in the water table, specific unsaturated conditions, which then support your subsystem modeling, your more detailed modeling, which then supports your total system analysis, which actually gets back to some very abstracted models.

And so, again, you need to think of 10 CFR 60 as building from the favorable and adverse conditions, and that is the test on how well you understand the site. So I think there's a lot of similarities, again, in what you're saying and in how we envision 10 CFR 60 being applied.

DR. KAPLAN: I think I laid out the issues that have to be addressed. It's just as we build up and the process becomes more complex, there's a body of interested parties to the decision that we're not communicating with, and I think that's one of the issues that we ought to deal with when setting criteria.

I'm far more comfortable with the modeling as it becomes more complex. The complex geometries, the complex flow fields that Rally is laying out are intuitively far more satisfying to me, as a hydrogeologist, than the simple one-dimensional models we were running a couple of years ago. It looks more like reality to me. I've got more confidence we're going to capture the important features.

I would not want to have to explain that to the Navajo Nation.

DR. BROOKS: Yeah. But, again, just to reiterate, the idea here is to break the site characterization down into discrete pieces which you can get your arms around, and that is embodied in the favorable and adverse conditions. Your understanding of those fits directly into your understanding and your analysis of the subsystem requirements, both the engineering and the GWTT, which then gets rolled up into your total systems modeling.

Now, what I'm hearing is, is that it sounds like there is perhaps one group that's starting at the top, and not looking all the way down to where the data is being generated, and then there's another group that's generating data, and maybe there's not enough communications with the people that are up doing the "total systems modeling."

More realistically, those arrows probably should be going both ways, as we're iterating through this process, collecting data, looking at the subsystem modeling, and then doing your total systems modeling.

I don't see a great difference between what you described and what 10 CFR 60 envisioned.

DR. KAPLAN: Could I make a comment?

DR. DOMENICO: Okay.

DR. KAPLAN: As task leader for ground-water travel time, I was shocked by some of the things that the NRC laid out yesterday in terms of information they would like to accompany the ground-water travel time calculation.

What I heard was that the "any other time" that the Commission might accept was dependent on information about quantity of flow, geochemical environment, geometry of the problem. Now, I ran and managed that task for many years, and laid out plans to 2001 under any number of exercises. Not once was it our intention to provide that information. We had no reason to believe it was wanted, and it would be part of the decision with respect to ground-water travel time.

And I guess this is what I'm saying is wrong with the current regulatory environment, is the information you want is not specified to the point where I, as the provider, am likely to give you the basis for your decision, and that's a problem. That shouldn't come as a surprise to me after spending six years in consultation, also, with the NRC. I shouldn't hear about it after the fact.

DR. DOMENICO: Any other questions?

(No audible response.)

DR. DOMENICO: Thank you very much, Paul.

The next presentation is comments from the State of Nevada from Carl Johnson.

MR. JOHNSON: I'm going to be somewhat different here, and not use overheads. Linda, could you make sure we get the lights on? Thank you.

For the record, my name is Carl Johnson. I'm the Manager of the Technical Programs for the Nevada Agency for Nuclear Projects. The Agency is responsible for the state's oversight of the national high-level waste program. We've been interested in the determination of ground-water travel time as it applies to the siting and licensing of high-level waste repositories since the beginning of the waste program.

I, personally, have followed the development of the concept and its application to the repository program through regulation since the early eighties, and have provided comments on this subject on numerous occasions. My comments today will focus on Nevada's view of ground-water travel time for the determination of site suitability, and the DOE's approach to the determination of ground-water travel time for its use in a repository application.

Groundwater is the most likely means by which radionuclides could be transported from a geologic repository to the accessible environment. As we have observed at Yucca Mountain, gas transport may also be a likely means. Hence, a long groundwater or gas flow travel time between the underground facility and the accessible environment is a highly favorable condition for waste isolation. Consequently, our confidence in the ability of a geologic repository to isolate wastes is directly dependent upon the understanding of the groundwater and gas flow between the repository in the accessible environment.

The state's view is that the ground-water travel time criterion in the NRC regulation 60.113(a)(2) is a test of the hydrogeologic character of the site from the perspective of pre-emplacement rates of ground-water movement, or natural transmissivity of the site. The intent is to provide assurance that the natural, unperturbed geologic environment is capable of isolating waste, independent of the engineered barriers and the disturbed zone. The criterion provides a quantitative measure of the site's ability to meet the Environmental Protection Agency's standards for radioactive materials.

The ground-water travel time criterion under 10 CFR Part 960, the DOE siting guidelines, provides a similar but different test of the site's natural conditions. The siting guidelines present a series of criteria which a site must meet, or is likely to meet, or the site must be deemed not suitable for further consideration as a repository. The guidelines contain a site-disqualifying condition for ground-water travel time, a quantitative measure of a site's ability to meet the standard.

A site shall be disqualified if the pre-emplacement ground-water travel time along the fastest pathway is determined to be less than a thousand years. If the travel time is greater than a thousand years, a site may qualify as a suitable site, subject to other criteria. In my view, the criterion is clear and unambiguous. The site either meets the thousand-year threshold, or if it does not, the site is disqualified.

At Yucca Mountain, pre-emplacement unsaturated zone travel times are very critical to the determination of site acceptability. Measurements of hydraulic conductivities and estimates of effective porosities in the fractured welded tuffs of the underlying saturated zone indicate that travel times on the order of 100 years from the repository area to the accessible environment. Therefore, for this site to be acceptable for a repository, the unsaturated zone is the key to meeting the thousand-year travel time criterion.

If all the unsaturated zone is restricted to matrix flow, travel times will likely be long, measured in thousands of years. However, if there is fracture flow between the disturbed zone, a boundary which is yet to be determined, and the underlying saturated zone, travel time within this zone will be likely to be very short, measured in terms of years, or hundreds of years.

Information gathered to date confirms that tens of millions of fractures occur within the unsaturated zone of the proposed repository block. It is yet to be demonstrated that these fractures do not transmit water. It is also possible that faults function as water conduits, but this is yet to be fully determined.

There are two approaches for gathering information to assess fracture flow; direct measurement of hydrologic properties, and ground-water dating.

Direct measurement of properties is difficult in complex, fractured, heterogeneous conditions driven by pulsed recharge fluxes. At Yucca Mountain, reliable representative measurements of hydraulic properties for fractures do not exist. It has necessitated the use of theoretical properties in equations which predict the ground-water travel time. These theoretical properties and relationships, which have their origins in soil physics, have yet to be demonstrated to be representative of a fractured porous media like Yucca Mountain.

Furthermore, the few fracture model calculations applied to date by DOE, such as the infiltration flux distribution used in the WEEPS model, have been overly simplistic.

Despite growing field evidence of flux rates on the order of 4-10 mm/yr in some areas, these models still utilize, for the most part, flux rates an order of magnitude or more lower. Therefore, the utility of using these models for predicting ground-water travel time with a reasonable level of confidence has yet to be demonstrated. Depending on the assumptions used, ground-water travel times have varied over four orders of magnitude, and we just heard Paul say six orders of magnitude.

Direct measurement of ground-water flow in fractures and age dating of fracture water offers the best approach for obtaining confident travel times in the unsaturated zone. Flow in fractures has been well-documented by downhole video camera in various boreholes at Yucca Mountain. Direct measurement of in-flow rates and sampling of these flows for dating has been sparse until recently. Results from these samples have been slow in coming, but some of the results from Drillhole UZ-16 were reported at the NRC's Advisory Committee on Nuclear Waste last December.

In a hydrologic working group session, USGS and Los Alamos National Laboratory researchers reported post-atmospheric nuclear weapons testing ages for Chlorine-35 and tritium found in fracture water encountered at depths of 1450 feet. That depth is approximately 400 feet below the proposed repository horizon, and strongly indicates a flow path to that depth of less than fifty years.

I anticipate that as the program places more emphasis on samples of opportunity with respect to unsaturated zone moisture, additional evidence of fast pathways confirmed by age dating will emerge.

Let me now turn to the subject of ground-water travel time calculation, and its particular problems, and then I will conclude with some comments on DOE's approach to ground-water travel time requirement.

According to the NRC regulations and DOE siting guidelines, the calculations of ground-water travel time is along the fastest path from the edge of the disturbed zone to the accessible environment. The disturbed zone is defined as: "That portion of the controlled area, the physical or chemical properties of which have been changed as a result of underground facility construction, or as a result of heat generated by the emplaced radioactive wastes, such that the resultant change in the properties may have a significant effect on the performance of the geologic repository."

Subsequently, the NRC has clarified the reasoning behind the disturbed zone boundary, and I quote: "The disturbed zone criterion is intended to prevent the reliance on only the zone directly adjacent to the engineered facility for the major portion of the geologic barrier protection, and to avoid the complication of consideration of coupled processes close to the emplaced high-level waste when demonstrating compliance with the ground-water travel time performance objective."

The last quote is highly significant to the proposed repository. The majority of travel-time credit from pre-emplacement conditions is likely to occur between the disturbed zone beneath the repository and the water table. Within the saturated zone, travel times are believed to be relatively fast through the fracture networks to the accessible environment. Pumping tests of the saturated zone at the C-Well complex have confirmed this belief. Therefore, the determination of the disturbed zone boundary is crucial to the calculation of ground-water travel time.

The subject of the boundaries of the disturbed zone, where the calculation of ground-water travel time must be initiated, has been the source of much confusion and discussion, which part of that has gone on here. DOE's new proposed program approach, the PPA, as we understand it, only adds to this confusion.

Referring to my earlier remarks, the disturbed zone definition includes not only repository-induced physical and chemical changes, but also thermal changes. As the Board has heard in past meetings, DOE proposes a high thermal-loading strategy for Yucca Mountain. That strategy has proposed thermal loads which could drive the temperature range up to and above the boiling point at the water table, and yield significant temperature increases at the ground surface. Under such an extreme thermal load, the location of the disturbed zone boundary could lead to a calculation of an extremely short, or even zero ground-water travel time.

Using a high thermal-loading strategy, the specific load controls the location of the disturbed zone boundary. However, as we understand the PPA, the final thermal load for the repository will be defined only after the site performance confirmation period, which is planned to be up to 100 years in duration. If that is the proposal, then a final disturbed zone boundary cannot be defined, and, thus, a final pre-emplacement ground-water travel time cannot be established for possibly up to 100 years. That approach certainly violates the spirit and the intent of the ground-water travel time criterion, as promulgated both by the NRC and the DOE.

A concern relative to the calculation of ground-water travel time in the disturbed zone is the use of average values. The unsaturated zone is a complex, dynamic system, where average hydraulic parameter values have little meaning relative to addressing travel time along the fastest pathway. Of special concern is the influence of short-term, high-intensity precipitation events that could lead to very rapid travel times to the water table.

Tyler, in 1986, reported occurrences of downward water velocities as high as 60 meters per year in fractured tuff environments in the vicinity of Yucca Mountain. Given these past occurrences, it is clear that the DOE must determine conclusively if the magnitude of water movement consistent with these recorded events is occurring at the repository site. It will not be adequate for site characterization to estimate the probability of such flow occurring, but, rather, site characterization must determine if it actually does occur. If this flow is found to occur below the repository horizon, then, clearly, it represents a fast path and other slower paths, such as matrix pathways, should not even enter, or be averaged into the calculation.

Now I will turn to the subject of DOE's approach to determining compliance with the ground-water travel time criterion.

As we understand both the April, 1994 presentation to the Board, and the June 10, 1994, to the NRC, letter to the NRC on the subject, the Department proposes that pre-emplacement ground-water travel time be considered a distribution that defines the likelihood of each water particle reaching the accessible environment at a specific time. Thus, ground-water travel time distribution is a distribution of water particle transport times.

The approach involves developing separate distributions for transport from the boundary of the disturbed zone through the unsaturated zone, and in the saturated zone from the water table to the accessible environment. Then, the approach would sum the resultant distributions, and evaluate the significance of the travel times on system performance.

The State's position is that the DOE approach violates both the intent and language of the travel-time criterion, and should be rejected by both the Board and the NRC.

As I've stated earlier, the NRC criterion was established to test the natural system's ability to isolate radioactive waste, and the DOE, in its own siting guidelines, established a disqualifying condition to reject sites which could not, or likely could not meet the waste isolation natural system test.

A key element of that test is defining the fastest pathway. Definition of the fastest pathway, and whether a water particle, traveling along the fastest pathway, takes longer or shorter than a thousand years to accomplish the trip, leads to a pass or fail grade relative to the criterion. For a particular site, there can be only one fastest pathway of radionuclide travel. The DOE's PPA approach fails to address identification of the fastest pathway, and possible methods which could help define the pathway.

Distribution of water particle transport times has a place in evaluating site performance through cumulative distribution functions. It is important to assess radionuclide transport performance by evaluating the extremes of these distribution functions. Those distribution functions may or may not include the pre-emplacement ground-water travel time fastest pathway. The Department has interpreted the ground-water travel time criterion as a sub-element of site system performance. That is clearly a misinterpretation.

The proposed DOE approach acknowledges the NRC definition of the disturbed zone and its application to establishing a boundary for the disturbed zone. The approach recognizes that the disturbed zone includes physical and chemical property changes which could significantly alter repository performance. However, the approach fails to acknowledge that the thermal changes, and the resultant changes in the physical and chemical properties must also be considered in defining the disturbed zone around the repository. The omission of the thermal aspects, in our view, is contrived, since its inclusion would have the effect of enlarging the disturbed zone, and reducing the ground-water travel time.

The approach also fails to recognize that the definition of ground-water flow may include two phases of water; liquid and vapor. The Board is well aware of Nevada's view that the definition of ground water includes both phases and, therefore, the ground-water travel time criterion must include vapor phase travel time. This is the basis of Nevada's insistence on timely characterization of pneumatic pathways at Yucca Mountain.

In conclusion, the ground-water travel time criterion, as promulgated, is a test of the ability of the site's natural system to isolate radioactive waste. It provides an objective, numerical standard by which all parties can judge the goodness of a site. The DOE approach introduces a subjective re-interpretation, which places premature importance on meeting performance objectives by relying on assumed parameters, instead of attempting to directly assess site suitability. I request that the Board evaluate DOE's approach very carefully before giving any endorsements.

With that, I'll take any questions, Pat.

DR. DOMENICO: Thank you very much, Carl.

Any questions from Board members? John?

DR. CANTLON: Yeah; Cantlon, Board.

Carl, it seems that you've taken a much harder view than NRC itself in sticking with the fastest pathway. The language, as I understand NRC's presentation in the written material, is that DOE would have to justify a fastest pathway that exceeded the thousand years, or was less than the thousand years, in terms of the risk that's imposed. In other words, if only 10 ccs gets down in one small fractional part of the repository, so what? How can you stick to a single criterion and expect that to be disqualifying to the site?

MR. JOHNSON: Well, the--and I don't mean to misrepresent that the NRC's criterion is not a disqualifier. The ground-water travel time criterion under DOE's siting guidelines is a disqualifier.

I think the point that Mal Murphy made yesterday, and we certainly agree with that, and in his presentation, that if there is a fastest pathway identified that is less than a thousand years, that DOE is going to have to present a very compelling argument, and that compelling argument, I believe, is going to have to be supported by empirical site data, not assumed parameter values from theoretical models.

DR. DOMENICO: Further questions? Staff?

DR. PALCIAUSKAS: I wanted to ask a question concerning what appeared to be two somewhat conflicting statements. One was the ground-water travel time was, of course, instituted as a measure of a geologic subsystem's ability to isolate waste. You stated that.

And then, yet, you also implied that the ground-water travel time calculations should not involve anything of particle transport. They seem to be somewhat conflicting statements; in other words, diffusion or dispersion, or anything of this nature.

MR. JOHNSON: I don't think there was a disjoint between those. What I was attempting to say is that the travel time is a measure of--pre-emplacement ground-water travel time is a measure of the site's ability to isolate waste. The transport, the water particle transport time may be a way of getting to that if it's used to calculate pre-emplacement ground-water travel time.

However, it appears like the approach is to--the Department's approach is to use that to calculate, or initial calculation of site performance, which would be a different animal than the looking at the site's ability to isolate waste in a pre-emplacement calculation.

DR. DOMENICO: Further questions from the staff?

DR. CANTLON: Carl, could you sketch for the Board your view of the role of water vapor as a transport vehicle for radionuclides?

MR. JOHNSON: The repository may have, or will have gaseous radionuclide components, C-14 being the primary one there. I envision that water vapor could move from the repository vertically to the ground surface. The gaseous radionuclides would be caught, if released, could be caught up in that, and our concern has always been, is that there is likely a pathway from the repository horizon to the ground surface.

That needs to be verified, and that's why we've asked for the early characterization activities, to do the, or collect the field evidence to say whether there's a fast pathway for water vapor, to get from the repository to the ground surface, or the accessible environment.

DR. DOMENICO: Any questions from people out there?

(No audible response.)

DR. DOMENICO: Carl, thank you very much.

What we're going to do now is have a brief fifteen-minute break to gather our thoughts, and when we come back, it would be nice if all of today's speakers we're here at the table. We're not going to actually have a round table. We'll just return to our places.

I would like the round-table discussion to think about what you've heard both yesterday and today. Any topic related to ground-water travel time is fair game, and, especially, there are maybe a few things that we should think about, like:

Is there agreement on the definition of disturbed zone, and what we feel about the role of expert witnesses in that game. Is there agreement on what transport process in the computation of ground-water travel time? How should the significance of travel times less than one thousand years be assessed? We heard that yesterday. Is there any problem with the approach that was outlined by the M&O yesterday?

Lessons learned from past attempts at struggling with the ground-water travel time, the fact that we learned that it's model-dependent; and, then, what is the role of chlorine and tritium in the calculations. Seems like if we wait long enough, we're going to have enough of chlorine and tritium to make some real good estimate of ground-water travel time that I, frankly, would trust a lot more than some modeled calculation, but that's something we might talk about.

And, would an alternative criterion be preferable, or should we even be thinking of one at this stage? So, those are some of the things that I think that--I would hope that everyone would think of, including the people out there, and we'd be very glad to have any sort of input on those ideas, but, right now, let's take a fifteen-minute break, and reconvene.

(Whereupon, a brief break was taken.)

DR. DOMENICO: Let's all return, please.

We threw out some ideas, and I've just been told that Dwayne Chesnut would like to talk to us a little bit about the significance of travel times less than 1,000 years.

Have I got that right, Dwayne? Go right ahead.

DR. CHESNUT: I'd like to take a couple minutes to show some results of things that I've been working on off and on for a number of years. The roots of this actually go back about 20 years, into a totally different career, but I've just recently gotten a bunch of things to come together in looking at the nuclear waste problem, and as of last Friday, I got a paper approved for the MRS meeting in Elko later this fall, so that's why this is kind of coming in at the last minute, but their verbal came through, both the project office and the lab, last Friday.

This piece of the story many of you have probably already seen. That was presented at the high-level waste. It's part of a fairly long paper on dispersion in heterogeneous permeable media, and why I think the classical approach doesn't quite describe the real situation.

The important part of this is this curve on the right, which shows the breakthrough of an infinitesimal pulse released at time zero out to some accessible environment boundary. So, if you can imagine that at early time, I release a unit mass of material at the repository, or the edge of the disturbed zone, and I watch the concentration breakthrough out of the accessible environment, and this is normalized so that this is on the mean travel time, the average travel time for this system.

There is a parameter in here called sigma, which describes the spread of that pulse, about the mean, and it's a measure of the heterogeneity in the system. When sigma is zero, that pulse breakthrough curve has one single point. It's a delta function centered on the mean travel time, so it would have an infinite spike right here.

As I increase sigma, the peak starts to flatten and broaden, but then as I increase it still further, it starts to get a long right-hand tail, and the location of the peak moves to earlier and earlier time. If I continued that process in the extreme limit of a very heterogeneous system, all of the breakthrough occurs at time zero. So, I have a system which continuously grades from totally dominated by a single pathway, to one in which the average travel time totally describes the breakthrough curve.

And up in the upper left-hand corner is an example from a detailed simulation done by Luis Moreno in Sweden and Chen Fu Tsang at LBL, showing that this is not just an artifact of a simple analytical model, but you can actually see this when you put in a real distribution of permeability.

Now, why is that important? It's important because if I take the convolution of that function, with a release function for the engineered barrier system, I can give you the total breakthrough at the accessible environment for a time-dependent release. In other words, I can look at the entire transport of a radionuclide inventory out to the accessible environment.

Now, this is a very busy slide, but this is everything; a radionuclide inventory over here on the left, which has 29 radionuclides in it. This is a plot of the curies present in one metric ton of initial heavy metal as a function of which radionuclide. The red spikes up here show what it is in emplacement. The blue curve is what it is in a thousand years, so you get some idea of a few radionuclides decay fairly rapidly. The rest of them don't see much decay in a thousand years.

So I take the decay constants and feed into an engineered barrier release function. That release function is one that Pat Domenico and John Bartlett used a few years ago. This is exactly what the regulations require; complete containment for a thousand years, 10^{-5} release until the inventory is exhausted.

So, we take this as a bounding function. This is what we're required to do, by law. Put that together with the ground-water model. I have to combine that with retardation factors for each individual radionuclide. These are the lowest values that you can calculate from the kDs given in TSPA-93. Take the lower bound, put all this stuff together, and we have two parameters we have to get back to the ground-water system; sigma, and the mean ground-water travel time.

This particular example, a sigma of 2.2, the mean ground-water travel time is 70,000 years. The 2.2 number is basically based on pump test data from Calico Hills in the saturated zone, so there's some connection with site data. I'm not saying that's necessary the right number, but it's something in the ball park.

This curve shows, by individual radionuclide, the release from the EBS, which is the red curve. The blue curve is what reaches the accessible environment in 10,000 years. So, the difference between these two things will give you an idea of how much the geologic system is doing for us in retaining radionuclides within the repository.

I've set all these things up, and I get release to the accessible environment slightly less than one, so I've just complied with the total system release standards, and also calculated, what is the normalized release from the engineered barrier system itself, and it's almost a thousand. So, in other words, I get a thousandfold reduction in the amount of radionuclide release from the geologic system.

Now, you can see there are lots of interesting things you can play with, and relatively simple, small number of parameters in this thing that can be tied back in to site data, and, to conclude, I'd just like to show this plot.

I'm assuming, again, that I've completely complied with the subsystem requirements for the EBS. That, by the way, is itself a big job, to demonstrate that we can do this, but we only have to bound it within that step function. If we can make a good, rigorous bound, we're in good shape.

Given that, I've plotted the combination of mean ground-water travel time and sigma parameter heterogeneity that is required to just comply with the EPA sum equal to one. So, the green curve here is that boundary, where the EPA sum is exactly equal to one. The region up here above the curve, the EPA sum is less than one, and we're required to show that the probability of being in that region is at least .9. So, if we can come up with ways of getting these parameters and demonstrate the EBS subsystem requirements, then we can show the required probability without going through a very complicated calculation.

Anything below that curve gives me an EPA sum greater than one, so any combination of heterogeneity and mean ground-water travel time that falls over in here, we are not demonstrating compliance. The red curve is the combination that gives an EPA sum equal to ten, and, therefore, we can put another boundary in here. We're required to show the probability of being in that region is less than one in a thousand, according to the current regulations.

Now, what does this have to do with some of the issues that were raised yesterday, the tails of the distribution? Just as an example, I took this curve, come in here at a sigma of two, the mean ground-water travel time there is about 30,000 years, and if you go through and calculate what is the right-hand tail--I mean, the left-hand tail, the small travel time that we worry about, the 10 percentile on that distribution, you find that that number is about 300 years. So, I have 30,000 for the mean, I have 300 years for the tenth percentile, but I can still comply with the regulations.

So, I think this is a way of getting at this question of how important are the tails of the distribution. We can quantify the relationship between compliance with the EPA release limit, and a point on the tails of the distribution.

That's where I'll stop.

DR. DOMENICO: Thank you very much, Dwayne.

DR. LANGMUIR: Langmuir; Board.

Obviously, the value of sigma is a critical unknown here in this very simple model. Where are we with getting sigmas? How much do you need to know that we don't know, and how long is it going to take, do you think?

DR. CHESNUT: That's a very good question. Where are we? Right now, my best guess is that we're somewhere between about one and a half and two and a half, and we have a mean ground-water travel time somewhere in an ellipse around that curve. There is some evidence that says we're on the wrong side, and there's some evidence that says we're on the right side, and I think, with some more Chlorine-36 data, that we can start to zero in on where we actually are on this thing. That's a separate topic that I could address later, but...

DR. LANGMUIR: You don't need tests from surface-based testing, or underground tests to--

DR. CHESNUT: Well, I need sample to measure for Chlorine-36.

DR. LANGMUIR: From which you will infer, then, the right--

DR. CHESNUT: I go back to this particular conceptual model and see what value of sigma and mean travel time is consistent with the observed distribution of the isotope in the subsurface.

DR. LANGMUIR: So you're not using any of the surface-based testing information that's being provided?

DR. CHESNUT: The only piece I can use is the distribution of permeability in situ, including the fractures. Theoretically, if I calculate a sigma from the permeability distribution, that's the same sigma that I should use in this calculation, so I have two different ways of trying to get at the number.

If I have a lot of packer tests in the unsaturated zone is one possibility. The other one is to use Chlorine-36 to back out the parameters, because that's a transport test on the spatial and temporal scale that we care about for repository performance.

DR. LANGMUIR: You were telling me yesterday that the Chlorine-36 data is on the way, that June Fabryka-Martin is analyzing much of the recent data, the data from the last year and a half, and it will be available, perhaps, in the next few months to us? Do we know?

DR. CHESNUT: The current status? That I don't know. I understand that June Fabryka-Martin is in the process of putting together a milestone report, and I don't know the schedule for that, and where she is as far as reporting some additional data.

DR. LANGMUIR: Do you have some sense of how much of such data you'd need to have confidence in the value of sigma that you're selecting?

DR. CHESNUT: Well, let me show you what the problem is, if you want to take this up a minute here.

This slide, this figure up in the upper left is UZ-16 Chlorine-36 data plotted versus depth, and what I haven't shown here is the number of years per foot of depth at which the sample was measured.

Now, if you look at that, the cluster of points in the upper left-hand corner is PTn, so the longest time to get to the depth that we see any place in UZ-16 is up near the top. It took seven or eight hundred years per foot to get down to that depth.

Then you get the cluster of points in the TsW which range from about a hundred years per foot--oh, by the way, the blue dots here give you the error bars on these age determinations, so the red point is the mean value, and then blue dots tell you where the upper and lower standard deviation is.

These points down here are in the Calico Hills, so the fastest observed travel time of any of these particular measurements is that lowest point in the Calico Hills, and it took something less than a hundred years per foot at the lower error limit. This does not include the bomb pulse material that someone mentioned this morning, at a depth of something like 1400 feet.

Now, the conceptual model says that parameter should be log normal, T over Z, or time over depth should be a log-normal function. The problem is that it's truncated. We can't see less than 50,000 years with Chlorine-36, we can't see more than about a million. So we have a truncated log-normal distribution with both the upper and lower tail chopped off, and what I've plotted here is a cumulative frequency plot of the observed numbers, and the zig-zag curves give you the error bars on the observed frequency distribution. This is on a log scale for time over depth, and this is a linear scale for time over depth.

The curve, which may be difficult to see, is by taking a particular set of values, two and a half for sigma, and what amounts to 2500 years per meter of residence time; in other words, it takes 2500 years to go one meter down through the system, on the average, and, as you can see, that particular combination, the parameters gives me a pretty good match to the data.

Now, I should caution you that it becomes very insensitive to the exact combinations of sigma and travel time per meter when I get up into large sigma values, above about one and a half. So, at this point, what I have to say is we need more holes, and we need more data at a specific narrow depth range in order to see how this actually works out.

DR. LANGMUIR: Are your needs being factored into what's being done in terms of sampling, for analysis of Chlorine36?

DR. CHESNUT: I understand there are several hundred samples in the queue for analysis at this point, but that's, again, this is Los Alamos's program, so I'm not intimately familiar with the details.

DR. CANTLON: Cantlon; Board.

If we're going to rely on the isotope data here, Chlorine-36 and some of the bomb materials, it would seem to me most of these values are coming in perched water, and so on, below the repository zone, aren't they?

DR. CHESNUT: No. That's one thing I forgot to mention. This particular analysis is cosmogenic Chlorine-36. It's not bomb pulse.

DR. CANTLON: Okay.

DR. CHESNUT: And so this is distributed throughout the vadose zone.

DR. CANTLON: Throughout the vadose zone?

DR. CHESNUT: Throughout the vadose zone, and the maximum depth down here on this particular plot is 1200 feet from the surface.

DR. CANTLON: Okay.

DR. CHESNUT: So the log-normal transport function accounts for a very broad range of time that you observe in the cosmogenic pulse.

Now, one other thing I did forget to mention, the groundwater itself, based on three samples that June gave in her Focus 93 paper, has a Chlorine-36 age of much less than 50,000 years. It is basically not decayed, relative to the input cosmogenic pulse. So, there is an interesting problem here; how to reconcile what's happening in the vadose zone with what we see at the water table. Those samples, by the way, are not at Yucca Mountain, but they're in the vicinity.

DR. CANTLON: But if we're going to relate this to the safety of the repository, then we need to know something about the pathway by which that material is getting in. If it's entering--take the groundwater, for instance--along some of the intermittent streams, coming down as pulses, that doesn't mean it's flowing down through the repository.

DR. CHESNUT: We need measurements above and below the potential repository horizon.

DR. CANTLON: Right. Now, is anything being done? Before we get all excited about this data, I'd like to have something that would relate it to the safety of the repository.

DR. CHESNUT: Yeah. The point is, I could relate it to the safety of the repository if I have the data in the right place. At this point, I don't have the data in the right place. I suggest this as an approach, that we might be able to focus effort to answer the questions.

DR. CANTLON: Right, but are we focusing on getting some pathway, understanding how this material is getting there?

DR. CHESNUT: Well, the point here is that I don't care. The model does not attempt to trace individual pathways in detail, like the large models. What it does, it makes an underlying assumption that these pathways can be treated as independent flow paths from surface down to wherever we observe this Chlorine-36 data.

There is an assumption of independence that is very strong, but it is not as critical as it might seem, because you can actually go back and run detailed models on a small scale, remove the assumption of independence, and see what it does to the sigma parameter and safety of the distribution.

As it turns out, in all the cases that I've tried, it's still log normal. It still obeys a distribution like this, but I have to change the parameter a little bit. So, if I'm deriving the parameter from an actual field scale transport experiment, which Chlorine-36 is, then I have some reason to believe that this thing actually, at least, is a bounding approximation for what I'm going to see in radionuclide transport.

DR. CANTLON: But, if the primary pathway is outside the footprint of the repository, it's irrelevant.

DR. CHESNUT: Right; you're right, and so what this suggests is, first of all, we're going to need to get a lot of Chlorine-36 data when we get the tunnel-boring machine in operation, because that's going to give us very broad coverage, only at one stratigraphic location, but that tells us where we are along the upper edge of the repository. And then, we have to have samples from above and below the repository horizon.

DR. DOMENICO: I think Bill Nelson has a question or a comment. Bill?

MR. NELSON: I think we're going to find that as the process models start to become available to us, we can get--we will have available to us distributions that you will start to look at your sigma values from, which would suggest that the pathway ideas that he's asking about, and the--that makes it a very unique summary tool for the very detailed data that's gone into these models, and I'd like to suggest that that would be a way to do it.

Now, I have one concern. We may find that the sigma that you're using is highly related, a two-dimensional nature of flow systems, and may not be appropriate in the three. That remains to be seen. I think that would be a way that would--it grows from the bottom up, like NRC was asking us to do.

DR. CHESNUT: Yeah, let me address that. The problem with the large problems, in my opinion, and if you really want a long story, read my high-level waste '93 paper of why I think it's wrong.

I do not believe that you can define a proper shape of a breakthrough curve with large-scale models, because you cannot put in the right dispersivity. Dispersivity is a notoriously difficult problem in large-scale transport, and in my paper, it addresses that I think that picking an appropriate value of sigma is a better representation of the breakthrough curve for a large-scale process than anything you can do with dispersivity.

In other words, you average out the source of most of the heterogeneity when you put a grid box 100 meters on the side, and try to put a dispersivity number on it. So, my suggestion is that we use very small-scale grid box, where we can really put in a heterogeneous distribution of permeability. We run the calculation, we come back and we fit that breakthrough curve with something like this and determine the parameters that go with it. We can systematically investigate known spatial distribution to permeability, and see what it does.

In the paper, I did that, to some extent, with the stuff that Chen Fu Tsang and Luis Moreno did, and I found that if I took a sigma value that was about 30 per cent of what they used as the input to their 20 x 20 x 20 cube simulations, that I could reproduce their breakthrough curve.

So, I can adjust--in fact, this was almost independent of the input value of sigma, so the ratio of simple model to complex model sigma parameter was about constant, and so that's the sort of suggestion of where I think we can start to come up with the simplification, and still connect the site data, and it will use the complex models, but I think, just looking at some of the curves yesterday, it looked like that sigma values were about one and a half, and maybe if that's where we are in the real world, maybe that's okay. But the question is, is that going to be consistent with Chlorine-36?

DR. DOMENICO: Dwayne, sigma's your standard deviation; correct?

DR. CHESNUT: Yeah.

DR. DOMENICO: The diffusion theory of dispersion, that's equal to 2 times the square root of DT anyway.

DR. CHESNUT: It's a standard deviation of logarithm of the breakthrough curve.

DR. DOMENICO: It's the logarithm of the breakthrough curve?

DR. CHESNUT: Yeah.

DR. DOMENICO: Okay.

DR. CHESNUT: And that's why it gives you a very different behavior for transport over long distances. I can match it at short distances, and find that if I try to extrapolate at twice that distance, with a dispersivity parameter, I fail; whereas, if I match it with a sigma parameter and do it twice the distance, it would still work.

There's a good paper by Jury and Butters a few years ago where they had tried to fit in a porous medium soil, tried to use a convection/dispersion equation, and they found that if they matched the data at 30 cm and tried to predict 60, they couldn't with a classical dispersion model. With the log-normal approach, they come much closer to seeing what happens in the next part of the paper.

DR. DOMENICO: Any other comments?

DR. MIFFLIN: Dwayne, I'd like to kind of be the devil's advocate here. That was the cosmogenic Chlorine-36 that you were trying to get the sigma relationship. Yet, in that same hole, there's a report at a greater depth of a bomb Chlorine-36; right?

Okay. Now, the question I have is how, then, would that enter into the sigma calculations or determinations that you're trying to do if, indeed, that is bomb tritium at a greater depth? Because here we have the, you know, the paradox of the vadose zone hydrology. We don't quite understand what's going on, or if we understand, we can't quite prove it, and so we have perhaps rapid pathways, and then we have slower pathways that average things out.

DR. CHESNUT: I think that's a good question, and I actually was trying to talk about Chlorine-36 in the discussion this afternoon, but since it's brought up, let me go ahead and do it.

This is another piece of data we got from June Fabryka-Martin, and this is just in the PTn, and I've done that because it's a more restricted depth range, and the analysis is not quite as difficult. This is three different wells, UZ-16, and a couple of N holes. There are 20 data points here. The depth range is from about 110 feet down to about 230, and out of those 20 points, five were bomb pulse, and two were less than 50,000 years.

Now, with a big enough sigma, I can account for all of those observations, but I have nothing to fill in the gap between zero and 50,000 years. So, what I have to do is say, all right, I have 30 per cent of my data, or 35 per cent of my data that was less than 50,000 years. That gives me a point on the cumulative distribution to tie it to, so I start my empirical distribution at .35 and look at what happens as I go up from there. And, this is the kind of shape that you get, and, again, you can play around with this curve.

I think there is some potential for being able to accommodate simultaneously the bomb pulse material, the long average age, and the relatively slow release of radionuclides to the accessible environment, and that, I think, is three fundamental requirements of any performance model that we try to take into licensing.

DR. DOMENICO: Any further questions of Dwayne?

(No audible response.)

DR. DOMENICO: Thank you very much.

MR. SHETTEL: I have one comment to make. Don Shettel, representing the State of Nevada.

There are some experimental data on spent fuel under unsaturated dripping conditions that suggests that the fractional release may be greater than 10^{-5} for certain radionuclides under certain conditions, so your use of 10^{-5} is not necessarily conservative.

DR. CHESNUT: What I'm trying to do is to show what is the relationship of regulatory subsystem requirements to overall performance, and what we see here, if we can--what you're pointing out is we haven't bounded the problem within that envelope yet.

MR. SHETTEL: Right.

DR. CHESNUT: And that is a difficult job, and it says we can focus on looking at the EBS release independently of how we handle the ground-water travel time problem, and then put these two things back together in an internally consistent way. That's the real point here, is not that we've demonstrated this, but what is the pathway to a constructive demonstration.

MR. SHETTEL: Well, those types of experiments need to be continued by DOE, not have their funding cut off just because they show high releases.

DR. CHESNUT: Okay. I think what's needed is to look at where are we on the entire sequence of things that have to happen in order to get a release out of the EBS. We have to have a waste package failure, we have to have contact by water, we have to have solution of radionuclides, we have to have transport of that out into the rock, and it's that point is where we're worried about the one part in 10^5.

MR. SHETTEL: What my point is, you have to use experimental data that are appropriate to the environment.

DR. CHESNUT: I agree, but the point is that let's say I get a solubility of, I don't know, one curie per liter. How many liters of water do I have to make the transport happen? That has to also be factored in, and what is the chance that I'm going to have a waste package actually fail in that sort of period of time? That's another subject that I addressed a couple years ago.

But the point is that we have to sit down and put together a consistent, bounding set of calculations, based on experimental data for the solubility, for the amount of water, for the failure probability, for the mean lifetime of the waste packages, and all these other things, and then ask ourselves, do we meet the subsystem requirements or not. If we don't, we have more work to do. If we do, then we don't have to worry about that particular piece of the overall demonstration.

The other thing is that we are not relying on extraordinary engineering measures. We're saying we're only going to do enough engineering to comply with the regulations. If the site has these characteristics, we can meet the regulations.

And one other interesting point, if the system is totally homogeneous, the travel time required to get the sum equal to one is 1,057 years, so I think the NRC was very smart, all those years ago, because they set a standard that if the system really were homogeneous, would it be absolutely adequate.

MR. SHETTEL: That's just my point. We need to use the experimental data that's available, rather than assuming, say, the upper limit.

DR. CHESNUT: I'm entirely in agreement, and what I'm saying is that we don't necessarily have to look at that part of it in the context of an elaborate total system model. We can look at the release from an EBS subsystem model and see if it's within that boundary.

DR. DOMENICO: Is anybody in the audience from the National Academy of Science, or from the National Research Council? Anybody out there? We know they're very actively working on this, and we'd like them to share their views, but, apparently, we don't have any participation from them here today.

I think one of the points that has come up that Paul made that's disturbing--maybe it shouldn't be disturbing, because maybe this happens many times--is the model dependence of the calculations, several orders of magnitude. This is, to me, rather disturbing, and "You can get what you want," I think, is one of the statements.

Would anybody like to address that? Let me just--did I hear--what was the order of magnitude, Paul? Was it six orders?

DR. KAPLAN: It was six orders of magnitude in the simple model we published about three years ago.

DR. DOMENICO: And in a stochastic analysis, how many values of permeability were you really looking for?

DR. KAPLAN: Okay. The stochastic analyses I've done more recently and published as part of a sensitivity study for ground-water travel time actually ranges there, because we varied boundary conditions, ran about six to seven orders of magnitude.

DR. DOMENICO: Six and seven orders of magnitude on those?

DR. KAPLAN: Yeah. But I also got the same phenomena, using a different approach than Dwayne does, and that is something with a high coefficient of variation on permeability. I'll get a multi-modal response out. I get a very rapid breakthrough, and then it'll go down. I'll get another peak, so that I get a very high mean or median travel time, yet I can get substantial portions of my breakthrough coming at the, let's say, very early arrivals.

And the longer times, in my report, I sort of threw out and said, "I don't care about these." Then, what I did was the sensitivity to the input of the rapid arrivals.

DR. DOMENICO: I see. I'm going to throw this open to the group. Does anybody have anything that's pressing that deals with anything that has been discussed either yesterday or today? And that includes people in the audience.

DR. MIFFLIN: I'd like to make a comment on what Paul found in modeling, and my first comment is, is that ever since the early to mid-eighties there has been modeling, which seems to be of a nature that is scoping models, and my impression is they've become very refined over the years, and probably are very powerful in terms of performance assessment and some of the hydrologic modeling or transport modeling.

However, over that same period of time, which now equates about 12 years since the vadose zone was officially adopted as the target at this site, there has been starts and stops with respect to actual site-specific data collection, and some of these problems that are showing up in the modeling, assuming that most of the processes and some of the site-specific media characteristics have been correctly put into the model, show that site-specific data to constrain some of these uncertainties is the only way in which there's going to be any degree of confidence in terms of characterizing and necessitating how extensive waste isolation might be.

I want to trigger that comment back to the concept of the ground-water travel time. I strongly disagree with some of what I've heard here. I think it's a good measure. I also recall, within the regulations, that a site has to be simple enough to have confident characterization.

Now, I forget what, perhaps, Mal or somebody remembers whether that's a favorable condition or a--I can't remember exactly how that fits into the regulations, but it's a disqualifying condition, as I recall, and it's my impression that this site can be characterized, but it's also my impression that it hasn't been with respect to ground-water travel time, vadose zone hydrology, et cetera, and so, when we start talking about ground-water travel time and disturbed zone, I think it's important to go back to this other requirement in licensing: Can this site be a confident site with respect to some of these licensing criteria?

And, I think it can be if there is site-specific characterization in terms of field tests, but it's not going very rapidly, you might say, and some of these hydrologic aspects, with respect to perched water, how fast is it traveling, isotopic signatures with respect to why, you know, why do we get a sigma of cosmogenic Chlorine-36 that looks one way, but yet, way down deep in the hole, we're picking up bomb Chlorine-36. Come on, guys. It's time to try to find out what's going on, and it can't be done on limited data.

DR. DOMENICO: Anyone care to address those statements, or have other statements that are pertinent here?

DR. CHESNUT: Have you seen a paper in the May, *Water Resources*, on some modeling in a heterogeneous system by Luis

Moreno and Chen Fu Tsang? That's an extremely interesting paper, because it has pictures of flow pathways in very heterogenous systems, and even though the predominant direction of flow is one-directional, it has some tortuous, contorted pathways that develop because of the statistical distribution of permeabilities.

And if you imagine drilling down through a system like that, you can easily see where you would intersect some pathways that have extremely long travel times, and some which have very short travel times in the same borehole, and as yet, it's described by an underlying simple statistical distribution of permeabilities. I think that would be a good picture to look at. I think it was May of '94, *Water Resources*.

DR. MIFFLIN: I'd like to respond to that.

I think that the field type of hydrologists, or hydrogeologists realize that, in fact, they're conditioned to understand that there's rapid pathways and very long pathways by observations in field relationships.

The point is that the ground-water travel time criteria recognized that, that there are some rapid pathways. That's what relates to a first and important measure of the hydrogeology of any given site, in that it's not a measure of how much might be transported by the rapid pathways, it's a measure of whether there's waste isolation over time, and it's a kind of a rough and dirty measure of hydrogeology, and anything dealing with fluid flow, either gas-phase or liquid-phase, is not going to be fully characterized. Anybody that thinks it is going to be characterized is living in a dream land, but there are at least some measures that can be made of a given site.

And so, I think NRC's attempt to sort out sites and their potential for waste isolation is well-founded on the concept of ground-water travel time, and we do have some tools, such as the Chlorine-36 and tritium, that allow a somewhat net measure of what's been going down those fractures as fracture flow.

The point I was trying to make in my earlier comment, Dwayne, was that you've got to get site-specific data, and below the repository horizon as well, to have any idea at all whether any of that's happening, and it's been 12 years, right now, for that type of data to be developed, and it just isn't there.

So, the point is, is that a very sparse database is not going to allow some of the modelling efforts to be constrained.

DR. CORDING: It seems to me that, you know, you're looking at these--obviously, pathways are tortuous and very affected by the degree of fracturing, faulting that's present at any one location, and I think that part of the problem in looking at some of the borehole data is being able to identify what, other than lithology, what other features, structural features are present in the vicinity that you could tie into, and I think that's going to be one of the advantages of being able to see these features in larger exposures and horizontal exposures on some of these near-vertical features.

And it would seem to me that by taking the sampling across from fracture zones into--and fault zones and other features like that, into regions of less-fractured material, you should be able to start to see some things of a much greater degree of--much more information. In other words, there should be almost a wealth of information once you get down at several different levels, perhaps, and as well as below the repository level, to be able to see that, and be able to tie in, then, to what the specific structural features are, as well as the lithology.

DR. CHESNUT: I agree partially with that. There are really two very different conceptual pictures of flow in heterogeneous systems that are on the table. One is that the heterogeneity is as a result of large-scale mappable features that I can go out and measure in the field, without having to do a flow test. The other picture is that these flow pathways are a result of a statistical distribution of things that are too small to necessarily map, and I think the real system is somewhere in between.

An example of that is in ASPO, at the hard rock laboratory, where a considerable amount of effort has gone into geophysical and geological mapping from surface surveys, surface-based boreholes and some hydrologic testing, and what you see are certain major identifiable fracture zones, only some of which actually contribute water. A few of those are major sources of inflow.

If you take out all of the mappable stuff, you can account for about 60 per cent of the flow into the tunnel. The other 40 per cent is coming from little fractures that you see everywhere, and I think the ratio may change, but that picture of a heterogeneous fractured system is probably a reasonably good conceptual model for any system of fractured rock.

The problem that we're going to have is that we don't have any flow to measure. How do we characterize the system when it's in a relatively dry part of the rock system? That's really the fundamental issue.

DR. CORDING: I think it also points out that one observation at one location doesn't tell you the whole story, and I think you need to have--

DR. CHESNUT: Yeah, we've got to have more data. There's no question about it.

DR. CORDING: There are some areas that look like they should carry a lot of water, and you see nothing, because even on a given fracture system, obviously, it's following a path that's tortuous and not over the full plane. I think that's important.

MR. JOHNSON: Let me add a little bit to that, Dwayne. All one has to do is go to the observations that have been made elsewhere on the test site; specifically, in the Rainier Mesa area, where it's been reported on numerous occasions in the past that the faults that have been encountered in those tunnels are, for the most part, dry, yet when a precipitation event does occur, within a matter of hours to days, you've got moisture flowing in some of the fractures. I think that is good observational material that needs to be transferred to the consideration for Yucca Mountain.

I guess I, to follow on with what Ed just said about our observations we hope to make in the ESF, I have this lingering problem with how this whole discussion of increased, or that we need more data and more observations, fit with the Department's proposed program approach, where it's going to be 18 months to 24 months before we have a complete tunnel, and such observations can begin, yet, in 1998, there is a site suitability decision that needs to be made, and that the information from those observations are, or could be key to determining ground-water travel time, site performance, a whole host of things, and, frankly, I don't see where the data is going to be available at the time that decision needs to be made, or DOE proposes to make that decision.

MR. MURPHY: If I could just follow up on that, I think it's even worse than that, because as I read the--and try to put some time lines on DOE's proposed process for making the site suitability determination, which is a good process from the point of view of public input and stakeholder involvement, Paul, and all that, I think it's very good, but it looks to me like that there is an 18-month to 24-month period right there from the time they cut off data gathering until they can get these reports through the drafting stage, through the internal peer review stage, through the NAS peer review stage, through public involvement and input stage, et cetera, so that it seems to me we're looking at a period of 12 to maybe a maximum of 18 months from today before they will be required to stop gathering data, or even maybe analyzing data, and begin writing the documents which will go through this suitability evaluation process.

And the real question is, in my mind at least, not how much data there is going to be available in 1998, or 2001, when DOE hopes to go to licensing, but is there going to be sufficient data available a year and a half from now, or a year from now in order to make these kind of decisions?

MR. BROCOUM: We have a whole meeting, I think, the TRB is scheduling a whole meeting over site suitability, I believe, the middle of October.

We've been working very hard to come up with what we call a five-year plan, which will describe our studies over the next five years, how it all fits togethers.

I wanted to make a point here, because it keeps coming up, about data cutoffs. We don't plan on cutting off data. There may be a data cutoff in the sense that that data can go in this particular report, but the data itself will continue to be collected, okay. So, I think there's always confusion when we say, 18 months, we'll cut off data. That is really not true.

I want to make two other points here. One, I want to address some of Paul Kaplan's earlier comments today about public involvement. I want to just, for the record, remind people that when 10 CFR 60 was promulgated, it went through lots of public comment periods, drafts, public involvement that way; 960, the same thing. We had several--at least two public comment periods on 960, so that there was a public input on preparing that information.

I also want to point out, and it will be discussed in October, we have--as Mal just said--a very involved public involvement on the part of the process that DOE is responsible for, which is the suitability part. In that, we've come up with a fairly extensive public involvement process, including Federal Register notice, including several, three or four public meetings we've had this year, lots more coming up, and step-by-step evaluation suitability so that we can do it kind of a bite at a time.

So, we'll see how that goes and, you know, another year or so, we'll know how we're doing. Maybe we won't be on schedule, maybe we will. I can't predict that. So, I just want to remind you of that.

Also, we talk about stakeholders here, and like it's just the people in this room, or just the people that are opposed to Yucca Mountain. You have to remember, we have stakeholders in the utilities, the rest of the country, the Congress. Dan Dreyfus is a Presidentially-appointed head of the program. Congress have set up the TRB to oversee the program. There is a lot of public involvement in the process already. We could do better, but I think the implications that there is not enough involvement of the stakeholders is really not a accurate description of this program. This program probably has more oversight and involvement of the various parties than many other programs in the country.

Finally, I'd like to make a statement about--I would like to read a paragraph from the report that we submitted to the Technical Review Board on April of this year. On April 8th, Dan Dreyfus sent a letter report to the Nuclear Waste Technical Review Board, and this represents

DOE's position on subsystem requirements, and since that's what we're really talking about at this meeting, I'd like to read this statement just for the record:

"The NRC implemented Regulation 10 CFR Part 60 contains subsystem requirements in Section 113 that addresses the performance of specific subsystems to radionuclide release, rather than the overall performance of a geologic disposal system. While it is important that the regulations require the use of multiple barriers, both engineered and natural, there is no need to specify individual, quantitative subsystem requirements.

The quantitative subsystem requirements presently contained in 10 CFR 60.113 have been justified as providing assurance of defense in-depth for a repository. However, they have the potential to unnecessarily constrain the repository design without a corresponding health benefit, since there is no technical linkage between these requirements and total system performance.

An overall performance limit, such as individual dose, together with other provisions of 10 CFR Part 60 provide adequate assurance of defense in depth. Specific subsystem requirements, if provided, should be qualitative in nature. If the NRC chooses to provide any quantitative subsystem guidance to supplement the regulations, such guidance should be derived from and consistent with the overall system requirements."

So, this represents DOE's official position, as stated by Dan Dreyfus, in this submission to the National Academy of Sciences Panel.

DR. DOMENICO: It's beginning to sound a little like yesterday.

Go ahead, Mal.

MR. MURPHY: And Steve, I apologize for putting you on the spot here, but you do have a stamp on your head that says, "I'm the senior DOE official," so that's a target.

What is the Department of Energy's position in its proposed program approach with respect to changing the current statutory and regulatory framework? It seems to me what you just said--and I read that letter, you know, back in April myself, and came to the same conclusion myself, and I've had lots of discussions with lots of people from DOE on this subject.

Are you suggesting, is DOE asking the NRC to change its regulations? The PPA, it seems to me, is founded on the proposition that the Department is going to be able to make a technical site suitability decision in '98, go to licensing in 2001, get its license, do a hundred years worth of stuff, et cetera, et cetera, without any changes to the regulations, but we keep hearing things, like the letter from Dan Dreyfus, and like your statements yesterday, and statements from other

DOE representatives, that these regulations are mushy, or this is wrong, or we really need to change this, or this needs to be tweaked, some of which I agree with, you know. I mean, I don't think you can do this without changing the regulations.

Are you asking NRC to change its regulations?

MR. BROCOUM: We have asked the National Academy to consider this in their recommendation report, which will be submitted to the EPA, I believe, in December of this year; that the 1982 Waste Policy Act mandated a three-step process, the National Academy study, and the fact that the EPA would then look at that study and modify its regulations, and, finally, that the NRC would conform their regulations to the EPA standards.

So, we saw that as a process to improve the regulations, so this recommendation was in that spirit, to improve those regulations. Until they're changed, we are proceeding ahead on our suitability and licensing evaluations under the current regulatory regime.

MR. MURPHY: Well, let me put it more specific with respect to the subject we're here in town to talk about today and yesterday. The NAS committee's mandate from Congress is to take a look at a health-based standard based on individual, or based on doses to the public, so I would not assume that the NAS--if I were in your shoes, I wouldn't assume that the NAS committee is going to make a recommendation that goes beyond that narrow mandate from Congress. I certainly wouldn't assume that the EPA, even if the NAS committee does, that the EPA will go beyond that recommendation, but let me pose a specific question.

With the current confusion, or the current lack of specificity with respect to the application of the definition of the disturbed zone to the ground-water travel time issue, can DOE make a technical site suitability determination, and can it write a license application by 2001 with information in that license application showing the Nuclear Regulatory Commission that you're complying with the 1,000-year ground-water travel time, or if you're not complying, there's other reasons why you should still get your license, without some further regulatory action to either eliminate the disturbed zone requirements from the regulations itself, or to further pin down what that definition means?

It seems to me you're telling us that you intend to build a deck on the side of my house, without knowing where the living room wall is going to be. You're going to build a 19-foot deck, starting from the place that most of the people usually sit when the weather is warm in my living room, and can you--and yet, you're still saying, don't worry about putting the wall, the outer wall on the living room. I can still build your deck, and we'll hang it out there somewhere and you'll be satisfied with it. Don't worry. You'll be reasonably assured that this is a good deck at the conclusion of this proceeding, of this process. I don't know how you can do that.

MR. BROCOUM: I don't understand your deck analogy, but we--

MR. MURPHY: It's the only one I could come up with.

(Laughter.)

MR. BROCOUM: We are taking, as best as we understand, the ground-water travel time requirement of the NRC, ours and 960, and coming up with a methodology which was explained yesterday. We're not assuming any changes in the law, in our methodology, and we have a three-year schedule to get that done, which I think you've heard described.

So, the answer to your questions are yes and yes, for the suitability and license, at this point in time.

MR. MURPHY: Do you know yet where you're going to start measuring ground-water travel time from? I mean, you're going to have to start writing a report in 18 months--

MR. BROCOUM: We don't--

MR. MURPHY: --on whether or not the site is technically suitable for a recommendation to the President, and for taking a license application to the NRC. If you go to the President with a suitability recommendation, it must be accompanied by a certificate from the NRC saying:

"Characterization is complete. There is an adequate database here for us to begin processing a license."

Where are you going to start measuring ground-water travel time? Is it going to be 50 meters from the canister? Is it going to be five, is it going to 500? Where is it going to be?

MR. BROCOUM: I don't know. I don't know where it is, and that's fine, because I'm not the person responsible for doing this. These fellows here are. It's their responsibility to come up with that methodology and that recommendation.

MR. MURPHY: They don't have "Senior DOE Official" stenciled on their forehead.

DR. DUGUID: Anyway, let me respond to that. I don't think we know where the boundary of the disturbed zone is.

MR. MURPHY: That's my point.

DR. DUGUID: Nobody has ever tried to define it. We have an approach that we are going to take to define it, and if you look at the schedule in that approach, the report that will be used in the site suitability analysis, the final report--I can't remember the date on it, but it starts sometime in fiscal '97, so we are going to take a couple of iterations in defining this, and until we get into it, I don't think we know how difficult it's going to be, and that's why it's going to take a couple of iterations to get in there, see if we need more data, see what we need, see what difference it makes.

Let's say we find a disturbed zone, and let's say we say it's about 50 meters, plus or minus 25, or plus or minus 50. It's not going to be very exact, because there are a lot of uncertainties. We still have a repository that we haven't pinned down the loading for, so that, alone, is going to make that boundary very indeterminate. You know, for each different loading, you will have a different disturbed zone.

So, I think we need to get into this, find out what the difficulties are, before we decide it can't be done.

MR. MURPHY: Well, I didn't suggest that it can't be done. I'm suggesting it can't be done by 1998, or by 2001. We're not suggesting that you can't do this. We're saying you can't do this in the amount of time that the Department has given you.

MR. BROCOUM: Yeah, but we have to--

MR. MURPHY: And with the data that's going to be available by that time.

MR. BROCOUM: But, I mean, we'll see how we do. I mean, I don't--that should not be an argument for not proceeding ahead, and it sounds like you're trying to make an argument that we shouldn't be proceeding ahead.

MR. MURPHY: No, no, no, no. I'm not arguing that you shouldn't be proceeding ahead. I'm arguing that you shouldn't--that you need to continue gathering site-specific empirical data, and I'm, you know, it's the scientists that I'm relying on in that respect. I'm not saying that you shouldn't proceed ahead. I'm saying that you're being overly optimistic, which has been a characteristic of DOE's attitude in the past, throughout this program, I think.

You're being very overly optimistic in thinking you can do this by 1998 with the amount of data that it seems to me you're going to have available at the time, and with the continued uncertainties surrounding the interpretation and application of very significant parts of the regulatory scheme.

MR. BROCOUM: But, remember, we have, in our suitability evaluation, we have a step of, you know, putting together all the technical information and having it reviewed by the National Academy of Sciences, and they will tell us whether the data is adequate, and whether the, you know, the analyses from that data are reasonable. So, we have built in a totally independent review group, of which we will have no control over, nor will any of the other stakeholders, and so, you know, and that will all be discussed in October. But, there is a step in there for some, how shall I put it, disinterested party to help define how much information is really necessary.

DR. DOMENICO: There's probably time for one more comment from anybody, a question. Yeah, Paul?

DR. KAPLAN: I have a fundamental problem with some of the things I'm hearing here that goes back to ground-water travel time.

We've had a number of parties get up and say ground-water travel time is a wonderful criteria. I know exactly what it means, but, yet, we have had very different definitions and interpretations, but this morning, Marty got up and made it very clear, wonderful criteria, here's what it means, and he defined it independent of quantity. We had the NRC get up, and others get up and say, wonderful criteria. Despite the fact that we don't mention quantity in it, it's implicitly there, and we're going to consider all these other things.

I am having problems seeing where there is consensus among the parties here as to what it is we're talking about calculating, or what it is that's going to be the basis for the suitability analysis, and until those fundamental questions are resolved, I have no reason to believe things aren't going to continue to go around in circles.

I thought hard before coming here, do I want to present an alternative criteria? Do I want to throw the baby out with the bath water? And after my eight years here--and I know there's people here whose experience goes back at least half a decade past that--I have no reason to believe we're not going to meet eight years from now and experience the same debate.

Some of the faces will be the same. Others will sit here and assume it's the first time we've put it on the table, but, basically, some of the discussion we're having doesn't make sense until we have at least agreed to the rules, and we have not agreed to the rules.

DR. DOMENICO: Marty wants to respond, or agree; join, or something.

DR. MIFFLIN: I would buy into your comment, but for one particular point, and that is that the ground-water travel time is focused on the rapid pathway, and that goes back to the general concept that, if possible, that the site should isolate the waste from the seismic geologic environment.

So, I don't have any problem whatsoever if somebody comes up and demonstrates there's bomb Chlorine-36 at some depth in the mountain, and saying that represents a measure of a rapid pathway. There's no question. I don't know how much water went down. I don't care. It just proves, beyond much scientific doubt, that there was a rapid pathway, bomb Chlorine-36 got there. It's a good measure. It shows that liquid water can get to such-and-such a level.

Now, I agree, that's not the average travel time to that depth. So, you know, in a site-specific sense, there is certain tools that are available, and it's possible to determine within the intent of that site selection criteria what travel times are.

So, I find that, you know, DOE selected this site. They said it was a good site. I find it a little surprising --well, I guess it's not surprising, but when we find it's difficult to get back to meeting some licensing or site selection criteria, that we want to change the rules of the game, I find that a little bit, you know, like crying about spilled milk. It's time to settle down and characterize the site, with site-specific data; that's all. There's ways to do it.

MR. BROCOUM: I want to make one correction. DOE did not select this site. It selected three sites to characterize. Congress selected this site.

DR. MIFFLIN: DOE assured Congress that any of the three sites could be licensed.

DR. CANTLON: I just am raising a question to Marty. If you find bomb chlorine below the repository level, which is my understanding, it's in the perched water below, if one assumes that that pathway is outside the footprint, so what? What does that say about containment of the repository?

DR. MIFFLIN: Very good point. There is no sampling in the footprint; right? And we have the type of hydrogeology where you have great variations in probably recharged pulses in terms of areal distribution. We have great variations in fault and fracture networks. We have a very, very complex, geology at that site.

So, a site characterization program needs, in terms of data collection, needs to deal with those types of problems and focus in on that we've some reasonable assurance that the range of possible site-specific controls on the hydrology have been adequately tested. Then there's other ways to back into it, too. It's not just bomb Chlorine-36. There are some other approaches as well, but it does have to be characterized with an actual site-specific database.

DR. DOMENICO: I'm going to call this to a halt here.

MR. BERKOWITZ: Pat, I'd like to say something, if I may, please?

DR. DOMENICO: Yeah. Go ahead, Les.

MR. BERKOWITZ: Les Berkowitz, M&O.

I'd like to say this: The mandate we have been given is to determine whether the disqualifying condition in 10 CFR 960 is applicable. Our mandate also is if we find it isn't applicable, to go ahead and determine whether or not ground-water travel time at the site complies with the requirements in 60.113(a)(2). That's the job we've been given, and that's the job we're going to try to do.

Now, the essence of the process that we're involved in is that we've got to produce something to provide to DOE, and for DOE to provide to any other entity it feels it should provide this, to support the decisions the Department has to make. That's the essence of the process.

Up until this point in time, there has not been information of that kind provided to anyone, and, in fact, we're exercising both 10 CFR 960 and 10 CFR 60 for the first time. We're learning, as we go, how to operate within, if you want to call it that, a regulatory environment. It's a different environment from the environment in which the Department and most of the people working on this program have ever operated, and it takes time to learn how to do it.

Now, we're going to be working as hard as we can to meet the schedule. We're going to use the data that the various people involved in characterizing the site are going to obtain, and as we conduct our work, and at appropriate points in the process, there will be opportunity for review by the Nuclear Waste Technical Review Board, and other appropriate bodies.

We can't say any more than that. I think we're beating a dead horse. I think it's time for us to get on with the job.

DR. DOMENICO: I think we have a large, long-term discussion later in the afternoon. I think at this time we should, in order to keep up with the program, take a very rapid ten-minute break to clear the room here, and then invite Dwight Hoxie up to give his presentation.

(Whereupon, a brief break was taken.)

DR. DOMENICO: Can we reconvene here?

I want to alter the topic a little bit by addressing the site characterization work supporting performance assessment, and the ground-water travel time, and the first presentation is going to be by the fellow I was looking for yesterday, Dwight Hoxie.

DR. HOXIE: My role in these proceedings today is to serve as a bridge, a bridge over troubled waters, methinks, but what I want to talk about now is, essentially, the connection between performance assessment, of which we've been discussing just a little bit in terms of ground-water travel time, one aspect of performance assessment, and the site characterization issue, and this afternoon, we're going to hear a couple of talks, primarily dealing with hydrology that are going to be looking at site characterization, that is providing data as input to performance assessment, including ground-water travel time.

So, let me, first of all--oh, let me say one other thing. When we talk about performance assessment and site characterization, frequently, we speak in terms of a dichotomy, as if they were two separate, non-related entities that somehow interface in some nebulous gray area in between, and this has led to the enunciation of something that may be called a performance assessment paradox, or, perhaps, more appropriately, a pseudo paradox, and it was originated, as I understand it, by Piet Zuidema from NAGRA, which is the Swiss program, and it was the subject, essentially, of a rather lengthy workshop that was sponsored by the DOE and NAGRA in Albuquerque, hosted by Sandia, this past May.

And what we were looking at is the interface between site characterization and performance assessment, and, essentially, the idea, going into this workshop, is that, well, site characterization collects data that performance assessment can't use, but performance assessment needs date that site can't measure, and so this seems to be the, perhaps--we hope, anyway--is not an irreconcilable difference between the two.

So, what I want to do is talk a little about how these two, or how to resolve this particular pseudo paradox. First of all, I want to suggest, we don't have a dichotomy, but we really have a triad, because what we have is performance assessment, we have site characterization that feeds data into performance assessment, and we have engineering design, that also feeds data into performance assessment, and by engineering design, what I'm really referring to here is information on the waste package, the waste form, the waste container, and so forth; this kind of information.

But these all interrelate. We have performance assessment directing site characterization and design, and vice versa, so I'm showing it this way to try to indicate that we have a continuum of activities that are going on, and what I've done--well, one danger that I have here is that by showing it in this circular sort of mode, this is a very dangerous diagram, because it may accuse us of circular reasoning, of course. So, what I'm doing is saying, look at this in the vertical, and look at these down here as being foundation activities that support performance assessment.

So, what do we have? We have site characterization. What does it deal with? Well, it's really looking at the properties of the natural barrier system, the geologic setting, the hydrologic setting, geochemical, and so forth. Engineering design, now, are the properties of the engineered barrier system and its components, so this is what I mean now by foundation activities.

So, now we can resurrect a pyramid, and we saw the pyramid shown this morning, briefly, and it's indicated here as kind of a hierarchy of different kinds of models and activities, but over here, we have performance assessment grading down to our foundation activities. The foundation activities are the pedestal on which our pyramid rests. We have site and design down here, which involve field and laboratory data collection, and then, rising out of all of this, we have a whole host of models. We have conceptual models, we have framework models, process models, subsystem models, and, finally, we get up here to total system models.

And we talk about all these different kinds of models, and, you know, it's one of the things about human beings in science that always like to categorize things, so we always have to put things into our little boxes, and so we have boxes for all our various kinds of models.

One thing I'd like to have you notice about the pyramid, it's just a little bit too light in here to see this, but if you were to go out tonight, perhaps, and look off towards the intersection of Tropicana and Las Vegas Boulevard, you might there just sort of see the radiance of enlightenment projecting heavenward from the apex of the pyramid.

So, one of the things that we've been debating here this past, well, half day yesterday, half day today, has to do with semantics, definitions, and I've taken just a little attempt here to be Humpty Dumpty myself, and provide some definitions of all these various kinds of models. These have not been reviewed and approved by DOE, USGS. They are my own little concoctions, if you will.

But, just so we have some idea of what we'll be talking about this afternoon, when you'll be hearing people talk about the various kinds of models, a conceptual model is really a fundamental thing, and that is hypotheses concerning the state of a system and those processes that determine that state, control that state, and cause this system to evolve, and these are testable hypotheses, as Paul Kaplan was talking about this morning. We need to be able to test and disprove these hypotheses.

A framework model, really, is just a representation of the structure and geometry of a system, so, by example, it's the geologic framework model at Yucca Mountain which would be prone to this kind of category.

We have process models, and, generally, these are a mathematical representations of the processes that are controlling a particular system. Darcy's Law, for example, I would argue is a process kind of model, formulating that into a mathematical equation.

We have subsystem models, and these, generally, are going to be quantitative representations of the response of a system to outside influences, to processes internal to the system, and also to the boundary conditions, initial conditions, and so forth, which govern the system's behavior. So that's what I mean by a subsystem model. A subsystem model, for example would be the UZ hydrology model at the site, or a saturated zone flow model for the site.

And then, finally, getting towards the apex of the pyramid, we come to our total system performance assessment models, and, again, this is a quantitative representation that is intended to do something like evaluate the behavior of a system with respect to some specified performance objective, like ground-water travel time, for example, if we can agree on that as being an appropriate performance objective.

When I say quantitative representation, that term, quantitative, would include things like deterministic-type models or stochastic-type models.

So, these are the kinds of models, and I would like to emphasize that in this game of assessing the suitability of the Yucca Mountain site, evaluating the performance of a repository system at the site, and, indeed, just in understanding the geologic, hydrologic processes that are going on at the site, we rely on models.

We rely on this suite of models, and so they are not really a hierarchy, they are really a plexus; the idea of their being interconnected. They interface, and this is where we interface with performance assessment. By "we," I mean, the site program interfaces with performance assessment. Site characterization has to be intimately involved in the development of performance assessment models. Performance assessment, in turn, has to come down from the pyramid and interact directly with site characterization to develop the fundamental foundation-based models on which the evaluations are going to be made.

And we have done this, to some extent, when we developed the TSPA iterations that have gone on thus far, and the most recent one is the TSPA, total systems performance assessment, in 1993, and out of this particular exercise, there developed a series of recommendations or data needs, if you will, that performance assessment was asking site to provide, and so, I've just highlighted the most important ones that I identified out of TSPA-93, and what I'm going to ask you to do is that this afternoon, you can just take this particular slide, and you can check off when my colleagues from the Survey give their talks, to make sure that they address all of these things, and if they don't, then you can raise questions at the end.

But, one of the things that performance assessment has said is that we really need to know "How is flow occurring at Yucca Mountain?" and we've heard that repeatedly today. Do we have an equivalent porous medium kind of system out there, or do we really have preferential pathways that are controlling the flow of groundwater into the system and through the system?

We're concerned about gas-phased releases of Carbon-14, and the transport medium in this case is air, so we need to know what the rock mass bulk permeability is; that is, the air permeability of the rock mass. We need to measure that, and, of course, we also need to know what kind of sorption properties that carbon dioxide may have for the tuff rocks in the environment which exists at Yucca Mountain.

Another very important thing is that our mode of transport at, essentially, any geologic repository site, means the dominant mode, that we heard yesterday, is going to be groundwater, so we have to get water into the system that can contact emplaced waste and transport radionuclides to the accessible environment. So, we're really concerned about what the spatial distribution, the flux is at the repository horizon, and how that spatial distribution is going to change in time, for 10,000 years, 100,000 years, whatever the long-term performance requirement is going to be.

We need to look at the horizontal and vertical dispersive properties for the saturated zone system. If we are going to be dealing with a dose standard, we're going to be concerned about dilution of potential radionuclides getting into the groundwater system, so we need to know what the effective thickness of that system is going to be.

And, another very important problem that needs to be addressed, we've talked about it many, many times. It's a fundamental problem with any kind of hydrologic or system modeling, and that is we do all of our measurements, usually, in a laboratory or in very small-scale field experiments. How do we scale up to model scale kinds of applications? That certainly is a problem, and these are the kinds of things that need to be addressed.

So, once again, I recommend that in the talks this afternoon, check them off and see how well we're doing in addressing these kinds of issues.

Now, what I'm going to do is, I'm going to digress, and I'm going to go on to just a slightly different topic, and this was a special request, actually, and that was to give you a very brief update on what the planned testing is for the exploratory study facility at Yucca Mountain, and I initially have to apologize, because I am not involved with ESF testing. The ESF testing and planning and all this kind of thing is being done by Los Alamos National Laboratory, but those good, kind folks, along with my supervisor, Bob Craig, are, as we speak, somewhere in Sweden, and so I'm here to sort of fill in.

By saying that, what I'm really saying is that I probably am not going to be able to answer many of your questions. Technical questions probably would have to be directed, perhaps, to Ed Kwickless and Dick Luckey this afternoon. I can try. I'm going to show you two different slides here.

First of all, this is the north ramp, descending down here towards Topopah Spring. This would be the main test level, and the south ramp over here, the north ramp extension. One thing I should probably point out to everybody is that I hope--and this is a quality check--the Board should all have this view graph in color; right? This is red down here, and there's some blue and some green, so I hope that worked. Everybody else gets it in black and white, which is probably not as wonderful.

The other thing is, is that what you see right here is a test alcove location. It is the same slide that's shown over here, and I'll try to stand in the middle. So, right now, there are planned to be seven test alcoves that would be constructed in the ESF, and I think the plans right now would be to daylight the ESF at the south ramp sometime in calendar year 1996, so this is the kind of testing that would be done, and that would be available of a, say, in 1998, suitability decision, for example.

Our first alcove is Alcove No. 1. That alcove exists. Tests are being conducted in it, presumably, as we speak.

The next alcove that we're going to be talking about, Alcove No. 2, is shown right here, and that's at the intersection of the north ramp with the Bow Ridge Fault, so we would want to investigate the properties of the fault at depth.

We have two more alcoves down here, No. 3 and No. 4. These are going to be at the top and bottom contacts of the Paintbrush non-welded hydrogeologic unit, so we understand what that contact is doing hydrologically.

When we get down to No. 5, is presumably this alcove would be constructed in the hypothesized Drillhole Wash structure. We think that that might be a fault, but we don't really know. We don't know what its hydrologic properties might be.

Coming on down into the main test level itself, or the main ramp, we have an alcove that would be constructed at the Ghost Dance Fault, and where it intersects, presumably, the Sundance Fault, to try to understand the hydrologic properties of these, of that fault intersection.

The seventh alcove would then be, again, on the Ghost Dance Fault. There are also plans, of course, perhaps, to construct alcoves on the south ramp, as we ascend towards the surface. There is also discussion that we need to have some kind of access over to the Imbricate Fault zone that bounds the east side of the Yucca Mountain block.

Also, it is planned, currently, as I understand it anyway, is that once we got to depth, essentially, down here at Alcove No. 5, we would then, with a second TBM, excavate the north ramp extension over towards the Solitario Canyon Fault, and we would construct alcoves in the north ramp extension to conduct heater tests. So, it's going to be some time before we would have in situ heater tests, but those tests would then be capable of providing some data to support a definition of the disturbed zone, for example.

So, over here, is, again, the test cove location, the alcove locations, and the tests that are going to be conducted in those particular alcoves. I won't go into any great detail on those, because you should all have some additional material provided in your package, which includes a brief description of the ESF. It also includes a rather more detailed description of the testing that is being done in Alcove No. 1, and there is also a listing, with study plan identifications of all the tests that are planned to be conducted, as I just outlined here, in this seven planned alcoves and accesses from the ESF.

So, with that, I have constructed my bridge, and this afternoon, after lunch, we will return and we will talk about the problems of ground-water hydrology, site characterization as they relate to performance assessment.

Thank you very much.

DR. DOMENICO: Thank you, Dwight.

Any questions from the Board?

DR. CORDING: The extent of the exploration you described is to be finished for use in the licensing process; is that correct?

DR. HOXIE: Yes. The test that we have--in the seven alcoves--there's also construction-phase testing that goes on continuously as the tunnel is excavated; geologic mapping and sampling within the facility, also.

DR. CORDING: But the current plan is to have a north ramp extension that would go across and to the Solitario Canyon before--early in this program; is that correct?

DR. HOXIE: Yes, that is correct. At the present time, that is the plan.

DR. CORDING: I see that as an extremely important aspect, because it's one place where you are getting, at least at the north end, you're getting across those north/south trending major structures, and if that isn't done, there's a lot of real estate that you don't ever get a chance to see what might exist between the Ghost Dance and the Solitario Canyon Fault.

DR. HOXIE: That's very true, and there is no plan at the present time to access Calico Hills. There is a systems study to be conducted in 1995 to evaluate that question, so this does not include any ramps into the Calico Hills at present.

DR. CORDING: That aspect of it concerns me some, and I'd be interested in hearing, perhaps even this afternoon, as well as your own thoughts on what are indicators of how one makes the decision to go to that, and how rapidly you can then mobilize to do that, if one feels that the Ghost Dance is important. I had understood that the Ghost--or, excuse me--if the Calico Hills is important. I had understood that the observations in the Ghost Dance would be used as an indicator of whether you went down to the Calico Hills.

DR. HOXIE: Well, one plan would be to access the Ghost Dance, and if it's dry as a bone, perhaps we don't have to worry so much about the Calico Hills. If we find that it's dripping water, then maybe we have to be concerned that it might provide us a fracture connection through the Topopah Spring to the Calico Hills.

DR. CORDING: I see that as--in making decisions at that point and not being ready to proceed could be, I think, physically almost impossible, then, to get down to the Calico Hills in a timely fashion. I think that you may have already gone beyond the time that one can do that and accomplish it, certainly, before licensing, so I'd like, perhaps, some further thoughts as to what decision one really would be able to make once you hit the Ghost Dance, or whether you can make that decision right now as to whether you think it's appropriate to go to the Calico Hills, and whether plans need to be made at this point to do that.

DR. HOXIE: Well, there was a plan one time to go to the Calico Hills first, so--and it is--

DR. CORDING: I'm not talking about first, but I'm just talking about a plan that takes into account what one might be encountering here, and being ready to do it in a timely fashion, because there's a lot of time constraint here if you're going to meet the licensing date.

DR. LANGMUIR: Langmuir; Board.

Dwight, you mentioned that in the scheme of things, the thermal testing would be starting after the seventh alcove.

DR. HOXIE: No, no; no.

DR. LANGMUIR: What did you say about the timing of the thermal testing?

DR. HOXIE: I think the idea, actually, I think with the timing and the way it's planned right now, as soon as they make the turn, the north ramp makes the turn at the main test level, that we would then come in with the second TBM and excavate the north ramp extension, and get over there and get the heater tests underway.

I don't know what the exact schedule is. May Bill Clark or somebody from Livermore would know, but I think the plan is to get there by 1996, and get those heater tests underway, so it's not to wait until the seventh alcove.

DR. LANGMUIR: Okay.

DR. DOMENICO: Questions from the staff?

DR. PALCIAUSKAS: What are the specific hydrological tests that are being planned for the investigated major faults? Are they purely observational, or is there something more specific?

DR. HOXIE: Oh, I think we would do radial borehole testing; that is, air K testing. We certainly would do hydrochemical sampling, probably sample all the fracture minerals that we could find to get some idea of when those deposits were--and of ages of deposits, what they are, where they came from; Chlorine-36, certainly.

So, I mean, I think there's a whole--if you look at the back of the package, in the additional information, you can see the entire suite of tests that actually would be conducted, and I think we also have to have some latitude, some freedom to design tests once we see what the actual conditions in the fault zone are. We don't know yet. We haven't examined the fault subsurface. That will be, probably, a very enlightening experience.

DR. DOMENICO: Any further questions?

DR. CORDING: In looking at the southern access there to the Ghost Dance, like six and seven, recognizing that the Ghost Dance is not just a plain, but a zone, and that there are other zones that one is likely to encounter, I'm wondering what extent of the east/west driving, say, at Location 7 there will be, what extent of driving of these tunnels east/west there will be, because you could be hitting several different zones within the Ghost Dance. There could be hundreds of feet, as well as adjacent features that would be desirable to get some idea of, and at that level and at that position in the repository; in other words, near the southern end, to get something that goes east/west that allows you to see something of those typical structures.

So, I think that, perhaps, my perspective would be it's important to recognize that Ghost Dance isn't a plain, it's a zone, and that there are others, certainly, that are going to be parallel to it, and offset from it.

DR. HOXIE: Well, I think you're absolutely right, and we know, on the surface, that it seems to be a zone, so I'm sure that the PI, who will be in charge, principal investigator who will be in charge will have the freedom, I would hope, anyway, to determine how extensive the excavation has to be to ensure that, you know, they're sampling a fault in its entirety.

DR. CORDING: And I think, maybe, it'd be interesting to see if that really, you know, if that gets written into the plan so that there's an understanding that that sort of thing can be done or will be done.

DR. HOXIE: I think most of our study plans allow that, for the PI to make field changes, essentially, on the basis of conditions that they find.

DR. CORDING: Or even coming up with what you think is a reasonable estimate ahead of time, so that you don't make an assumption that it's going to be a lot less than it needs to be, at this point.

DR. HOXIE: Right.

DR. MIFFLIN: I've got a question with respect to the extension there on the north ramp, and the concept of the heater experiment with respect to timing.

Did I understand that that extension would be made after the loop is made, or--

DR. HOXIE: No, no, let me correct that misunderstanding. The plan would be to come down the north ramp and, just as soon as we get around the turn with the big TBM--actually, we would use the big TBM to excavate an alcove, as a starting point for a smaller TBM. Then, that would construct the north ramp extension. That is the plan right now, so the idea is to begin excavation of the north ramp extension just as soon as we get to this point in the mountain, and not to wait.

DR. MIFFLIN: And that would be a smaller diameter TBM?

DR. HOXIE: Yes, I think so, like 15 feet or something.

DR. MIFFLIN: Then, the other question I have, just in rough terms, when would the actual emplacement of the heater test occur, timing, this two years from now, three years from now, or what?

DR. HOXIE: I think, actually, they're looking at 1996, 1997 time frame.

DR. CHESNUT: Dwayne Chesnut, Lawrence Livermore.

As I understand it, the current thinking, that we'll go with this north ramp extension as soon as it's operationally feasible, and excavate that part of the tunnel, and then the test alcove, and I believe that about the earliest we could actually get the tests started is about '97, because there's a lot of instrumentation and a lot of underground stuff that has to go on after the access is there. So, I think it's about a year after the access, is what I understand the current schedule is.

DR. HOXIE: But, according to the present schedule, we should be here in 1995, so that's why I was saying '96 or '97.

DR. MIFFLIN: I was leading to another question, actually. How long does it take to get useful results on the heater tests once you start it, once you're set up? As I recall, depending on the thermal load and the scale of instrumentation, and so forth, there is quite a time period for useful results as far as above boiling-type of temperatures.

Is there some type of rule of thumb that comes along with this? Because it sounds like we're out around 2000 before we might have actual data, and then that has to be reduced and interpreted.

DR. HOXIE: I think the idea was, for the initial heater tests--and Dwayne, correct me if I'm wrong--was that they wanted at least a period of two years of data collection.

DR. CHESNUT: Well, the problem at the hole--I mean, this is a problem with the timing, and all we're going to get, probably two to three years worth of actual data before we go into license application. The idea is that we will have some tie back into the large-block test, which is much smaller scale. We will look for nasty surprises in the first couple of years in the underground tests, but the final set of data and analyses and things that have to go into the ultimate thermal loading decision cannot be made until after 2001. I mean, I think that's the current schedule that the project has laid out, and the whole strategy for the proposed program approach is to defer the final thermal loading decision until sometime that we can actually get the data on a scale that we think is meaningful for looking at the thermal effects. Dale Wilder could probably give you a better idea of the schedule, but I think that's pretty much where we are right now.

DR. DOMENICO: I think, again, we're allowing time this afternoon, and we're running five minutes behind schedule, so the schedule calls now for lunch, and a return at one o'clock.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. DOMENICO: Well, we have to start here, keep on schedule. Ring the dinner bell. We're going to continue where we left off this morning. Looking at the site characterization work, supporting performance assessment and ground water travel time, and we're going to be talked to here by Ed Kwickless from the USGS. Ed?

DR. KWICKLESS: Good afternoon. When I gave a talk to the Board a few months ago on Bo's behalf, little did I realize I'd be seeing you all again so soon. I couldn't imagine what else I could have to say that I didn't tell you a couple months ago, but I managed to find a few things.

I'm going to report on progress that we've made recently in the unsaturated zone studies, and unlike much of the discussion that you've heard here in the last day or so, I'm going to try and concentrate on what we know based on site data, as was called for by Marty Mifflin, amongst others.

The presentation outline shown here. What I'd like to cover is pretty wide-ranging, and more or less follows the sequence from the ground surface to the water table. I'll start by attempting to summarize a lot of the work that Alan and Laurie Flint and others have done on studying infiltration at the surface of Yucca Mountain. I'd like to follow that by presenting actually some numerical simulations that are pretty closely tied in with data from a wash in the northern part of Yucca Mountain that I hope will illustrate a couple of concepts that I think are key in terms of unsaturated zone flow phenomena.

I'd like to describe our laboratory experiment that we've done on a block of rock in Denver to try and better understand the behavior of flow and fracture rock, unsaturated fracture rock under fairly well controlled conditions. I'd like to describe to you some ideas we have about important matrix properties that aren't always measured, and present some correlation amongst matrix properties that may be important when you do the types of calculations that were described by Rally Barnard yesterday.

Getting back to the site data, I'd like to talk about the permeability and fracture data from UZ-16, what isotopic evidence exists for deep, transient fracture flow, talk a little about gas isotope data that's been collected over a period of many years and which we've just begun to try and model at the USGS. I'd like to summarize what we know of perched water systems and just talk a little bit about the nature of the unsaturated zone-saturated zone boundary, and finally conclude with a summary.

When you speak about infiltration, you have to talk about one of the most valuable data sets that's been collected over the last ten years or so, has originated from roughly 90 shallow boreholes that are located in various topographic and geographic locations across the mountain.

The work that I'm going to describe to you was presented by Laurie Flint at the last high level waste conference, and she very nicely laid out the purpose of these boreholes, and that is to identify locations where infiltration is both presently occurring and determine the dominant controls on net infiltration. And these included the type of outcropping geologic formation, the topographic position, the slope aspect and depth of alluvial cover. So the next several slides that I show will illustrate some of the trends that they were able to identify showing the influence of many of these different factuals.

The neutron holes as I said, have a great geographic and topographic distribution, and were located with the intent to capture the controls that I've described earlier. One important aspect is the outcropping geologic formations.

This map shows the outcropping geologic formations located beneath the alluvial cover, and the matrix permeability is typical of those types of outcropping formations.

On the ridges and sideslopes of the moderately densely welded tuffs are the predominant outcropping geologic formations. In the washes and on the Solitario Canyon, we see moderately welded and non-welded outcroppings, and these are generally exposed only in the washes or along the Solitario, and their alluvial cover is fairly thick, at least in the washes in the northern Yucca Mountain.

What's important to remember is that the moderately welded to densely welded intervals have permeabilities on the order of just a few millimeters per year, and so any infiltration that exceeds that value is likely to occur as a result of fracture flow.

This shows some typical moisture profiles measured by neutron logs. And the data that I'll be showing you originated and were obtained from monitoring during the winters of 1992 and '93, and these were some of the wettest conditions that were out at the site within the last ten years, and so provide a record over a fairly hydrologically active period.

What we see is at the ridgetop where the alluvial cover is either thin or absent and the rock type is predominantly moderately or densely welded, that we can get moisture penetrating to depths of 10 meters or more in several of the holes. And the idea here is that where the alluvial cover is thin or absent, there isn't much buffering capacity to hold the water in the near surface where it can later be removed by evapotranspiration, and so the contact between the overlying alluvium and the rock becomes wet relatively easily compared with locations where the alluvial cover is thicker.

And so we see that for two holes in WT-2 Wash, which is also home to UZ-16, which we'll be talking about, we see fairly deep penetration of moisture over the monitoring period, and in Pagany Wash, which is located north of Drill Hole Wash, we also see where the alluvial, even though in spite of an alluvial cover of about two meters, we see some deep penetration of moisture at this hole.

DR. LANGMUIR: Would you maybe stand to the side a little bit so we can see them when you're talking about them.

DR. KWICKLESS: I'm sorry. What you see here is the moisture content profiles for holes on the side slopes, and what these examples tend to show is the effect of slope aspect. And slope aspect is important because it controls the amount of net radiation reaching the ground surface, and on the north facing slopes we see less net solar radiation, and so what one would expect, all other things being equal, they would be more likely to experience deep penetration of moisture on a north facing slope compared with the south facing slope.

And in this data set, each of these three holes has about the same amount of alluvial cover, and the only difference between them is the slope aspect. On the north facing slope, N-53, we see a fairly deep penetration of moisture, to a depth of below 12 meters or more, where on south facing slopes for the same wash and for the same depth of alluvial cover, we see a much shallower penetration of moisture. And while there might also be some local features such as localized runoff, a nearby flow fracture might also have influenced these results. There have been enough of these types of observations to suggest that slope aspect may be an important factor in determining where deep penetration of moisture occurs.

This shows moisture content profiles for the channel, active channels, and adjacent terraces. Channel N-51 was also from W-2 Wash and shows how a relatively thick alluvial cover prevents moisture from reaching the alluvial tuft contact at least during the monitoring period, and presumably has prevented water from entering the bedrock.

Channel N-7, which is located in the active channel of Pagany Wash, was also shown that during the fairly wet couple of years, moisture didn't penetrate below a depth of about three or four meters. Similar observations were for a terrace in Pagany Wash at N-14.

So it appears that in the absence of runoff, that thick alluvial cover is very effective in preventing deep penetration of moisture. There is evidence that following runoff events, however, that moisture can propagate below the depths that would be expected.

This actually shows the change in moisture content as a function of depth for two locations. One is Pagany Wash. It shows that for five holes located in Pagany Wash, the moisture content changes did not penetrate very far at a location that didn't experience runoff, but in the same wash, N-13 is located at the mouth of Pagany Wash, in a location that did experience runoff, we see that moisture content changed and propagated to probably below 10 meters or so.

So there is also additional evidence from a study done by Chuck Savard for the deep penetration moisture following runoff, and this was done in upper Fortymile Wash. Infiltration and redistribution from two runoff events in the winters of '92 and '93 were monitored with neutron logging at N-91, which is 10 kilometers north-northeast of Yucca Mountain. A smaller event of '92 filled only part of the channel, and moisture content changes didn't occur for this event below a depth of about 5 meters. However, a larger runoff event of 1993, which filled the entire width of the channel, resulted in moisture content changes all the way to the water table at a depth of 18 meters.

The conclusions of that study were that the first wetting pulse stopped after satisfying a preexisting moisture deficit in the upper 5 meters, but that wetter antecedent conditions and greater width of the runoff during the second event allowed moisture to penetrate the channel to considerable depths.

So the extrapolation of these results to Yucca Mountain is uncertain because of carbonate layers, at least in the older alluvium in Yucca Mountain, may impede infiltration. However, we know that after development of calcrete layers it is a function of the age of the alluvium, and where the alluvium is hundreds of thousands of years old, such as on terraces, we would expect well developed carbonate layers. Whereas, in the alluvium in the active channels, such carbonate layers may not exist. So the extrapolation of these results to Yucca Mountain is under investigation.

DR. CANTLON: Before you remove that, is the water table actually at 18 meters below the surface?

DR. KWICKLESS: At that location.

DR. CANTLON: Ten miles north?

DR. KWICKLESS: Yes.

DR. CANTLON: Okay. Thank you.

DR. KWICKLESS: This slide here summarizes what we've been discussing all along, is that moisture appears to penetrate to greater depths on the ridges and sideslopes where the depth of alluvium is either thin or absent, and that on the terraces of sideslopes we haven't observed water penetrating to the alluvium outcrop contact.

I should point out though, however, that the periods of observation have been relatively short compared to the periods that we're describing. We've only had ten years of observations and have made observations on relatively few runoff events, and it seems logical to assume that in a wetter past or in a potentially wetter future, the washes might assume a more active role than this data indicates because the washes after all are where they are because a lot of water has flowed over them, at least sometime in the past.

And so another example of the neutron logs I'd like to show you is for N-35, and I have, in your handouts, I have prettied up the slide a little bit, but I wanted to keep the color here. N-35 is located just to the west of the surface expression of the Ghost Dance Fault. And why I'm showing you this is that it appears that water flowing in the Ghost Dance Fault has been captured in these series of neutron hole logs.

N-35 intersects the main trace of the Ghost Dance Fault at about 110 meter depth, and we see that from November, 1992 to March, '93, that we began to see changes in the moisture content at 110 meter depth, and that these moisture content changes continued until April, '94, presumably in response to fairly wet winters of 1992 and '93. So this is very direct evidence that flow is occurring in the plane of the main trace of the Ghost Dance Fault, and because we don't see it in the overlying rock, but only where the borehole intersects the Ghost Dance Fault, we can assume that the flow has been channelled down the fault.

DR. LANGMUIR: Ed, has that water been age dated or collected for N-36?

DR. KWICKLESS: I'm sorry?

DR. LANGMUIR: Has anybody attempted to age date the water intersected in faults?

DR. KWICKLESS: I'm not sure. The hole is encased, which is required for neutron moisture logging. And so I don't think this water was accessible for sampling.

DR. LANGMUIR: It would have been tritium anyway, presumably, it's young.

DR. KWICKLESS: I'm sorry?

DR. LANGMUIR: It's young water anyway.

DR. KWICKLESS: Yes.

What I'd like to do is summarize what isotopic data is available. And I don't think this copied very well in your handout, but hopefully you can see the slide. What I've tried to do is color code the types of isotopic information available at various holes throughout the mountain.

The purple in this figure represents gaseous C-14 samples, and we have those available at UZ-6S, UZ-6 on Yucca Crest and UZ-1. We have tritium data for UZ-6, UZ-7, UZ-4 and 5 and UZ-16. We've got C-14 data from the aqueous phase for NRG-7 and UZ-4 and 5 and UZ-16, and we've got chloride-36 data for three of the holes that I showed you earlier, N-53, 54, 55, UZ-16, UZ-N37 and UZ-14, and another hole which is located up by G-2 which is way to the north of where this map terminates. So there is an additional measure of Chloride-36 to the north.

The chloride-36 and tritium data provided isotopic evidence for near surface, transient fracture flow. And by near surface, I mean above and within the Paint Brush non-welded unit.

In three of the five boreholes that June Fabryka-Martin examined, she found bomb pulse chloride-36 in the non-welded units underlying the near surface fracture units, suggesting that sometime in the last several decades, water has infiltrated down fractures in the near surface. Her specific observations were that bomb pulse chloride-36 was found in the PTn at N-11, which I said was to the north of the map at 65 and 80 foot depths, and at N-53 fairly deep at

144 and 183 feet depths.

Bomb pulse tritium was found and reported by Al Yang in similar stratigraphic positions in UZ-4 and UZ-5 in Pagany Wash. Bomb pulse tritium was found in the Tiva Canyon member and within the bedded tuffs as deep as 42 meters in the Pah Canyon member at UZ-7, which is just up slope from the Ghost Dance Fault in WT-2 Wash. And bomb pulse tritium was found in UZ-6s at depths of 20-30 meters in the densely welded Tiva Canyon member, in a bedded unit at about 133 meter depth, and in the upper non-welded part of the Topopah Spring member at a depth of about 145 meters.

These two observations were actually reported to the Board sometime ago by Alan Flint, and it was in looking through some old handouts from these meetings that it's the only reference that I know of to these two observations. And recently, Al Yang has described at an NRC presentation bomb pulse tritium found within the Paint Brush non-welded unit at UZ-16.

So the isotopic evidence for deep penetration in alluvium is much more limited. There is one observation of bomb pulse chloride-36 to depths of 8 meters at N-37, which I think is located kind of towards the north. There is also bomb pulse chloride-36 at the alluvium tuff contact at 13 meter depth at UZ-16, and because bomb pulse chloride-36 was absent immediately above that, we assumed that this probably occurred as a result of flow along sideslopes, which suggests an additional infiltration mechanism.

I think I'm going to skip the next slide, and just say that while it's important that we identify infiltration as it exists today, it's also important to characterize the physical properties of the surficial materials either through measurements or through model calibration, so that we can determine what combination of climatic events, and that is what combination of successive wet years are necessary to produce recharge in certain magnitude.

And then the intent would be to use that climate record to allow the creation of stochastic models that would indicate the likelihood of that combination of climatic events occurring. So it's clear that characterization of surficial materials and not only characterization of present infiltration is going to be important to the study.

And the next slide merely summarized what we've been talking about. Thick alluvial cover appears, in the absence of runoff or ponding, appears to be effective in storing infiltration until it can be removed by ET.

The exception to that may be during or following runoff events. Where alluvial cover is thin or absent, water can enter fractures and move to depths of many tens of meters over weeks. Net solar radiation and slope aspect appear to be important.

Most importantly, at this time, it doesn't appear, at least to my eye, that any topographic setting or outcropping rock type can be eliminated as a potentially significant source of infiltration.

What I'd like to describe to you briefly is a series of simulations that I did in Pagany Wash, which we talked about the problem of temporal scaling and that we have relatively a limited period of observation on which to determine the role of various topographic and geographic settings for their long-term infiltration characteristics. And one of the ideas that we had was that we could use the saturation profiles from deeper in the mountain to get at the long-term role that various locales play in the net infiltration processes. So we estimated percolation rates from saturation, water potential and isotope data at UZ-4 and 5.

What we also wanted to do was identify important processes and stratigraphic intervals controlling the lateral and vertical movement of water within the non-welded and bedded intervals, and also establish a sense of the time scales required for the penetration of the PTn by infiltrating moisture.

The available data for this whole porosity, saturation and water potential measurements at two boreholes, UZ-4 and 5, and also tritium and C-14 data from UZ-4 and 5. The tritium data suggested the occurrence of lateral flow and the entry of water along multiple flowpaths.

The C-14 data were obtained from water squeezed from core sampled at approximately 100 meter depths in UZ-4 and 5. At UZ-4, which is located directly beneath the wash, the C-14 ages were about 1,000 years. At UZ-5, they were about 4,900 years.

We estimated matrix properties based on statistical correlations, and the fracture properties were estimated based on an aperture-scale fracture flow model.

The physical setting and simulation for each of the boundary conditions are shown in this slide. We basically simulated the channel as being 10 meters wide and having 120 meters of rock outcrop on either side of the active channel. As I said, UZ-4 is located in the center of the wash; UZ-5 on the bedrock sideslope.

The simulation domain extended from the ground surface to the water table. We assumed that all the flow, except for .1 millimeter per year, was concentrated within the wash. And this is an idealization of the reality, in that there's clearly isotopic evidence from the tritium data that suggests that there are multiple flow paths, but I think the simplification will allow a couple of points to be brought across that would otherwise be obscured if we made the boundary conditions too complicated.

What I'd like to show you is the flux vectors and flow lines that exist at steady state when you apply 20 millimeters per year into the wash. What this figure shows is that there are certain stratigraphic intervals which are very important in promoting lateral spreading of spatially focused infiltration at the ground surface, although we applied 20 millimeters per year at the ground surface, by the time it enters the potential repository horizon at elevation above the water table of about 360 meters.

I'm only going to show you the upper 120 meters of the simulations, which is where we have the data for. What we see is that there is lateral spreading above a low permeability vitric caprock that not only greatly reduces the peak values from the surface, the values of flux entering the potential repository from peak values, but it also significantly delays the entry of that water into the underlying unit.

We tracked particles released along the ground surface for 20,000 years, we see that in the rock outcrops, that the particles don't penetrate very deep, but in response of the greater inflow of water, the particles begin to move down, and as they encounter that low permeability vitric caprock, begin to show considerable spreading.

One of the reasons I want you to see this is that it's clear that if one drilled a borehole at this location, it's clear how one could go from younger water into older water and back into younger water, and so you get this kind of depth inversion of ground water ages. And although this lateral flow may occur for various reasons, in this case it's a capillary barrier between the pores of the overlying non-welded units and the fractures of the underlying welded units, the principle is the same elsewhere at other stratigraphic locations throughout the mountain.

When we compared these calculated travel times against observed ages based on C-14 data, we found that directly beneath the wash at 100 meter depth, we observed a 1,000 year old age, we predicted about 2,500 years. At UZ-5, which was located here, we predicted about a 5,500 year age for the ground water, which compared with the 4,900 year old age. So we felt that this was a pretty good match between predicted and observed ground water ages, and it suggested to us that the flux is--the long-term recharge through the wash has been fairly high on a millennia-long time scale, in spite of the fact that in the last decade or so, the washes have appeared to be hydrologically inactive.

So this just summarized the results, in that the washes appear to be important for recharge over millennia-long time scales. Capillary barrier effects appear to decrease flux rates from peak values at the ground surface and significantly delay, perhaps by even thousands of years, the entry of surface derived moisture into the potential repository horizon. And it illustrated the process by which depth-inversion of ground water could be accomplished.

We do have evidence at UZ-7, which is the site of some--that there does appear to be capillary barrier effect overlying that low permeability vitric caprock, in that to 20 meters or so, the vitric caprock are--capillary equilibrium, implying that although the profile is fairly wet, there is no moisture moving, or at least not a lot of moisture moving through this interval.

I should point out, however, that we're kind of at field capacity here and that any additional moisture that came down here would simply push moisture out the bottom into the potential repository horizon.

Changing gears again, I'd like to describe some results of a block experiment. There's clearly a need to establish an experimental basis for many of the underlying assumptions in numerical models, particularly the assumption that fractures become nontransmissive at small water tensions.

We want to also provide experimental support for the modeling results, for example, that capillary barrier effects between unfractured and underlying fractured formations inhibit the entry of water into the lower interval. And we also want to make comparisons--make estimates of water percolation rates on the basis of pneumatic testing, water potential monitoring and fracture mapping and compare these with the applied percolation rates.

The block that we worked with was taken from the densely welded unit of the Tiva Canyon member. And it's a little hard to see the surface expression of the fault traces, and we drilled 18 small diameter boreholes into this block with which we monitored water content, moisture potentials--water potentials.

The experimental design was that the block was encased in plexiglass and we placed a sand load over the top of the block in order to mimic the hydrologic interactions between alluvium and underlying densely welded unit or between a non-welded unit and densely welded unit. And then we applied--although the experiment has been plagued by various experimental problems, including bacterial growth and possibly the presence of entrapped air, we think that we've identified some supporting evidence for some of the assumptions that we're making in our numerical models.

For instance, when we applied two sets of water sufficient to saturate this overlying sand layer, what we first observed was that initially water moved very rapidly down these fracture traces. However, when the water potential in the sand, as measured by a tensiometer, became even slightly negative, water flow down the fractures ceased and this suggested to us that the capillary barrier effect that we were observing in our numerical simulation was in fact a realistic mechanism and was being observed in a block.

In the later stages of the experiment, we've created a constant flow rate through the block, and by varying the amount of water that's applied here, we can control the tension in this overlying sand layer and get the relationship between flow through this fracture as a function of water potential, and these flow-through experiments also seem to confirm our assumptions that fractures drain at fairly small tensions and become non-transmissive.

So although this experiment has been plagued by a lot of experimental problems, I think it provides some support for some of our basic assumptions.

Fracture properties; I just want to say the assumptions about what you're seeing with the pore-scale are going to influence your model results at the site-scale, and this is going to be true whether you model under ambient conditions or whether you're doing thermal hydrologic modelling.

At present, capillary theory and the application of pore-scale accessibility criteria form the basis for these numerical models which consider aperture variability in thin, rough-walled fractures. However, a large body of experimental data doesn't exist to support these. And recent fracture mapping of the ESF suggests that this conceptual model of fracture flow needs to be expanded to consider wide, that is to say noncapillary fractures, mineralized or otherwise filled fractures, and fractures with obvious controls such as so-called wormtubes.

The next figure shows you some values of capillary pressure curves and permeability curves that we've calculated with our numerical model. And in the absence of hard experimental data, these remain the most solid basis for some of the assumptions that we make about fractures. I don't have enough time to go into these.

As a result of mapping of the starter tunnel in the ESF, we've observed that there's many different types of fractures that need to be considered somehow in our numerical models, and these include fractures wide enough to fit your head in, fractures several centimeters wide that are filled with clay, and also it's been observed in these tunnels that carbonates penetrate within fractures down to depths of 15 meters or more, and while the hydrologic implications of these fractures are not certain, they are being investigated as part of the calcite-silica study which has provided a lot of indirect evidence of about how water moves in fracture systems.

Through the study of the minerals lining these fractures, Joe Whelan of the USGS and Dave Vanniman of Los Alamos have made a lot of inferences about how flow behaves in fracture networks. What they observe is that frequently, only a small percentage of fractures in a drill core will contain calcite, implying that the flow pathways occupy only a small percentage of the fracture network. And this was something that came out in the discussion earlier, in that when the permeability is much higher than the flow rate, you can expect that even in a well connected fracture network, only a very small sub-set of the available pathways are going to be flowing, even under steady flow.

On the basis of these studies, they've also observed that fracture flow has been episodic but repetitive. And one idea that they've had is that fracture coatings frequently contain dissolution surfaces which may actually represent the periods of greatest recharge, in that it's hypothesized that water moving very rapidly through the system is undersaturated with respect to calcite, and may dissolve previously deposited calcite coatings.

They also made an observation that--observations have also been made that calcite rarely occurs in fractures within the PTn, suggesting that flow has occurred primarily through the matrix in that unit. And recently they've been able to date the C-14 ages of 14 samples from the unsaturated zone, which yielded three values greater than 51,000 years, which is the limit of their age determination. They had one value as young as 21,000 years, and ten values between 33 and 45,000 years, indicating that calcite formation occurred as recently as the last glacial period.

I'm not going to have time to go into the detail on the correlation between fracture properties, but one of the ideas we've had is to use porosities and master variables and to tie all the other capillary and permeability characteristics to porosity. And you heard Rally Barnard yesterday describe how they would use indicating creating to determine contacts and generate the porosity values stochastically within a unit. And in the absence of any other solid information, these correlations can provide you with a best guess as to what the hydraulic properties are at those locations.

Some data that we feel has a lot of relevance is this imbibition data that was collected. How this data--let me describe how this data was collected.

The cores were first saturated with CO_2 and then filled under a vacuum to determine their accessible porosity. Later, they were subjected to imbibition in one of two ways. They were either immersed in a bucket of water and the imbibition rate versus time recorded, or they were placed on a wet film of water and allowed to imbibe from one end. And what the data show is that when allowed to imbibe from one end, only 30 to 80 per cent of the available pore space is filled.

So why is this important? Well, it's important for several reasons. One is that if we're going to use matrix saturations as an indicator of the flux through the mountain and infer the presence or absence or fracture flow through the use of numerical models, it's clear that we need to keep in mind that the matrix may not fully saturate under field conditions, and that the use of artificial laboratory techniques, such as I described for the porosity determination, basically may be misleading if you transport matrix properties that were determined under these conditions to field conditions.

Another thought that I've had in connection with this data is that it may be important for the thermal modelling of the mountain in that the water that's driven away by waste generated heat, should waste be implaced, may not return as readily into the matrix as one would expect, given the use of a non-hysteretic model. And so you may be generating a lot of condensates that may not find its way back into the matrix, but may be forced to drain through fractures.

I think that these seemingly innocuous experiments might have some large implications about how one is going to model the site and interpret those modelling results.

I'd like to move along to UZ-16 air permeability and fracture and isotope data. UZ-16 is unique in that it's one of the few holes in which permeability and fracture data and several different types of isotope data exist.

The last time we met, we discussed that the Chloride-36 and C-14 data are in apparent conflict, and I'm going to try and attempt to resolve that conflict.

I'd also like to mention that UZ-16 is not a hydrological; it's a VSP hole, and it's presently in the process of being instrumented for imaging of nearby fault structures, of which there are several, and this may help resolve some of the remaining difficulties.

One of the questions that the Board has asked on several occasions in the past is what's the relationship between permeability determined using air as a test fluid and permeability determined using water as a test fluid. And although the survey did not do these tests, I think they're very relevant to any discussion of the air permeability data. They were actually done by the University of Arizona at the test site, and they made several comparisons between cores and boreholes, in which the cores were taken from the intervals in which the tests were done, or they were done using air and water as a test fluid.

I think I'll just start. When they compared water saturated core versus air saturated core, they found that the air gave slightly higher values to the slip effects along the pore walls. When they compared water injection into a water saturated borehole versus water saturated core, what they saw was that at high permeabilities, the field determined permeabilities were much higher than the lab determined permeabilities because of the presence of fractures. No surprises there.

When they did the same thing with air, which is more relevant to our--the air permeabilities boreholes was much higher, but that in the unfractured intervals, the core actually gave higher values because of the slip effect that we identified here.

Most relevant to our case, though, is what happens when you compare tests done in a water saturated borehole versus injecting air at ambient conditions. And what they found was that when the permeability was fairly low, the water injection tests gave slightly higher values because the matrix was already fairly saturated and impeded the inflow of air. Conversely, when there was evidence that there were fractures, the air permeability tests gave much higher values because of injecting air, when you inject water into an initially air dried fracture, you often get blockage of the water by air.

So there are some slight differences, but overall they found a very good correlation between the air determined permeabilities and the water permeabilities, suggesting that air was an excellent surrogate.

So this data which was collected by Gary LeCaine and Jerry Walker through air injection testing of UZ-16 is some of the most extensive permeability data to come out of the project so far. And I think one of the most striking features about this profile is that it varies probably no more than an order of magnitude or so within an 800 foot interval within the Topopah Spring, which I think is remarkably uniform, especially given the relatively short packer length of only 12 feet.

The few values that we have for the Tiva Canyon are slightly higher, on the order of 10 to the minus 11th meters squared. All these values fall roughly around 10 to the minus 12, or about a darcy. There are five values that were taken from the Calico Hills unit, four of which gave values of about 10 to the minus 15th meters squared, and one gave a value of about 10 to the minus 14th meters squared, which suggests that at least locally, permeability is augmented by fracturing within this unit. The Calico Hills tuffs generally have a permeability of two to three orders of magnitude lower.

It's not surprising that these gave slightly higher values, since the packer tests were located in part on the basis of the fracture logs for this hole, and so we intentionally sought out the most fractured intervals within the Calico Hills, so that these values, although they're higher than the Calico Hills matrix, may not be representative of the permeability as a whole through this unit.

There were also a few intervals such as the basal vitrophere of the Topopah Spring which was too fractured and the borehole too rough to seat the packers, and so we were only able to obtain one value permeability in the basal vitrophere.

Similarly, another interval of obviously great interest is the non-welded embedded units that overlie the Topopah, and these were generally too washed to seat the packers. So one has to wonder if somehow we're censuring our data set by not being able to test along the most broken up intervals, so we may have had higher permeabilities at several locations if we had been able to seat the packers.

Bill Thordarson, who has just recently retired from the survey, has done detailed fracture logs of both UZ-16 and UZ-14. This is the fracture log for UZ-16, and what it shows is some very high fracture densities. This is recorded as number of fractures per ten feet as a function of depth, and you can see that throughout the Topopah, most of the fracture densities are on the order of probably 20 to 40 fractures per 10 feet of core.

DR. LANGMUIR: And what are the dimensions of those fractures? How big do they have to be to be seen?

DR. KWICKLESS: Bill was very meticulous and saw many things that wouldn't be observed with a borehole camera, in that he examined every piece of core, and if it was broken on five faces and those five faces were mineralized, those counted as a fracture. So these densities are higher than were observed for the same hole by television camera as a result of this very meticulous work.

DR. LANGMUIR: There's no telling, though, which of those would conduct fluid flow?

DR. KWICKLESS: No, there's not, and I'll get to that in a minute.

You will also notice that fractures are relatively sparse in the Calico Hills. Now, one of the explanations I'd like to offer for the apparent discrepancy between the chloride-36 data and the C-14 data that we touched upon the last time we met is the first thing is that they actually come from different stratigraphic intervals. The chloride-36 data originated in the basal part of the Topopah Spring member where the fracture densities are quite high. The C-14 data originated between 12 and 1,500 feet in the Calico Hills.

If you recall, June Fabryka-Martin's data here showed ground water ages of somewhere between 200,000 and 500,000 years, where Al Yang had measured nine C-14 age dates between 1,000 and 5,000 years. And, in fact, one of those C-14 ages that was 97 per cent modern was associated with the tritium value of 44 tritium units. So the data appear to be consistent, at least from the C-14 and tritium point of view.

One of the models that I'd like to propose for this apparent discrepancy in the data is that within the densely welded fractured units, water flow is occurring along relatively few and relatively isolated flow paths and is relatively difficult to detect from a borehole when the flow is occurring along very discrete pathways. When the fractures die out at the contact between the Topopah and the Calico Hills, a transition is made from fracture flow to matrix dominated flow, and in fact some of the water may spread laterally across the top of the Calico Hills, which is, by the way, where we see much of the perched water throughout the mountain.

When the water makes this transition from fracture flow to matrix flow, it spreads out and presents a much larger target area that is more likely to be detected by a vertical borehole. So the model that I'd like to propose is similar to what Dwayne described earlier, one of very localized and tortuous flow through the Topopah Spring member, and then finally a diffuse matrix dominated flow through the Calico Hills member.

As further evidence for this model, Al Yang has observed numerous intervals throughout the Topopah Spring that are fairly erratic, in which he's observed tritium values of 20 or greater tritium units. So we're just catching little glimpses of this flow through this fracture network in the Topopah Spring, but we're not really able to sample it in any quantity until it gets to the Calico Hills.

DR. LANGMUIR: Is the implication that if you got the chloride-36 samples from fractures in the Topopah Spring, they would also be too young to--

MR. KWICKLESS: Well, I think the implications are that if June had sampled from the Calico Hills, we would expect much different ages than what was sampled.

DR. LANGMUIR: Much younger perhaps; right?

MR. KWICKLESS: Much younger. And, in fact, I was handed this morning a fax from June to Russ Patterson in which she said that she needs to correct for dead chloride in her samples, and she'd been basing that correction factor on a chloride ratio that has since been updated and has led to much younger years even within the interval that she sampled.

So it sounds like more information on the appropriate chloride/bromide ratio to use has substantially revised some of the ages she measured at the lower part of the Topopah.

So I told you earlier that for gas flow, the Topopah appeared to be behaving as an equivalent porous medium, that the permeabilities varied only within an order of magnitude, and now I'm telling you that flow through fracture networks is extremely tortuous and localized. And the reason is that the first measurements were made using air, and these reflect the way the water moves through the system. And I said when you have a permeability value that's many orders of magnitude larger than any conceivable flux through this unit, then only an extremely small subset of fractures are going to be filled and flowing at any time.

I think it's beyond our understanding at this point to say which fractures are going to be filled and flowing, and maybe some insight will come as a result of some fracture network modelling in some variably saturated fracture network models that may evolve from this data, but right now all that we can do is, you know, qualitatively assess what's going on.

So we did attempt to do correlation between densities, fracture depths and permeabilities, and essentially found that there was no correlation between permeability and the fracture density for the densely welded units. And what this implies to us is that it's local apertures that are controlling the flow and not the fracture density, and this is a finding that actually one would have expected.

So in summary, permeabilities determined from air-injection tests appear to vary over a very limited range over depth intervals of many hundreds of feet, suggesting porous media type flow behavior, at least for air.

The intermittent appearance of tritium throughout the Topopah Spring member suggests that only a few of the many fractures are conducting water, and again, this is understandable given the permeability.

The effective permeabilities determined for the Calico Hills suggest that the matrix permeability is locally augmented by fracture contributions, although as I said, we identified the test locations on the basis of the fracture log.

And the apparent discrepancy between the carbon-14 data may be resolved by considering the relative stratigraphic positions and the fracture densities in their respective sampling locations, and the relative likelihood of sampling fast paths in structured versus unstructured media.

So some of this data has been touched on. Isotopic evidence for deep, transient fracture flow, I mentioned the tritium measurements of 20 plus tritium units occur sporadically throughout the densely welded Topopah Spring member.

There were nine C-14 age dates of 1,000 to 5,000 years, one of which was associated with a water sample having 44 tritium units.

Some data that I think was mentioned this morning; C-14 age date of 3,500 years for a water sample from the perched water zone encountered at NRG-7 within the Calico Hills at approximately 1,500 feet, and chloride-36 value from the saturated zone at UZ-14 having a chloride-36/chloride-35 ratio that indicates possible bomb pulse contributions. So it does appear to be water getting into the system, both from a wide variety of isotope data, and from the neutron hole logging.

I'd like to change gears and talk about the gas isotopes. I'll have to summarize this pretty quickly. Basically, there's been several years worth of gas isotope data collected at UZ-1 and at UZ-6 and UZ-6s, and one thing that they all have in common is that the C-14 ages in the stratigraphic intervals over the non-welded units show C-14 levels that indicate bomb contributions, whereas the gas beneath the non-welded tuffs is clearly pre-bomb.

In fact, Don Thorstenson, who's here to answer any questions, has shown that at least for UZ-1, that the gas column seemed essentially stagnant, and that you can model the C-14 activities at UZ-1 using a simple diffusion model, and that the lower part can be modeled with a steady system and the upper part with a transient model that accounts for a change in the boundary condition when the drill pad was constructed. And UZ-6 shows that samples were well mixed within the Topopah Spring, but clearly pre-bomb, and that the

Calico Hills layer was relatively stagnant in terms of gas flow, as indicated by the low carbon-14, the small carbon-14 measurements.

What it indicates is that there's clear segregation between the shallow and deep flow systems, and that the non-welded tuffs appear to be effective at segregating these systems.

In addition to those data, there have also been estimates made of the size of the CO2 reservoir necessary to contribute to the outflow of CO2 from UZ-6s, actually, and that indicates that flow from the east side of Yucca Mountain is a major source. And this was also consistent with estimates based on methane consumption rates. Essentially we don't see any methane coming out of UZ-6s.

And based on the calculations, an average downward advective velocity of about 50 meters per year has been calculated as necessary to capture the soil CO2 along the east slope at the rates observed from UZ-6s.

So given this background, we tried to create a numerical model that would at least capture some of this information. And while the modelling is very preliminary, I'll show you just some simple sensitivity analysis that we've done that look at the effect of assuming whether the PTN is fractured or unfractured.

These are the temperature contours for average annual temperature conditions and an assumption that the rock is uniformly fractured. You can see that there is some effect of the surface topography on the temperature contours. The gas column is essentially at static equilibrium, and the only thing that really drives flow is a buoyancy induced flow caused by cooler air on the slopes of the mountain sinking and being heated by the natural geothermal gradient and then rising up out of the crest.

For the uniformly fractured rock, we see that the flow lines penetrate quite deeply into Yucca Mountain, and although there is flow along the east slope as well as the west slope, the flux vectors that are calculated with this model clearly indicate that the gasses that would be sampled here would be post-bomb, which is in obvious conflict with what the gas isotope data suggests and might suggest to us that the PTN may not be as fractured as the rest of the rock, which would be consistent with fracture logging, observations made from fracture logs.

When we assume that the PTN doesn't have any fracture permeability, the flow field's contained considerably and you can barely see the flux vectors within the Topopah Spring they're so small. But the model up here captures many aspects of what the gas isotope data indicates, that there is a large component of flow up the east slope of Yucca Mountain, that the downward invective velocities are fairly large on the east slope, and that the shallow and deep flow systems are fairly segregated from each other.

So although the modelling is very preliminary and we've just started trying to capture some of the things that the isotope data is suggesting, at least this very preliminary model suggests that the Paint Brush tuff non-welded unit is reasonably unfractured and doesn't have substantial fracture permeability. And that's so summarized in that slide.

I'd like to get on with the perched water. Perched water has been observed at five boreholes now, UZ-1, UZ-14, UZ-16, NRG-7 and 7a, and SD-9. I want to point out that all occurrences of perched water occurred where a zone of fracturing was underlain by an interval of low matrix permeability and either low fracture frequency or filled fractures.

The primary means of determining the magnitude of these perched bodies has been in response to pump tests, and this appears to be a very good method for determining the magnitude of the perched zone.

So far, all encounters with perched water or freely-draining fractures have occurred below the stratigraphic levels expected to be penetrated by the north ramp or the main test level of the ESF. However, should drifting proceed to the Calico Hills, encounters with perched water are more likely. And that's based not only on our experience at Yucca Mountain, but I think it was raised this morning, it been observed at Rainier Mesa. There was a classic study done by Bill Thordarson that studied the occurrences of perched water at Rainier Mesa and found perched water occurred quite often in stratigraphic intervals with similar characteristics as the Calico Hills, namely that they were heavily altered.

This shows the occurrences of perched water at UZ-1, UZ-14, SD-9, NRD-7, UZ-16. I've also thrown the N-35 well on there where we observed flow in the Ghost Dance Fault. Basically, the story is told by correlating--these are actually from a publication by Paul Burger and Kevin Scofield and they've nicely summarized all of the available information on perched water.

Perched water was encountered at UZ-14 just above the contact with the basal vitrophere of the Topopah Spring. Although there is still considerable fracture density within that unit, the fractures that have been observed and logged are largely sealed with clay minerals, and the vitrophere is very reactive chemically relative to other formations, and that's why most likely that the fractures have been turned to smectite clays. There are also further incidents of fracture flow at the top of the Calico Hills and at the Prow Pass member.

Within UZ-16, we had encountered perched water within the Prow Pass, which is a zone of relatively little fracturing. Similar observations were made at NRG-7 where we observed that while perched water was observed, there was relatively little fracturing and a very tight matrix, and that at SD-7, we observed that perched water again occurred, as at UZ-14, at the contact between the lower non-welded, the lower lithophysal unit and the basal vitrophere.

So that basal vitrophere which has been heavily altered and although it has fractures, has mineral filled fractures, it appears to be very significant interval for perching water.

The last topic I'd like to cover is unsaturated zone/saturated zone interactions, and these can be divided into physical interactions, information transfer and numerical model coupling. And one of the more interesting physical interactions is suggested by this map of the geology intersected by the water table. And when you think of coupling between the saturated and unsaturated zones, it seems fairly straightforward, but when you begin to look at the units intersected by the water table, you see that there are some very high permeability units and some very low permeability units.

The Topopah Spring is known to be the most conductive tuff aquifer where it's submerged, whereas, as we've discussed, the Calico Hills member and the Prow Pass member are heavily altered, non-welded tuffs that have very low matrix permeability.

It's also known that the degree of alteration tends to increase towards the north where hydrothermal activity was most severe 11 million years ago when the zeolites are believed to have been formed.

So what this map of the geologic units at the water table suggests is those locations that are most likely to provide points of entry from water flowing from the unsaturated zone into the saturated zone, and those likely points of entry are shown in green, which are in the Topopah Spring member. It's also a likely point of entry is along some of the major faults, although in a regional sense, we know that even where the Calico Hills and Prow Pass members are faulted, those faults are only slightly leaky, because those units are able to sustain head differences between different aquifers.

And so I think when you start to think about how we begin to couple the saturated and unsaturated zone models, it all starts with a map of which units are intersected by the water table, and this is going to significantly narrow where we need to search in trying to understand how the saturated and unsaturated zones may transfer nuclides.

The other interesting bit of information that may come out of the saturated zone studies and its relevance to the unsaturated zone is that they were able to use a map of heat flow--temperatures at the water table, and this distribution of temperatures at the water table suggests that in the northern part of Yucca Mountain, there's a down-welling of colder, shallower waters that are depressing the temperature contours near where the there's up-welling along the Solitario Canyon Fault suggesting that in spite of model calibrations for the saturated zone which have suggested that these are very impermeable structures, that they do possess vertical permeability and that the lateral, the low lateral transmissivity is most probably due to offset of beds of contrasting permeability rather than closure of the fault by mineralization or another such process.

The last thing I'd like to show you from here is an explanation of the perched water that we see in the northern part of Yucca Mountain. Oh, I'm sorry, all of these figures come from a publication by Chris Fridrich, Bill Dudley and

John Stuckless, and they have a wonderful summary paper in a recent issue of *Journal of Hydrology.*

One of the explanations that falls out of the work that these authors have done is an explanation for the perched water in the northern part of Yucca Mountain. This cross-section through the northern part of Yucca Mountain shows that the water table coincides with the top of he Calico Hills unit in the vicinity of G-2, and that the water table falls over 300 meters between G-2 and G-1.

Well, the water table is believed to be limited by permeable layers within the lower part of the Topopah, so that the lower Topopah effectively serves as a drain limiting the water table elevation north of G-2. However, once in these permeable zones within the lower part of the Topopah, it has a very difficult time draining back through that low permeability vitric caprock through the Calico Hills member to follow the decline in the potentiometric surface. And so what we see is a lateral diversion of water from what was originally the saturated zone, along some very low permeability beds in the unsaturated zone, namely the basal vitrophere of the Topopah and the Calico Hills. And this is where we encounter them in G-1. And <u>now</u> the holes in Drill Hole Wash.

Is there isotopic evidence to support this? I don't think one could probably discriminate between water that's infiltrated from the ground surface and perched along there and water that's infiltrated up here and moved laterally. I think they both would be fairly young waters, and I think they would both be immature with respect to their chemical evolution, which have been some of the criteria used to try and see if this is really perched water or saturated zone water or drilling fluid.

So the summary of physical interactions just summarizes what we basically just discussed.

There's been potential for a lot of information transfer between the saturated and unsaturated zone studies, and I summarized here the kinds of information that could come from the saturated zone that would be very helpful to the unsaturated zone studies.

And, finally, an issue that keeps coming up is the coupling of the saturated and unsaturated zone models. I presented a map of the geology of the water table which I said is I think going to form the basis for this coupling. This coupling could occur either through a very large numerical model that embodies both systems or it could occur through weak coupling such as through maps, physical or digital, with contours describing either intensity of recharge or nuclide concentration or arrival times.

And, of course, in the future, consideration of repository generated heat may actually necessitate the use of a more strongly coupled unsaturated zone/saturated zone flow model.

Summary and conclusions. I think neutron logging and isotope data have suggested that near surface fracture flow is a relatively common occurrence, and at this time, that no topographic setting or outcropping rock type can be eliminated as a potentially significant location for infiltration.

Capillary barrier effects in the PTn may significantly reduce peak surface fluxes from peak values at the ground surface, and significantly delay the arrival into the potential repository horizon. And although we've learned a lot about fracture and matrix properties, there are still significant gaps for certain types of data.

The UZ-16 data and fracture data suggest gas flow may be described by porous media type models, but isotope data suggests that water may be moving along a much smaller subset of available fractures.

The C-14 and tritium data for UZ-16 indicate relatively short, that is, probably less than 1,000 year travel times as a result of fault or fracture flow near that borehole. Although relatively few locations have been studied, available gas isotope data indicate the PTn effectively separates the shallow and deep gas flow systems, suggesting little fracture permeability for the PTn.

The perched water zone detected in the northern part of Yucca Mountain may be due to the steep decline of the potentiometric surface between G-2 and G-1, and that direct connections between the unsaturated and saturated zones may be localized and restricted to the Topopah Spring member and major faults.

DR. DOMENICO: That's very good, Ed. Thank you.

Are there any questions from Board members or Staff? Well, I think maybe the proper time is after Dick Luckey speaks when we have both Ed and Dick up here in the roundtable discussion would probably be more appropriate.

So with that, we'll hear a progress report on the saturated zone by Dick Luckey of the USGS.

DR. LUCKEY: I'm going to try to give you an update on saturated zone studies that have occurred at Yucca Mountain. Just in case any of this might sound familiar to someone, I thought I'd like to point out that we've given similar talks previously. Going back through my records, I find that I've talked to the Board at least three times. I'm missing a file for a fourth one. The most recent one was in April of '94, where I outlined the USGS, Los Alamos, LBL saturated zone hydrology studies. So that's very close to today's title, so there is a fair amount of this that's going to be a repeat. Maybe I'll get a chance to do some things better than I did last time around.

I'd like to talk about where we've been in the past year, what we've done, and where I think we're going to go in the next year. I'd like to start out by talking about the process models and give you an update on that, talk about our regional model. We're a little further along with the regional modelling, so I can give you an update on that. And then I'm going to talk about the site modelling efforts that we're doing, and then we're going to get to the question that the Board keeps asking about data, and I'm going to talk a little bit about what new data we've gotten in the last year, what we've done with the data, and then look more about where I believe we're going to collect some more data in the next year. And, finally, I'm going to try to relate some of this to the technical site suitability decision.

I'll put this up here just to talk about the regional modelling that we're doing to try to give you an idea of what the region really looks like. This is an outline of the region. It's a three degree latitude by three degree longitude area. You can see Las Vegas here, Death

Valley, test site boundary, Yucca Mountain; the point being that when we talk about a regional model in the saturated zone, it's a fairly large boundary, fairly large area.

We've put a boundary around this thing based on the potentiometric surface that is in large part what we believe to be a no-flow boundary. As we work on this further, we're not so sure that it's really a no-flow boundary over the entire region. We've created a regional hydrologic framework for this entire area as a basis for doing our numerical modelling.

This is a piece of a typical cross-section that was done to build the regional hydrologic framework in Gross and Smith, USGS Professional Paper 1370. This happens to be a cross-section going through the Yucca Mountain area. Gross and Smith have the only complete, consistent set of cross-sections that cover the entire Death Valley region, so that was one of the major inputs to this framework. Even these fairly simplified cross-sections had to be simplified more to actually build the hydrologic framework.

We used a three dimensional GIS, or we call it a GSIS to emphasize that it's three dimensional, to build this hydrogeologic framework. We built the framework independent of any model grid. We felt this was fairly important to give us some independence in redesigning our model grid as we needed it. We used four geographic information systems to do this; Arc-Info, Intergraph, CP-3 and Stratamodel. There was no single GIS that would serve all of our purposes, so we did various parts of the processing in various pieces.

The three dimensional system was actually built primarily in Intergraph. We divided the system up into ten units going down to a depth of about 3,000 meters. We used this to provide a complete integration of the available hydrologic data. We call it "Quality-Assured 3-D Data", because this was based on published information and publicly available information.

We started out with geologic maps of Nevada and California, the Nevada State geologic map and the California 1 to 250,000 geologic maps. I already talked about the cross-sections. We brought those two pieces of work together by draping it over the digital terrain data. We used individual data points coming out of either published sources or data bases such as the National Water Information System, and we also incorporated remotely sensed data into this data base, to primarily look at recharged/discharge relationships.

One of the things that the USGS has spent a considerable amount of time in this regional study is looking at faults. Obviously, we believe that faults can be either conduits or barriers to flow, and probably are important in putting together our regional hydrogeologic framework. This map shows nearly 40,000 fault traces. These fault traces were divided into three categories; either conduits, barriers or neutral. They were divided into different categories based on different areas of the map.

We revisited the regional potentiometric surface to try to better define the boundaries of the Death Valley flow system. This is a current depiction of our regional potentiometric surface. This is a continually evolving effort. This is not a final map by any stretch of the imagination.

We constructed this surface to define the boundaries of the system of interest and also to give us some ground truth to attempt to calibrate a model, again, so the model would be calibrated against this potentiometric surface and also the observed outflows from the system.

This is the other piece of the model calibration information that we have, our recharge/discharge relationship. This is a regional water balance put together by Frank D'Agnese. I'd like to point out a couple of things in this, and even though we tried to reach out to what we believed were true boundaries of the system, we got a fair amount of interbasin inflow calculated. It actually turned out to be larger than the local recharge. There's probably some people out there that discovered this many years ago and pointed it out. We keep learning over and over again.

In this particular case, I think it's important, though, that we were able to put numbers on this. On the discharge side of the equation, we noticed that evapotranspiration is by far the largest discharge from the system. Spring flow is much smaller. This presents somewhat of a problem because ET is fairly difficult to measure.

There's a certain amount of unknown in this water balance because we don't really know what the long-term change in storage is, although it's probably closer to the 53,000 acre feet per year pumpage than it is to zero.

DR. CANTLON: What's the AF designation?

DR. LUCKEY: Acre feet per year. I'm sorry.

DR. CANTLON: Okay.

DR. LUCKEY: I think in acre feet; other people think in cubic meters or something.

We took the regional hydrogeologic framework and turned it into a regional numerical model. This was based on the USGS MODFLOW Code. This is a three dimensional finite difference code that's widely used in ground water studies.

The initial grid was based on 190 rows, 150 columns in four layers, so we had a cube 1,500 meters on a side. We had about 75,000 active nodes in this initial model.

We're in the process of revising this model grid, simplifying it considerably. We're currently working with a model grid that has 150 rows, 150 columns and three layers of various thicknesses. We're looking at a much shallower system. The deeper part of the system didn't contribute too much to the model, and we're looking at simplified boundary conditions around this model.

On our site modelling efforts, we're not near as far along as we are with the regional modelling effort, but we've already built a considerable conceptual model of the flow system. I'm going to try to describe that in some detail.

Our site model for our area of interest is not going to be built on definite hydrologic boundaries, although the numerical model itself may reach out to considerable distance. Our area that we're going to be concentrating on will be north somewhere beyond the large hydraulic gradient because we want to include that feature in our site model. East and west, it will extent out into Jackass Flats and Crater Flat, and somewhere south down to about Highway 95.

We're going to get the boundary conditions for this regional model. The boundary conditions for the site model will come from the regional model. These boundary conditions will either be head or pressure specified along the boundary, or fluxes specified along the boundary.

I'd just like to point out that the site model for the saturated zone is considerably larger than what we talk about for the site model for the unsaturated zone or the site geologic model. That's necessary just because of the processes that are involved.

For the purposes of generating the site model, we have divided the hydrogeologic system into three aquifers and two confining units. I'd like to emphasize that this is appropriate only for the site model. This is not a good subdivision for a regional model, and it's not a good subdivision for something when we get down to the scale of the C-wells.

We've defined an upper volcanic aquifer which is basically the highly fractured part of the Topopah Spring. It's beneath the water table only J-12, J-13 off to the east of the repository block over in Crater Flat and perhaps off to the south. It's above the water table over much of the area.

We talked about an upper confining unit that's primarily the Calico Hills unit, but includes the very base of the Topopah and the very uppermost part of the Prow Pass.

The Crater Flat tuff is what we talked about as the lower volcanic aquifer. The older tuffs and lavas below that we call the lower confining unit. What we call the carbonate aquifer is the lower carbonate aquifer defined by Ike Winograd.

In our conceptual site model, one of the things we obviously have to deal with is the large hydraulic gradient. There are a number of hypotheses about he large hydraulic gradient, the cause of the large hydraulic gradient, that are consistent with the existing data set. So we'll have to deal with all of these in the site conceptual model.

The first model I call the "No Big Deal" model. I got this name one day talking with Ike Winograd out at the cafeteria. He said what's the big deal; the flow is in the Calico Hills at that point. Calico Hills is tight elsewhere on the Nevada test site. Why does this large gradient surprise you guys. He didn't name this model, but that's where this name came from. I just made this up I think probably before the Board meeting the last time.

We have alternately thrown around either dike models or tight fault models that we say there's some sort of a structure in Drill Hole Wash because the large hydraulic gradient is north of there, so there must be something there. There's no geologic evidence for this other than the existence of the wash and the large hydraulic gradient in that area.

There's a spillway model that's been numerically modelled several times that says the underlying rock is either fairly tight because of hydrothermal alteration or a less widely accepted model would say that the Eleana Formation, even though it's probably at depth to the north, may be affecting the hydrologic system.

The drain model as discussed by Chris Fridrich, Bill Dudley and others talks about a buried graben, a fault to the north of Yucca Mountain that diverts water out of the volcanic system down into the carbonate aquifer. It's a very pleasing model in that it supports the low heat flow beneath Yucca Mountain and Ed Kwickless showed some of the evidence for that model.

Another model is the semi-perched model that was discussed several years ago in Tucson when we had a meeting there. The semi-perched model envisions that we're just trying to contour two separate surfaces, one on the upper aquifer, one on the lower aquifer, and that the large hydraulic gradient come about because we're looking at really two different surfaces. What little data we have up in that area is in the upper confining unit and that data is very difficult to interpret because the flow is predominantly vertical and we're getting our information from vertical boreholes.

Probably when we finally figure out what the large hydraulic gradient is all about, we will have eliminated all of these models and come up with something totally different.

Let me talk a little bit about where we are on the site numerical model. We have now made the decision that our initial model will be based on the FEHMN Code from Los Alamos. It's a fully 3-D finite element heat and mass transfer model. We're not going to be using the heat aspect of that model initially. It has very flexible geometry in it. Initially, we'll be using it as a porous media model without heat, and this was pointed out earlier that this is the same code that's one of the two codes that's being used by performance assessment, which will make it very easy to exchange information with performance assessment.

The site grid is currently being designed. It's going to be a very fine grid within the area that I described, a coarser grid out away from that area. We're designing the grid so we have a grid point at every data point within the system, plus lots of other grid points.

The model will be fully 3-D. Previous USGS models have been 2-D, although the performance assessment model that was done for TSPA-93 was a 3-D model. The model will handle faults somewhat realistically. We argue back and forth about how realistic this realistic is going to be, but at least we're going to handle faults in a much more realistic manner than we did in the past, and the site model will include the carbonate aquifer, which it has not done in the past.

We've already outlined some concepts that we want to test with this site model. One of them is how important are the faults to the flow system. We all intuitively feel that faults are very important to the flow system. We'd like to use this model to determine if the faults and the faults alone could explain everything known about the saturated zone. That probably won't happen, but that's something that we want to test.

We need to make some tests on our steady state assumption. We want to look at one concept to where there is no modern recharge. All water that's there was recharged during the Pluvial period and it's just a draining system.

We want to look at episodic modern recharge, something with a very large recurrence interval, perhaps 500 or 1,000 year flood type thing applying the only recharge, and of course the last set there was to look at somewhat modern recharge, see if we have to have relatively modern recharge to support the system as it exists.

We're going to be looking at focused recharge within major washes to see how viable that concept is for the saturated zone.

I already mentioned several hypotheses about the large hydraulic gradient that seem to be consistent with the available data. We want to use our models to see if we can eliminate any of those at this time. We want to look at the sensitivity of the models to climatic change.

We keep coming back to wondering where is the data for the model going to come from. I'd like to talk a little bit about some of the new data that we have either processed or obtained, and then say where I think we're going to be going in the next year. I'm going to talk about the regional hydrochemistry data set, an update on the potentiometric surface, what new water level data points we've gotten in the last couple of years, touch a little bit more on the large hydraulic gradient and perched water, then see where we're going to go in the next year. I'm going to talk about testing at G-2 on the north end of the large hydraulic gradient, the C-well tests, and the clean out of the WT holes.

Dianna Perfect and her colleagues recently compiled a fairly large regional hydrochemistry data set for the Death Valley region in a Lotus spreadsheet kind of data base. This data base contains 4,700 analyses with 39 constituents. They did a chemical balance on all of these analyses and found out about 3,700 of them are chemically balanced within about 10 per cent. We put this data set together for release to others. We're nearing completion of the process. It's already received USGS and DOE approval. We're just putting the camera-ready copy together now, and I think some of the people in this room have already seen this data set.

Another thing that's happened in the last year is publication of the revised potentiometric surface for Yucca Mountain. The last time I talked to the Board about this, I don't think I made it very clear what we were talking about. Let me try again.

Here on the left is the revised potentiometric surface 1994 published by Irvin and others. It's for the small hydraulic gradient area only. It's on quarter meter contour intervals. You'll notice the dark contour there is a 730 meter contour. On the right of this is the 1984 potentiometric surface map published by Jim Robison. His map is basically only for the large and the moderate hydraulic gradient area, and the contour interval on that map is 100 meters down to 10 meters.

Again, the 730 meter contour is dark. So the only correspondence between these two maps is that one darkened contour. In the 1984 map, the characteristic of the 730 meter contour is primarily just inaccuracies in the data. It's a little serpentine because the data were not as accurate. The contour is fairly close to where the revised contour is. It's within a meter. If you take from the 729 to the 731 meter contour on the new map, the old 730 contour is completely within that. So while they look somewhat different, they're really maps of different things.

We have two new water level data points in the last year, thanks to the unsaturated zone drilling program. We have UZ-14 and UZ-16. This is the area of the large hydraulic gradient south of G-2 and WT-6. So we have one data point, UZ-14, kind of at the toe of the large hydraulic gradient; the other data point at UZ-16 down in the small hydraulic gradient area.

UZ-16, the information we've gotten from that, the gradient is so small there that until we get a final borehole deviation correction to it, that data point is not very useful, although the water table is very well constrained at that area.

UZ-14 is considerably more interesting to us. The water level at UZ-14 has not stabilized yet. We encountered the producing fracture beneath the water table in the May time frame, and this is plat of the recovery of the water level in that borehole. On May 10th, we blew the borehole dry and let the water level drain from the single fracture, and time zero on this plot starts when the water level in the borehole passed this single producing fracture.

We've already learned that the Crater Flat Tuff in this hole is extremely tight. Probably the final water level in this hole will be at somewhere in the 778 to 780 meter vicinity, which puts this somewhere near the base of the large hydraulic gradient.

Ed Kwickless talked about in the connection between the unsaturated zone and the saturated zone, the possibility that the perched water at Yucca Mountain on the north end of Yucca Mountain was simply water that had been shedded off the saturated zone off to the north. This map shows both saturated zone and perched water levels. I've used this to just show that the perched levels are consistent with that hypothesis. They don't necessarily prove that hypothesis, but the perched water in UZ-14, NRG-7a and SD-9 are all lower than the saturated zone off to the north and higher than the saturated zone off to the south.

Now, where do I see us going in the next year? One of the first things we're going to be doing is testing well G-2 on the north end of the large hydraulic gradient, be doing some flow surveys and some logging in that well, determine the hydraulic properties of that well and obtain hydrochemical samples. We're looking at doing that somewhere late this calendar year or early next calendar year. The June 2nd drilling schedule says that that will be done in

December.

Borehole WT-23 is planned for late in FY 95. WT-23 and WT-24 are two boreholes that are targeting the large hydraulic gradient. They split the large hydraulic gradient into about three pieces. WT-23 is about a third of the way up the large gradient, as we understand it now. The total depth of that hole is targeted to be about 150 feet below the base of the Calico Hills unit, so this ought to help us determine whether the semi-perched model is viable or not.

WT-23 and WT-24 together will allow us to understand at least the shape of the large hydraulic gradient.

I'd like to talk a little bit about what we're going to be doing at the C holes in the next year. I really need to preface that by talking about or at least acknowledging that we have a prototype site out in Raymond, California at a granite quarry where we're looking at fractured rock. We've been testing instrumentation and methodology there for the last couple of years.

We're now in what we call Phase One work at the C-well complex. We've got the packer string in place. We're collecting data, water level data from discrete packed off zones. We've developed a PC-based data acquisition system to collect the data. We were hoping to be into Phase Two fairly soon, which is pumping at discrete intervals. Now we're probably looking at at least early calendar 1995 before we get to this phase where we actually place a pump and a shroud in one of the holes, probably C-3, and start actually pumping water out of these holes.

This will be followed up with tracer tests. Current plans are for doing hydraulic tests for nearly a year, and then following those up with tracer tests. We may try to figure out how to combine some of our early tracer tests with our later hydraulic tests to try to accelerate the process.

In the next year, we were planning on cleaning and testing a series of WT holes in preparation for hydrochemical sampling. The WT holes were drilled in the early 1980's with air foam. They were drilled and never developed, never cleaned. The plan at that point was to go back in fairly soon and get them cleaned. We haven't done that yet. So this is primarily a development cleaning of the holes. We will take water samples during this process, but they're not the kind of sampling and detailed hydrochemical characterization that is envisioned in the study plan.

We will get some sort of a qualitative information about the hydraulic properties of the holes during this process. These are not going to be very controlled hydraulic tests, but we will monitor the discharge and water levels and be able to calculate a specific capacity from these tests.

Originally, we had planned on doing about half of the WT wells in FY 95 and the other half in FY 96. Now I think budget constraints are going to limit us to three in FY 95.

The last couple of slides are my attempt to see what the saturated zone studies can and cannot take to technical site suitability analysis, the information that we will have ready by the end of FY 97. I see that we will have our regional framework and numerical model fairly well completed and calibrated to the existing data set. We'll have our site framework and numerical model completed based on whatever site data is available at that point.

We're going to have a somewhat improved characterization of the large hydraulic gradient, at least we're going to know a lot more about the large hydraulic gradient than we do today. We're certainly not going to know everything about it. We should have information on flow and transport properties at the C-Well complex. We should have at least a qualitative understanding of hydraulic properties based on the cleaning of the WT holes. We're not going to have the second tracer complex that was talked about in the SCP and we're not going to have testing of the existing H holes by the end of 1997.

Now, the bad news is what we're not going to have in time for TSS, and that's a good understanding of the role of faults in the flow in the saturated zone. We're not going to understand to any large extent the interaction between the fractures and matrix at the site. We're not going to have full characterization of the large hydraulic gradient, although we will know a lot more about the shape of it and can possibly eliminate some of the theories of the large hydraulic gradient.

We're not going to know transport properties throughout the site. We're only going to know them at the C-Well complex. We're not going to have a significantly improved understanding of the role of the carbonate aquifer beyond what we have now. At one time, I thought that we'd have G-5 drilled to the carbonate aquifer by the end of FY 95, and I don't see that happening at this point.

DR. DOMENICO: Very good. Thank you.

I'll make a suggestion here. Why don't we get Dwight, Dick and Ed together during the roundtable, and why don't we take a 15 minute break, which is scheduled for now, and then we can have the three of them together. Is that okay?

(Whereupon, a 15 minute break was taken.)

DR. DOMENICO: Can I get you're attention? Okay.

We have probably some general questions to most of the presenters this afternoon, and we also have some items that we would like people to discuss, including the presenters this afternoon who probably know as much about this as anybody. But one thing we were asking about, the pneumatic pathway issue, and where do we stand today, and Marty Mifflin has volunteered to give us some information. I would expect that USGS would have something to add to that or refute, or DOE as well. But Marty, would you lead off on that?

DR. MIFFLIN: Okay, I was asked to say a few words on pneumatic continuity and testing, and I didn't realize it was supposed to be a formal presentation. But I'll go through--

DR. DOMENICO: It's informal, Marty. It's informal.

DR. MIFFLIN: Okay, informal. I'll go through a few points as to--it's a relatively new issue and it's raised to some importance with respect to the timing of the ESF construction and the types of testing that might demonstrate either pneumatic continuity or lack thereof with respect to the repository horizon and surface pneumatic continuity.

The question appears to be a rather important one if the thermal loads that have been discussed extensively, such as the so-called reference thermal load of the SCP or the dryout heavy thermal load where the idea is to perhaps keep the repository horizon above boiling for several thousand years, some scenarios up to close to 10,000 years.

The question that comes out if you consider large envelopes of above-boiling rock around the repository horizon is that there likely, with the information we have from heater tests, the mobilization of the entire moisture content from the matrix once it goes above boiling for any length of time. When you make some back of the envelope calculations as to how much moisture that is, with the reference thermal load of the SCP, you come out with something like 20,000 acre feet of water.

And if you do a very simple calculation and assume that half goes up and half goes down, and you estimate fracture porosity, effective fracture porosity at around 1 per cent, you find that above the proposed or modelled above-boiling zones, that you have the possibility you could fill the fractures with water, assuming they're not too much imbibed back into the matrix or that the matrix is already up at 80 per cent saturation.

That was recognized quite a while ago, back in the mid Eighties during review of I think it was the environmental assessment, and there were some comments to that effect made at that point in time.

So given that there is a question of what happens to mobilized moisture with an above-boiling thermal loading scenario, one then has to ask where this moisture will go, and then the fate of that moisture. And this is something that you can't do on small scale site permeability tests, et cetera. It starts to become a question of repository scale or footprint scale boundary conditions. And the obvious, based on the information that's been developed, is that there are some units which may well be relatively low permeability in terms of, say, bulk hydraulic conductivity, such as the PTn. And then there's also other features such as the faults or highly fractured zones and fault zones which may or may not have good vertical hydraulic continuity in terms of fractures or faults or both.

And this is forgetting about the idea that perhaps under certain types of engineering barrier scenarios, that we have to worry about gas phase radionuclides.

So if one asks these questions and approaches it from "What type of information base would allow some type of judgment on developing performance models with various thermal loads?" One has to have stresses on that system, of repository scale, in order to test these possible boundary conditions at a repository scale. And the frontal type storms that come through in the winter months, usually starting sometime in the fall. Two or three or five a year is something of a typical year where very strong frontal storms come through. And they usually are between something like September, late September is the earliest I recall seeing one, and usually late in March is the latest period of time where you see a real strong frontal storm coming through.

The amount of barometric change that occurs in one of these storms is such, based on the direct observation of UZ-6s and H, one of the H wells, I can't remember which one, H-4, I believe, and some of the other holes, suggests that there's differential barometric pressures set up, or differential pneumatic pressure set up so that the wells will blow due to a confined state. And in order to have them blow for any length of time, it's necessary to have fairly large reservoirs that are responding to the differential pressure.

So there's already some data that suggests that there's a degree of confinement, because under a pressure change, if there was nice open, very highly efficient pneumatic continuity to depth, there would not be the differential pressure set up to allow for those wells to blow or suck.

So the problem that comes forth is that there's not extensive knowledge as to what the bulk permeability or pneumatic continuity is with some of these features. Once the ESF gets into the mountain, and let's say it penetrates to the Ghost Dance Fault zone or any other highly fractured zone and that there's very high pneumatic continuity along those fracture zones, then you have basically inoculated that zone with outside barometric pressures. You no longer have an opportunity to measure the lag time between, say, land surface barometric pressure changes and the pressure changes that would occur at depth.

Conversely, if these data bases are established prior to the ESF construction, you have the opportunity to measure in vertical profile at any point at which you put the measuring ports in, which is just a matter of isolated transducers up and down holes, you have the opportunity to recognize if, say, the PTn is a low permeability unit or not with respect to pneumatic continuity, just by virtue of the lag in the pressure changes which are induced by the barometric changes at land surface.

The other methodology that's available and was referred to today was the geochemistry of the gas, where we saw at two sites at UZ-1 on the north edge of the footprint, there appears to be a systematic increase in dead carbon with depth, or a decrease in modern carbon with depth, and which would suggest that there's very little circulation there.

It's pretty much a dead situation. And then at UZ-6 and UZ-6s where unfortunately the configuration of UZ-6 is such that there's a possibility that there is a mixing of the shallower gas with the in situ gas, and there was that vertical profile, what, 75 per cent dead carbon or something like that at depth in the Topopah Spring.

So there's a suggestion that there is not good pneumatic continuity, at least locally, in the general vicinity of the footprint. But at the same time, in order to be able to model--

The sampling of the soil gas is a critical thing to do, again, before the tunnel gets in, because the tunnel, of course, during the tunnelling operation and construction-- I'll continue. The actual pressure changes at points and the combination of the sampling of the gas for the gas geochemistry seems like an adequate way to demonstrate on the site scale with two cross-checking types of data bases whether or not you have active circulation and whether or not some of these more obvious pathways, such as the fault zones or the faults, are indeed confining, to determine whether or not there is--well, what I have to say is not too important, so I'll continue on.

So, anyway, there are two apparent approaches that look at the problem on a repository scale, which possibly, or in my opinion probably would be compromised by trying to establish the data sets after the ESF has reached a significant part of the Topopah Spring and into the zone where several of the faults start to show up.

My feeling is is that if one is an optimist or pessimist, depending on which side of the fence you are with respect to the site, that probably the site will be used for some type of purpose, and regardless of what configuration it may settle into, it would be rather important to have a good idea of the pneumatic continuity with land surface, whether it's highly localized or whether it's quite uniform or whether it's quite confined, which it could be.

If one envisions a mobilizing or going to gas phase water in some unknown but significant envelope around the repository, that gas phase movement, or the vapor, will migrate to cooler regions. It will condense and it will be in some position for either flow back to the repository horizon, or if it's pumped out through one conduit or a few conduits such as typical geothermal areas, it may not be there for condensation and re-invasion of the repository.

So in a practical sense, forgetting about gas phase radionuclides, this seems like an important data base to establish to try to get at the major boundary conditions of the site, and that's about it.

DR. DOMENICO: It was my understanding at a meeting eight months ago that the Department of Energy was going to put in some holes immediately to address those concerns. Has this happened? Does anybody know?

MR. WILLIAMS: Dennis Williams here, Deputy Assistant Manager for Scientific Programs, DOE.

I think that we've had a variety of discussions with the Board and we've also had some presentations to the NRC with regard to the instrumentation that is going in the ground to establish this background pneumatic condition on the mountain. We're well underway in that program. I think Ed has probably talked about that a little bit as we've gone through it, or through some of the presentations today, and from the site perspective, we're out there actively doing tests in boreholes and preparing boreholes for instrumentation. So I don't really know what more to say about it, other than I probably wasn't heard.

I mean, I could itemize the drillholes, I could itemize the results of a preliminary ground test that we have conducted here recently that shows that we can grout the instruments in, I mean those types of things have been underway over the last couple of weeks and months.

DR. DOMENICO: Well, I recall at the pneumatic pathways meeting, Joe Dlugosh more or less guaranteed the people there that there was going to be an immediate program to put some instrumentation in before there was any further compromise by the tunnelling.

MR. WILLIAMS: Well, I guess, I'm sorry, but in response to further compromise, I don't think we've got the TBM going yet, which may be a positive and/or a negative.

DR. DOMENICO: Don Thorstenson is here and he knows something about pathways and pneumatic and otherwise, and maybe he'll have something to say about this.

DR. THORSTENSON: I can expand a little bit on what Ed said regarding flow in the Tiva Canyon, the separation from the Topopah by the PTN unit in the vicinity of UZ-6s and UZ-6. The Tiva Canyon in that area is an extremely dynamic system. Ed was talking to you about model flow velocities on the order of tens of meters per year.

We have some recently acquired data on atmospheric CFCs, Freon 11, Freon 12, that appear to be telling us that the flow time from the presumed east side of the mountain, which a whole variety of indirect lines of evidence say is where the gas must be coming from, flow time from the east side of the mountain to UZ-6s is on the order of two years, not the 40 years that's imposed by the fact that it's all full of post-bomb carbon. But it's very, very rapid, so that

that unit in fact at least on something approximating a footprint scale, you can say the Tiva Canyon is extremely permeable.

We're beginning to learn enough about it that I hope soon we may be able to start to say something about the degree to which the atmospheric carbon-14 dioxide sees or does not see the pore waters in that system.

When you look at UZ-6, you're looking at a deep major borehole that's been drilled, left open, it does in fact penetrate the PTn, it's not cased either above or below it, and so the borehole itself is providing a conduit for gas flow between the two formations. And, again, a variety of lines of evidence, the Freon data being the most recent and the most definitive, are saying yeah, there is shallow gas that's getting into the Topopah.

I think that I would make the following generalization that the degree to which this is analogous to the question of the impact of the ESF, et cetera, is argumentative, but I think you can say one of the things that's been lost by virtue of UZ-6 having been drilled and left open is the possibility of getting a real uncontaminated sample of Topopah Spring rock gas from that borehole. All of the information that we have to date suggests that that's not been accomplished. My instincts are saying I don't think we're going to, because even with the borehole closed, there's still a tremendous amount of cross formational flow between the shallow system and the deep.

So you've lost the ability to directly sample, what do you want to call it, the primordial Topopah Spring rock gas. You can maybe back some parameters out in terms of what did it look like with varying modelling exercises and stuff like that, but on the other hand, what we've gained by having the boreholes open is the ability to draw a lot of conclusions about pneumatic processes in the mountain that are not obtainable, for instance, from UZ-1.

I mean, the fact that UZ-6s, which blows continuously essentially all winter long, is principally thermally driven and only very, very seldom over-ridden by the barometric wave that Marty was talking about, is consistent with all of the other information that says it's permeable as can be. I mean, the equilibration times between the atmosphere and 100 meters is very, very short, and so most of the time, temperature dominates.

When you look at UZ-6 from the physics portion of the thing in terms of what we know, it's extremely permeable down there, there's huge volumes of gas that moves in and out of the borehole, but it does respond principally to barometric effects all winter long. And so you have something that just freaked us out when we first observed it. There's an access to the annular casing of UZ-6 which draws formation gas from some unknown but shallow depth.

And so depending on what the barometer is doing at any point in the winter, you can find a situation where the small hole on the outside of the borehole is exhausting gas at three or four or five meters per second. At the same time, the main borehole is inhaling gas at three or four or five meters per second. There are things to be learned from that, and I think I would just simply say so far at least my own opinion is having UZ-6 there in terms of getting at pristine chemical data has posed problems. On the other hand, we have gotten a lot of information that probably wouldn't be there in its absence. For the moment, let's let it go at that.

DR. DOMENICO: Ed, is there future testing plan before the presumed mountain is compromised by the TBM if we ever see a TBM in there? What's on the horizon in terms of more planning, in terms of the pneumatic pathways?

DR. KWICKLESS: I may not be the best person to answer this. But my recollection is that there was going to be an accelerated drilling program that would attempt to have boreholes implaced and instruments implaced in order to measure the dynamic impacts of the excavation of the ESF.

And so there could be a before and after picture of the types of information that Marty described in trying to look at barometric pressure as propagated through the mountain, and also get into the mountain before gas samples might be contaminated by gas entering through the ESF. Whether that accelerated schedule for drilling is actually progressing, I can't comment on that.

DR. DOMENICO: It was my understanding, again, that there was going to be an accelerated program, and this was essentially told to the people at the pneumatic pathway meetings by DOE representatives. So I don't know if it's underway or if some problems have come up.

MR. WILLIAMS: A comment on that. Again, Dennis Williams here. I think that's exactly the program that we're talking about, and our objective was to get especially the instrumentation of NRG-6 and NRG-7a completed before the onset of the winter season when we had the barometric stressing, and of course before we got the TBM in that vicinity. And I think right now, we're looking at the TBM schedule calls for them to turn the corner sometime in the winter of '95. So the way we're going at the present rate, we've already done quite a bit of gas phase testing in some of the north ramp boreholes, and of course we're progressing with the permanent instrumentation in the two that I mentioned. We should be in place well before the TBM arrives on the scene.

DR. DOMENICO: Well, to change the topic a minute, maybe I can address this to Dwight or Dick, in the sense that we may not be going down to Calico Hills, what do we have to know about the Calico Hills? What information do we have to know if we're not going to go down and look at those rocks in this characterization procedure? Any thoughts on that?

DR. HOXIE: It seems to me that one thing we really need to know is what the distribution of the zeolites are in the Calico Hills, because those are the minerals that will provide the barriers for radionuclide transport.

I think the other thing that we would like to get information on is what are the effects of the faults that transect the Calico Hills, particularly like the Ghost Dance structure, because that might provide us with pathways that somehow could provide transport pathways from the repository to the water table.

But I think one thing that we really need to do also, since we're discovering that we can perch water on the vitrophere at the base of the Topopah Spring, is that we need to know how extensive is that perching, because we may be able to perch water on top of that and cause it to move laterally away from the Calico Hills, which would be our primary barrier, so that it would bypass Calico Hills, maybe getting into something like the Ghost Dance Fault and then going on down to the water table.

So I think we need to answer the questions of how extensive is this perched water phenomenon over the areal extent of the repository itself, especially in the direction to the southeast where radionuclides would be going.

DR. DOMENICO: Well, how are you going to determine these; how are you going to measure these things? It seems like that's pretty difficult, determine the distribution of perched water when the surface drilling program is not progressing that rapidly, and that seems to be the only access we have to the Calico Hills now.

DR. HOXIE: Well, that's the only one that we have in the plans right now that I know of. I mean, we still have the possibility of an ESF extension to the Calico Hills. Maybe we should have gone there first and tried to identify just what was going on in our primary barrier. But you run into the danger there of uncertainty. The principle thing is that in order to access our primary barrier, we have to perturb it. So you want to minimize that disturbance also, so there's a Catch-22, I think, folded in there.

DR. DOMENICO: Don Langmuir has a few I think questions to pose to somebody.

DR. LANGMUIR: One of the things I've come out of the meeting with, and actually I came in the meeting with, was a concern that we didn't know enough about fracture properties. If we're going to consider them the fast pathways and the most likely avenues for gas releases or ground water releases, we've got to characterize them and we've got to get at them somehow and determine their properties. We've heard a lot today about measurements of how many there are, and Ed mentioned that we didn't know from the number which of those were relevant to transport.

Ed made a comment in one of his overheads that said fracture property estimates were based on an aperture scale fracture flow model. I guess I don't know what that means. Were those estimates, truly estimates, or do you have real data that you're using to get at your fracture properties?

DR. KWICKLESS: I believe that slide was made in connection with the series of simulations that I'd done for Pagany Wash, and there was a need to estimate properties in the absence of any experimental data on what the capillary properties of fractures were and what the relation between the transmissive and capillary properties of fractures were. And the model I alluded to was a numerical model that considered small scale aperture variability within the plane of the fracture, and it was based on purely theoretical constraints and allowed us to generate, in the absence of any experimental data, what are the most credible capillary and permeability properties for a fracture, given the assumption that we know what the geometric variability of aperture within that fracture are.

So it was just a modelling assumption made with another fairly sophisticated numerical model that allowed us to generate theoretical fracture properties.

DR. LANGMUIR: How much information do you have that's really measured on fracture properties in the unsaturated zone at this point that you can go to the modelers with with any confidence? I mean, where are you on the sigma in Dwayne Chesnut's log normal distribution function? Did you have a warm fuzzy feeling when Dwayne said he was using two and a half, I believe, or one and a half?

DR. KWICKLESS: I guess I'd have to question whether a model that was developed to model heterogeneity in a porous medium would be the right model to apply to a fracture network and a fracture system. And so the issue of whether sigma was one value or another may not be relevant if the conceptual model is itself not the correct conceptual model to describe flow in a fracture system.

In terms of your first question, I think there's been some extensive mapping of the starter tunnel and of the test alcove and also of the pavements around Fran Ridge where the block experiments are to be, and the intent is to analyze that data with a discrete fracture model to calibrate the relevant fracture apertures and fracture transmissivities. And we could do such a calibration now that we have the permeability data for UZ-16, we have the fracture data for UZ-16. It's a question of calibrating parameters for UZ-16, going to another hole which we already have fracture geometry information, such as UZ-14 and for which we will later do permeability tests, and now begin the process of iteratively calibrating and predicting flows based on the fracture data.

I recently visited Golder and Associates who have developed this fracture network model called FRACNET and I was asking Bill Dershowitz who's a developer of the code if it would be appropriate to do a fracture network analysis, given the apparent fracture density, and Bill's comment was that a fracture network was an analysis that probably wasn't appropriate. What you'd probably want to do was a sieve analysis.

DR. LANGMUIR: Say that again?

DR. KWICKLESS: A sieve analysis, given that you basically had gravel based on the fracture densities.

On the other hand, the tritium data that Al collected indicates that the fracture spacing is much greater than what they suggest.

DR. LANGMUIR: We heard from some optimistic thoughts from Rally Barnard yesterday and Jim Duguid that they thought that GS was going to give them information next year which would give them some more comfort in terms of being able to model fractures in their three dimensional model of the mountain. I don't know that you were here to hear that comment, if I'm correct, I believe that's the sense I got. What can you give them next year that's going to give them a warm, fuzzy feeling about their 3-D models? And when you see a model with 4,000 nodes based on six or eight or ten holes, do you have a warm, fuzzy feeling? I'm concerned about the data you're being able to feed to them to give them confidence in their model of the mountain.

DR. KWICKLESS: In terms of fracture data?

DR. LANGMUIR: Fracture information, but also just rock properties generally. I have no trouble with matrix properties. That seems to be easy to do from lab tests, but the importance of fractures and their properties. My sense is that's one of the most important inputs to their models, and it's coming from the GS.

DR. KWICKLESS: A thought that I've had is that water flow is probably not going to be very sensitive to overall fracture permeability, in that what we're measuring is something like the intrinsic, single phase permeability of the densely welded units. It may not be that important whether that permeability is a million millimeters per year or a trillion millimeters per year if the fluxes are only a millimeter or ten millimeters a year. Their conclusions may not be very sensitive to overall fracture permeability.

What it may be more sensitive to is how preferred pathways form in that network. I think that in part, we're at a very primitive stage of understanding just how those pathways form through a fracture network. I've done a very schematic representation of the fracture network and looked at just that issue, but there's basically no experimental data to corroborate the findings of that model.

DR. LANGMUIR: How much will you be depending upon the ESF and the alcove tests that are going to be done in the ESF, that are projected to be done there? I mean, the fractures are largely steeply sloping vertical type features, so that it's very tough to get at them, I presume, in the surface based testing effort. To what extent are you depending upon the underground workings as a basis to get at them and characterize them?

DR. KWICKLESS: Well, that is one of the benefits of the ESF, was that it was hopefully going to correct for some of the vertical boreholes. Many of the air permeability tests in ESF are of course going to have essentially horizontal boreholes that will probably be able to sample the predominantly vertical fracture network in a way that the vertical borehole can't.

We have several tests planned similar to the test that Livermore has proposed for the heated block experiment, only in our case, the block wouldn't be heated. We would, as for the small block experiment that we described during my presentation, we would heavily instrument a block and apply a known flux to the upper surface of that block and monitor through small diameter boreholes just which fractures in that block, wet or filled, flowed water, and try and understand the basic mechanisms controlling water movement in variably saturated fractured rock.

This is cutting edge as far as hydrology is concerned, and whether such experiments would yield data that would be transferrable to the kind of calculations Rally Barnard described or just what the outcome of such an experiment would be. I don't think that we can say ahead of time. I know that often times in the Yucca Mountain project, we're asked to say what our results are going to be. I don't think we know what these results are going to be and how useful or how transferrable they would be.

DR. LANGMUIR: I had one more for you. I was surprised and impressed with the capillary spreading business, the idea that you could explain the reversals in ages because of different properties of the adjacent zones. I had inferred, I think perhaps naively, from looking at the reversals and dates, that they reflected the different properties of 14, CO2 and Chlorine-36, so that when you had, for example, different dates in the same horizons, the younger 14, CO2 was reflecting the fact that it was partially gas, partially going into fluid phase, whereas the Chlorine-36 could only get there as a fluid, and so, therefore, it was truly showing you a real age.

But today we learned that it was quite likely that the older Chlorine-36 ages, certainly those above I believe the Calico Hills in the Topopah reflected waters that were really not in pathways of any great importance to the movement through the system. When you sample a heterogeneous system, you're not going to likely hit the rapid pathways, whereas, as you went down to the lower horizon where you get spreading, you're going to hit a representative sample and get a representative age.

I guess I'd like you to comment further on that, and I suspect that changes Dwayne Chesnut's model a little bit, since he assumed some things about his Chlorine-36 ages in his model, not assuming perhaps that effect was taking place.

DR. KWICKLESS: We did catch glimpses of those pathways through the Topopah with the tritium sampling. We do see several instances where we sampled tritium above 20 tritium units which would indicate that there was relatively recent water. We went through fractures around that hole. So in terms of catching little glimpses, I think we did catch some glimpses, but in those kinds of instances, it's always difficult to say, well, was that sample contaminated or, you know, why am I seeing this erratic distribution of values. And I think that the fracture network model that we described helps you understand why you just catch these intermittent glimpses of a fast path.

Don Thorstenson can describe better the possible inter-reactions between the gas C-14 and the liquid C-14. I think I'll defer that question to him.

DR. LANGMUIR: Let me ask Don a nasty question then. What I come out of this discussion with is the possibility that travel times through the unsaturated zone are one or two orders of magnitude faster than we thought if the Chloride-36 data does not reflect the real ages of water that's come directly and continuously down from the surface, but rather water that's been bypassing and has been sitting there out of the flow regimes, and that if in fact we can't sample, say we're looking at the Topopah Springs, we cannot get at it except accidentally, samples of rapidly moving ground waters for Chlorine-36 dating, we can't get those dates in that horizon except by accident, and we haven't perhaps hit them yet. And the dates for Carbon-14, CO2 really are ground water dates, perhaps, in the Calico Hills, and they're younger and they're suggesting more rapid flow.

Is that stretching it too far, Don?

DR. THORSTENSON: No, I don't necessarily think so. I mean, the way that I at least try to think about this kind of stuff is just in terms of relative rates of different processes. And, I mean, I'll just cite a super-simplistic example.

You could have water percolating either in fractures or matrix or however, but let's just say you have water percolating very slowly through the Tiva Canyon down to the Paint Brush non-welded. Everything we think we know about the Tiva Canyon says the gas fluxes are high enough that basically if that were to happen, if you had very slow water percolation, the Carbon-14 signature in that water would be governed by gas flow processes and exchange with the water, not by the rate at which the water is moving through the envelope, in this case, of C-14 dioxide.

If the water in fact is moving very rapidly, then the question is one of how much interaction is there. If the interaction is very little, it's hard to tell what the water may show up in the end with in terms of Carbon-14 completely independent of whether it got there in two weeks and it's got 100 tritium units in it.

I'm not sure if that's exactly answering your question or not, but I don't particularly have a problem with discordant dates by different methods if you can construct plausible mechanisms within the state of our knowledge of the hydrology by which they could be explained.

DR. LANGMUIR: What I come back to is the implications of all of this to ground water travel time. If the longest travel times are in the unsaturated zone and now we're saying that they're ten-fold faster than we thought because Chloride-36 does not really measure the bulk of the water that's going through the section, then we've got some problems.

Dwayne, do you have a comment?

DR. CHESNUT: Let me try to address a couple of these points. The UZ-16 data was one set that I showed this morning. The other one was the compilation of data from the PTn from three different boreholes, N-37 and UZ-16 and one other.

On that particular data set, there are 20 measurements of Chlorine-36. Five of those are modern. Five are bomb-pulse. 25 per cent of the entire data set for the PTn from those three boreholes is, you know, it's 40 years old, whatever number you want to put in there. The rest of the data is essentially within the cosmogonic levels.

Now, my model represents an extreme assumption, which is that there is no mixing among different flow paths. It is an end member of the system in which you have pore mixing among different flow paths. So when you go out and make a measurement and you take a sample, you're going to get something that is connected by a hypothetical flow path back to the surface. It doesn't necessarily have to be vertical. It doesn't have to be a straight line. But the assumption is that it's independent of the next flow path that I sample.

So if you consider sampling a medium like this over and over again, what you should see at a given depth is the log-normal distribution of the apparent travel time.

Now, to make things even more complicated, there are two different kinds of times that I can actually get. One is what's called a resident concentration that's left over in the rock itself. And this is a volumetric average of essentially a steady state distribution. If you put in Chlorine-36 for 100,000 or a million years, you're going to reach some kind of a steady state log-normal distribution.

The other part of that is what flows through that mass of rock and hits the water table. There's a flux coming down through the system. The average age of that water will not be the same as the average age of what you see in the pores and what's left behind.

Now, this is a consequence of this particular conceptual model. It has interesting implications because the Chlorine-36 dates that we see at the water table are modern, less than 50,000 years old. And Ed reported one figure this morning in the Calico Hills that's 600-some odd Chlorine units, which may mean there's some bomb-pulse mixing at that particular location in the saturated zone.

So the point is that at least in a cartoon sense, this thing can start to explain some of the large degree of variability that we see in the data. It doesn't necessarily mean that it's correct, but it may be a good approximation, and in one sense I have tied this back into fractures.

The flux distribution of inflow into the mine is a perfect log-normal function and has a sigma of about 1.6. If the flux is log-normally distributed, the transport process is going to be pretty close to log-normal. So there is a lot more work to be done. I'm not saying that this is a validated conceptual model. It's an approach that may give us another handle on the problem.

DR. KWICKLESS: I don't think the problem is with Chloride-36. It's a problem of sampling in a structured flow field where your chances of intersecting the active flow paths are relatively small. It has nothing to do with the fact that it's Chloride-36 versus--

DR. LANGMUIR: No, but that's just what we have data on. We have to explain what we're measuring in an intelligent way, and it's the stuff that makes it through that's going to be the ground water travel time issue, not the average age of the pores and fractures in one spot.

DR. CORDING: I wanted to continue some comments that I was hearing this morning or earlier today on the importance of--just to go to one item, the importance or the timing on going to the Calico Hills. And, Dwight, you were describing that if you got down to the Ghost Dance Fault and it was bone dry, then maybe you wouldn't have to go--but realistically, what do you think one is going to be able to see and make a decision on at that time? And then what is the plan then to actually--I believe you say then one should go down to the Calico Hills. How is that going to be done and achieved and how do you get the results from the Calico Hills? I mean, are we talking about something that's a real plan here or just talking about it?

DR. HOXIE: I may have to pass to Dennis, but let me just say that I know that there is going to be a system study in 1995 that is supposed to be looking at reasons for going to the Calico Hills and how to get there in a timely fashion if we should decide that we need to get there. I don't think that answers your question, but that's my knowledge.

MR. WILLIAMS: And that's about where we stand right now in our system study. I think it's still in the program. I think we're talking about a January or February time frame on that.

DR. CORDING: Of '95?

MR. WILLIAMS: Of '95; that is correct.

DR. CORDING: Of starting it or getting a result?

MR. WILLIAMS: Getting the results of the system study. And then from that point, of course, then we will plan according to the decision that is made on that system study.

DR. CORDING: So that system study would involve what components?

MR. WILLIAMS: Do we have anybody here from the systems group that could tell us exactly what's entailed in that? They're probably better qualified than I am to talk about it.

It looks like we are lacking on that.

DR. CORDING: If you could update us on that in some fashion later, I would appreciate it.

MR. WILLIAMS: Could we do that, Claudia?

MS. NEWBURY: Yeah, I'm sure that Steve will discuss it at your next meeting, Steve Brocoum.

DR. LANGMUIR: I had a different, a general question. Those of us who have been watching and have seen the dry drilling program appreciate that it's very slow and very expensive, and it's only part of the way that it's planned to go. I don't happen to know the answer to this, maybe you do, perhaps Dick Luckey does. How far have we gotten in the total plan of surface based testing as proposed by the DOE sometime ago for the characterization effort at this point in time, not projecting what you might accomplish in another year or two? But my sense is you've got--I think we computed at the present rate of drilling those dry holes, it would take 23 more years or more to finish the ones that had been proposed originally. This is more, of course, to the surface based testing program than the dry drilling. But where do we stand on collecting the information that was proposed by the DOE originally in the surface based testing program?

Can anybody answer me on that, in general terms at least?

DR. LUCKEY: I can't give you any specific percentage value, but you are correct that it's a small percentage of what was initially planned in the SCP. I'm more familiar with the saturated zone studies, and we haven't done any of the holes that are part of that study to date.

As I indicated, I see one coming next year and another one coming the following year. The total number of saturated zone holes was probably something close to a dozen, and that's not going to happen in the next couple of years. So it is going very slow.

There are some positive aspects here, though, at least from my perspective, is that we have integrated the drilling program a little more. Now we're getting saturated zone information from unsaturated zone holes. There is no easy way to compute the number that you're asking for, but it's going to be a small percentage anyway, however we compute it.

DR. LANGMUIR: Well, certainly you're going to be asked in this PPA to prioritize what has not been accomplished so far, or help the DOE make that decision. And I wonder how you feel about that? That's a very loaded question, since you will have only been a short way through your program when you're asked to decide what you can do without in the next few years.

DR. LUCKEY: I think we've tried to answer that question by saying what's the most important piece of information. My perspective is that to fully understand the hydrologic system, we needed everything that was proposed in the SCP, and the DOE as managers of the program saying well, we probably can't afford to understand everything, we've got to proceed at some risk. And so what the USGS has tried to do is to tell DOE what we believe the most important components are that can be looked at early and at some reasonable cost. And so in the saturated zone, probably our biggest discomfort is the large hydraulic gradient, and so we have targeted a limited number of holes at the large hydraulic gradient.

Let's not further define the small hydraulic gradient area; let's go at probably the biggest discomfort that we have and do it first, and then we can fill in later.

DR. LANGMUIR: How many holes will you be putting in to better characterize that between now and '97 or '98?

DR. LUCKEY: Under the current plans, two holes, which is all that was written into the SCP explicitly. There were two holes to look at the large hydraulic gradient with the assumption that after we learned everything from those two holes, we may need to do something more. When we wrote the original plans, we weren't convinced that those two holes were going to do it, but those two holes were needed to start to do it, and those I do see coming before the TSS decision is made. There are other things that are not going to come, so we can get those two holes in the ground.

DR. HOXIE: Dick, just a point of clarification. The two holes you're talking about are the two WT holes?

DR. LUCKEY: Yes.

DR. HOXIE: But there's also a plan to drill G-5 also in the large hydraulic gradient, and that was supposed to be a very deep hole, presumably going all the way to paleozoics so that we could get some idea what the vertical distribution of heads would be at depth, which is something we don't have and something that we will not get from the water table holes. It will give us the configuration, but they're not going to give us anything about how the head might vary with depth, whether or not it's decreasing with depth or increasing with depth, which would be an indication of which way the water is actually moving. So I think we need, in order to really address the large hydraulic gradient, we need that G-5 borehole.

DR. LUCKEY: I think that we need to realize that this is a constantly evolving program. You don't have to be real bright to know that we're not going to get everything, and we keep looking at sort of backup positions, and the testing of G-2 that I've talked about today is not discussed in the SCP at any place. It's kind of a backup position because we knew that we're not likely to get G-5, which would give us that kind of information in the near future, so we're looking at secondary, lesser quality information to try to at least flush out some of the concepts we have of the large hydraulic gradient.

MR. WILLIAMS: Dennis Williams here; a comment on your beginning question with regard to how many drillholes we're dealing with right now compared to what we dealt with before.

My only frame of reference is going back about three years when I started on the project. And at that point in time, I think you all had a drilling schedule that called for 40 deep holes, and we had a drilling consolidation shortly after that that possibly eliminated a couple of those holes. And, of course, since that time, we've gone into quite a program of trying to combine as many programs as we can in individual holes.

But I guess to specifically answer your question how many have we drilled in that period of time, basically we're into the fourth one. We've done UZ-16, we've done UZ-14, we're almost done with SD-9 and we're some two-thirds of the way through SD-12. So that's kind of the sad state of affairs.

DR. LANGMUIR: I understood there were going to be some more conventionally drilled holes going in which didn't require the cost or the time scale to get some information of value.

MR. WILLIAMS: SD-9 is not a dual wall hole. That is a conventionally drilled hole with the Stratmaster. It's not dual walled reverse circulation. And, likewise, a couple of the holes that we're instrumenting at this point in time, NRG-6 and NRG-7a, are also conventionally drilled holes done with LM-300 drill rigs.

One of the things that we're really interested in is comparing the results of a lot of the gas permeability and the instrumentation values that we get in these conventionally drilled holes versus the LM-300 type holes. Is there a difference? That's something we really want to know.

DR. LANGMUIR: Any more questions from the Board or from the Staff? Bill Barnard?

DR. BARNARD: Bill Barnard, Board Staff.

Dennis, you said you started out with 40 deep holes; you've done four after this consolidation. How many more do you think you'll need to do over the next several years, few years?

MR. WILLIAMS: Well, we don't know for sure how the consolidation is going to work out. What we had targeted as a possible goal is to reduce the total number of holes by half and still get all the technical data that we needed.

Of course, the schedule for TSS has thrown a little bit of a new wrinkle in that, and that's caused us to prioritize some of the work, like the large hydraulic gradient. So it's a little bit up in the air, and of course we don't know for sure how effective we will be until we get to the end of the process.

DR. LANGMUIR: I just had one last set of questions which I think are going to the hydrology folks in the GS.

Dick Luckey had a very useful last slide in the overhead collection, which said what saturated zone studies cannot take to the technical site suitability table, and I wanted to have him talk about them specifically, and maybe I could change a couple words here and there in them before I ask him to do it. But what the studies cannot take to the TSS and of course it's not just going to be saturated zone studies; it's also going to be Ed Kwickless and the unsaturated zone studies that are providing inputs like this, a good understanding of the role of faults.

These sound like very critical issues to me that cannot be addressed before the TSS, a good understanding of the role of faults, and I would suggest that maybe we don't have to--I don't know what the word full means--but a good understanding of fracture matrix flow at the site, and I think you said you felt you could get a good characterization of the large hydraulic gradient with two more holes. Maybe not full, but maybe enough to have a comfort zone about it.

This list strikes me as a very important list in terms of what needs to go to the site suitability table. I wonder if you could comment a little bit more about this list, and perhaps others could here, too. It's the very last page.

DR. HOXIE: Could I make just one comment--this is Dwight Hoxie--while Dick is finding his notes?

DR. LANGMUIR: By all means.

DR. HOXIE: One thing we will get from the ESF, however, is probably a good characterization of the faults in the unsaturated zone. That will be a plus because we will have alcoves on all of what we identify right now as the major faults that transect the repository block.

DR. LANGMUIR: And I guess this shouldn't be a list that I throw at Dick; it should be a list for the program as a whole. The saturated zone studies will provide inputs to this, but I guess the issue is how much will all the players bring to these questions.

DR. HOXIE: Presumably, some of the information that we would get from the UZ in the faults would also transfer to how they would behave hydrologically in the saturated zone as well.

DR. CORDING: Dwight, how important do you see getting east-west across the site, at least between Ghost Dance and the Solitario Canyon Fault?

DR. HOXIE: Well, I guess personally I'm not particularly concerned about Solitario Canyon Fault because I don't really see that necessarily it is going to be a problem for us in the UZ. I think the information we'd really like to have is what is it doing for the saturated zone system. Is it acting as a conduit or a barrier? We have some preliminary heat flow data that indicates we have some upwelding along the Solitario Canyon. So it really may have more significance for transport in the SZ as opposed to any influence it may have on the UZ.

DR. CORDING: Okay. What about anything beyond the Ghost Dance, I'm talking about in the repository, or things that would be going through in the main part of the repository, proposed repository block?

DR. HOXIE: Well, I think anything that we encounter along the way is something that--anything we find that's unusual, and it was indicated yesterday we're certainly going to find surprises, we need to have the flexibility to be able to stop and test if we need to.

DR. CORDING: Do we need to go across and get beyond the Ghost Dance Fault? In other words, you've got some penetrations into the Ghost Dance. Do we need to get to the west and get across at least towards the Solitario Canyon?

DR. HOXIE: Well, I would argue that perhaps more importantly--we going to have the north ramp extension presumably, which will be going towards Solitario Canyon Fault--but I would also argue that perhaps what we really need to do is to go to the east where we would be looking at the Imbricate Fault zone that bounds the block on the east. And there is a plan of course to run a drift over that way, but it's not in the 1997-'98 time frame.

DR. CORDING: Well, I guess in terms of the importance of the extension on the north ramp to the west, you do see that as an important part of this characterization of the fault system there that could be in the repository block?

DR. HOXIE: Well, I guess I really personally, this is just my own personal opinion, is that again for the UZ, I don't really consider the Solitario Canyon Fault to be that significant. The importance for the north ramp extension, in my mind anyway, is to provide an access point for the thermal testing. But other people may have different views on that.

MR. WILLIAMS: Dennis Williams here with an observation.

By the time you get your north ramp to the main level of the Topopah, you will have encountered the Bow Ridge Fault. There's numerous small faults that are probably at the north extension of the Imbricate Fault zone. You will have gone across the Drill Hole Wash Fault. You will likely have gone across the trace of the Sun Dance Fault and a couple of intercepts on the Ghost Dance Fault. I mean, if we're talking about characterization of faults and looking at the potential for fast flow paths, don't we have quite a number of intercepts there.

DR. HOXIE: Well, I would agree with that, except that I think the question was whether or not we need to get to the west bounding fault and characterize it.

DR. CORDING: Or just get to the west of the Ghost Dance Fault, I guess that's--I mean, I know that plans are in a flux at this point and I know there's different plans being proposed and all. I'd just like to know whether we think it's important to get to the west of the Ghost Dance Fault, which involves about two-thirds of the volume of the proposed repository.

DR. HOXIE: Well, certainly the north ramp extension will get us to the west of the Ghost Dance Fault.

DR. CORDING: I'd agree with that and I understand there are different--you know, plans are in flux and I think it's--at this point, I'm not willing to assume that anything is going to be accomplished, and I think we as people looking at the technical side of it need to be very clear as to what they think is really important for this facility.

DR. LANGMUIR: Well, let me come back to my question, and I'll take Dick off the hook a little bit because in fairness, it's a question for the whole program, because as Ed is suggesting, you're going to get fault information from the ESF, critical information on faults, and of course Ed's program is looking at the unsaturated zone fault properties to some extent, at least.

So the question really, if I can rephrase it, is this list of issues that Dick has very nicely fed me, because I asked him to make such a list up, is a question for the program and how far we're going to get with this before the decision in '98, whether it's through the ESF with very limited data or a small amount of additional surface based testing in the unsaturated and saturated zones.

My sense is we're going to have some more understanding of faults by '98. I don't know whether the word good is the right word. Could you all pick up on this? Do you see where I am on the overheads here, this very last sheet? Fracture matrix flow; I'm still quite concerned about this one. I'm not persuaded that we're going to know very much about it in a year and a half or two, and I think it's critical to what models we select to determine the flow of the mountain, whether it's the "WEEPS" model or composite model, which have very different implications in terms of travel time and movement of contaminants. I'm not supposed to be answering this, though.

DR. LUCKEY: Let me take a crack at it, since at least I started with this list, and I'd like to make it real clear that this list is just for the saturated zone.

We're going to know more about faults by the time TSS comes around. We're going to know more about what they look like at depth, which we don't know now. I put this first bullet in because it was a specific test to look at the Solitario Canyon Fault in the saturated zone, because it looks like an important barrier to ground water flow, and we were actually going to test that. And sort of a fallout from that test was perhaps we would also get some information about what the Ghost Dance Fault is doing in the saturated zone.

I put this on the list because I don't see that happening, because other higher priority items are going to prevent it from happening. I don't have a great comfort with it not happening, but I still believe we should do first things first in this program, and this is a second thing. So that's why that first bullet was on there, is the role of faults in the system.

We probably will learn something about the Drill Hole Wash structure, what the nature of that structure is, and perhaps that will tell us something about our conceptual model of flow in the saturated zone. But we're not going to have anything that we can point to as a measured value or a real test. Just by knowing more about the characteristics of faults at depth at the site, we're going to gain some information, but it's going to be pretty limited.

I think the second item, a full understanding of the relationship between fracture and matrix flow at the site is probably a very critical item, maybe more critical in the unsaturated zone than it is in the saturated zone. In the saturated zone, we're already pretty convinced that most of the flow is in fractures. It's just which fractures and what size and what percentage.

It's not quite so clear to me, listening to people, that we know that yet in the unsaturated zone. We do know that under transient conditions, there is some flow in fractures. Now, whether this occurs for great distances most of the time or is a very local phenomena, I think that's a critical question that has to be resolved, maybe not with complete certainty, but we have to make some great progress at knowing what percentage of the flow is in fractures in the unsaturated zone and how important that really is.

Perhaps if occasionally there are very fast pathways through the unsaturated zone, but it doesn't contact waste canisters, I don't know whether, for performance, that is really a great big issue. I'm certainly not a chemist, but I know that solubilities of some of these things are important and perhaps if you can move the water fast enough, then the contact time is not there for having solubility. So I don't think it's an easy question to answer on this whole ground water travel time and fast paths.

I think I'll stop with that much and let somebody else have a crack at it.

DR. KWICKLESS: I just have a comment. I'm amazed at how much was learned from this one hole of UZ-16. And you have to realize that it's the first dry drilled deep hole from out of this program in all of the years of drilling, and when you see the variety of different kinds of information and the different kinds of insights that have come out of this one hole, I think here's a lot of hope that we're going to understand a lot about the basic mechanisms of flow at Yucca Mountain. We can essentially triple our knowledge this year with the analysis and testing of UZ-14 and UZ-7 and 7a.

So in terms of how far we've come in our understanding, just on the basis of this one hole, in a sense, it's very encouraging. And if some of the surface holes, particularly around the faults, I know Joe has talked about the UZ-7 complex, which I think straddles the Ghost

Dance, and the UZ-9 complex, which I think is in the Imbricate Fault system, his will allow some cross-hole testing. We'll have not just one hole at these fault locations, but we're able to test, you know, parallel and across the faults.

I think that if UZ-16 is an indication of just how far you can take information, real data, there's some reason for optimism that we can at least understand this site better than we have in the last ten years.

DR. LANGMUIR: But you have to have confidence that whatever you're determining from one hole can extrapolate and that there's some consistent properties laterally within each of these formations. I do worry about the areal coverage that you have so far of the mountain. To what extent do you think it represents the mountain?

DR. KWICKLESS: I don't think we have enough holes, deep dry drilled holes, to even begin to understand the variability from one location to the other, to understand how--I mean, to have a sense of your uncertainty, you have to have more data than one dry drilled deep hole, and I think it's really far too early to say. You know, everything is uniform at this point, okay. We have one dry deep hole. There is no variability, there's no uncertainty. You know, if we drill a second hole and things are completely different, you know--

DR. LANGMUIR: Well, two holes make a line.

DR. KWICKLESS: Then you've got to drill a third. I mean, if you're going to do this rationally, you would keep doing it until you converged and the collection of a small amount of additional data didn't change your understanding.

DR. CHESNUT: I keep coming back to Chlorine-36 because I think it's something that's extremely important if we can understand it. And I believe this gives us the best information we can get on the fracture matrix interaction problem because you cannot explain what we see with the porous model. You have to have fracture flow to get the stuff to give you such a strange and wide distribution of ages and to help bomb-pulse at 1,400 feet in the mountain.

So I think systematic collection of data is going to help us resolve that fracture matrix problem in the unsaturated zone.

I'd like to point out that when we get that main ESF tunnel, the north-south tunnel through there, we're going to have probably, what, two kilometers of sampling at essentially the same stratigraphic location at about the same depth from the surface, and I think that's going to be very interesting data because it's going to give us one slice at one depth that tells us something about fracture matrix interaction from the surface down to the repository level.

It doesn't answer what's going to happen below the repository, but at least it gives us a handle on the probable waste package contact mode possibly, what the flux distribution looks like, and possibly some way of bounding what actually gets to the waste package. And ultimately we can contact waste and form a source term. So I think it will give us a lot of information on the source term part of the whole problem.

DR. LANGMUIR: Any more questions or comments from folks at the table here?

DR. PALCIAUSKAS: Just one brief one. Dwight--I'll make it very brief--Dwight promised us something here and he didn't quite finish it. He gave us a big list to follow as Ed and Dick were speaking to make sure that they clear all the topics, and I'm looking at one glaring topic at the bottom there that really relates to your PA paradox. Basically, it's how you transfer measured data into models or vice versa, and that's usually--you know, modelers always demand data that isn't measured, and people measure data that isn't being modeled and you have to make the connection, which is really a very difficult task, as we all know, and I was just wondering if you could touch a little bit on the last point that was incurred.

DR. HOXIE: Well, actually, the instructions were that you were supposed to keep score to see if the speakers actually addressed all those issues, and I'm going to defer to Ed because he did talk about the scaling problem in his talk. So do you want to--

MR. PALCIAUSKAS: Maybe I could just limit the question. Which properties are you having the most problem scaling up into the models, which physical properties are we referring to?

DR. KWICKLESS: I guess the problem I don't think is so much scaling; it's something that Dwayne has touched on. Is your conceptual model the right conceptual model? I think that the major source of uncertainty is not parameter values for equivalent porous media model; it's is this the correct model that we should be thinking in terms of. Is flow through fracture block a completly different type of behavior than what we assume in our models, and is any parameter value that you pick going to be really meaningful. So I kind of beg off the question by saying that conceptual model uncertainty is at least as great as parameter uncertainty and the whole issue related to scaling.

DR. LUCKEY: If I could, Ed, I'd like to amplify that a little bit more. I think that Don left the impression that our important job in site characterization is measuring things and giving them to PA, you know, where is PA going to get a particular value for a particular model. And the way I've always seen PA operate is they're going to use a random number generator to get that value anyway. And I think the much more important thing that we've got to give PA is just the concepts, you know, this concept is good, this concept is inconsistent with the data, and at least reduce their number of models.

So I think our job is not quite so much to feed PA information, but to feed PA concepts so they can put it into their kinds of models. And I think we're learning a lot about the flow system at the site, not everything we want to know, and I think it's important that we pass on those concepts, and so we are making lots of progress because we're early in the program.

DR. LANGMUIR: Rally Barnard?

DR. BARNARD: Rally Barnard, Sandia Labs.

Victor, let me try to answer this. With all due respect to Dick, we'd really love to have a lot of information, okay. Concepts only go so far. The parameter that vexes us the most in our analyses is the hydraulic conductivity parameter. And the problem there is that when you look at the water retention curve, it can be very steep, and that, in essence, is the entire non-linearity in a flow problem, and it's the thing that makes your codes work the hardest. It's the things that can give you the most divergent answers and all like that.

The kind of experience we have is that the water retention curves that we have seen and the ones that are measured generally have a fairly uniform range of core sizes, and so they fall like a complete waterfall. That may be because those are the most competent rocks, the easiest one to measure and they're all, you know, nice and tight, and that's what you get. And if you had used another rock with a wider range, you would get a more gentle slope. A more gentle slope is easier to model numerically, and we can relax a little bit in some of the numerical constraints to get the job done.

So from the customer's standpoint, now you know what the property that we like to see scaled the most is.

DR. LUCKEY: I think the point I was trying to make is I'd like to give you that information, but I could probably-- I think I could help you more if I could tell you whether the dual permeability or the dual porosity model was a better model. I think the permeability value that we give you is probably trivial compared to which model. If we could tell you which model to use, you'd probably be much better off.

DR. CHESNUT: Well, I was just going to point out that I agree with Rally basically that this permeability or conductivity or whatever you want to call it is really the crucial problem, and I think particularly difficult because we're dealing with a fractured system. We cannot take samples in the laboratory, measure permeabilities and apply them to the mountain. So we have a scale problem right from the beginning. How do we measure anything on a scale that we care about?

We could get some handles on this by measurements in the saturated zone by packer tests. We could get some handles on it by measurements in the unsaturated zone with pneumatic testing. We're still left with the problem of how this stuff is connected in three dimensions, and that to me has always been the most difficult thing to grapple with because that's what's necessary for transport calculations, not the flow calculations so much, but how do these things hook up.

And I think that you can get a handle on what the problem is by looking at the Calico Hills data that's been published for the saturated zone. There's a paper, a Los Alamos report published a couple of years ago by Colleen Lobin (phonetic). She pulled together all of the data for the Calico Hills zeolitized, vitric and pump test data, and put it in one report, and you have three log-normal populations. You have the zeolitized, that's the lowest mode, the vitric stuff has about a hundred times the value, and then you have the pump tests, which are a hundred times the vitric mode.

So the interesting thing is the sigma for all three distributions is very near the same number. It may be purely coincidence. But the point is that when you go out and measure the permeability in the well, you're getting a hundred times or more what you see in the matrix. This is characteristic of the fractured system. So that's really to me the difficult scaling problem, is how do we take this stuff, lab scale, well scale, the inter-well scale, and the site scale.

DR. LANGMUIR: We promised earlier in the meeting to give those in the audience a chance to comment or ask questions, and maybe this is an appropriate time to do that. Do we have some interest? I think we do.

MS. LEHMAN: My name is Linda Lehman. I'm representing the State of Nevada. And I would just like to make some comments about the hydrologic data needs for the ESF and also maybe by way of an example, show you some of our frustration that we have as stakeholders actually trying to influence this process and having really very little effect, in my opinion.

In about 1989, I believe, we presented the State's alternative conceptual model on ground water flow at the Nevada test site, which entailed mainly recharge through the western side of the mountain, which has been pretty much ignored in most analyses. We feel that the Solitario Canyon Fault region is important primarily because the repository location at that point is the closest to the surface. It comes within about 500 feet of the Solitario Canyon Fault there.

We later did an analysis of potential recharge, and I believe we also presented that to the Board a year or so ago, that indicated that quite high volumes of water could come through that side of the block because of the potential to runoff into Solitario Canyon.

Now, today we further heard Ed Kwickless talk about the PTn unit being sort of a buffer to any infiltration coming into the mountain. Well, one of the concerns is where are your pathways, where are the faults that can influence the flux through the repository block, and several Board members have brought up what about the Solitario, what about the western side, where is this flux.

Well, it seems pretty clear to me that almost all of the efforts in this site characterization and the ESF are directed towards the Ghost Dance or the faults to the north. It seems logical to me to see some work done on the western side of the mountain and in the areas which Ed pointed out have the highest potential for infiltration, which is where the Topopah Spring is exposed, and that is along the western side of the block.

So in my opinion, we're still lacking a lot of information regarding this flux through this unsaturated zone along the western side of the block, and unless we take some of the shaft drifts out there and actually perform hydrologic tests, not just heater tests, then we're liable to be missing a very, very important pathway.

DR. LANGMUIR: Any comments or responses from the table? Ed Kwickless?

MR. KWICKLESS: I think it's a good suggestion, and I think Linda is right that if our interpretation of the behavior of the Solitario Canyon Fault in the saturated zone is correct, and that is that there does seem to be some vertical permeability along that fault based on the temperature contours of the water table, and that low permeabilities do more to the juxtaposition of beds of contrasting permeability, but the fracture fault system itself is open, then it does seem that by virtue of its topographic position and its apparent transmissivity in a vertical direction, it's a logical place for water to infiltrate. And I think it's a reasonable suggestion and one that needs to be pursued.

MS. LEHMAN: Could I just add one more thing? Thank you, Ed.

With respect to the modelling as well that's being done by Sandia, that fault, as you know, is always used as a no-flow boundary, which Pat brought up again today. Now, there's only been one other model that I've seen that uses it in anything other than that context, and that is the Bo Bodvarsson model, but he uses the faults as dry, which has the same effect in the unsaturated zone of having a no-flow boundary condition. So what does it take to get Sandia to model what we feel are realistic conditions along these fault zones?

And also with respect to the flux rates that are used in the Sandia model, Nevada has argued for at least the last ten years that I know of to see a flux of on the order of millimeters utilized in a meaningful fashion. And we commented on the last iteration of the Sandia model and argued that the flux should be skewed a little bit more to the higher values.

In response to that in this last iteration, they divided what they call the wet climate scenario and the dry climate scenario. And in the wet climate scenario, they did use a higher infiltration flux as an average. But in the dry climate scenario, they lowered it again. And I asked them, I said, "Well, is today dry climate or wet climate?" Because we're measuring fluxes on the order of millimeters, four to ten millimeters we think is a reasonable number to use. But yet they actually lowered it such that, you know, it's included in there, but it's at such a low probability because the distribution is exponential, and the zero flux is the highest one that's being utilized, and when you get down to the millimeter type fluxes, you're way out on the tail and you have essentially a zero probability that it's ever going to happen.

So those are some of our frustrations, and we would like to somehow have a means to see that these things get accomplished, or at least looked at.

DR. HOXIE: May I make just one comment? There is a plan in the SCP to drill essentially a horizontal borehole into the unsaturated zone in Solitario Canyon to look at just exactly what Linda was talking about.

I guess a few minutes ago, I was saying I was a little skeptical about the method of accessing the Solitario Canyon Fault from the south, the north ramp extension, for example, because I wasn't really sure, I'm not sure that that's going to be the place where we really want to look at its hydrologic properties. And I think that the horizontal borehole was the place where infiltration may be taking place. I don't know where that borehole is in the schedule at the present time.

DR. LANGMUIR: Any more questions or comments from the audience? From the table?

DR. MIFFLIN: Marty Mifflin; I have one.

I'd like to ask, Ed, in your own opinion, how many more of these dry drilled boreholes do you feel would be appropriate based on the new level of understanding that's come forth in the last year or so from the UZ-16 and UZ-14 results?

DR. KWICKLESS: Well, I think what our position has always been is that you would calibrate on one hole and predict at another, and when you finally begin to converge and you demonstrated through calibration and prediction of new holes that you understood your system sufficiently, then you are probably ready to stop characterizing the site and make a determination. I'm not sure that we've drilled enough holes right now to make a meaningful calibration of anything. I think it's going to have to be an iterative procedure, and when we stop being surprised, you know, when we see exactly what we thought we were going to see, then maybe it's time to stop.

DR. MIFFLIN: Can I follow up on that? How many times do you think you'll be surprised?

DR. KWICKLESS: How many holes have we drilled? I mean, this is why I do site characterization, and I think that the experience with UZ-16 is reason enough to pursue site characterization, is that you're still finding new things, you're still developing new understanding. And when that's no longer the case, then it's time to just make a decision.

DR. LANGMUIR: Maybe that's a place to stop.

I want to thank all the presenters for their efforts on behalf of this meeting, and I've certainly appreciated and learned a lot from it, as I think the rest of the Board has, and it's been very edifying.

I'm going to call us adjourned. Thank you very much for attending.

(Whereupon, at 5:55 p.m., the meeting was adjourned.)