UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

Hyatt Regency - Denver
Anaconda Tower - Room 210A-B
1750 Welton Street
Denver, Colorado

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BOARD MEMBERS PRESENT

Don U. Deere, Chairman
Melvin W. Carter
Donald Langmuir
Ellis D. Verink
Dennis L. Price
John E. Cantlon
D. Warner North - not present

William W. Coons, Executive Director

P R O C E E D I N G S

DR. DEERE: Good morning, ladies and gentlemen. I am Don Deere, Chairman of the Nuclear Waste Technical Review Board.

Since many of us are new to you and vice versa, I would like to give some background on the Technical Review Board. The Board currently has eight members of a total of eleven. The three other appointments are actively being pursued by the Presidential Appointments Office. Since two of the three new appointees would have a strong interest in today's and tomorrow's sessions, we had hoped--obviously optimistically--that they would be aboard by now and present to hear the scientific presentations.

Now I would like each board member to introduce himself, giving his affiliation, his major area of technical and scientific expertise, and finally, the panel or panels on which he serves. If we may start at the end of the table with John?

DR. CANTLON: I'm John Cantlon, Michigan State University, Vice President for Research, Dean of the Graduate School. My area of specialty is environmental science and I'm on the environmental and health panel.

DR. PRICE: Dennis Price. I'm from Virginia Tech, Professor of Industrial Engineering and Operations Research. My area of specialization is in transportation, particularly human factors and system safety.

DR. VERINK: My name is Ellis Verink. I'm with the University of Florida, a faculty member there, and I'm a member of the panels on containers and transportation and risk and performance analysis. My field is corrosion and metallurgy.

DR. LANGMUIR: I'm Don Langmuir, Professor of Geochemistry at Colorado School of Mines, chairman of the panel on hydrogeology and geochemistry and on the risk analysis panel as well.

DR. CARTER: I'm Mel Carter from Atlanta, Georgia. I'm a consultant there. I'm also Professor Emeritus from the Georgia Institute of Technology, and I'm certainly not used to the weather we're having here in Denver this morning. I also chair the environmental and public health panel and serve as a member of the containers and transportation panel. My fields of expertise are radiation protection and waste management.

DR. PRICE: Yes, I didn't give my panel. I'm chairman of the containers and transportation panel and I'm also on the risk assessment panel; Dennis Price.

DR. DEERE: As you see, the Board has established a number of technical and internal panels consisting of two to five members each. We chose to do this for two reasons: First, because of the very large scope of the scientific and technical subjects that DOE and its contractors are, of necessity, involved in, it seemed best to match the expertise within our membership to that of principal topics being studied so that smaller meetings could be held to pursue subjects in greater depth. And secondly, because of the difficulty in bringing all eight--in the future, all eleven--of the members together for full board meetings sufficiently frequently to accomplish our task of monitoring the DOE's technical and scientific program and reporting our findings and recommendations to Congress and to the Secretary of Energy, it appeared really necessary to form the smaller and more specialized working panels.

The panels are responsible to me, as chairman of the board, and ultimately to the full board. Each panel will submit reports of its activity and findings every few months to the full board. These will be discussed for acceptance by the board and will form the basis for parts of our reports to Congress and the Secretary. The board and the panels also make use of individual consultants of national and international renown to assist it in analyzing certain specific topics. To date, six consultants have been used. Two of the six are present and they will be introduced later.

The Board was also authorized by Congress to utilize up to ten professional staff to assist it. To date, we have hired three highly qualified professional staff in technical and scientific fields, plus one on half-time loan from OTA, and we have three administrative professional staff. I would ask our Executive Director, Mr. Bill Coons, to introduce those who are present.

MR. COONS: The professional staff currently here, I'll ask them to stand as I introduce them; Mr. Russ McFarland, who is on the Structural Geology and Geoengineering Panel; Dr. Sherwood Chu, who is here in the first row, our Transportation Panel, Container and Transportation; and Dr. Jack Perry right next to him, who is on the Environment and Public Health Panel.

I would like to add that all of these gentlemen have interdisciplinary interests, so they are predominantly on one panel but help out in others, and that's, I think, to our benefit because then we have a pretty good analysis.

We have also with us today the Deputy Executive Director who we were very fortunate to obtain. He is Mr. Dennis Condie, standing in the back there. Dennis has had a long, I guess, number of years working with Congressional commissions, the establishment of commissions, Presidential boards and commissions over the last 15 years, and frankly, without his help we wouldn't be as far along as we are today. So we're very fortunate in having each one of these gentlemen with us.

DR. DEERE: Thank you, Bill. I would add, however, that the staff members that you introduced are not members of the panel. They are liaison contacts between the full board, the panels, and the administrative staff.

Now I will turn the meeting over to Dr. Don Langmuir, who is chairman of the panel on Hydrogeology and Geochemistry.

DR. LANGMUIR: Thank you, Dr. Deere.

This is the first official presentation of the Department of Energy and its contractors to the board panel on hydrogeology and geochemistry related to site characterization activities at Yucca Mountain.

I'd like at this point to introduce our two consultants on the board, and I'd like to ask them to stand as I do that; Dr. Roy Williams and Dr. Patrick Domenico. The other member of the panel on the board is Clarence Allen, and because of illness in his family he won't be able to make it perhaps until about Wednesday this week.

This is also the first official presentation of DOE in this subject area to other board members, as well as to our panel. Those involved in preparation for these presentations know that the panel provided DOE staff with an extensive list of preliminary issues and concerns related to hydrogeology and geochemistry. We did not expect all of these issues or concerns to be addressed at this first meeting. Several, including the effect of near-field temperatures on radionuclide behavior and the stability of zeolite minerals are slated for further discussion at the Waste Package

Environment and Containers Panel meeting at Lawrence Livermore in January, 1990.

The DOE and its contractors have done, I feel, an outstanding job of organizing the next day and a half of talks. Given the limited time available, the panel and DOE have agreed to emphasize some topics and give only very brief overviews of others. This was considered preferable to the alternatives of less-detailed coverage of all topics, or total neglect of some. The panel's intent will be to request future DOE briefings on subject areas of concern not covered in detail at this meeting, perhaps in the form of workshops beginning in early 1990.

Before we start today's presentations, I'd like to point out several ground rules for the next day and a half. One is that no questions of the presenters or of the Board members are allowed from the audience, although presenters themselves may call upon colleagues in the audience for expert comment and answers to Board questions related to their presentations. This is expected in that we know much of the material being discussed by individual representatives or researchers is the work of colleagues in the audience.

A second ground rule--which is, I'm afraid, going to set us back time-wise a little bit--is that Board members would like to be able to ask questions during presentations as they come up rather than afterwards. We know this may make it difficult to stay on schedule and hope that some time allowance has been built into the presentations for this purpose.

Finally, I'd like to turn the meeting over to Max Blanchard of DOE, who will introduce the presentations.

MR. BLANCHARD: Thank you, Don.

Speaking for Carl Gertz, we're pleased to be here. We hope to have an effective dialogue, one that's very candid this morning and tomorrow with you all, and we brought support staff to help answer questions, anticipating that you may get into details beyond what we've presented in the viewgraphs. Carl will be here tomorrow, approximately noon, and he's looking forward to discussing the results that you see presented here today.

As we began preparing our presentations, it became obvious that there were some things that I would have to relate to you that we adopted as organizing principles, and so what I'd like to do is to discuss these briefly with you. Like Don mentioned, presentations were developed to focus on the topics that were requested. That means there were things that were left out that a number of principal investigators or a number of the programmatic people would like to see discussed at some future time if it becomes your wish.

The presentations as they have been developed describe our current understanding about the topic and the basis for that current perception and, of course, as you mentioned, the time available determined the level of detail in which we could go. I'd like to make sure that you're all aware that the observations are preliminary interpretations. We have an ongoing program. About 36 per cent of our program is still collecting information either in the field or in a laboratory, and that the information our speakers will be discussing with you will go well beyond what's been published. And so in many areas, it's a result of their own investigations and their opinions about their data and the interpretations that go with that data, and so you need to appreciate the fact that they have not necessarily discussed this among their peers in their own organization. Some of their interpretations or conclusions that they might be discussing today won't have had the benefit of their internal organization technical review, nor will it have had a broader review within the Department or its own outside or inside technical reviewers, and so this information you will see published in one form or another as we continue to proceed in our characterization program, but in an attempt to make sure that we were presenting as candid information as possible, we felt we wanted to present our current data and our current understandings, which goes beyond that which is in Chapter 8.

Also, then, since this is new information and the people in the program office and the integrators have not had the opportunity to think about what some of these conclusions might be, or at least the interpretations, the regulatory information or the implications to compliance with the regulations have also not been evaluated, and so bear that in mind. We are not in a position at this stage to determine how that links with the demonstration of compliance.

This little pictogram, I think, illustrates the point that you made, Don. Our site characterization program outlined in Chapter 8--8.3.1, is the site investigations--is a program that responds. It's not a program that goes out and gathers for first principles. It responds in that we started with the regulations, 40 C.F.R. 191 from EPA and 10 C.F.R. 60 from NRC. From that, we focused in on performance and the design requirements. From that, we developed information needs which then requested information from the site in the classical earth science disciplines; geology, hydrology, tectonics, and so forth. And so the program that we have described in the SCP has a much larger topical area in geochemistry than that which we're talking today, or in hydrology from that which we're talking today about the unsaturated zone. So just looking at the scope of that program outlined in 8.3.1, we have some topics in geochemistry, geohydrology and rock characteristics, waste package, and some in performance assessment related to groundwater travel time. These are pieces of a bigger program on each one of these topics, and depending upon how strong your appetite is in looking at the whole picture and how that picture folds together in a performance and design standpoint, and then how that makes the next step into demonstrating how you can comply with the regulations, we'll be glad to work with you for future meetings.

The speakers this morning, we'll start with Alan Flint from the USGS, who will be discussing characterization of the infiltration in the unsaturated zone. After that, Alan, Bob Trautz and Joe Rousseau will talk about measurements that have been made in the unsaturated zone and discussing the properties as we know them now. After lunch, Paul Kaplan will be discussing the performance assessment part, fracture versus matrix flow in conceptual models. Ted Norris will augment that with some geochemistry studies in Chlorine-36, and Ed Weeks from the Survey will be talking about his observations on air flow and water flow in fractures.

Later on in the afternoon as we move into radionuclide gas releases, Rich Van Konynenburg will be discussing the gaseous isotope issue with respect to waste package containment, and then Ben Ross will be describing the preliminary status of Carbon-14 modeling effort that he has finished. Then we'll be closing with Dwight Hoxie, who will be discussing the model validation strategy in the hydrologic zone of the unsaturated zone. Then I will describe the agenda for tomorrow, tomorrow morning.

I assume that we'll make it through today. I ask that our speakers be brief and that we'll try very much to stay on time and give you the opportunity to decide whether you want to pursue questions further or continue with that particular topic.

With that, Alan, I think we're ready to start.

DR. FLINT: Okay. I'm going to talk about the infiltration program. The infiltration program is mostly that studied over Yucca Mountain itself. There are other people in the audience that deal with infiltration and recharging areas like Fortymile Wash or farther south, and if you have questions about those we can talk about that.

The objective of the infiltration program is to define the upper flux boundary conditions of Yucca Mountain under present day or simulated wetter climatic conditions. That's the overall objective.

The way I'm going to go through this talk is to discuss three different areas: the characterization of surficial materials, which is the physical and hydrologic properties of those materials using surface-based and borehole geophysics, mapping GIS system. The next will be characterial of natural infiltration, which deals with precipitation, evapotranspiration, neutron logging, geochemistry; and then finally, characterization of artificial infiltration. This outline is going to show up several times in the talk so you know where we are and where we're going to be going. If you have specific questions about these areas, you'll know where we're going to get to it. If there's something you're interested in that's not up here, it may come up, it may not, so you can ask about that.

The first thing I want to talk about is the area of interest of the program. We look at three different areas in different details. The first area is the large scale. That's the Upper Amargosa watershed. That's going to be more with the precipitation, meteorology studies. An intermediate scale is Yucca Wash and Fortymile Wash, which are--we're going to concentrate a little more on, but less than we do the large scale. And the last one, the small scale are those smallest definable watersheds that cover the repository itself. That's the area we're going to do most of the work.

This is the watershed boundaries for the Upper Amargosa watershed. Yucca Mountain is centrally located. You see the outline of the Nevada test site. Spring Mountains, Las Vegas is out in here; Death Valley and California. The outline you see of Yucca Mountain, the repository right in here, this is that same outline again of the repository. This is the smaller scale, although we have part of the borders of two other scales. This is the Yucca Wash drainage, Fortymile Wash drainage. These are the three--it says Fran Ridge, but it's really the Drill Hole wash drainage, part of Solitario Canyon and Busted Butte drainage. The repository is underlain by these three. We're trying to find definable boundaries, although we will concentrate more on the surficial materials here, because of our other studies, we're interested in what's going on out in this area. It's very important that we put the whole program together that way.

I just put up some definitions so that we could sort of have a common understanding of the way I'm looking at this. Infiltration is simply the movement of water across the air-soil interface. Percolation is the downward movement of that infiltrated water, and net infiltration is percolated water that has moved below the zone of evapotranspiration. The zero flux plane is one way people look at that.

We're going to use a water balance approach to discuss the infiltration program and looking at net infiltration, which is simply precipitation minus evaporation minus runoff, plus or minus some change in storage. We use a framework to evaluate the component parts. So first I'm going to talk a little bit about the conceptual model and how we're putting all this framework together.

Most importantly, net infiltration is spatially and temporally variable; and second, sort of a caveat, is that the water balance approach is used as a framework but it's not solved specifically for net infiltration. The errors are just too large, but we need to look at the component parts.

We have--and I'll have a map of this in a couple slides--we have the topographic settings and the hydrogeologic conditions that we're dealing with on Yucca Mountain; ridges and slopes, upland bedrock channels, alluvial channels, lower canyon walls, old channels, terraces, recent channels, and these become important to the way we deal with precipitation. What happens? What's the fate of rainfall? What's the fate of snow melt? These are all very important to that.

So the mechanisms we want to put with those settings, precip, surface runoff, infiltration, matrix flow in alluvium and rock or fracture flow in alluvium in fractured caliche layers or rock, evapotranspiration, recharge. Can we get it recharged from net infiltration? I'm not sure that we can, but we can define the upper flux boundary conditions. That's what we're trying to do. But we need to put all this together.

So I want to talk for a minute about the conceptual model in terms of a schematic. This is just a cartoon. This is not a real cross-section that we're looking at. You can see there's a rock outcrop in the middle. That's not real. That's just to give you an idea that these exist, and the purpose of this is to discuss in some detail how we're viewing the fate of rain water. What we're looking at on the graph is at the top is the ridge slope conditions that we see; shallow soils, residuum, some fractured rock. We have upland drainage channels which have a drainage network but don't have any soil covering. Now, these areas become important for rainfall that--or snow melt that can enter directly into the fractures into the rock. This is where fracture flow becomes important. Snow melt becomes important in these locations. They're higher elevations. You can have a snow melt in the winter time and you have very little evapotranspiration demand, and you can move the water fairly far. You can take the alluvial cover very thin and you can saturate that quite easily. Underneath saturated system, the water can move into these fractures.

We have upland channels getting a little thicker with alluvium. If we have enough runoff, enough snow melt, enough rainfall, we can get saturated conditions down at the base and we can move into some of the fractures there. Otherwise, we're moving through matrix flow through the alluvial--alluvium.

The margins of some of the washes become important because we get overland flow that can get on top of the alluvium. They can move through fractures at the base or they can move underneath the alluvium. We see a lot of times that snow melt will be able to penetrate some of these kinds of boreholes. What we're looking at on this side, also--on the left side--is the more welded type of rock. Again, this is just a schematic. You don't have welded on one side of a channel and non-welded on another. We have the thicker alluvium, some old channels, some terraces, and recent channels with alluvial fill or recent channels that have--are welded rock, that are exposed. These become important when you're looking at snow melt or rain water moving through this alluvial channel. To get to the saturation to move into fractures you have to have a lot of water; in this case, too much for these thicker alluvial channels.

In this situation where you have fractured rock, you can move in quite quickly. If you have runoff collecting in the system, you can move into these fractures. It's a really important mechanism, or we have--dealing with the less welded materials, we have the same kinds of conditions on Yucca Mountain, and I'll talk a little bit more about these boreholes when I get to the neutron logging section.

But some of the conceptual models we're dealing with are: What areas are most likely to take water through matrix flow? These are the alluvial channels, maybe some of the non-welded units, which one are fracture-flow controlled. Most likely, the higher elevations, more rainfall, these areas may be the most important to moving water down deep enough to get below the zone of evapotranspiration. When you get to this thick alluvium, you have a lot of storage in the surface. You can move water down, but that water can be evaporated quite easily and I show some slides that kind of explain that. We'll see this slide again, too, in a couple more places.

Well, the first thing I want to talk about is the surficial materials, how are we going to characterize the surficial materials and give you some idea of the work we're doing. Now, keep in mind these are some of our ideas that are coming out now that haven't been reviewed. We're still putting all this stuff together. Some of it is further along than other parts, but it's going to be somewhat disjointed, perhaps, in the whole talk because different parts of the program have gone along at different rates and different intensities, so we'll try to--I'll try to put everything in a perspective, again, going through this outline.

This is the surface of Yucca Mountain, a photo of an aerial photo. You see the UZ-6 drill pad up in the upper left corner, the Coyote Wash in this area in the southeast corner of the photograph. This wash ridge system is a system that we studied in some detail looking at surficial materials, looking at physical properties and hydrologic properties. This is just to the north of the Highway Ridge.

There's another small area up in here which I'll show later in the infiltrometer study, but just so you know, it's located in this same watershed, and I'll remind you that we're looking in that area. So we're studying this watershed; see a few trenches in here where we got some of our information. This is a map of that watershed. Just to give you an idea of the kind of work we're doing, we're trying to look at geomorphic properties or soil physical properties, soil developmental properties, hydrologic properties that we can base these units on. This is just an example of some of the different kind of units whether we're dealing with--well, here's a cross-section that shows a little more detail.

Up on the upland side we have residuum. We can get fairly thin soils where, again, it's very important to recharge or to net infiltration anyway, getting below some zone of evapotranspiration by getting through fractures. We have some colluvium, some creep. Some of the soil is moving slowly down. We can see evidence of that. We can define different horizons, zones where we see slides. There are quite a few slides on Yucca Mountain where we have soils that have built up. In some cases, they've blocked the channels and moved some of the channels around. The alluvial fill in the floor, and again we see this colluvium horizon on the side, and we do see some caliche development in here, but we're just trying to come up with some ideas of how this system is developed that we can identify certain things.

This next slide is a geostatistical analysis of one parameter that we measured in this watershed, and that's sand content. We're trying to use classical statistics and geostatistics to define this watershed system. In this case, we're looking at lag distances of about--and these are in thousand feet increments--so if we take the a priori variance as a seal in this case, we're looking at a little over a thousand feet in correlation length. It tells us something about how we're going to have to sample this site. It tells us what the variability is. Is sand important? Is sand something you can relate, like others have, to infiltration, to water storage, to bulk density? This tells us how well we can estimate sand where we don't measure it, how close-spaced the correlation is between measurement points. We can use that and help design our sampling program.

Another one for clay. Maybe a little less of a distance there, maybe 500 feet, and a fairly good seal at the a priori variance. Dry bulk density, we don't see a range in this case. This is the variability of bulk density. If we wanted to know at an unmeasured point what the bulk density is, our best guess would be just the a priori variance and the mean of all the samples. So if you want to define bulk density, if that becomes a very important parameter, you're going to have to sample it at much closer intervals if you want to look at that range of the variogram. So it helps us out in our sampling scheme.

Now, gravimetric water content, in this case we're looking at two different scales. The red one, the squares are at hundred-foot increments, and two hundred foot for the little plus signs. We see a fairly good spatial correlation. Is it because of the physical properties? Maybe it's the sand/clay relationships. Or is it because of the distribution of rainfall? Maybe the distribution of rainfall is spatially correlated at this fine scale, and that's why the moisture distribution is correlated. We look at--this is a simple direct variogram. If we look at a cross-variogram--this is gravimetric water content with elevation--we find good spatial structure. So there is a relationship, spatial relationship between moisture--as you would guess--and between moisture and elevation. Again, it may be due to the increase in elevation controlling the precipitation; therefore, controlling the soil moisture.

Well, this is a summary of some of the properties we've measured, just to give you an idea that we have gotten some information out and you can look through this in more detail later if you wish, but we're seeing what you would expect in terms of means, fine soil, 1.24; coefficient of variation, fairly low values as has been reported in the literature for density. The high values are things like cobbles larger than 75 mm rock fragments at the surface, pretty high variability. You can see the range, 1 per cent up to 46 per cent.

DR. DOMENICO: Alan, excuse me.

DR. FLINT: Yes.

DR. DOMENICO: Roughly, what percentage of Yucca Mountain would be considered unconsolidated material like you have here and what percentage solid rock at the surface?

DR. FLINT: That's something that we're working on now. We haven't finished that. If I were to shoot from the hip, I'd say 80 per cent is covered by surficial materials; 80 to 85 is--

DR. DOMENICO: Where these are important?

DR. FLINT: Where these are important, where you have no direct exposure of the bedrock, or if you do have direct exposure, you have surficial materials nearby enough that it becomes important. And I have a diagram later that shows where we have some bedrock outcrops and some soil materials, and how we have to put the two together in an analysis like this, and I'll show that in the artificial infiltration study. If anyone out there has a better guess, I'd love to hear it. I'm just sort of guessing at about 80 per cent.

Now we're going to look at the natural infiltration. Again, I didn't present much information on this, but we do use a lot of surface borehole geophysics, do a lot of GIS mapping, trying to put all this stuff together, but that's sort of where we are right now. A lot of the new stuff is prototype and we haven't finished the work enough that I could present.

In natural infiltration, one of the most important things that we have to deal with is precipitation, so we're going to do a little more thorough analysis on precipitation at the outset and we'll catch the rest of these in a little more detail later, but let's look at precip for a moment.

This is the southern Nevada area, Las Vegas down in the lower right corner, the Nevada test site boundary is outlined, and this is the Upper Amargosa River watershed with Yucca Mountain centrally located. These are historical records of rainfall. We went to the literature and got out some information published by DRI and National Weather Service. These rain stations that are on here have at least eight years of record. Not all eight years were continuous with each other. Some were started and stopped. Others were started halfway through that time period, but we did get eight years of record. We are assuming that eight years was enough to come up with an analysis of this data.

So once we take those records, we went through and calculated an experimental semivariogram and then fit a model to that. We see a range of about forty miles for precipitation in our variogram. We had a total of 42 stations, again with eight years of record. Once we have this analysis--and we had to use the log transformation of precip--it's a log normal variable--we multiplied it by a thousand to get rid of some numeric errors in our modeling efforts--we can then calculate rainfall. This is the rainfall distribution in southern Nevada; low in Death Valley. We see higher up in the Spring Mountains, but we just have an estimate. This is a direct variogram. There's a mountain range--

DR. DOMENICO: What are the units?

DR. FLINT: These are in millimeters per year, I'm sorry; millimeters per year. There's a mountain range over here which you don't see when you use the simple variogram, but I'll--this is the watershed, the Upper Amargosa watershed and the variances for that. Now, the variances is very important. That's one of the major advantages of geostatistics over other kinds of interpolation schemes.

You can see the high variability of the south end of the watershed and to the north end of the watershed. Those are indications that we would need more information at that point, but we don't want to use that simple technique of geostatistics. We want to go on to something a little more complicated. This is an elevation direct semivariogram. There is a spatial relationship with elevation, as you would guess, and we see a range of--and this range that we're going to deal with, about 140,000.

DR. DOMENICO: Is that variogram based on kriged data or actual stations?

DR. FLINT: Those are--okay, this variogram is based on 1,551 grid points and the 42 precip stations. Those were digitized off the USGS topo maps.

DR. DOMENICO: So it was not the kriged values that--

DR. FLINT: It was 10,000--no, these were not. These were real data points taken off a map at 10,000 foot spacings to come up with the spatial correlation of elevation. We put that together with a cross-variogram, elevation and rainfall, and then we can take that information and we can use our kriging, co-kriging technique with elevation and rain together to come up with a little better map. This is the old kriging map on the right. Again, this mountain range does not show up.

When we add the co-kriging and when we add the detail of elevation--because elevation is correlated with rainfall--and when we add that detail, we get a little better estimate of what the rainfall distribution is. You can see the mountain range starts to show up. The Spring Mountains show up quite well, and these high rainfall zones that we see also correlate quite well with the vegetation maps, so we feel pretty comfortable about this analysis.

This, again, is just that co-kriged precip map of the Upper Amargosa watershed and the co-kriged variances. Again we see high variability to the south, maybe somewhat to the west and to the north. We're using an elevation grid that goes beyond the watershed so that we don't have to extrapolate the boundaries, get rid of some of the boundary conditions. But this helps us out in our estimating of rainfall. We're really looking at a much larger area in support of some of the other studies, as well as infiltration. We can look in a little finer detail at Yucca Mountain itself.

In this case, we go back to kriging for just a moment. This is the Drill Hole Wash watershed, UZ-1, UZ-6 in the south and the boundary of the repository. So a kriged estimate doesn't account for the elevation. It only accounts for the distribution of rain, and the distribution, since it was controlled by elevation, you see somewhat of an increase in precip as we get to the higher elevations.

DR. DOMENICO: How many points did you squeeze on that? How many gauges do you have on Yucca Mountain?

DR. FLINT: These are from the analysis using the southern Nevada, or the Upper Amargosa watershed.

DR. DOMENICO: But how many gauges do you have on Yucca Mountain proper?

DR. FLINT: Okay, on Yucca Mountain right now, we have--the USGS has six rain stations. We're putting out a network of more rain gauges, up to 30 is our estimate right now. We're using geostatistical techniques to try to site some of those rain gauges. SAIC, in their program, has two stations. I think they're adding some more, but we collect the data of rainfall that we're--eventually what we want to do is take this information and calculate experimental variograms and elevation on the--specifically on Yucca Mountain. We don't have enough information to do that. We have done a few storm variograms, and it looks real hopeful, but we take the kriged information. We've constructed our own digital elevation model for the site, which you have here. We can take the correlation between rainfall and elevation and make a co-kriged map of average annual precip. We can make an individual map of a storm, a month, a season. Unfortunately, this is where we are right now. We have not done the co-kriged estimates of rainfall, but that's something we're working on. The information I've described here is going to come out--well, it's going through--going to go through survey review starting next week and it'll be two papers in the _Journal of Applied Meteorology_, and then we'll go on to more specific details for the watershed right over the repository.

DR. DOMENICO: Why are you so much interested in recharge over that total area, as opposed to recharge in, say, Yucca Mountain proper? Why is that important?

DR. FLINT: Well, this is all of Yucca Mountain. Do you mean just over the repository itself?

DR. DOMENICO: No, within the total watershed where the studies are being done. Is recharge an important variable, outside of Yucca Mountain?

DR. FLINT: That's something--it is important, and some of the ideas are that the major recharge of the saturated zone has occurred in areas, say, south of Yucca Mountain in the more gravelly materials in Fortymile Wash, but we're not studying recharge in this particular discussion. What we're trying to look at is the distribution of rainfall. We're trying to get some boundary conditions. We looked at a larger scale because we had the information and we wanted to see where we had high precipitation/low precipitation regions, and then we're moving specifically to these watersheds even though some of the interest may be just over this area, because we want to calculate the total rainfall in this watershed. If we're going to have flumes measuring runoff, an important part of our water balance is the runoff, and we need to define--if you were just to look at, say, rain over this area, you would have to have a lot of information to correct for, say, if you measured runoff down here, how much of that came from an area outside of the repository. So we're trying to find manageable units that we can deal with.

On the small scale, this is the area the infiltration program is specifically interested in. The infiltration program is not interested in that larger scale more than to try to characterize it in terms of the development of rain storms, the location of rain storms, the rain shadow effects from the ridges from the Funeral Mountains to the west and down to Death Valley. We're trying to understand how Yucca Mountain itself is seen on a larger regional scale in terms of precipitation. I'm sort of integrating part of the regional meteorology program into this because they're an integral part. Rainfall is very important to characterize.

Now we're going to look at evapotranspiration that we've tried to define precip, and again, you know, I'm saying with the precip we want to make sure we can get it down to a storm-by-storm event and estimate what the distribution of rain is and how much we get in a particular watershed.

Evapotranspiration is what happens to most of the precipitation water. What doesn't get evaporated or runoff again becomes--may become net infiltration. We're using a lot of techniques. This is a Bowen Ratio Station. It measures temperature and humidity at two heights and uses some assumptions about the resistance to vapor flux and to heat flux in this system, and you can calculate evapotranspiration. We also measure wind speed, wind direction, temperature, humidities.

Another technique we use is Eddy Correlation. We have a sonic anemometer and a krypton hydrometer to measure directly evapotranspiration. The Bowen Ratio has to use the energy balance approach to calculate evaporation. This technique uses direct measurements.

We have just a Class A evaporation pan that we use to get a potential evapotranspiration, so we have some measurement techniques we use in the field. We also use modeling techniques, Priestly Taylor, Pendum Montif (phonetic). We try to collect all the information and look at all the variety of techniques that we have to make our best estimate from different directions. We want to be able to confirm some of the information that we get by other techniques. Bowen Ratio versus Eddy Correlation are two good techniques to compare against each other. Fast response pscyhrometers is another technique using the flux variance method. Evap pans may give us an upper limit. We can just--I have some data on this that just gives us potential evapotranspiration using a .7 pan coefficient for the site. We don't know if that's right or not, but it's just our first guess, and we're looking at some reasonable numbers; 4 mm a day evapotranspiration potential. It's going to be less than that because of water-limited systems, and increasing up as we get toward the summer, up fairly high over 16 mm a day. This is about as high as we've seen. So we have some estimates of evaporation.

DR. DOMENICO: How many of those are scheduled for Yucca Mountain?

DR. FLINT: Right now, just two.

DR. DOMENICO: You have two on Yucca now, or none on Yucca now?

DR. FLINT: We have none on Yucca Mountain, no. We're working on getting authorization to put these on Yucca

Mountain. This sits in Jackass Flats. It's about 15 miles from Yucca Mountain, but it is an important area to study. It's close enough to the site that we can make some inferences. We have a large fetch for the site, which gives us good confidence in our measurements.

The energy balance approach is one technique we use in calculating evapotranspiration. Net radiation minus ground heat minus latent heat minus sensible heat is equal to zero. This latent heat is evapotranspiration component, so if we can solve the component parts, then we can get at latent heat. The Bowen Ratio uses a ratio of sensible heat, the latent heat. The Eddy Correlation technique measures these directly; the flux variance technique, the same thing.

The radiation balance, this is wrong. I should have changed this, but incoming shortwave plus incoming longwave minus outgoing shortwave and outgoing longwave is equal to net radiation. That's the energy available to heat the soil, to heat the air, or to evaporate water. Those are the three places it can go. So we're going to try to do radiation balance and get this information. Again, when we do this analysis, we start component by component and try to understand and describe each one in detail, make sure that the instruments work for Yucca Mountain, make sure we understand the theory behind the instruments, and make sure we can get the measurements in. And then when we get one detail worked out, we'll go on to the next. We're going at a detail-by-detail level.

This is some instruments. It's not in your package, but I wanted to put this in. I just wanted to show you some of the detail that we're looking at in radiation, because that is the energy that's going to be available to run the system. We have measurements of incoming shortwave or outgoing shortwave radiation, incoming longwave and outgoing longwave radiation. We use those--those are secondary standards to the World Radiometric Reference. We have instruments that measure incoming long and shortwave and outgoing long and shortwave. You can calculate net radiation from those. We have instruments that measure just outgoing longwave--in this case, two net radiometers, a single dome, a double dome. We're using this as a way to test the physics of the instruments, how they work on Yucca Mountain, are there corrections we need to make to take these measurements. This is an important component and we're doing a lot of investigation trying to understand this system.

I'm going to talk about now, I'm just going to talk about incoming shortwave radiation and how we're trying to analyze that. These are a series--right now there are five weather stations on Yucca Mountain that the USGS is operating that collect air temperature and relative humidity, solar radiation, wind speed, wind direction and precipitation, and one station collects barometric pressure.

We can take this radiation data and use it to calibrate a model specifically for Yucca Mountain. That's what we have here. Solar radiation, watts per square meter over a one-day period. This is how we calibrate the model for the site. At first we fit the clear sky radiation model to our best clear sky data. Then we account for blocking ridges. Blocking ridges take away direct solar beam. Blocking ridges also take away a circumsolar diffuse radiation that's following the sun around in the sky; important when you start dealing in mountainous terrain, and so we can correct that and come up with a pretty good match. We can calculate solar radiation, model it on sloping soils, soils that are blocked by ridges and down deep in the washes.

We then take this model, our digital elevation model for the whole Upper Amargosa watershed to try to look at the distribution of radiation. We want to overlay radiation, net radiation, evapotranspiration with precipitation and try to look and see what the distributions look like. If you measure, let's say, 100 mm in one location and measure 150 mm of evaporation, you need to know, are you getting 50 mm from the saturated zone, or are you getting a transfer of 50 mm of rain from a different area, and so you start looking upgradient from that area and maybe you find 150 mm of rain and only 100 mm of evaporation. In that case, you know where

50 mm might have come from. So we're trying to put that together and we're trying to get our model working.

This is just two maps of solar radiation in megajoules per square meter per day for the watershed, the Upper Amargosa River watershed; up to about maybe 32 megajoules per day in the summer, and about 11 megajoules per day, per square meter per day in the winter. Notice the distribution is much more detailed in the winter. The low solar angles become very important in the distribution of radiation in this mountainous terrain in the wintertime. In the summertime, the sun more directly overhead, it doesn't seem to be as critical.

We look at experimental semivariograms of the site. Again, when you're looking at a model, you can model radiation at any location. You don't need to do a geostatistical analysis and interpolate. You can model it for any point you want. But how long does it take to run your model? That's a constraint that we're going to run into. It takes, in this case for that watershed, it takes a couple of hours at 10,000 foot spacing. Well, we look at the relationship, spatial relationship and we find, are we willing to live with this variability at an unmeasured location? If we are, we can set our sampling at, say, the range of this experimental variogram of 70,000 feet. Rather than using 10,000 foot spacing, we use 70,000 foot spacing. We can run the model in a couple minutes rather than a couple hours. So this is where we're trying to take a model, do analysis of the spatial variability of the output, and this spatial variability depends on the elevation of the site, the slope, the aspect, blocking ridges. So we're really looking at the overall components of the site rather than doing a variability analysis on slope, aspect, elevation. We put the component that we want, solar radiation, and do the analysis on that. That integrates all the other systems. So we're trying to get a driving mechanism, trying to understand it in some detail and how it's distributed in the watershed, and we do this for the smaller watersheds and we're working on that right now.

Okay. So we've taken precip and looked at its distribution. We've taken evapotranspiration. We've had some measurements of evapotranspiration. Some models, we're looking at their distributions. We're going to try to overlay this together. Now we want to look at another component part, something to deal more with the net infiltration measurement directly or with storage of soil moisture, what happens to the water that's not evaporated or during a transient period, when precip occurs before evaporation occurs, so we'll look at the neutron logging and then we'll get on to the geochemistry data.

This is Yucca Mountain, a site repository boundary again. These are the locations of our neutron monitoring holes. There are 74 holes out there that range from 15 feet to about 150 feet in depth. It's in a five-inch steel casing, but we have ways we have been able to calibrate, rough calibrations right now, but we are fairly comfortable that we're collecting good data in terms of neutron count. When we get a better calibration equation, we go back and do some of these analyses again, but right now we think we're pretty close.

I'm going to concentrate later on in the geochemistry section on Pagany Wash, which is up in this location, one of our best instrumented washes on Yucca Mountain. We have a few more locations similar and when we get the next 30 or so holes we plan to drill, we'll have a few more watersheds instrumented with the neutron holes to this degree.

Again, this is that schematic we saw earlier, and what I wanted to show was where we have measurement locations. These holes on the slide, the boreholes represent where we have at least one or more measurements in that case. What's really important to point out is that the less-welded, bedded, non-welded or partially welded parts of Yucca Mountain, we do not have any neutron holes in the ridge slope positions, the upland drainage channels that are--or the upland small alluvial channels or even at the margins. This is an area where we're missing information. We hope to go back and put in some holes in these kinds of locations because this non-welded unit becomes quite important. We do have some measurements in a channel that is exposed non-welded, and that becomes real important in some of the discussions that we'll have in a little bit in geochemistry.

This is a photo that's not in your package. I wanted to add it because I have a graph--the next graph you see in your package is--so you might want to look at that now, is the next graph. We're looking at two holes. The graph on the left in your package is N14. It sits up on a terrace. About 10 or 15 feet to the right of the slide is N13. It's in an active channel. That's this slide. This is a fairly gravelly material, much more gravelly here than it is as you go further up the wash, as best we can tell right now. So you have the terrace, the active channel, and we had a major runoff event, rainfall runoff event, and we're looking at the two different data sets in time. This is the depth of the hole with water content.

You can see in the--this is N14--we had a lot of flux at the surface, increased in moisture content from a rainstorm event, but it decreased again due to evaporation and it didn't go very far, maybe a meter. We saw that, and then we don't see a pulse any farther than that. N13, in a very gravelly, active channel, if you were to go through and follow these carefully, you'd find that there was a pulse of water that moved all the way through the system that reached to the bedrock. It took about nine months to get there. N13 is the only hole where we have this kind of detail of moisture flux getting to depths of 14 meters. It's in an active channel, very gravelly. This becomes real important. If we're dealing in these gravels, we can move moisture through them and we can move it fairly fast in the drainage system. In most of the other boreholes that are in active channels, we haven't seen this kind of transport.

This is out at the--near the bottom of the wash, so we're looking at, from our conceptual model perspective--the high up part of the wash become very important, fractured rock, thin soils, we can move the water in very quickly. The mid-part of the slope, we have finer materials, good storage, less drainage, restricted drainage in some cases, we can evaporate that water. We get out to the bottom of the channels, lots of gravels, we can move the water through fairly quickly again, so we have upland areas, important to recharge, midslope and the washes, maybe not so important, and then the bottoms of these washes out in the flat areas, alluvial fans become important again in getting water through the system fairly quickly.

DR. CANTLON: You haven't mentioned the drawing up of water by plants, free edifidic use of water in these washes with the deep-rooted shrubs and so on.

DR. FLINT: I have a slide that comes in the geochemistry section that sort of fits in with that, but there is a lot of evapotranspiration. That's what's going on here. Remember from that slide, you can see a lot of plants around there. There's soil surface evaporation. There's plant root evaporation, and a lot of our evapotranspiration models use soil resistance, plant resistance, plant root resistance to calculate evapotranspiration, so the plants are important.

MR. WILLIAMS: Alan, is 1984 the last time you obtained such data?

DR. FLINT: We have, from 1985--or I guess it is 1984. '84 and '85 are the only two runoff events where we have any data at all. After 1985, we have not had a runoff event and I show some slides that show what's happening to the Yucca Mountain because of the decrease in the rainfall and because of the--well, it's about a three-year drought now we're facing in southern Nevada.

This is Neutron Hole N7. It's in the middle of Pagany Wash, and you'll see that again later in another discussion in the geochemistry section. This is volumetric water content in days since 1984, so this is that storm event in 1984 that we saw in that previous slide, and you can see we're looking at the top meter, the top five meters, or the average of the entire borehole for this one hole, N7, and this is representative of all the other holes we've seen.

So you saw this pulse of water during that storm, and then we see the system dries out, we get some more precipitation in the wintertime. The system dries out a little drier than it was the year before, wets up again, drier, wets up, and drier yet. Right now what we're looking at is the driest we've seen Yucca Mountain since we've been monitoring it. The top five meters, the same thing. Of course, there's a little more moisture down at five meters, and then all the way down to the bottom of the borehole. What we're seeing is a trend of drying.

When we put the boreholes in we were looking at about 11 per cent moisture. Now, in the surface, we're looking at 2 or 3 per cent; very, very dry, and this is the longest dry period we've had. It's important--it may be very important, if we can, to get out and drill some of these neutron holes now while we have an opportunity to get the lower limits of the system, because how dry it is now may be very important to what's going to happen if we have a two-inch rain or a three-inch rain at one time, and in a wetter system. But anyway, we're just trying to give you an idea that Yucca Mountain's been drying out because of this extended drought that we're dealing with.

This is a map of water content of the top meter of alluvium or alluvium in rock, depending on whatever the hole is. This is for all 74 holes. In general, we're looking at about 8 per cent, 6 to 8 per cent moisture over the repository boundary, quite a bit higher up in Pagany Wash, and I put that in a three-dimensional graph to give you a little better idea with the repository boundary as draped over the three dimensions.

It's important to point out this is, right in this location, is that N13/N14. This is Pagany Wash moving up to the north, and the highest point is Neutron Hole N10. It's in exposed bedded tuff, quite a bit higher moisture content in the surface, the upper one meter to the north. Is that because there's more exposure of the non-welded units up there, or is it because of increased rainfall? That's something I don't know yet. This is data we put together about two weeks ago so we haven't spent a lot of time with it yet.

You can see that the lowest moisture counts are over the repository. Those are also the shallowest soils. This is the top meter of bedrock. Now, one of the arguments was that the washes are the most important component of recharge in this system. If that's the case, then you would expect--or I would expect, anyway--to see increased moisture content in the bedrock underneath the alluvial channel. Our conceptual model says that maybe the upland areas are the most important, or the very deep areas where you have lots of gravels, but not necessarily the alluvial channels. So we did analysis, looked under the alluvium at the bedrock. We found--and I'll show you the three-dimensional view and then tell you what the answer is, because it really isn't apparent from here.

We do not see an increase in moisture underneath the alluvial channel, although I do have some data. That's what we got from this information. Again, up to the north we have an increase in moisture content in the top meter of bedrock. I'm not sure if it's due to the increased exposure because of the non-welded units that can hold the moisture, or because of the increased rainfall, but we do see increased rainfall to the north of the site.

This is a summary slide that we've just put together. We haven't done a lot of analysis, but we do have a few numbers for you on what's happening at the top meter of bedrock, and again, it's a test of our conceptual model. In a wash or in a sideslope, if the washes are the most important part of recharge, do we see more moisture underneath the washes? We don't. We're looking at 8 per cent, 9 per cent. The sideslopes under alluvium are about the same as under the washes.

If the bedrock is exposed at the surface, pretty high moisture content. When the bedrocks are exposed at the surface, they're also up in the washes, in the narrower parts of the channel. Maybe they're protected more from evapotranspiration. Sideslopes, about 7-8 per cent moisture content. So this becomes a very important area, the exposed surface, and you can see that from that other slide.

DR. DOMENICO: Is this--of course, you've had the three-year drought. Are these in the last year, or are we looking at the three-year average?

DR. FLINT: Right. These were data from June and July of this year.

DR. DOMENICO: Of this year?

DR. FLINT: Of this year. There's a fairly large data set and I didn't want to take too much time going through the whole thing, so I just put together one set of information.

Let's look at a wash specifically in a channel itself, and you can see some more detail in that, to distinguish between the less welded and non-welded in particular. Under the alluvium, less welded, more welded, it didn't really make any difference. They were all about the same moisture content, 9 per cent. If it's exposed, the more welded, only 10 per cent; the non-welded is 20 per cent. I think that this would be expected. It fits with our conceptual model of infiltration, that the more welded fractured rock, the water's going to go through the fractures and move into the system fairly deeply. Fractured water flow is something we cannot measure with a neutron moisture meter. There's not enough volume and you have to be there--if there were, you have to be there right at the time of the movement.

But we see an increase in moisture content, most likely because of the rock's ability to take in the moisture, the matrix flow, slow movement of water. We don't lose it through fractures. And again, it may be high because of evaporation, and I'll show you in the geochemistry data some reasons that that could be the case.

DR. DOMENICO: Is there much experience with neutron logging in bedrock?

DR. FLINT: We have five years now, and we've done some experiments in the laboratory and feel pretty comfortable with it. There has been some. Scott Tyler DRI did some work in the deep boreholes, or in the neutron holes, and there have been some models run by Oak Ridge National Labs which have agreed with our results, so we feel pretty comfortable with it, although the calibration is most critical in this case, and that's something that we have a request in from DOE to drill two calibration holes to do a very in-depth study of different materials, and when we get to that we'll have a much better understanding of our ability to make these measurements. But we think we have a rough curve--a good curve, but it's still a rough curve. But a lot of our analysis has to deal more with changes in moisture content, and with an approximately good curve, the changes in moisture content are easy to do because you're measuring the same material, just at different times.

This is the relationship that we've come up with for volumetric water content versus the thickness of the alluvium in the channels. The thicker the channels, the lower the water content of the top meter of bedrock. It sort of makes sense. The error bars, 95 per cent error bars, they would go away if we could get rid of this one point. Unfortunately, this consists of only two data points and we just don't have enough information. That's an area where we want to expand our work, is to try to understand what's happening at these intermediate ranges.

The zero flux plane, the zone where which water gets below that, it becomes net infiltration; above that, it's still subject to evapotranspiration, may be somewhere in this range, four to eight meters. We have plant roots down at, in some cases, eight to ten meters, and it may be below that zone so this is an important area to look at in a little more detail, but this is rough. This is our first shot at trying to put something together on the influence of the alluvial channels on bedrock in trying to test our conceptual model.

Okay, now I want to talk a little bit about what we've learned in geochemistry and in one case, it's sort of a case study from Pagany Wash, that wash up to the north that had those high moisture contents in the bedrock. We're going to look at tritium. There's some oxygen-18, deuterium, looking at whether it's snow melt or a rain melt, and some

Carbon-14 data. The tritium data is some stuff that we have in my program, some of Al Yang's data, Carbon-14 from Al Yang and others.

This is an area that I want to talk about. This is Pagany Wash. This is a cross-section that we're going to look at of boreholes. This is a drill pad for UZ5. UZ4 is in the center of the wash. Down at the mouth of this wash was N14 and N13, the two that I showed earlier. Up in here is Hole N1 and N10. Up in this channel at the very top is that real wet spot. I added some extra photographs that aren't in your handout just to give a better idea of what this looks like. We'll look from this side, and from this side, and then finally from down in the wash.

This is looking down on--this is an instrument trailer for UZ4, which is located in the center of the wash, UZ5, and then our ring--our string of neutron holes across the wash. Looking up the wash now, again, this is a trailer. You can see UZ4 and you can see some of the neutron holes across here. This is the alluvial channel where we have more or less bedrock that's covered by a very thin veneer of soil, and then we start to get thicker soil materials and you can see how that sort of fits that cartoon that I showed earlier.

And then in cross-section, looking up the channel you can see some gravels in the channel itself, UZ 4 again, the trailer, the UZ5 drill pad, and then we have neutron logs across this wash. We've gone out here and run surface seismic to get depth of bedrock over the whole channel. We're running ground-penetrating radar--hopefully, they're running it right now out in this area--trying to get some information. We're using our prototype techniques in testing this methodology out, and putting that together with our seismic work.

This is distribution, a schematic just for your reference to show you where the holes are from N2--that's an important hole. This is one you've probably heard about. This is one where we see water show up a week or so after a major rain or a snow melt event. It's all in welded rock, fractured rock, and in the last storm we had we got five foot of water in there, and I'll show you a diagram of that a little bit later, and then the rest of the boreholes across the wash; the active channel, and that instrument shelter, and then the road for access.

DR. DOMENICO: Those are all presumed unsaturated zone?

DR. FLINT: Those are all unsaturated, right.

DR. DOMENICO: Unsaturated, but you're still getting five feet of water in that hole?

DR. FLINT: Right. It's transient water, and it--I'll try to explain that a little bit later on what we think is happening at that hole.

DR. DOMENICO: Is this where you got your water samples for tritium?

DR. FLINT: No. There have been water samples collected on here. The water samples for tritium, which I'll show you on the next slide, came from core that was collected when the holes were first drilled. There has been tritium analysis on the N2 water and I'll talk about that when I show N2.

This is a cross-section of that channel. This is the borehole. This is N2. You can see its topographic position is right at where you have the alluvium starting in the channel, and then the holes going across UZ4, located in the center of the channel. This was the moisture content collected on a Friday in 1984. That's what the profile looked like. On Saturday, they had a little sprinkling, not much rain. On Sunday they had a fairly good rainstorm, maybe one or two inches of rain that was estimated, some indirect measurements of runoff. They think that there was probably runoff in the channel for maybe an hour, and then on Monday we came back out and logged all the holes again.

The difference between the log that we got on Monday after the runoff event, and the log that we got on Friday are represented in this second graph. You can see an increase in moisture content in a layer underneath the active channel. This is an old channel to the left of the graph that shows some increase in moisture from flow down the side and flow coming from upgradient. But we saw in 24 hours flux of water about, maybe 5 meters in 24 hours. That's a fairly good rate, with some high-water contents, maybe 10 or 12 per cent. On--24 hours later, on Tuesday, they logged the holes again and they saw--we calculated a new moisture profile between this and the Friday log and found that we had a decrease in moisture. We had very little movement further down. We couldn't detect any movement, really, and we start to see a loss of water, most likely due to evaporation.

Again, in this case, the water moved down quickly and then it evaporated, we think. Down the wash further on, where N13 was, the water moved down, and because it was a gravelly material, moved fairly quick.

This is some neutron data and some tritium data, tritium data collected from UZ4 and neutron data collected from N6 and N7 on either side of UZ4; depth of bedrock. This may be a little confusing. This is not a moisture profile. This is the highest water content we've ever recorded, minus the smallest water content we've ever recorded. What that represents is a change in moisture content. What we were trying to do was put five years of record and depths all together in one graph. This represents where we've seen changes in moisture content. If we don't see changes in a location, then we assume, or might assume that there haven't been changes there, or if we see minimal changes.

So what we're looking at in the first, say, two or three meters of bedrock, we see up to a 10 per cent change or, in some cases, quite high as you would expect from surface. These data points are not necessarily related in time. The high water content here might be at a different time than the high water content here, as you might expect, and it's hard to represent the data unless we had a series of graphs, so we put it in this form. What we notice now is that there is a lot of flux that goes on in the top, say, four meters, and this is pretty consistent in a lot of the boreholes. The top two to four meters is the most important part for flux, or changes in water content.

Below that, we see fairly constant, a lot of noise. From our analysis of this data, we believe that somewhere around two to three per cent change in moisture content is probably an unreal change in moisture content. There may be nothing happening there. Due to random decay, neutrons, spurious counts at different times, the likelihood of having one value show up at any one time becomes fairly likely, and the three per cent value is an indication that there may not be a change in moisture content. There may be some change. It may be small. It's probably smaller than two per cent, or one per cent.

Overlay the tritium data now from UZ4, the tritium data collected before the boreholes--before the storm event when the UZ4 was drilled, we see tritium units over--we see modern water, over 10 tritium units, down to the same zone where we saw that flux from the storm, and then the tritium drops off to below ten units. This is pre-'52 water. It's consistent with what our thinking is, that this water moved down quickly down to this level, and then evaporated away, and that's why we may be seeing this reduction in tritium units, why we don't see a large pulse from the '59 or 1963. So this system--this seems to be fairly consistent with that thinking, that water flux moves down and then evaporates away.

One thing that I wanted to note here is that when you make calculations in the alluvium, particularly in tritium, you can't go down and say, "Well, this is a lower limit. It went five meters in 30 years," because we saw from that storm that it went that distance in 24 hours. So with the alluvium you have some question here, but--in making that calculation, so we just simply say we see this peak at this point right now and we can't calculate the travel time from that.

This is N8. It's not in your package. It's just another example in the wash; the same system. We see an increase in the water content, or a change in water content down to about six meters. We also see the tritium drop off. So we're down below, 10 tritium units at about seven meters; again, fairly low values, a little less than 30 units. Probably modern or new water mixing and evaporating and diluting this down.

One peak, which is also consistent with our conceptual model that water can move down under the alluvial channels, so we may not be seeing any water flow through this system in the last, say, five years, but we do see a pulse of water. If we had a tritium level, that tritium level would probably go pretty high, or it might go fairly high. Actually, I'm not sure now because, thinking about it, tritium levels, this water is probably current rainfall and I'm not sure what the tritium levels are right now; I imagine fairly low up there. But this is consistent--I have a diagram. This is the exposure of caliche. This wash was covered with alluvium and, I think it was in '84 they had a washout. The alluvium came up a little bit higher than this. This was all washed out, and you can see the caliche development underneath what was the old alluvium at the time, just an indication that there is a lot of water flow into there and there is some in some fractures, and you can see that.

This is a photograph not in your handout, but this is Hole N1 in a wash, upland wash; tritium units indicating all post-1952 water, quite a bit of movement, changes in water content up to eight per cent, an area that's low again. Maybe we don't see a change here, but then we see these pulse right at the contact, so we do see water moving down under the channel. The water can't--may not be moving through the channel. It may be moving under the channel. Some high water contents at the base. This is in a non-welded unit. We're not sure what's happening right at this point, at least at this time.

I want to show N10. This is at the high wash, high part of the wash. This is a non-welded, exposed--there's a very thin veneer, maybe a quarter of an inch, a half an inch there of material, well-protected, non-welded unit protected from radiation. In this system we see the highest water content and we look at some of the highest tritium levels that we've seen in any borehole. Again, looking at max minus minimum water content change, activity not very low, not more than about a meter, as you would expect. We don't see a lot of flux in that system. We do see a couple pulses. Maybe there are some fractures in here. We're not sure yet, but an interesting thing is these high tritium levels.

In this case, if we don't have a lot of mixing and evaporation of water--because it's only happening in the near surface--water from the 1957 to '59 or 1963, the high tritium level waters could quite easily get down into this zone and then continue on downward. So this may be a fairly good indication. We don't know below this what the tritium levels are. Is this a peak or not a peak? I don't know, but this could be simple movement down through matrix flow. This comes out at a conductivity, if you were to just estimate it, of 10^{-6}, 10^{-7} cm per second, and for this rock, saturated conductivities range around 10^{-4}, so it's not inconsistent.

DR. DOMENICO: This is rock at--

DR. FLINT: This is rock. This is all non-welded. This is--

DR. DOMENICO: You squeezed--again, you squeezed the water out of--

DR. FLINT: This water was taken out using a vacuum distillation technique from the material, so we--if we don't have evaporation mixing, we can keep those fairly high tritium units. So this is sort of support that the low tritium in the near surface, in the alluvium, is from evapotranspiration and that this water got below that zone and is continuing on downward, but you're looking at about 20 meters. This is moving at maybe 60 cm a year, something like that.

Now this is another diagram, N90, which it was in Solitario Canyon, the same pattern as we see before, increased changes in water content in the near surface, top two or three meters, and then it--the system drops off with a pulse around the contact below ten tritium units at this point where it drops off, and then we get to the higher tritium units of the surface. Probably, again, they were much higher than that. It's a mixing of most recent waters and evaporation, causing a dilution of the tritium.

DR. DOMENICO: But yo don't have tritium beyond ten meters?

DR. FLINT: We don't have tritium below that.

DR. DOMENICO: Did you measure for it?

DR. FLINT: Pardon?

DR. DOMENICO: You didn't measure for it?

DR. FLINT: No. These--we didn't collect the core. There was only spot core taken for these. One of the things that I'd like to do when we get back to work on the program is to deepen some of these holes maybe up to five or ten meters, and take core samples out for tritium to see what's happening below that. And then, also, on the other holes that we drill, the other 30, we'll collect some tritium data.

Now, in contrast to this typical pattern that you've been seeing where you see the high tritium levels and then they flatten off at something below '52, below ten tritium units, or 1952 levels, we look at some data from Fortymile Wash. This is in alluvium and then some in bedrock. Over 140 tritium units. We see a pulse of tritium at about 10 or 12 meters, maybe another pulse, at the surface fairly high tritium units in Fortymile Wash; not much change in moisture content that we've seen, but still, some pretty good tritium units.

N91 in Fortymile Wash, the bedrock contact, the water table, again, fairly high tritium units; more interesting in the top, maybe--I think it's about a foot and a half, high tritium units, over 30 units. Well, the reason for this, I believe--or at least, I'm guessing now--is that I believe Hans Clausen has measured fairly high tritium units in the snow up on Ranier Mesa, Pahute Mesa from--associated with the weapons program. If that's the case, then these numbers wouldn't be at all surprising that you have snow melt, or rainfall and snow, and you have flow down the wash. You put these large pulses of tritium into your system and you see these values, values that you wouldn't normally expect to see. There's a lot of mixing, a lot of evaporation going on here. This water in this case, and the previously slide--I'm guessing now--is mostly due to tritiated water coming down out of Pahute Mesa from the weapons program. I'm not sure how it gets there. I'm not sure of the fate of this water. I'm not sure how far it goes down Fortymile Wash. You can see it hits the water table and you might have some dilution with some of the older waters. I don't know.

There may be something that we can do with this information. I just got this data on last Friday. I've only had it for a week, and really haven't had a lot of time to talk to people about this and what it means. Is there some significance to this? Can we follow Fortymile Wash on down to see how much tritiated water we get in the subsurface? Will that tell us anything? I don't know, but there--I'm sure there are some things that we can do with this.

DR. LANGMUIR: Alan, have you been doing tritium and precipitation on a routine basis?

DR. FLINT: No, we haven't. The tritium was done by several people back in--I think it was '84, '85 and '86. It hasn't been done and, because of some of the results I'm starting to look at--we're just getting some of this data back. We've been sort of put on hold for some time. We will be collecting more tritium data in the future from rainfall. We'll be maybe reevaluating the meteoric water line for Oxygen-18, deuterium, and some more other geochemistry interests in rainfall, but we have--I have not been involved in that. That's been done by people from the Denver office.

DR. LANGMUIR: And snow melt, as well?

DR. FLINT: Snow melt is something that we will also look at, yes.

Now, N2 is that hole we talked about earlier. Some of the measurements from N2 have shown that in one or two weeks after a major snow melt or rainfall, we see this borehole fill up with water. The last rainfall we had took about three days and we had five foot of standing water in the bottom of the hole. It didn't last very long because it was sampled, but in most cases it doesn't last for more than a week or so, and then it's gone, and I have some data that help to show maybe some of the fate of that water.

What I wanted to point out in this slide is into the UZ5 drill pad, this is one of those large flow units that I showed in that surface characterization mapping. Channelization is probably occurring here. There's a large source area. It channelizes. You can see the rock stripes and you can see what might be a low spot on the topography. Maybe it's a fracture-controlled system. The chances of all that rainfall/snow melt moving down this system is very good. A fracture under this system like we've seen in other locations is fairly good, and this water may move directly down to the bottom of that hole fairly quickly. So when you have this system, fractured system, very little soil cover, very significant, can make a significant contribution to net infiltration below 50--well, 50 feet. Below 50 feet, you're down below the zone of evapotranspiration. That water most likely becomes net infiltration.

But this data helps to explain some of the tritium data in the next slide. This is from UZ5. The bottom of that drill hole, N2, is at about this location in the profile. So we know now we have, in one borehole, five foot of standing water at this location, at the TPC on the graph. If our conceptual model is right for infiltration, and for the subsurface flow, we would expect to see this water continue on down through the fractures, hit the bedded unit, and start to travel through matrix flow, and the slower matrix flow will cause a decrease in tritium and you can see just what we would expect; the high tritium units polyimbibed in the matrix when the water was sitting in fractures, the fractures terminating in the bedded unit, and then we get lower tritium levels and then, finally, no tritium at depth. This is in the Pah Canyon. We don't have data in other places. This is the Yucca Mountain member and then the bedded tuff.

Now, there is a dashed line on here. This, from the dashed line down to the bottom of the yellow bedded tuff is what it's mapped to be bedded from the original logs. In looking at some geophysical data and in looking at profiles from UZ4 and A7 over in Drill Hole Wash, its most likely that this unit is much thicker than that, and we didn't have enough core from that zone to define that bedded unit. So this is sort of an artist's interpretation, and we think the bedded unit is thicker than that. So we're seeing probably fractures through here and maybe terminating in the bedded end flow. This is on the side of the wash, and it may be that it is the welding that becomes most critical, rather than just the unit names; whether this is densely welded, moderately welded, and then we get to this non-welded, so over here we're looking at non-welded. The 60 tritium units may be, in a non-welded unit, the--at the base of the Tiva, and this may be all matrix flow again in here.

Okay, we look at over in the center of the wash at UZ4 we see a little different story. In the top of the alluvium, the same thing you remember from before, high tritium units from the surface and then a decrease to pre-1952 tritium levels at the base. Right at this contact, if we had another sample from there most likely we would see maybe it's more modern water.

In the unit below that, in the Tiva, we see 28 units and then three units. This may be that that sample came from an area that was not connecting a fracture, that was not getting the most--or some of the older water in it, but then we start to see the higher tritium units again. In this case now, in the old--you remember from the previous slide, they died off in this location and there were zero tritium units down here. Here we're picking up fairly high tritium units, and then finally we get down to the bedded tuff, we get zero tritium units.

There is something different in this system than the one right next to it. We have some Carbon-14 dates from the water from Al Yang, a thousand-year water and a 5,000-year-old water, but as was pointed out earlier to me by Ed Weeks, that this system is an open system, and making interpretations about Carbon-14 in the water from an open system, you can't really say that the water is that old. It may be an exchange process with Carbon-14 in the atmosphere now, and these waters are quite possibly much older than that. So let's say in this case, 5,000 years or older. That becomes important in another slide I'll show a little bit later, underneath the wash.

But why do we have these high tritium levels? I'm not sure. There is some evidence that--and I'll show--sort of present that here--the washes in Yucca Mountain in some cases are fault-controlled and in some cases they're controlled by fracture sets. This case could be a fracture set. In the center of the wash you have a lot of fractures through some of the bedded units. This diagram shows another possibility, and that is a possible fault.

We're looking at--in this case we just outlined three units. We have the Upper Cliff, the Rounded Step, and the Hackley. If we look on the other side of the wash, we see the Upper Cliff doesn't line up. The Rounded Step doesn't line up--that one should be down in here--and the Hackley. So what we're looking at is a possible rotation at this point. So if there is a fracture controlling this system, or a fault controlling this system, there may be a lot of fracturing in the non-welded units. So in the center of the washes, if they are fracture sets or fault controlled, you might have water flowing underneath the alluvium or down through N2, which is in this location, through fractures, migrating through the fractures and then moving downward through this fault system. That's one possibility, and that's something we're going to have to investigate in other washes and with other tritium data.

DR. DOMENICO: Again, all of these waters were squeezed out of the rocks?

DR. FLINT: No, these water--

DR. DOMENICO: Except for UZ4 or wherever you--

DR. FLINT: I'm not sure how the water were taken out of the deep boreholes.

DR. DOMENICO: I don't see any water table indication.

DR. FLINT: No, there's no water table here. The water table is probably 2,000 foot.

DR. DOMENICO: No, but you do have some perched water and UZ.

DR. FLINT: In the N2 it was--

DR. DOMENICO: At 30 meters, where your four feet of water comes in.

DR. FLINT: Right. That was in the one borehole, probably a temporary zone that had some saturation and then water moved into the matrix of the fractures. We just put more water into the system than it could imbibe into the matrix for maybe a one or two-week period, and then--

DR. DOMENICO: You did, or nature did?

DR. FLINT: Oh, nature did; okay.

DR. DOMENICO: Well, I was curious.

DR. FLINT: Yeah, no. Yeah, nature.

I wanted to show UZ7, another borehole in a wash. Two neutron holes nearby show that same high volume of water change near surface, and then it drops off. The same thing is true with the tritium. A lot of mixing going on here, so the lower tritium probably not unexpected in this case, and we're getting down maybe toward pre-1952 water. This is in the alluvium. Now, I have a diagram that shows UZ7. These are those values again; high tritium units. We are missing some date from the Tiva. That data is still in the lab. We should get that back soon. Now we're looking at fairly high tritium units, 194 units, 141, and on down, starting to decrease.

This may be, again, in the middle of a wash, fault-controlled or fracture sets, likely fracture sets, but if there is a fault there it could be controlling these high tritium units in this system.

Now if we look at--these are alluvial-covered. Now, if we look at UZ6, for example, UZ6S--that's on top of the crest of Yucca Mountain--there is no alluvial cover. We're getting--these are data that we just got in last Friday; fairly high tritium units in the top of the Tiva. Two data points were missing. They could be high; we don't know yet. 73, in the bedded unit, 102; 85 tritium units in the top of the Topopah Spring member, so this is the deepest we've seen tritiated water.

You remember from UZ4, that water from the Carbon-14 was 5,000 years or older. This water is 30 years, 30 years old. If it's direct flux from the top through fractures, it got through the bedded unit, which we expected to be less fractured and retard some of the flow, you're looking at maybe 400 mm a year vertical flux.

There may be another mechanism for this, and most likely is, and if we look at the topographic setting for UZ6S, this is UZ6. UZ6S sits right on the crest. This is the unit where we see that high tritium levels at the lower part of the borehole. The chances of rain water along the side moving down through open fractures, hitting these units through open fractures, directly going in to the top of the Topopah Spring unit through fractures and then moving laterally, a very likely possibility. In that case, you're looking at maybe 40 mm a year of lateral flux.

So is this the mechanism? I don't know yet. I haven't had a chance to sit down with people and talk about this, trying to put all this together. I'm sure that others have ideas on this. Ed Weeks will have some discussion about UZ6S. He might have some ideas on the movement of tritium at these depths, but this is just some of the information we're trying to put together.

MR. WILLIAMS: Is there a source of the 85 tritium unit number that would make it younger than 30 years?

DR. FLINT: I don't know. It is possible. The difference between, say, Pahute Mesa having those high tritium units in the snow, and those events that--from the weapons program--getting to Yucca Mountain, it's less likely that they did. That's something that you see up in that part of the test site not uncommon. Getting them in this location is maybe less common. In fact, the way the test site operates is that the winds have to be going in a certain direction before they do any testing in case of a leak or something like that, and if that's the case, and if they have the winds going in the right direction, Yucca Mountain may be about as far away from the source of the tritium--if it is coming from the weapons program--as you can get, if it has to go atmospherically all the, you know, all over the globe before it gets back. So this may be as far removed as you can be, but I don't know. I think there are going to be some talks later with some--on some Chlorine-36 data that may be somewhat similar to this, but that didn't come from the test site weapons program.

Now we're looking at--I don't have too much left to go, but I just want to show you some of the work we're doing in artificial infiltration. This last system we dealt with was dealing with natural infiltration. Now we're going to look at wetter conditions. We're going to put water on the system and try to see how the system operates.

The first example is an infiltrometer study. That's the study I showed you in the earlier photograph on the top of Yucca Mountain, above that watershed that we studied for the surficial materials, where we have a lot of bedrock exposure.

This is, say, one of our surficial mapping units. This is just sort of a rough approximation of what a grid might look like. It might be a triangular grid, but we have grid points--somewhat out of focus grid points, but grid points where we will take measurements, and then at one location, centrally located, we'll have a smaller set of grid points at, say, five meter spacing, and we'll go in there and we'll study that area in depth to look at our close spaced spatial variability of properties.

This is an example of one of those less than idealized sampling grids because of the exposure of bedrock. Even though we went in, we're going to do it at five meter spacing. You can see the exposed bedrocks in the slide and you can see where our final sampling points were located. In this case, we ran a double ring infiltrometer. We sampled these for bulk density, sand, silt, clay, gravel content, fine silt density, porosity, trying to relate the physical property of the soil with the infiltration measurement. The bulk density measurement is fairly easy, and we can get a lot of data fairly quickly. If there's a correlation between the bulk density measurement, the sand, silt, clay and infiltration, we can use our infiltration measurement in, let's say, 500 locations, bulk density and sand, silt, clay, whatever in 5,000 locations and use co-kriging to estimate our uncertainties of the infiltration rates and then we can go back and test our mapping.

We add to this some of the surface rocks, not just bedrock, but we add to this the surface rocks, the ones that are not in hatch marks. These are not connected to the bedrock itself, but do have some influence on the measurements we take. This had a conductivity of 6 x 10^{-3} cm per second on average, with a standard deviation of about 5 x 10^{-3}, or a coefficient or variation of near 100 per cent, not unexpected for infiltration.

Now we're going to look at the infiltration data, first, just using the infiltrometer information, just the hard data that we have. We're going to be looking at a three-dimensional graph, looking from this corner looking down the slide. So remember that there is this rock layer here in the next diagram. There is the rock layer. Now we're looking backwards down the slide. This is the infiltration rate that we measured in three dimensions. In this zone, we can handle about four inches an hour precipitation.

DR. DOMENICO: What am I looking at there? Four to 16; what does that say?

DR. FLINT: Four to 16 cm per second x 1000, so that's--the average, again, was 6 x 10^{-3} cm.

DR. DOMENICO: You keep giving things in millimeters per year. I hate to keep multiplying things. What does that mean?

DR. FLINT: This is at centimeters per second. That's 4×10^{-3} cm per second. I like to change units because it keeps me thinking. When you deal with a big program, some people like it in some units, some like it in other units. Wait until we get to megajoules and kilojoules per gram and stuff like that. That's more fun.

This is not accounting for the rocks. This is only taking the measurements. This is the hard data that we have. We add to that some information, some soft data, our inferences about the rocks not conducting. We don't have direct measurements. We try to put that together, we come up with a little different diagram. This is those rock outcrop zones. We're just estimating that there's zero infiltration rate, relatively speaking. This is that high zone, so we have reduced the ability of this system to handle a rainfall event by having those rocks in there. Because this is downgradient, the water that runs off from this system is going to move into these soils that have higher infiltration capacities, so we are going to have some surface runoff that's going to be taken up. So this is where that mix of, you know, how much bedrock exposure is there is one question, and then, how is that distributed, how is that related to the distribution of soil upgradient/downgradient, and how much influence does that have?

These are fairly thin soils around in this area, so you can saturate them fairly easily and move water into the fractures, and you get some of the thicker soils that actually have some of the higher rates, they may be able to handle the moisture, keep it held up in the system, and then evaporate it where you don't have movement.

DR. DOMENICO: What's the difference between this one and the one you just showed?

DR. FLINT: The one I showed before did not account for the fact that all the rocks were there, and I'll go back. This is just taking and fitting a inverse distance squared map to the data points where we actually have measured data. We went through--

DR. DOMENICO: It's only into the soil?

DR. FLINT: This is only into the soil. We went back through, took the rock information, and digitized the rocks and assumed that that was zero infiltration, just for the moment. It's not fractured. It's fairly low in comparison to the soil, and we added the information about the rocks into our inverse distance squared model, and we get a little bit finer, a little bit better detail of what's happening there. Quite a bit of variability in that. This is that rock layer that you saw in that other--and you have it in there, surface outcrops. Zero infiltration rate. A few pockets of soil surrounding those and inside here, more rock outcrops, low infiltration rates. So we get a different estimate of infiltration when we average all of that together.

DR. DOMENICO: What's the basis of assigning zero to those rocks?

DR. FLINT: Relative to the soil, it's a fairly low number. We don't know what it is, so zero or, you know, 1×10^{-8} or $^{-7}$, relative to the 10^{-3} is several orders of magnitude, so we feel that this is just a close approximation.

DR. DOMENICO: Wasn't your moisture content higher in the rocks than it was in the soil?

DR. FLINT: No. These, it was pretty much the same.

DR. DOMENICO: I thought when you summed up the moisture content a half an hour ago, it was higher in the rock than it was in the soil.

DR. FLINT: It was higher in the bedded, I'm sorry. It was higher in the non-welded units. In the welded unit, it was the same in the surface alluvium as it was in the welded unit. The welded units being fractured, the water moves through the system or it evaporates fairly quickly and it doesn't move into the matrix as fast. So the welded units are, on average, about the same as the surface of alluvium, the top of the alluvium, and this is residuum or some wind-blown materials in this, and they vary in thickness from zero to maybe two or three feet in this small 25 x 25 meter plot.

We've just put this data together. We haven't done a lot of analysis on it yet. We just wanted to present some of the information, some of the kinds of things we're working on. We'll go out and we'll take more samples at larger, different locations and try to put all this together in a big picture, but this is where we are right now.

The next is small plot, large plot and ponding.

DR. CANTLON: Let me back you up a minute on the rocks. Have you done any look at natural physical sample? It would seem to me reasonable to expect that if you have a soil mass with rock material in it and you're looking at an infiltration event, the moisture goes down, hits the rocks, moves around the rocks. You actually might get a net increase in flow with the rocks there, not a decrease.

DR. FLINT: Well, this is surface infiltration. You're going to--if you're moving around the rocks, you're going to put moisture in another part of the system that is supposed to be taking up rainfall. Now, the system--these rocks that we're dealing with, from that diagram that you have two ago, are bedrock, are exposed bedrock. There is nothing underneath them. It's just continual bedrock. We do have surface rocks. These do become important in evapotranspiration, because underneath these rocks you get water moving up, it collects underneath the rocks and can concentrate there, can actually stay there for a longer period of time underneath the surface rocks.

Now, this is--I'm went to the next slide and I'll talk about that information on this slide. The large plot/small plot rain simulation where we do rainfall experiments, we would get one number, maybe, from this whole system rather than all these little points from the infiltrometer study. We'd get one value. We'd put some boundaries on it. We'd put a rain--a sprinkler there. We'd collect runoff and we'd find out what's the infiltration rate we have--or the rainfall rate we have to use before we start getting runoff and try to define the whole system.

The small infiltrometers are easy to run. They're quick, and we want to do a thousand, or 2,000 or 5,000 of them, and we want to use that to define the upper one foot or so of soil. The larger, small plot/large plot, we want to get deeper and deeper soils and put this whole system together from a variety of perspectives. So we're working on some prototype development in that case. We haven't done any measurements, other than one laboratory experiment, and so all I have is the slide to show you that that's something we're going to try to do, but we haven't done yet, and it's an area that's on hold right now pending any changes in the budget. And if there's no changes in the budget, then this will stay on hold.

DR. DOMENICO: What point do you have to--the infiltrometer study, you've actually applied a lot of water to the surface.

DR. FLINT: Not a lot, but--

DR. DOMENICO: That's the results?

DR. FLINT: Right.

DR. DOMENICO: What good are those to us when the rate of infiltration will depend upon, you know, basically the moisture content of the soil and the soil may never ever get as wet as you made it?

DR. FLINT: Well, what you're looking at here--what you were looking at there, a minute ago, is more or less a transmission zone conductivity. What is the transmission zone conductivity? If it is greater than the expected rate of rainfall, then those soils will take on moisture. If the conductivity of that area is less than the expected rainfall, then you're going to have surface runoff, and that's what we're trying to get at.

DR. DOMENICO: So you're calling those infiltration rates a saturated--

DR. FLINT: No.

DR. DOMENICO: You're not?

DR. FLINT: A satiated transmission zone conductivity. There is an air entrapment--

DR. DOMENICO: What the hell is that?

DR. FLINT: If you were to take one of those soil cores into the lab, you would measure it. You would come up with a saturated conductivity that is one order of magnitude greater than what you see in the field, or maybe not quite so much, but the transmission zone conductivity accounts for entrapped air. It accounts for the system not being completely saturated, some of the larger channels or mid-sized channels not conducting for one reason or another; mostly entrapped air. The term they use in soil science, soil physics, is satiated. It's not saturated. It's just the best it could do under the circumstances, and when I say transmission zone conductivity, if you're, you know, looking at moisture profiles with time, you're looking at the movement of the water through the transmission zone, which is slightly less than saturation. And if you're, say, five per cent less than saturation, you're looking at maybe an order of magnitude decrease in the conductivity.

So you can go to the laboratory and take these measurements, and you're going to have to make some assumptions about how much of a reduction you're going to have in the field. We're really trying to look at how the system will respond to wetter conditions. That lowest rate I saw in the soil would allow rainfall event--and again I'm changing units--of four inches an hour. We have seen four inches an hour rainfall occur twice in Topopah Wash, which is to the east of Yucca Mountain, and we've had a major runoff event in Topopah Wash, so that's the kind of thing we're trying to understand. How fast can these soils take on water? There is going to be a period of time where it can take on water at the rainfall rate, and as we saturate the soils--and these are shallow, they will saturate--at what point do they start getting runoff, and it really is important. The history is important.

A one-inch rain storm may not be very significant, but it is if it followed a two-inch rain storm a week earlier. That's when we see the major runoff events. So you're right, that it's very important to know what the conditions are in the soil, soil water contents. These are, in cases, the wettest we might see. The infiltration rates of drier soils can take on water faster than that for a long period of time until they start to reach saturation, and then they start--they decrease because of the large gradient when you have unsaturated conditions.

DR. DOMENICO: Any infiltrometer studies planned under natural rainfall conditions?

DR. FLINT: The natural infiltration studies are set up such that where we do an artificial infiltration--not an infiltrometer, but an artificial rain simulation site--right next to that is a fully-instrumented site identical to the artificial infiltration site that is used to monitor natural infiltration events. We'll have psychrometers, heat dissipation probes. We'll use cross hole gamma, time domain reflectometry, tensiometer transducers, psychrometers, and we'll try to see the impacts of the natural system because if you're doing an artificial infiltration program and you add a half an inch of water and it rains an inch and a half on you, you have to be able to account for that, and we do that in our fully instrumented--what's called a control plot for the small plot or the large plot rain simulation sites. The control plot, we measure more in depth the characteristics of the natural event.

MR. BLANCHARD: Alan, you only have a couple of minutes left. Can you reach your summary?

DR. FLINT: In summary--I knew. I was waiting for him to--I wasn't going to do anything. I was going to sit there until he said that, then I went for it.

I have two summary slides, actually. One is infiltration is spatially and temporally variable. We already went through that. That makes sense. We need more data on the temporal and the spatial scale. The temporal scale is important, especially when you're dealing with the neutron moisture logging program. We need to get some holes in as soon as possible so that we can get another five years of record. We learned a lot from having those. We need to get some of the sites that haven't been instrumented yet, neutron holes, and start collecting data. We'd like to do it while these soils are fairly dry so we can see how the system is responding to increased moisture contents or increased--wetter conditions.

And, of course, as you would guess, you refine the data set and the conceptual models and it's iterative. As we get more information, we refine the system. It's a dynamic system. The conceptual model's dynamic. The data sets are dynamic. In another year all of this stuff may change and I may change my mind on everything, but right now, this is what I'm thinking.

In terms of the conceptual model again, this is sort of a review. The important areas may be in the upland areas where you have--as we saw from UZ6--where you have direct access of fractures to snow melt and rainfall, you can move water fairly quickly and fairly deep. The alluvial channels are important in storing the water from the surface. It stores the water. The water's removed through evapotranspiration again. Fractured rock in the channels, that's a good spot to get a lot of moisture into the system. You've got the exposed fractures. You've got channelization, so you're concentrating a lot of water in one location. This is maybe some of the best places to get water in.

The N2-type example on the sideslope, you do get some good amount of water in that particular one channel that we happen to have a borehole in, probably not as much as in a fractured channel. Of course, these fractured channels, if they do get a lot of water, they may start to plug up some of the fractures. You may get fines in there. You may get calcium carbonate in there and plug some of that up.

Old channels become important sometimes. We saw in that one example, the Pagany Wash cross-section, where that one channel did pick up some moisture. That moisture in the old channel got down to the welded rock in 24 hours. Now, once it's there, if it's saturated or near saturation, it can move into some of the fractures and become important. And finally, when you get a long way down the wash, you're getting to these larger gravelly materials that are deposited, you can move this water fairly fast through this system. So snow melt becomes important. Two foot of snow provides two inches of water, which is 30 per cent of the total water we get on an average year, during a time when we have little evapotranspiration, little demand from large net radiation--or we have low net radiation, so that water can move through the soil, come in contact, flow into the matrix or into the fractures without much of a demand for it. In the summertime, a rainfall event hits the soil, moves in quickly, but it evaporates quickly, and the water that gets through these thin layers, this water may move through this, hit the surface, and be evaporated in two or three days. So the winter precipitation events may be the most important, and from the Oxygen-18, deuterium, the water we see in this hole, most of the time we saw it in here, occurred from snow melt, and the data that we saw in UZ4, that tritiated water was from snow melt.

The other data that we've collected, I don't know. We haven't done that analysis and I hope to collect some more data, and we also hope to get some data in these less welded units, not fractured, up in some of these locations on the site. That becomes important because these non-welded units are some of the wettest we have on Yucca Mountain.

DR. DEERE: Wouldn't it be true in the climate, the terrace deposits and all of these thick gravels would be extremely important, then, because they would be a constant source of infiltration along the entire contact?

DR. FLINT: Very, very important, yes. Right. The larger, the gravel alluvium areas down low in the washes down on the alluvial fans are very important. Those are zones that are gravelly enough that we can move the water below the zone of evapotranspiration. Once you move it below that zone, it can go on. It can take years, however long it wants, to move down to--through the system, and Hans Clausen, in his paper, showed what he thought was the source of the groundwater in the Upper Amargosa Watershed, the one we're dealing with, was probably from the gravelly soils to the south of Yucca Mountain where you can move through the channels, although he felt it was probably pleistocene in origin. That, on a larger scale, is the same thing we see here in a small scale. When you get down to where you're getting to the alluvial fans, a lot of gravels, the water can move fairly quickly through there.

So we have those, you know, the three regions; the upland areas, very important for fracture flow. Middle of the channels, the washes are not very important, at least in terms of the alluvium, because water moves in, nice storage capacity, it evaporates pretty quickly, although you do get some moving underneath the channels. And the sideslopes become very important, and then again, the alluvial channels become more important as you get way downstream.

DR. CARTER: I have a couple of sort of generic questions. One, do you have any particular problems in dealing with the small quantities of water that you're dealing with? I presume that the precipitation is on the order of less than a half a millimeter a day on an annual basis, at least, and a lot of variation, but what kind of problems do small volumes of water give you in terms of measurement and interpretation of data?

DR. FLINT: The small volumes of water make--create some problems in that you're trying to detect that in a neutron hole which sees fairly large--that's the large enough area. More importantly, though, is that when you're looking at, say, evapotranspiration, the water, even though you can say half a millimeter a year on an annual basis, it comes in spurts. It comes in large storms that last for a couple of days, and we might see one or two inches at one time. When you see an event like that and you set up your--and your evapotranspiration measurements are out there--you can determine the fate of a large percentage of that water. If you're dealing with an infiltration rate of--a percolation rate of half a millimeter a day, an evapotranspiration rate of six millimeters a day, if your Bowen Ratio or Eddy Correlation System turns off for a couple hours, there you lost the whole recharge for the year in one measurement.

But if you look at it from a different perspective in that you get six inches a year of rain at a location, and measure five inches of evaporation because you got that one or two-week period after a major rain storm, then you've at least been able to bound your system and say, well, there's only one inch of rain unaccounted for, and that's the system we need to work with. The low quantities of water are difficult, but then that what makes the site--one of the reasons why the site was chosen to study in this investigation, and that's why we go to the artificial infiltration studies.

DR. CARTER: Okay, the second question involves how you will intend to factor in or consider effects from the weapons testing program at the Nevada test site, and I presume this would be extremely important, not only in dealing with tritium, but Chlorine-36, Carbon-14, and a number of other things, and I gather that apparently efforts, major efforts have not been made to do that yet. Is that a fair assessment?

DR. FLINT: I'll make a couple comments, then I'll sort of have to open it up to some other people. From what I understand--you know, we did see some large tritium units in the Fortymile Wash, which is likely related to the weapons program. Chlorine-36, as I understand it, comes more from oceanic, aboveground testing and not from the land-based testing, but is there someone out there that wants to sort of address that question about is--in the geochemistry program, are you looking at the influences of the weapons testing program on Carbon-14? I see some heads. Does somebody want to jump up? Al Yang.

Al, do you want to come up front here and have a microphone? In the corner here, Al; in the corner, Al.

DR. YANG: Yes. We have measured, in our program for the geochemistry, we have measured in the past three years for all the tritiums from the precipitations. The maximum number we got is about 60 tritium units, or 70. We never saw such a high number he's talking about, 100 tritium units or even 200 tritium units, so we still don't understand where that comes from. But other than that, we think we should be able to tell from the precipitation going down through the core through infiltrations, then we can correlate between the precipitations and what it is penetrated down to the water tables using all the stable isotopes, Carbon-14's, tritiums, and the Chlorine-36 is down by Los Alamos, and we have some--pretty many data. It's feasible to do, despite a very small quantity of water, get from the cores, but we are planning to do all this and I think we are going to get some good data out from this program.

DR. CARTER: But you may well be interested in events that occurred prior to three years ago in terms of tritium and the other materials.

DR. YANG: Well, because that's where we started. We started through the core, so that's all we can get.

DR. CARTER: You may be interested in historic data as well.

DR. YANG: Yes, but our data is going to get to--right now, the gas/water sample, as I said, is about 300 feet, 350 feet about 5,000 years old, and I think that is a good data. There is no contamination from caliches because our bicarbonates, 13, Carbon-13, Carbon-12 ratio is about the same as a biogenic CO_2. It's not from caliche, because the difference is quite different. Caliche has a 13/12 ratio about -5 per ml, but from a biologic CO_2, it's about -20, and these data have a 13/12 ratio of about -20, so there is no contaminations for these Carbon-14 data. So I think I get a good data out, but it's a lot of work.

DR. FLINT: I agree with your point, though, and I think there is a possibility to go through and see some of these peaks of tritium at depth and try to relate that to snowpack and events. What you'd have to do is you'd have to lay out the tritium probably at some greater depths in some of these boreholes, look at the, you know, look at some historic events about weapons testing, when you might get some high tritium levels, put that together with a large snow melt runoff event and see if you can't find a correlation, but there is a need. It doesn't appear to me that the high tritium levels we see in Fortymile Wash, whatever is causing that--whether it's from the weapons program--most likely is occurring on Yucca Mountain. We see fairly low tritium levels except in one hole in ten. We saw those 200 tritium units. Most likely that was from, say, 1963 or '64, and that that water has just been continually moving down through a matrix flow.

DR. CARTER: Well, the point I would make is that this is a generic problem and certainly a number of the radionuclides that the programs, or the various programs are designed to measure and consider as far as modeling, and so forth, I would contend that the weapons testing program at the Nevada test site not only currently is, but certainly in the past has been a source of those radionuclides.

DR. FLINT: Yeah, I agree, and from my perspective, and what we have sort of done is we say that the water, when we see tritiated water, is post-'52 water. That's not--you can't--and I tried to make that point, I thought, but you can't say what date this water came into the system. You can't go that 1963 was a peak and 1957 was a peak, because 1978 could have been a peak or 1985 could have been a peak, and particularly at Fortymile Wash.

We can say that it's new water, like 84 tritium units down at the bottom of UZ6 in the top of the Topopah. That's modern water. We know that. We don't know where it came from or when it got there. It could have taken two years. It could have taken 30 years. It didn't take more than from 1952 to present because there were no tritium units that high at least we know. And, you know, there's another argument about whether it's 10 tritium units or 25 tritium units is background, so that question's not quite resolved, either.

DR. YANG: But I think that weapons program--

DR. BLANCHARD: Al, just a second. We have a scheduled break which was set up. Do you want to pursue, Don, continued questioning and postpone the break, or--

DR. DEERE: About another two minutes, Max.

DR. BLANCHARD: Okay. There's one point I'd like to make, Mel, and that is the Chlorine-36 discussion by Ted Norris will go into information that relates to more historical events. The other thing is, we do have a study plan which is geared towards studying the geochemistry that we can learn from the result of the weapons test program as an imprint on looking at current climate and hydrologic processes. We did not prepare a presentation to represent what was going on in that study plan, but we'll be glad to do that sometime in the future.

Do you want to pursue more questions?

DR. DEERE: Let's break. Let's have our break and come back in ten minutes instead of fifteen.

MR. COONS: For anyone who hasn't signed in, would you please go to the desk and sign in during the break? Thank you.

(Whereupon, a brief recess was taken.)

MR. BLANCHARD: The next topic is measurement of the unsaturated zone hydrologic properties. The topic is divided into three separate talks. One is an overview of the matrix properties. The other one is talking about the air permeability testing, and then measurement of fluid potential field in in situ monitoring.

The first speaker will be Alan Flint, talking about an overview of the matrix properties of the unsaturated zone.

Alan?

DR. FLINT: The way this is set up right now is we want to sort of talk about the unsaturated zone. The talk we had this morning was on the shallow unsaturated zone. Now we're going to talk about the deep unsaturated zone; some overlap between the two, particularly matrix properties, but this is set up in sort of a way to make sense from how we're going to do the study.

I'm going to talk about the drilling program just for a minute or so and why it's set up the way it is. Once we have the drilling program, we go in and we start drilling a hole, we're taking out core, and the core goes into the matrix property program and we do some analysis on that core. When the hole's finished, or during stages of the hole being drilled, then we'll go in with our air permeability testing. Rob Trautz and his group will go in and do some studies. When the hole is finished, we've taken the matrix properties. We have a basic understanding of what the distribution of state variables, like water content or water potential are in the hole. Rob has gone in and taken a lot of measurements of air permeability. We've looked at fracture zones, Tb logs and geophysical logs, and then we go in with the in situ monitoring. So this presentation sort of tries to put all this together. We matrix property analysis. We do air testing on the hole itself, and then we instrument the hole and try to put that all together in this system.

The program itself is responsible for really three major areas. One, it's to describe, measure the state variables on the rock matrix. Basically, that's simply describing the in situ water content and water potential.

When we do this initially, as quickly as we can, what we're trying to do is locate zones of high water potential, low water potential for Joe Rousseau and his borehole instrumentation. He has to calibrate his instruments, and he has to calibrate them fairly quickly. When he calibrates the instrument he wants to know what range he's going to be dealing with, so that's what we're trying to come up with.

We're also going to characterize the physical properties; bulk density, porosity, water retention, saturated/unsaturated conductivity, and I'll go into more detail on this in a little bit. And third, we want to develop these three-dimensional spatial structures. What does it look like in 3-D? And we use our geostatistical and statistical analysis for that.

The system is set up initially from sampling of borehole was on classical statistics. We simply did coefficients of variation, predicted sample sizes to estimate the means, but we really don't want to know what the means are of a property. We want to know what the distribution is. What's the high and what's the low, and where are they, and how are they distributed? So we're bringing in geostatistics. We look at three dimensions. We use multivariate analysis so we can add porosity, density, conductivity and the interrelationships for our co-kriging analysis, like we saw with the rainfall. We're going to do a structural analysis, look at variograms, cross-variograms between the different components. From there, we can go on to predictions. What is our best estimate of what the property is at a certain location? If a modeler's dealing with a 10 square meter block, we can give our best estimate of what's there even if we don't have a measurement point there. But better than just the estimate is this analysis of the variants. What is our uncertainty in that estimate? Is that uncertainty unacceptable? If it is, then we can say where you're going to need to collect data to reduce the variable, to reduce the variants, improve your estimate, how many samples it's going to take, and then finally, we can do simulations. Although the prediction is our best estimate of what's there, what could be there? A simulation will tell us, and this is a conditional simulation. It's picking from the distribution that we have at that estimation point, but it's conditional in that it has to meet the same spatial structure. It has to be consistent spatially. You can't have a high conductivity and a low conductivity if that's not the way they occur in nature. So our conditional simulation is real valuable.

Once you run a conditional simulation, here's the possible distribution. It's consistent with the data and it's consistent with the spatial structure. Then you can run your flow models. Then you can do another simulation and run more flow models and come up with a distribution of certain properties. So this, geostatistics is real important in our analysis.

With that in mind, I want to just show two examples of some data we have from G-tunnel. This happens to be two experimental semivariograms of neutron counts. We did this because we didn't have our calibration equation at the time, but counts is related to water content. So this could be counts, it could be water contents, bulk density, porosity; it doesn't really matter. What we're looking at are two holes that are about 90 degree angles to each other. Joe Rousseau's borehole and one of the cross hole testing for Rob Trautz was set up in G-tunnel. What we see here is simple, zonal anisotropy, a little different variability as you're going with the beds than if you're going across the beds. We have a pattern that you see develop at the a priori variance where we're looking at crossing beds again, the distance between bedding planes. That's in two dimensions.

We add the third dimension, which you would guess is the most variable, the highest a priori variance. This is the horizontal hole in the non-welded unit and we have a little bit different range because we don't have the great length of the hole of these. We're looking more in the order of 25 meters. We see the greatest variability. From this kind of information we can build that three-dimensional geostatistical experimental semivariogram, finally a model variogram, and estimate in any of three dimensions what our properties are.

The vertical is the most variable, as you would guess. It's not more than an order of magnitude more variable, and because you have good detail on the vertical variability in a borehole, you have as many samples as you can get, hopefully, you can do an analysis. Assume that this is the worst case and do a three-dimensional model using the vertical variability in the horizontal direction if you don't have enough sample points at a horizontal spacing. This might be an upper bound. It's a most conservative estimate if we don't get enough samples, but we set up a drilling program that was trying to look at the horizontal variability. The vertical we can get from a series of boreholes.

This is the systematic drilling program. We have our feature base sampling, the UZ9 system down the imbricate fault zone, the exploratory shaft, and the UZ2-3 complex up by UZ6. Add on top of that the systematic drilling program, and this is an analysis that's in the SCP. It was worked out between USGS and Sandia to try to come up with a plan that would give us the best aerial coverage, and also give us some spatial variability analysis. We have pretty good distances in some cases. Our closer-spaced sampling scheme is represented here. A lot of sample points at the south end that we're trying to put together a program so that we don't waste any of our resources. We try to put together with the feature based sampling at the UZ9 complex, add to that some more of our systematic holes and get a good estimate of the horizontal spatial variability.

The exploratory shaft location at the north, another place to get at horizontal variability, but in the worst case we use our vertical variability and say, "That's the worst case," and do our first estimates, do our variances, and find out how well we really did. And we have a few more spaces, but in--sort of in a summary to this slide is our sample pairs.

In classical geostatistical analysis it's recommended that you have at least some--some say 30, some say 50. We went with 30 data pairs at a fixed interval. With that program I showed earlier, this tells us that we have 35 data pairs at 1,000 foot spacing. That's enough to make a valid variogram model, and we went on and looked at other spacing. So now we have the drilling program that allows us to get aerial coverage of Yucca Mountain. It gives us our feature-based drilling program, and it gives us enough sampling to do three-dimensional geostatistical analysis. These samples can move around. These holes can move around. Once we finish the analysis from the holes, then we can recommend new hole locations if we find that there are areas we have a lot of uncertainty, the highest variances.

Okay, so we have the drilling program. We're going to go in and drill the holes and start looking at some of the matrix properties from the core that we get out. Water content, water potential, the two state variables. Permeability, gas or liquid, saturated/unsaturated, and then some models of these components. Liquid isn't very important. Gas permeability is real important to the work that Rob Trautz is doing, some of the work that Ed Weeks is doing, and others on how the matrix functions. We also need to account for fractures and that's what other programs will talk about.

Water characteristic curves. How do we deal with hysteresis and can we model these? A lot of data points come out of these measurements. We want to simplify things with models. And then related properties, bulk density, particle density and porosity.

A core sample that we hope to get from our drilling program, it's preserved in a Lexan liner. That's a good way to preserve the state variables. We can preserve the geochemistry data. We'd have no contamination with C-14 or tritium. You can photograph the core. You can see fractures, and this--we propose to have one sample out of every three feet preserved in the Lexan. This is an inner core barrel used in drilling, and we think we can get our best measurements on state variables from this kind of sample.

One of the things I want to talk about for a second are some different sizes of core that we're trying to deal with. What's representative? Is a small core, or would it be better represented by a large core? And how does this match the field scale? Are there field scale measurements different from this? I'm going to kind of go back and forth and talk about the size of the samples and some experiments we did to evaluate that, and also, how it relates to field scale.

This is where we try to put everything together at different scales and at different--different kinds of information. This is from our wet and dry drilling experiment in G-tunnel. We have volumetric water content versus depth in the borehole, distance from the rib. The bottom is some lithology information. The blue is a zeolitized band. The yellow is a silicified. Zeolitized, silicified. You notice that in the silicified zones you have the higher fractures in both cases and you see that these--some go through some of the zeolitized but they'll terminate. Even though this bed goes on above this, we don't see the fractures. It terminates.

We have our information from the core, fairly detailed information. We also have a neutron log. Now, why is a neutron log so different? Well, it's a different scale that it's dealing with. We have to be aware of that in this kind of system. Some of the lowest measured water contents came out of this silicified zone. Some of these fractures are open. You have air flow. You can keep these dry. Highest water contents come from the same zone in the neutron logging.

Well, the reason for this, we believe, is that you have, one, this core is gone now. We took it out when we did the drilling. You have the zeolitized on either side. Your neutron log is looking up maybe 200 millimeters into that zone. You have fracture flow during the wet drilling. You've wet up the zeolitized zone above and below. You see that right here.

In this case you're looking at a wet zeolitized zone, yet you're looking at the dry silicified on either side. So you have to account for the scale of your measurement and what it's seeing compared to what your core is measuring. So we have to be very careful in how we process these kinds of information.

In terms of water content, we can also look at water potentials. It's sort of a driving mechanism for flow. We see the low water potentials, down to -50 bars in these zones that are fractured. These fractures probably do not go into the zeolitized units very far, but they do extend laterally and probably intersect the opening of the drifts. So if you're modeling a ten meter section, what number do you use for water potential for that section? This is where a problem comes up. Do you use one bar or do you use -50 bars? If the water has to flow across that block, what's going to control water flow? So I'm not sure of the answer to that question. That's something the modelers are going to have to look at and they're going to have to do some sensitivity analysis on that, but this kind of information is what we hope to put together from the matrix program. Detailed geologic information, detailed hydrologic information, you've got to match the two together. You can't separate them. You've got to keep them together.

DR. DOMENICO: Was the dry-drilled and the wet-drilled due to compare influence of water in the samples?

DR. FLINT: The wet and dry drilling were to compare the influence of the drilling fluid on the in situ conditions, what's left in the borehole and on the samples. We've made some, yes, we have some conclusions on that. Do you want to hear them?

DR. DOMENICO: Yes. One minute, that's all.

DR. FLINT: Okay, one minute. What we've concluded is that drilling with water wets up the system. Now, in the fractured system, the water gets into the fracture and travels a great distance. Dry drilling, to some extent, dries out the system very little land it recovers very quickly. Wet drilling recovers in a couple of months due to some ambient conditions, but you have chemical contamination. Even in the welded unit you can add 20 to 30 per cent of the water in the welded is from the drilling fluid, so we like the dry drilling system.

MR. WILLIAMS: I want to ask you a question that's reflected by that slide as well as some others. How do you decide that the Calico Hills zeolitized unit or the bedded reworked tuff unit is the population which you will use as your sample population, or a sample substrata? What decision-making process do you go about using to decide what your population is for sampling and for sample analysis?

DR. FLINT: We hope to have, through systematic drilling program and through our geostatistical analysis, a classical description, a classical statistical description, and a geostatistical description of all of the samples we can get. We have intentions of taking up to 10,000 samples and we're going to have to do the analysis based on the spatial distribution. The population is going to have to use classical statistical assumptions that your sample that's distributed throughout the site is taken, you know, in the case of random sampling, that's probably not appropriate in a geologic system. Systematic sampling is probably most appropriate because these parameters are not randomly distributed. The Calico Hills system was all laid down at one time. Geologically, it's very similar, and a spatial program like we have is probably the best, rather than random sampling.

MR. WILLIAMS: So why don't you try something like clustering or just a canonical analysis or something like that?

DR. FLINT: There are lots of analyses we can--that we'll do. Cluster analysis we're doing--

MR. WILLIAMS: So why did you choose these categories of samples to put on this graph?

DR. FLINT: Oh, because this is all I had.

MR. WILLIAMS: Well, why didn't you group them some other way?

DR. FLINT: Because these are individual samples. Each one of these samples is in our matrix program. For instance, any one of these samples, an individual has been compared through the imbibition study, through a water release curve using a pressure plate, using a centrifuge, through gas stripe permeability measurements, through centrifuge permeability measurements. Eventually, it'll be used in the measurement of mercury porosimetry, psychrometer, microwave, water release curve techniques, a variety of techniques, and these are only listed because this is the characteristic of those cores that I showed you along that graph, or that photograph earlier.

MR. WILLIAMS: So let me just push you a little more. Why didn't you combine the data for the vitric unit of the Calico Hills and the Calico Hills zeolitized unit? Why did you put them separately?

DR. FLINT: Okay. I believe in looking at individual data points more than I believe in means. When you start lumping this stuff together, you're giving yourself a false sense of security, I think, in a way, that you can say that there is a mean imbibition or a mean water release curve for the Calico Hills unit. As you'll show in my summary slide, there is a large amount of variability, and I want to try to describe that by showing how two samples can be quite different, even from the same unit.

MR. WILLIAMS: What makes you think you've separated them correctly?

DR. FLINT: These are separate--just core samples.

MR. WILLIAMS: I mean, when you took the core samples, what made you--what makes you think you sampled the correct individual units? You're doing a very subjective job of doing this and I'm just trying to figure out how you're going to handle that subjectivity.

DR. FLINT: Oh, yeah, you're right. These are the core samples that are in my lab. That's all I have. I didn't try --I'm not trying to say that this is a typical example of the Pah Canyon vitric, only that that sample is Pah Canyon vitric. This is Calico Hills vitric. It may not be anything at all representative. One of these came from G4, one came from some other place. They're not representative. They're only representative of that sample. I'm not trying to say that this represents any of these units at all. I don't want to imply that. This is just some point measurements, so that's sort of--I guess I didn't understand. This is--what this analysis is for is to really, is to look at the influence of the sample size.

The Topopah Springs/Grouse Canyon, we did a lot of work on the Grouse Canyon out of G-Tunnel and Tunnel Bed 5, but you see that there is difference between samples. All I wanted to show was that in an imbibition experiment, where you just set this thing on water--wet filter paper, it absorbs water. You see a difference in the way it absorbs water. We can make some use out of that. It's a very simple technique and I wanted to show that here.

This is imbibition in centimeters timed to the one-half to linearize. If you linearize this, the linear part of the curve--using Phillips infiltration equation for horizontal infiltration, assuming no gravity, which is okay for us--we take, in this case, two relative saturations for the same core; about 20 per cent or 60 per cent saturation and calculate sorptivity value, so we get two sorptivity values.

We can look at sorptivity versus saturation. This represents one core. The zeolitized, the two red, or silicified, the blue, is the welded, sort of as you would expect. Now we can do an analysis. We're going to look at 20 per cent saturation and 80 per cent saturation, so we're going to take the data points off of these for the large core, 2 1/4 or 2.4 inch is the HQ that we're going to get from Yucca Mountain, and the small one-inch plugs, which is commonly used in the oil and gas industry because they're easy to sample horizontally.

But now think about this 20 per cent saturation/80 per cent saturation in the next slide. Sorptivity versus porosity. The red is 20 per cent saturation, the green is 80 per cent. You can see whether it's a small core, the small circles, or the large core, the diamonds, they fit this pattern pretty well. There was really no difference between the large and small core. There is a nice relationship to total porosity. Eventually we'll make a three-dimensional surface out of this, sorptivity, porosity and initial water content, but I just wanted to show from this example there was no difference between small and large core.

This is the porosity data from those core. Large core is blue; small core in yellow. What we did is took the large core and we undercored a smaller one, a one-inch plug. See, the one-inch plugs are easy to deal with in the laboratory. They're a lot faster to come to equilibrium. You can do more with them. We're developing technology for the large core, but we really want to make sure that large and small tells the same thing. The only differences you see, in some cases, is whether or not you captured concentrated pumice fragment or removed the pumice fragment from the edge of your core. The pumice fragments is what causes most of this variability. Again, that shows the small core/large core, pretty much the same.

I'd like to show some of the data now on water potential versus saturation, our water release curves. We use a variety of techniques. We have to because these core react differently and some were going to work better than others. We're still trying to figure out what works the best. We're not done yet, but this is preliminary data. Water potential in log and bars and saturation, SPOC cell--and I have a diagram of that to show you what that is. That's a submersible pressurized outflow cell. The centrifuge and the pressure plate. Just two of those samples that we showed earlier. In one case they seem to go together; in the other case there seem to be some differences, but you can see that yo might be able to make a composite curve, which is what we do in the next slide.

The SPOC cell works quite well at part of the curve. The centrifuge, we couldn't get any water out, or they couldn't get any out when they ran this in core labs up to a certain point, and then once water started coming out, maybe it is, maybe it's not at the right location, but we can make a composite curve. We have to put all this stuff together.

Now, the SPOC cell is kind of exciting for us because this is a thing, this is a way we really think we can get at some information at near the--at between zero and five bars. This takes a small, one-inch plug, a ceramic plate.

It's underwater and it's connected to pressure. You can put pressure in, works just like a pressure plate system, you drive the water out and you can continually weigh this. Right now we're doing it manually, and you get a desorption curve. But then you can take the pressure off, let water go back in, it goes back up into the rocks and you get the sorption, and that's what we're looking at here, water potential versus saturation for two rocks.

Now we're looking at a hysteretic curve. In one rock, the Calico Hills zeolitized, we probably didn't really get to the air entry value. We're just moving a little bit of water out of some of the--maybe a couple pores starting to form some meniscus in the system, but in the Paintbrush Tuff we see a pretty good indication of hysteretic effects.

Now if you're starting a saturation and you go down to desaturation at five bars, then you saturate again, you're back up here. This value might be more that satiated water content. Now, if you have an event where this occurs, when you look at a water release curve again or if you look at desorption, does this, you know, is this going to go back on the same line or are you going to find that your scanning curves are really somewhere down in here? I don't know yet. One of the exciting things that we want to do is take some of the core and test it, and start it and move it just a little bit and see if we can find out where it is on this curve, and then go back and measure the curve and see where we were. Are we at an absorption phase or desorption phase of that rock? Is that rock wetting or drying? That's something we hoped to do in these--when we get these cores preserved in Lexan liners and back in the lab, we can go through and do all of these tests without even taking the core out of the liner, so it's really kind of fun for us. We're looking forward to trying to put some of these ideas to work.

This is another technique. This is a psychrometer, psychrometer microwave technique. All it shows is some--that we can get up to about 100 bars. This is for the welded units. You use a very small sample, no bigger than a No. 2 pencil eraser to do an analysis. Is that representative? That's something I'm not sure of yet, but we can do the analysis and we can try to find out, and that's what we're working on now, but we can get some fairly high water potentials, so we take the SPOC cells and we go up to here, and then we take the psychrometer microwave and we go on farther. We take centrifuge. We try to put all this together. We're still working on it. That's an area where--this is current work that's being done right now.

Mercury porosimetry doesn't look as good as what we had thought. One curve is fairly typical of what you would expect to see. The other curve goes just backwards in the welded unit of what you'd expect to see. There are some problems with mercury porosimetry. We hope to try to understand why they exist, but we have other techniques which we think work, and we would only use mercury porosimetry at the final--as a final test, because it is a destructive sample, and hopefully we can get some information from that if we can get the system to work, but we tried to put all this stuff together and get a water release curve.

Once we take that water release curve, then we try to calculate hydraulic conductivities, unsaturated. That's what we're after in the end. This is a graph of unsaturated conductivity versus saturation. The data are using nonsteady state techniques and the lines that you see are models, models that are based on the water release curve and one measurement of conductivity, the saturated value. So those are our best estimates right now of what the relationship would look like.

Again, you're looking at a lot of data points. What you want to do is simplify that by putting it into one measurement, maybe a simple model. But right now we have a little problem in that the systems and measurement don't quite agree, and also the models don't agree. The models don't agree with each other, although they're supposed to be based on the same principles in many cases, and they don't agree with the data except at a few locations.

This is an exaggeration. This looks better than it really is. We had to add a lot of data up in here to get these titles in, so if you expand this scale it gets a little worse. But, you know, we can make this any way we want it to be depending on our point of view at the time.

Another example, the same system, a measured system, and then the models. The Van Genuchten approach, the Van Genuchten equation doesn't work in this case. What we think is that the assumptions he uses to solve his unsaturated conductivity make certain assumptions about the water release curve. Those assumptions, we've found, if we use them we cannot match his water release curve with the water release curves we have, which we think we have good data for. So the error in the Van Genuchten is probably not related to the conductivity measurements. It's because of the water release curve, which we think we have a very good understanding of.

Well, the warning in this, I guess, is that because we don't know why Van Genuchten equation doesn't fit our water release curves, you have to be very careful in the results you get from groundwater flow models that are based on using the Van Genuchten equation to get unsaturated conductivity. You can see that we can have several orders of magnitude difference quite easily, and you need to be very careful about putting too much stock in models that are based on an assumption in the Van Genuchten equation that doesn't match our data.

With enough data, we can put a lot of this stuff together. We can find out the best techniques. We can make the curves, find out which models fit the water release curves, which we measure with confidence, and then go on to the unsaturated conductivities, which we think we can do with the SPOC cells now, through new steady state techniques and gas drive steady state techniques and centrifuge, and we think we can get good data. We think we can take all of these models and put them together and find out which models work the best.

One assumption people use a lot of times in soils is this idea of similitude in scaling, where in similar media you can use one equation to describe everything. Similitude may not be the case on Yucca Mountain; that is, the Van Genuchten equation may fit the bedded unit. The Brooks & Corey may fit another unit. You may not be able to use the same function in all the units. You may have to use different functions. We're not trying to find any universal equation. We're trying to understand Yucca Mountain, and we think we can do that with the techniques that we have developed in the laboratory, and with the modeling, but we've got to get back to the modeling. We've got to make sure the modeling is going along in step with the measurements we're taking in the laboratory, and then eventually into the field.

This is some of the results that we are looking for, just a simple profile. This would tell Joe Rousseau the kind of information he needs. In the case of a water profile, if he needs to--log of kilopascals, just to change things--one bar of 10, 100, somewhere in here is 100 bars. If Joe is going to have an instrument zone in here, he needs to know what the water potential is there because he could calibrate for it. If he's going to be in this zone or this zone, he needs to know that. That's what we're trying to get some of this information for.

We get other information, bulk and grain density. We can add that information to the geophysical logs and help them to do some calibrations, or just porosity. I just wanted to show you a couple examples of the kinds of logs that we hope to get out of some of this analysis, and the core matches pretty much with these units.

This is a summary slide of the matrix properties again, and Rob is going to talk a little bit about the fracture properties in his talk, so this is the matrix information. This is where the information came from, the references. What I wanted to point out, look at the high variability, the Calico Hills large variability. We don't know what represents the Calico Hills and we don't want to use a mean value. Same with the non-welded Paintbrush Tuff. Look at the non-welded tuff. Here you're looking at a welded, one order of magnitude, variability in saturated conductivity; five orders of magnitude in the non-welded, maybe three orders in the welded, five orders of magnitude in the non-welded, and then maybe two or three in the crater flats. So the variability seems to be fairly high. When you're looking at saturated conductivities in the Calico Hills that are looking very similar to the saturated conductivities in the welded units, you need to make some effort to understand the distribution of those properties; where is it, when is it important.

The Calico Hills is a very important unit in our study, and you can see by this large amount of variability--and that shows up in porosity--we've really got to get information from the Calico Hills. It's critical. When you have--if you have fractured system, a fracture flow, if the matrix is at this saturated conductivity, it can't take up the water as fast as the fractures can deliver it, and you might have through-flow through the fractures. And this system, the matrix might be able to take up the water. Where is that? We don't know yet. We don't know how much of this Calico Hills is at that saturated conductivity. We don't know how much is at the other. We really need to get more information there.

MR. WILLIAMS: Do you think you can still stay with that hydrogeologic framework with that kind of range of data?

DR. FLINT: No. No, what we're trying to do--

MR. WILLIAMS: That's what I was trying to get at in my earlier questions.

DR. FLINT: Oh. Yeah, what we're trying to put together is a different kind of layering based on the hydrology of the system. You know, the framework that we're looking at here, these are the geohydrologic units that are supposed to be similar, but they're not. Geohydrologic out of Montezar & Wilson, that's the framework, but you're right, we've got to get some more information on the hydrology and we've got to break these into similar hydrologic units.

In that G-Tunnel work, we could see zeolitized and silicified. That was all one unit. It was all a non-welded tuff, yet layers in that non-welded tuff acted just like the welded tuff. There were fractures. The only difference is that the fractures were terminated by the zeolitized bedding zones. In a welded tuff, the fractures are not terminated at that close spacing.

Well, this just shows that we do have a lot of variability. We need to find out if there are specific zones where this is the case. You're right, we need to redefine the units based on the hydrology. We might find that we'll have a whole set of units based on porosity, and then another set based on conductivity, or another set based on density. Which ones do we use? Do we have to put them all under one unit? I don't know. I don't think so.

DR. DOMENICO: Help me out here. The Calico Hills non-welded is unsaturated below the repository; correct?

DR. FLINT: This is the unsaturated portion of the repository.

DR. DOMENICO: Below the repository?

DR. FLINT: There are parts of it that are saturated below the repository to the north end.

DR. DOMENICO: But this is the unsaturated part?

DR. FLINT: This is the unsaturated below the repository.

DR. DOMENICO: I always felt the non-welded one was the most uniform and low permeability. I see the highest values and the greatest variation in the values. Does that surprise anybody? It surprised me.

DR. FLINT: If you look at the top of Yucca Mountain, if you look in the Paintbrush tuffs, you've got a bedded unit-- under the Tiva you have a bedded unit, then you have the Yucca Mountain non-welded member, then another bedded worked unit, then the Pah Canyon, then another bedded reworked unit, then the Topopah, so you're dealing with a large variability and it may be that those bedded units are the most variable. They seem to be important for capillary barriers or for retardation with the zeolites, but they also are so variable that we need to really make sure we don't make that assumption and just say, "Well, it's one unit, we'll use a ten meter average," because I tried to show in that non-welded, you're looking at one bar water potential. Then your looking at -50 bars, then you're looking at one bar. That's all in one unit.

MR. BLANCHARD: Alan, we need--Pat, we need to make a point that--which Alan would have made, I think--is that the Calico Hills non-welded unit he's referring to has a vitric and a zeolitic portion, and so you'd expect the properties to vary depending upon where you're at. Since we have such a small sample population for characteristics for all of these geohydrologic units, we have a large range and we really don't know what that means statistically until we have more samples from any one of these units on any one of the rock properties.

Alan, can you get to your summary? I think we're running overtime on this.

DR. FLINT: There it is, the old summary slide. I was waiting for him again.

We're currently refining the methods to measure hydrologic properties. We're having some trouble with some methods. Other methods are working out well. This new SPOC cell is really going to be a good system for us. It's going to be fully automated. We can get those hysteretic curves quite easily and we can do a lot of samples. We can run 20, 30, 40 at a time, so that's a pretty exciting area for us to work in.

Once we have enough data and a thorough analysis, we can start working at the models. We can look and see which models work. Again, we're trying to take a model and fit it to a whole bunch of data so we can simplify stochastic model processes so we can provide one or two bits of information with one core, rather than 100 data points with one core. We want to put it all together.

We have to be careful about similitude again, because there may not be similar media that we can use the same Van Genuchten equation or Brooks & Corey or Mualem or some other technique.

Sampling testing is based on a geostatistical analysis which will help to define the uncertainties in hydrologic structure. This is the thing we're trying to get at, the hydrologic structure. We're going to try to stratify based on similar hydrology, not necessarily on similar geohydrology or stratigraphy.

This was the description of the matrix property program. These are rock matrix properties. There are fracture properties which become very important in this system. You already saw from infiltration program that the fractured properties at the surface are very important. How important are they at the subsurface? Are there fault zones? Are there fracture zones through the welded, non-welded, non-welded units? That's some of the work that others are doing. Rob Trautz is going to talk about some of his work and the importance of the fracture system, and then Joe Rousseau's going to talk about the borehole instrumentation.

MR. TRAUTZ: What I will be talking about is the, pretty much the role of fractures in transport of fluids through the unsaturated zone, primarily looking at the fractured units which consistently are the welded units up at Yucca Mountain, the Tiva Canyon, the Topopah where the repository is located itself. The plan here is to test the surface-based boreholes using a straddle packer system, straddle packer having four packers with an injection interval in the center, with two guard zones on either side. We'll lower the packer system down the hole, inflate the packers, and do a gas injection test, meaning we'll measure the flow rates that go into the borehole or into the interval, the crusher response both in the injection hole itself and in a nearby observation hole if it's there. We have a couple clustered borehole sites as well as single hole tests planned for Yucca Mountain.

Downhole measurements that will be made will be gas pressures, gas temperatures, and we are planning to use thermocouple psychrometers at least prior to the test to measure relative humidities.

I will start out with the reason for doing the tests, of course, the regulatory concerns. We're looking at release of gaseous-phase radionuclides, tritium, Carbon-14, which Rich Van Konynenburg will talk about later on, appears to be a big problem. We have movement of water vapor and its impact on the liquid flux, essentially taking water out of the system through vapor.

We have gas flow mechanisms I've listed here and I will not go into a lot of detail because several of our speakers will be looking at this, Ed Weeks. Barometric pumping, topographic relief, geothermal gradient, Ben Ross will be looking at some modeling of that. We have heat loading, which Karston Pruess from LBL has looked at in terms of modeling exercises, and then diffusion, of course, will be another transport mechanism.

The in situ pneumatic tests will hopefully provide gas flow modeling parameters; permeabilities, porosities of the fracture system, and then it will also provide parameters for transient fracture-flow modeling. We're hoping to get at least out of the permeability results, a saturated permeability for the fracture network.

I'd like to go through the analysis that we're going to use for these gas flow tests, and then I'll end the talk with a example of a test that we've run already out at an analog site in Superior, Arizona and give some test results there. The gas flow equation is given in Equation 1, with permeability, porosity, p is the absolute pressure of the gas itself. We have "fee", which is the porosity of the system, C_t, which is the total compressibility of the system, and total compressibility comprises of fluid compressibility plus the pore volume compressibility, and then the time variable, of course.

Underlying assumptions in this equation, the most important ones are the ideal gas law was used to develop Equation 1. We have saturation with respect to the gas phase in the fractures equal to one, so the fracture system at this point I'm assuming is totally saturated with the gas, and this equation, Equation 1 is a non-linear equation which cannot be sold, at least rigorously, using known techniques, analytical techniques, and what we're going to do, I'm going to essentially use TOUGH, a simulator, to look at an assumption of linearization of that equation.

Now, permeability is also a function of pressure, and that's one reason why that's a non-linear equation. Klinkenberg showed this back in the forties. I will assume that permeability is not a function of pressure, that it is a constant due to the fact that we're going to be measuring a fairly high permeability of the fracture network and the slip effect due to--Klinkenberg's slip effect will be minimal, I feel, in that case.

We have viscosity, which is also a function of pressure and gas temperature, but I will assume that it is--well, it is a very weak function of pressure itself and No. 3 up here, I'm assuming in the first place that the gas is expanding isothermally through the system and we--measuring the temperature, the gas temperature's injection during the test, we actually did see this. The temperatures didn't vary very much at all.

We have the pore volume compressibility here, which is, of course, a function of pressure.

DR. LANGMUIR: Rob, before you change that, Don Thorstenson was involved in a paper recently in which he suggested, I felt--I thought--that the ideal gas law didn't necessarily apply on saturated media. Is that simply stagnant conditions, or is that a flowing condition like this?

MR. TRAUTZ: I guess I'm not familiar with that paper.

DR. LANGMUIR: This is Water Resources Research, about two months ago. Don is in the audience, so I don't know--

MR. TRAUTZ: Due to natural vapor?

DR. LANGMUIR: Well, Don is back there, so if he--

MR. TRAUTZ: Well, in ideal gas, at least for, let's say, the compressible fluids that I'm going to be injecting in formation, which is air, or compressed air, ideal gas law usually holds over, well, up to ten atmospheres, so I guess I'd be willing to talk to Don and find out what he has to say about it.

MR. THORSTENSON: I'm Don and I have to say that's not what my paper says. Assuming we're talking about the same paper; that is, Water Resources Research a couple of months ago on fixed laws, et cetera, et cetera, that was basically aimed at trying to look at some alternative means of looking at combined diffusion flow than at least have been around in the earth science literature, but the paper itself never deviates from 25 degrees and basically it's perfectly happy with the gas law.

DR. LANGMUIR: Okay, sorry. My mistake, perhaps.

MR. TRAUTZ: The range of values of compressibility of the matrix and also of the two fluids here is shown in this diagram. Most of this was lifted directly from Freeze & Cherry, who have a table, and those values are indicated by an asterisk. I'm going to assume that the pore volume compressibility of the matrix is essentially negligible in comparison to the gas compressibility itself. As you can see here, our jointed rocks at Yucca Mountain, at least in the saturated zone, compressibilities are in this range here so we're going to be two to three orders--they're going to be two to three orders of magnitude smaller than the actual gas compressibility, so I'll neglect that component.

Now, Beta, which was the fluid compressibility, for an isothermal gas, that's one over p, or a gas expanding isothermally is one over p, Beta being the coefficient of isothermal compressibility. Now, I'm going to--since it's one over p, it's coming into the non-linearity of that gas flow equation in Equation 1, and I have to linearize it. I have to assume it's a constant and solve the equation in terms of the dependent variable, which would be p^2, and here I've introduced the dimensionless parameters and I'm going to take the same approach that hydrologist do with type curve matching and you essentially non-dimensionalize the flow equation. You can then solve that equation rigorously using an analytical technique like Laplace transform, inverting it into Laplace--or, pardon me--transforming it into Laplace domain, then inverting it back either numerically or analytically.

And that's what we've been working on, a series of programs that will allow us to do that for different boundary conditions and initial conditions. TMAKE.FOR is the program that will allow us to do that. TMATCH will allow us to throw the type curve up on the screen, throw the data set over the top of it, and then visually do it on the computer, a visual match, get the type curve match off of the screen, and then it goes into CALC.FOR, which will then calculate the permeability and porosity of the system. So it's the same techniques that hydrologists use, except we're using a linearization assumption to do it for the gas flow.

Now, how good is the linearization assumption? I took TOUGH, which is a variably saturated two-phase--or actually, three-phase flow code developed by Karston Pruess, and I did a gas injection test using the TOUGH code. It was a one meter interval with a radial cylindrical configuration. The bore hole was actually put into the simulation explicitly, and then I compared it--the data points are actually from TOUGH, what the generated pressures and the gas phase were--I compared it, or actually matched it, the type curve match, with two fairly well-known solutions in the oil and gas industry, Van Everdingen & Hurst, 1949. It's a finite diameter well solution, and they were the first ones to introduce the concept of well bore storage. Since the well bore was actually explicitly modeled in TOUGH, it did exhibit well bore storage and this is the data match that I have come up with.

The actual output, what I'll consider the true simulated data set, that's the true permeability that went into TOUGH was .8 x 10^{-15} m^2 and a porosity of 17 per cent. The type curve match that I got out of the program which would essentially be the linearized equation, was .8 x 10^{-15} m^2, so there's--apparently there's very little difference there, and the porosity value is 18 per cent. And when I played with the program quite a bit, it was usually within 2 per cent of the true permeability with the visual match, because the visual, you'll essentially get a little bit of air just being able to eyeball the data, and this was usually within 5 or 6 per cent, the porosity value.

Switching gears, we have--

DR. DOMENIC: Are you saying that this method's going to permit you to calculate the permeability of the unsaturated zone if it were saturated; is that what this is about?

MR. TRAUTZ: The fracture system, primarily concentrating on the fracture network because--

DR. DOMENIC: The permeability of the fracture system, assuming they were saturated?

MR. TRAUTZ: Assuming they were saturated with gas, yes; completely drained.

DR. DOMENIC: Okay.

MR. TRAUTZ: We have done some prototype testing out at Apache Leap tuff site in Superior, Arizona. This shows the borehole locations. They're drilled from the surface to a total depth of, I think, 45 meters along the access of the boreholes. They're drilled at a 45 degree angle, and these boreholes do not actually intersect. They are offset by 5 meters coming out of the plane of the screen, so the distance between the injection points, shown here in red, and the observation interval in the other borehole here, is about 11 meters.

This site is a non-welded to partially welded tuff and we performed essentially a 24-hour gas injection test and what I'm showing here is the actual dataset. We've got some stepping in this region where we really didn't have the resolution or accuracy of the pressure transducer that we needed, so I'm de-emphasizing the early time data here. Two solutions. We have the classical Theis solution and a spherical flow solution and you can see here we're getting a very good match, at least with the Theis solution.

Now, if you were to move the dataset down, you could get just as equally a good match on the spherical flow model, so it shows essentially the non-uniqueness of well test solution which many hydrologists, I think, are well aware of, and--but we do have one nice recourse, and that's the actual values of permeability and porosity that we measure from the two models. From the radial flow model, we get .3 x 10^{-11} m^2 for the permeability, and a very unrealistic porosity value of 123 per cent, so we can eliminate that model. The permeability of the system for the spherical flow is .9 x 10^{-13} m^2 and we're getting a porosity value of 7 per cent.

MR. WILLIAMS: What would your early time data have done had it been correct?

MR. TRAUTZ: Well, I don't really know. Let me give you some more information on the matrix properties first. The University of Arizona is actually the group that is operating that site, and they have a very good dataset on the matrix properties. Now, at the average field suction of 80 kilopascals, we have a mean permeability of about 23 x 10^{-16} m^2. We have a maximum from the core samples, maximum permeability of about 563 x 10^{-16} m^2, and we're measuring a permeability of 977 x 10^{-16} m^2, but the porosity value, I expected--so we have a permeability value which is much higher than the matrix, and we're seeing a porosity value which is essentially what I would consider much higher than what the fracture system would see if the early time data was indicative of the flow, gas flow through the fracture network itself. So what are we looking at?

One possibility may be it's truly acting as a double porosity system, but a double porosity system, at least the late time data will give you these parameters. It essentially gives you a fracture permeability and it'll give you essentially an average porosity or storativity term, so that may be one explanation for this type of system, and I think Ringarten (phonetic), in '82 showed that you don't necessarily get the full transition for a double porosity model; meaning you see the early time data, which is the fracture data. It goes through a transition where you get the linear slope on a simulog plot, and then you get the late time data, which is an averaging process, or it's looking as an equivalent homogeneous porous media. So you don't always see both curves, and it depends upon the transition variable, the block to matrix permeability value which he designates as lambda, or Warren & Root do. So we may be seeing a double porosity response, but we're not certain at this point.

We plan on going back and doing a great deal of more testing in that area. We're also going to try to look at anisotropy of the system using a method Paul Hseih developed, and we'll linearize it using the gas flow linearization assumption, so at that point I'd like to entertain any questions and...

Joe Rousseau will be speaking next, and Joe, as Alan pointed out, we've got the gas permeability test work, which will go on soon after the boreholes are installed and after the permeability testing is over, Joe Rousseau will move in, stem the holes and measure in situ flexes, or not flexes, but potentials, temperatures.

MR. BLANCHARD: Thanks, Rob.

We originally had anticipated that lunch break would start at about 11:45. Don, do you want to stay with that or do you want to take 15 minutes for Joe?

DR. LANGMUIR: Let's continue with Joe.

MR. BLANCHARD: Okay.

MR. ROUSSEAU: The title of my talk this morning will be, "In Situ Monitoring," and that is looking at the fluid-flow potential field. I've got about 30 overheads. I think I can get this thing done in 15 minutes. I've thinned the talk down by about 50 per cent. There is an addendum package that will give you additional detailed information.

In the presentation I'd like to cover four areas. One, I'd like to define the purpose and scope of the in situ measurement program. I'd like to talk about the measurements real cursory to give you an idea of what sort of accuracies and precisions we're trying to achieve. I'd like to touch on the UZ-1 experience, because here we have about four years worth of data. I'd like to also talk about the G-Tunnel experience where we have a year, about 13 months of data and we consider that an analog site, something that might be equivalent to the Calico Hills vitric unit. And lastly, I'd like to summarize by outlining what I consider to be the benefits of in situ monitoring.

The purpose of in situ monitoring is to define the liquid and vapor flux fluid-flow potentials. Here we're talking about pneumatic pressure, that thing that drives convective flow of gases; vapor pressure potentials, which is the diffusion potential mechanism; water potential, which is the liquid component of the fluid that involves both matric and osmotic, and finally, the thermal potential, which impacts every one of these different potentials, and also is a energy potential in its own right.

I'd like to touch this real briefly. Alan did some discussion about the site-specific, if you will, the surface-based borehole drilling program. We have approximately 31,000 feet of borehole that we'll be instrumenting. The deepest borehole's about 2500 feet. Now, these are feature-based. Four and five boreholes are in Pagany Wash. There's also indication there may be a fault in Pagany Wash. 14 in UZ-1. UZ-1 instrumented in 1983, and 14 will also go into use at this site here to take another look at this perched water occurrence that they encountered at about 1250 feet of UZ-1.

We have two boreholes across the Ghost Dance fault, UZ-7 and 8. We have a three borehole complex in the imbricate fault structure; boreholes 9, 9-A and 9-B. We have the topographic boreholes; that is those on the Solitario Canyon escarpment, 6, 6S and 2, 3; a pair of boreholes that'll straddle Solitario Canyon, UZ-11 and 12; a borehole at UZ-13, which was designed to take a look at the thickest portion of the Tiva Canyon unit, and UZ-10, which doesn't necessarily intersect any particular stratigraphic or structural feature that's different than what you might see in a systematic drilling program.

The scope of the program involves instrumenting 16 vertical boreholes, 12 1/4 inch diameter dry-drilled, cored. We've adopted a solid stemming design. We've looked at the options of a packer assembly, but putting in 16 instrument stations in a borehole with packers doesn't seem like that will be a very successful endeavor. We have 16 instrument stations per borehole. Now, we've gone with redundancy, in which we're going to have two pressure transducers per station, two thermistors per station, two psychrometers per station, one gas sampling "U" tube arrangement--and I'll get into some detail on that in a minute--two solenoid valves per station, which allows us to regulate the flow of gases during vacuum withdrawal; and secondly, allows us to do in situ recalibration of the pressure transducers.

We plan approximately 1600 sensors in this program. Currently there are no plans to instrument the Sandia systematic drilling program boreholes, though that is a possibility down the line. Read sensors once every five hours, and the reason for opting for five hours is to get away from temporal bias in the dataset; that is, we won't measure at midnight every day. We have everything fully automated on PDP 1173 computer systems. Duration of the monitoring, based on results of UZ-1 instrumenting and G-Tunnel work indicate that we're looking at about three to five years in which to get the data.

Types of measurements. Pressure. Pressure transducer does have an in situ recall option. We're trying to achieve an accuracy of .005 psi. We were fairly successful in G-Tunnel and coming very close to that. This is at two sigma significants. Our total pressure range from the top of Yucca Crest to the total depth at UZ-6--or UZ-2, I should say --2500 feet, so we have about one psi absolute to deal with. We have ability to resolve a ten-foot high column of air to two sigma significants. Temperature, .005 of a degree Centigrade accuracy at two sigma. Our temperature range will be between 15 and 40 degrees Centigrade. Water potential, we do have an in situ verification option which is related to gas sampling, which I'll talk about in a minute. The accuracy is relative, about .5 to 1 bar in the range of about -1/2 to a

-70 bars.

I should mention that we have a redesign on the psychrometer. This is not a standard device anymore. It contains six wires. We put a special pair of wires in there. We can monitor the current draw during excitation so that we can evaluate any problems related to drift.

Another portion of the measuring program is gas sampling. This particular component of the program is designed to inhibit condensation during the withdrawal of gases. One of the primary users of gases is the geochemistry program and they'll look at various isotope ratios. We also have a need to be able to preserve the mass of the gas as we bring in a pole, primarily to check those psychrometers. So we're shooting for about five years of monitoring right now and there's an active program by Dr. Brown, who is in his fifth year of monitoring using psychrometers to evaluate the long-term reliability.

The program requires introduction of a dry-carrier gas. We measure the mass flow of both the dry and mixed gases; the dry gases at the surface before they're introduced downhole and the mixed gases as they come uphole. We measure the dew point temperature of the mixed gas combination, and then we back-calculate what the vapor pressure is at source, having handled less temperature.

I'd like to get into the next section a little bit, and just very briefly highlight some of our UZ-1 experience. First, we found that there were significant reversals--and I should refer, Parviz did a lot of work here. Parviz is in the audience, and I'd like to give credit for that.

We did see significant reversals in water potential gradients both across hydrogeologic boundaries and within hydrogeologic units. There was a relatively high thermal activity at depths greater than about 100 feet. Now, what the significance of this is at this time, I don't know. Unit hydraulic gradient assumption across a thick section of Topopah Spring welded unit was not confirmed by field measurements. Equilibration took on the order of 18 to 24 months, though there were some stations that equilibrated in just a few months. So we have different levels of physical activity going on with that at Yucca Mountain. UZ-1 has also served as a useful prototype in which to evaluate the instrumentation, determine what sort of accuracies, precisions, stabilities and reliabilities we need. What do we need uphole in terms of electronics, instrument shelters, et cetera, to get good measurements?

This a schematic section of the borehole at UZ-1, basically instrumented with psychrometers and pressure transducers and heat dissipation probes. A typical psychometric station consisted of a 200 mesh sand bounded on bentonite on both sides, silica flour filler, and then concrete grout plugs. It also contained screens for sampling gases.

This is what we saw in terms of water potential. I referred earlier to the significant breaks in the water potential profile with depth. We see these occurring across bedded/non-bedded units up here where we get a very sharp demarcation. We see it occurring across the boundary between the Paintbrush non-welded unit, Topopah Spring unit. We see it also occurring within this thick section of homogenous/near-homogeneous type rock that does have variability in fractures with depth, so that might be a fracture effect right here, or it could be possibly related to the occurrence of perched water table at 1257 feet.

Temperature profiles here. Now, there's two types of data being shown on here. One is actually in situ temperature data, which is your out-of-curve. In a curve in here is data taken by John Sass, in which he inserts a tube down the central stemming tube and carries a thermistor down in a water environment, so there's about a half a degree Centigrade or Celsius difference in temperature here. What this thing shows us is that we do have sort of a convective up--or convex upward profile in temperature, and that has been assumed, perhaps, to be related to the movement of vapor from depth where the vapor is warmer, creating this sort of non-linear, if you will, thermogradient effect. You also see a lot of thermal activity here at about--greater than depths of about 100 feet, which is about twice what one would expect. I think with thermistors, we'll be way into the range of ability to try and resolve some of these thermal effects. These temperatures were measured with T-type thermocouples.

Now I'd like to get into some of the G-Tunnel experience. This was a prototype effort to look at using packers, mostly for exploratory shaft investigations. Two boreholes were instrumented, three stations per borehole. They've been monitored now for exactly 13 1/2 months. We pulled the horizontal borehole last week. The significant thing that we saw here was the liquid and vapor phases with the near-field wallrock were not in equilibrium, and they were being affected by changes in the annual temperature in the drift, changes of only about 1.4 to 1.5 degrees C. annually.

Convective transport is a predominant mechanism for drying the near-field wallrock. Preliminary calculations, looking at energy balance; specifically, the available heat, the heat capacity of the rock, the thermal conductivity of the rock indicate that the rock probably desaturated on the order of about one-tenth of a per cent per year. These are very preliminary calculations. I will be giving a paper in March. You're all welcome to attend, and this is the title of the paper in which I will produce more detailed information.

To give you some idea of the lay of the land in G-

Tunnel, we're about one mile underground beneath Ranier Mesa. Two boreholes were drilled, one 15-foot vertical. The vertical scale here is two times the horizontal. The horizontal borehole, 150 feet, 4.125 inch diameter, dry-drilled, continuous coring.

DR. DEERE: What do you mean, one mile underground?

MR. ROUSSEAU: One mile laterally underground, okay? We're about 6,170 feet in elevation, and I don't know how deep we are from the top of the mesa itself.

DR. DEERE: It must be about 13-1500 feet?

MR. ROUSSEAU: It's quite a bit, it's quite a bit, and I can't give you that number right now.

Okay. We instrumented six stations, A, B, C in the vertical hole. We used single packers in here. In the horizontal hole we instrumented D, E and F, locations 120, 90 and 12 feet. These little hash marks you see on the bottom here are the locations of fractures. These little other marks that you see in here are the silicified zones which Alan talked about a little bit. We measure the pressure, temperature, water potential once every five hours. We carried a mercury barometer inside the open drift, also carried pressure transducers and thermistors at Stations G and H.

The material that we're working in here has porosity anywhere from 35 to 50 per cent. Some of that porosity has to do with whether the unit you're working in is silicified or not. It's zeolitized. It's Tunnel Bed 5. It's closest analog would be the Calico Hills vitric unit. We had core samples run by Holmes & Narver, seven in the horizontal hole for various properties which I'll talk about in a second, and we had two samples in the vertical hole that were also analyzed.

As part of the addendum package, you'll find these little dividers in here and that's where you can start to insert the addendum sections there so you can get some continuity. I'm not going to talk too much about this. It's a pretty busy slide. What I wanted to highlight here is the porosities that we're looking at and our saturation, saturations in the vertical, 58, 68, and in the horizontal, somewhere around 56 to 73 per cent saturation.

The first station that gave me nearly a heart attack was Station B. We spent a lot of time on it, and all of a sudden, look at what we saw. We weren't sure whether our data was going south on us. This was within a couple weeks of collecting data and, in one sense, this station equilibrated almost immediately, but it equilibrated because it's very, very active and we intentionally went after a high angle vertical fracture in the vertical hole at Station B, isolated with two packers.

We see its water potential record jumping anywhere from the equivalent of 2 1/2 bars down to greater than what the psychrometer can actually read, so I took a section--this was about a year's worth of data in here. I took a section of the psychrometer record at Station B and blew it up, 4400-4900 hours, superimposed on that the pressure and the temperature waves, and what we're seeing here is the fracture communicating with the open drift. Now, mind you, it's only ten feet deep. With Station F, which was 12 feet in the horizontal, they didn't show any of this sort of activity, and intersects many, many very closely spaced fractures.

So what we're seeing here is as pressure builds up we're producing, or driving cold dry air from the drift into the station, shooting the psychrometer off scale. Things start to stabilize again and you get a very flat plateau on the psychrometer. The reverse happens. When the pressure drops in the station, it starts to stop either warm air, moist warm air from the matrix or from deeper, and then the psychrometer--okay, then the psychrometer gets wet again. Well, this was repeated many, many, many times.

There's something about this record which may not show up in some of your data is that it very, very closely resembles the high frequency amplitude changes we see in pressure. So in the wintertime you have your very, very high frequency, high amplitude stuff, and in the summertime you have just the opposite, low frequency, low amplitude stuff.

So here we're actually seeing the communication between convective flow processes, water potential and vapor. When I use the words water potential right now, put it in parentheses, because we're not looking at water potential in Station B. We're looking at a vapor phenomenon.

In situ temperature, like I said, I've taken about half the data so I'm going to be popping back and forth between a vertical hole and a horizontal hole. This is the temperature record in the vertical borehole DI-1. The only thing I want to show here is illustrate, or let's say, two-tenths of a degree C. in here. Our confidence in our measurements are way out in the .005 of a degree Centigrade range. When we were in this zone in here, we were way out to the .010 of a degree Centigrade resolution power, so it gave us a lot of confidence. You can look at Station B and look how noisy its temperature is, and that's the station that communicates with the drift.

Down here we have Station G being shown. Now I use this data and assume what we're dealing with here is a smooth, harmonic sinusoidal function of temperature with time in a later analysis, and these little close hashered arc sort of things are the weekend events in the drift when the ventilation system is shut down, half of it's shut down, okay, so we get a very coherent temperature record during the weekends.

I'd like to throw up a temperature profile here for the vertical hole. Now, this is where I think we saw our most significant information. What I have calculated and what I'm showing here on the right and the left are the calculated temperatures with depth based on some data that Sandia produced for the heated block experiments, looking at--well, the data that they provided me was basically thermal conductivity and the volumetric heat capacity, which I could compute the diffusivity of the rock and thereby compute what the predicted temperatures should be at depth.

What we actually saw, looking at the peak summer and winter temperatures, the low temperatures in the winter and the highs in the summer, was something slightly warmer in the wintertime and something slightly cooler in the summertime. Now, this has been attributed to the effects of evaporative cooling in the summertime, keep the wallrock cooler than they normally expect, and then in the wintertime, the condensation.

I'd like to throw up and quick and dirty in situ water profile, and then I'm going to show you all stations, A, C, D, E and F. I've taken B out because it would wash out the scale on the left-hand side and we wouldn't see any information here.

Stations A and C, reached the apparent position up here where it looked things were equilibrating, but then all of a sudden as temperatures started to go up, they started to get wetter, or apparently they were getting wetter. In fact, what was happening is they were evaporating interstitial pore water, producing a higher relative humidity at a higher temperature.

Stations D and E, which were located at 90 to 120 feet--and this record looks a little shaky in here, but we're looking at resolving tenths of a bar in here, equivalent--I'll show it to you in microvolts. These were the washout sections. I did that intentionally because we were doing an in situ pressure transducer recall and a 2-watt solenoid valve shot the water potential right off scale again. This repeated on every station.

So I think the one that was very drastic here is Station F. It just kept going up and up. Now, we got our last piece of data last week and, lo and behold, Station C has made a reversal, which is what we had predicted would happen. It would reverse. It has to reverse in order to maintain the energy of the system.

Now we used data here from the core samples that Holmes & Narver analyzed for us and we used the capillary pressure survey data to come up with--and Kelvin equation. Let's see what the vapor phase component looks like. So what we have in here are data from core, and we get a pretty nice little fit in there, but of course, vapor pressure is most dependent upon temperature, so we had to use a temperature that was measured in August, at the same time the systems were drilled.

What we see at D and E here over a year's time is basically no changes. Here are the calculated saturated vapor pressure. Our data are falling into a very smooth, and what you would expect, vapor phase diffusion gradient. In the summertime, the gradient is between the drift and the near-field wallrock and in the wintertime that reverses. Here are your saturated vapor pressures, winter and summer, for two actual measurements. Now, you have to look at the DI-1, which is the vertical hole to expand on this information, but basically this same thing was happening.

These are the actual measured water potentials, measured from core and from in situ measurements. Now, we see some slight different in here in the water potential measured from core, but not too bad. I mean, we're only a couple bars here difference, and this is what we actually saw in situ. It's showing slightly wetter from the core measurements than what we measured, but water potential is very, very sensitive to temperature, so there is some reason that these are different. One, they're measured at a slightly different temperature than what we had in the field, which is about 15 1/2 degrees C. Here you're looking at measurements in the lab, which is about 25, which isn't a lot, but it is enough to drive it in that direction.

Pressure transducer record looks something like this, and what I wanted to use this for, as I said earlier, I felt that the dominant mechanism for drying that rock is convective flow of gas from within the wallrock towards the drift. Here's Station G. Stations A and C in the vertical cannot be separated with the level of accuracy we've got right now, but what we are seeing is a positive pressure gradient from Stations A-C to Station G, driving moisture into the drift and I suspect that what the--the mechanism here, of course, is the vacuum ventilation fans as they draw air from the back end of the drift and outward, so we're maintaining that all year 'round.

In the horizontal hole we have a positive pressure gradient between all rock in the hole and Station H, but there are places in here where gradients reverse sporadically between D and Stations E-F, and haven't gotten to the full amounts as to the water potential record, but there's indications that those little slight rolls that you saw in there could very well be related to these changes in the convective pressure gradient.

Okay, in summary, I'd like to summarize the G-Tunnel--there is actually two summaries in here--work. Okay, pressure gradients indicate that convective flow of interstitial pore gases continually between the near-field wallrock and the drift, and measured temperature changes in the near-field wallrock were, in the summertime were cooler and in the wintertime warmer than predicted, and I believe this is due to the latent heat vaporization processes.

The last one, changes in what I called apparent water potential between winter and summer are really the--what we're seeing here is just a dynamic phase change process between liquid and vapor. So really, the thing is like a pump and it pumps out. Now, if we'd gone through another year here we probably would have seen a buildup, a buildup of water inside the rock while the water potential apparently goes lower, but it has to because the temperature's dropping. It has to because the relative humidity's going down.

I'd like to summarize with the benefits of in situ monitoring. These are the sorts of things I think we can get out of this sort of a program as opposed to a static one-time measurement from a core taken from the geologic environment. One, we can look at the impact of episodic events. These episodic events don't necessarily have to be precipitation. They can be a large pressure front moving in. They could be a very cold spell being developed. We can look at the impact of diurnal, seasonal and annual harmonics in here, and we're talking pressure and temperature and probably--well, pressure and temperatures. Leave that one right there.

We can obtain the pneumatic pressure measurements and the temperature measurements, things that we cannot get from the core measurement process. We can evaluate the equilibrium processes. Now, you saw Station B equilibrate almost overnight, but it's a very active, dynamic station, and those are the sorts of things that I think that we have to look at at Yucca Mountain. Some of those other stations probably took close to a year to get to an equilibrium platform.

We can isolate discrete intervals of interest as in fracture zones, stratigraphic and structural contacts, and hydrogeologic boundaries, and I should call one--I'm separating stratigraphic and structural here from hydrogeologic because we have water tables sitting down there and we need to know what the energy transfer system is between the water table and dry rock, and we can get that with in situ monitoring, too. It also provides a platform for isolation of rock gases for geochemical analysis, and that concludes my talk.

MR. BLANCHARD: Thank you, Joe.

You mentioned that you had pulled your equipment out. Is there anything left of your--

MR. ROUSSEAU: I had the vertical hole be pulled out Wednesday or Thursday, plus the electronics.

DR. DOMENICO: Joe, has this been done with conventional instrumentation that you would normally use in granular soils, or have you guys been developing other things?

MR. ROUSSEAU: Well, we've basically taken the existing instrumentation that's on the market. We've done a minor modification to the psychrometer. What we have been looking for, though, is sensors that give us a very high degree of resolution power. We are using this--we are actually calibrating in the lab ourselves. We cannot get the calibration relationships that we need that are both temperature sensitive, so we're carrying the instruments to their nth degree.

Thirdly, we are using specialized electronics, Keith Ly systems, Hewlett Packard systems. We're not using black box systems, nano voltmeter cards, where we know what our thermal offset is using the Jfet switch, which is like 10 nano volts. So we have actually taken off the shelf stuff and applied it to what I call high precision, high resolution, high accuracy, long-term stability and reliability-type measurements.

DR. DOMENICO: You say these are calibrated in the lab?

MR. ROUSSEAU: We have our own calibration lab that we have done prototype development work on. We're wiring it up right now. We'll go through a last phase of testing, in which we're going to test long wires. The longest psychrometer we have worked with right now is 500 feet. We have 2500-foot psychrometers which we're going to be evaluating.

MR. BLANCHARD: Do you want to break for lunch now, Don?

DR. LANGMUIR: Yeah. Let's try and get back as close to one o'clock as we can. That may be optimistic. There are three restaurants apparently across the street in the hotel. We may want to spread around between them, there's so many of us.

(Whereupon, a lunch break was taken.)

AFTERNOON SESSION

MR. BLANCHARD: The next general topic was entitled, "Importance of Fracture versus Matrix Flow," and our first speaker is Paul Kaplan, from Sandia National Laboratories, who will talk with us about the modeling work he's been doing in conceptual models for fracture and matrix flow.

Paul?

MR. KAPLAN: I'm going to spend the next 15 minutes trying to present you with a broad conceptual framework in which to put pieces of what you've heard this morning and pieces of what you'll hear this afternoon. Since we're going to be talking about modeling, I'd like to offer a definition first.

Modeling is a process, not a product. The process I'm talking about is the analyst's testing, the consequences of the assumptions he makes about the natural world. The purpose of modeling within the context of an engineering site selection problem is to identify the circumstances under which the site would fail to meet the criteria for performance that are specified.

The primary conceptual issue at Yucca Mountain is the transport of mass in a system comprised of fractured, porous rock under conditions representative of the unsaturated zone. Major conceptual assumptions are as follows--and I want to thank any number of PI's within the project for input to this particular overhead, and the list has been compiled over the years through a number of different methods, through observation. We've heard Al Flint describe his observations at the site today. Laboratory studies, numerical experiment, field studies and natural analog studies, everything on this list now I would argue that most of us believe are essential elements of the Yucca Mountain system.

Variably saturated. By this I mean that we recognize that there can be the presence of, again, positive heads in the system, perched water, if you want to call it that. The system is multiphase and multicomponent. Multiphase, there is both water vapor, liquid water, there is gas in the system. The water is not pure water. It's a geologic system. THere are heterogeneities within the system.

Non-linear. I put this up now as a prelude, hopefully, to the future when someday we'll discuss performance assessment. In terms of the prediction problem, the non-linearities can be the essence of the problem. The system is multidimensional. It's anisotropic. Some of it comes from, again, well-recognized mechanisms; the fabric of the rock, the fact that there are layers, the difference between layers here or layered here is one of scale, and within the unsaturated zone, at fluid potentials of less than zero, the permeability potential becomes a function of one of the state variables.

The system is transient, at least with respect to boundary conditions, how dynamic those conditions are, how deep those transient propagates, I think, is one of the issues that will remain unresolved, at least at the end of the next two days. You'll hear speculation as to that.

We've already heard talk of statistics and geostatistics. Implicit in that, if not explicit, is the fact that we are dealing with a non-deterministic system. Flow systems have been known to be non-deterministic for many years. We solved them deterministically because we couldn't do any better.

Getting down to issues that are specific to a fractured system, let's start with the diagram of the conceptual model first. Imagine that we have two intersecting fractures through a rock matrix. We slice a plane normal to that and we look down. We've started to desaturate the fractures, which are the void spaces here. Yellow grains are the rock matrix, green--for reasons I don't quite understand, but this was left to the graphic artist's interpretation--is the interstitial water, and we show a flow vector through here.

There are some very important assumptions just in the drawing of this particular model. One is that the mechanism that's governing, again, the distribution of fluid in here and the fluid potentials is a capillary mechanism.

One of the ways of expressing that is to say that the ability for this fracture to retain water is inversely proportional to its aperture width. As it drains, the large fractures or the large void spaces drain first, followed by smaller and smaller ones.

Another fundamental assumption, particularly in modeling the unsaturated zone, is that the fractures are rough wall. Again, the implications of that assumption are that you can describe the void space as something analogous to a pore size distribution.

As a consequence of those assumptions, you end up with a model that relates the saturation within the fracture to a function, again, of the suction head or the matrix potential and it's a non-linear function. And all we're saying here is that the harder you suck on the rock, the more water comes out until you reach a certain point in theory that, again, no matter how much more suction you point, there is some water retained within the system. The conductivity now is, again, also a function of potential, the state variable. This is a relative scale, one to zero. Again, as you start to desaturate the rock, the conductivity of the fracture decreases. This is the same way we treat, again, a soil or an unconsolidated material.

I'm going to put up what is basically one of a series of numerical experiments to try and show, again, what the implications of these assumptions are, at least in a modeling exercise. Two-dimensional domain, a thousand meters across the base, 600 meters--this is from the--what is called Hydrocoin, the Hydrologic Code Intercomparison Project. It's an international project. Level 3 refers to that stage of the project where they're trying to determine whether or not, again, the application of numerical problems to high level waste repositories is even feasible, and it's also the case, too, as a sensitivity study. There were 93 two-dimensional cases run.

In this particular one, boundary conditions are a constant one millimeter per year flux along the top boundary, no flow boundary over here, fluid potential zero, elevation potential zero here at the water table, and at least one way to represent a possible fault, such as Ghost Dance Fault, is a no flow boundary. This is a plot of only the fracture saturations that were generated in the model. The yellows, again, very low values, going up to blues for the highest.

We can see, as a consequence of the modeling--or one of the consequences of the assumptions in this model--that paths--these are paths here of particles released along the upper boundary. Travel times were calculated from a repository horizon here, but we can see that the substantial amount of flow in here--flow lines are not one-dimensional. One of the consistent things that came out of the two-dimensional modeling is that there are any number of circumstances, at least given the assumptions we're capable of modeling now under which flow is not one-dimensional, one of the other circumstances that came out that was no surprise to us--we had predicted it, the magnitude in some cases surprised us--is that with respect to a performance parameter like groundwater travel time, the 2D is conservative, or in other words, the 2D predicts much faster travel times than 1D given the same boundary conditions, the same material properties. Depending on, again, what you use for material properties and boundary conditions, it can be a ten order of magnitude difference.

DR. DEERE: Between which, the--

MR. KAPLAN: This would be--this could be 10,000 times faster than the same case in 1D.

DR. DEERE: And your D's are horizontal distance and vertical?

MR. KAPLAN: Yeah.

I don't want to trivialize what we've learned from now many years of numerical modeling and a tremendous amount of CPU time, but I'd say one of the major insights that comes consistently through--again, in modeling the assumptions we went through earlier, in treating this as a system where there are both fractures and matrix, is that you see within most of the systems you model a continuum of three flow regimes; a fracture-dominated flow characterized by high fluid velocities. And again, we've heard some of this described by the PI's out in the field. You'll hear more of this described during the course of the next two days.

Concurrent fracture and matrix flow with strong interactions, and then, again, at very low fluxes or depending on other conditions, matrix dominated flow, tortuous flow paths around the drained fractures, simply a function in the model. As you increase the suction, the matrix tends to dominate; decrease the suction, increase the saturation, the fractures tend to dominate. This is all--and I'm going to argue intuitive in hindsight. Some of this, had we been able--had it been this intuitive four or five years ago, we could have saved ourselves some grief.

Getting back to the purpose of modeling, we're going to take a very quick look again at the use of modeling and the importance of fractures with respect to the performance parameters. The parameter we'll look at is groundwater travel time. Again, as a consequence of numerical experiments, one of the consistent things that comes out of the models is that short liquid phase travel times are sensitive to the very existence of a continuous fracture pathway. Do any modeling without a continuous fracture pathway, with materials of permeabilities as low as we think they are, then it's very hard to generate a failure scenario.

If continuous pathways exist, perched water is not required to generate travel times of less than one thousand years, and I've got a quick illustration of that in this next simulation. This is a one-dimensional Monte Carlo simulation. It's at half-millimeter per year flux. There are ten parameters in the model; five that describe the matrix, five that describe the fractures. There are 11 hydrostratigraphic units in this, so all together there were 110 distributions that went into the model. The parameters are correlated and it's through, again, a section where I had both data and the geologist's description of USWG-4.

I want to illustrate two things with this. One, again, as we see in the Monte Carlo simulation, we see this three-phased continuum. We see fracture-dominated flow here, again, indicated by very short travel times, a separate peak in the output distribution. Area in here where the system is responding, again, both fracture and matrix are interacting. You cannot distinguish between the two of them. The long travel time's dominated by the matrix.

You'll notice here with respect to, at least, the thousand-year travel time, we have a substantial number of failures. I did a scattergram of the fluid potentials generated in the model. There is no perched water in this system. All the fluid potentials are zero or less, so we have no positive head in this system.

Whether or not those are credible failures at this time depends on two things. One, validating the models that are used. The other part is getting more data so you have more constraints on the parameters that go into the models, and I want to use this slide as a lead-in to what you're going to hear from Ed Weeks and Ted Norris, in that numerical models are not self-validating, and by validation, I mean the way Dwight's going to present it this afternoon, and that is, are you using a model that's applicable to the conditions at that site? Not, is the code verified? This is something very different, and I'd argue that you cannot validate numerical and conceptual models without independent data and observation, and the only place you're going to get that is, again, from the geochemical evidence, which is an excellent place to get it because it's independent of the hypothesis you used up here. It's either going to validate it or, if you're real lucky, it's going to invalidate it, because sometimes that sends you a much clearer message of what you've done wrong. Field observation and, of course, laboratory study.

And with that, I guess Ted is next.

DR. NORRIS: I'm Ted Norris. I'll be talking about ^{36}Cl measurements that have been done for the Yucca Mountain project, primarily for determining the rate of water transport there. An overview of my talk shows there are actually two purposes that I've been doing this work. The first is to get site characterization data from samples, primarily--and an illustration of them is from UZ-1, a dry-drilled hole that was done by the U.S. Geological Survey in approximately 1983, and I was looking primarily for evidence of matrix flow or that could be interpreted in terms of matrix flow. I've also done some work and gotten some data in G-Tunnel that I think relates to confirmation of conceptual models, as Paul Kaplan was just talking about, and I want to conclude this talk with additional work that I think is necessary because I need to say that the use of ^{36}Cl isotopic data, as I am doing them for unsaturated water flow, has not been done at these depths by anybody else. So in addition to conceptual model validation, which is important, I think it's also important for this first-time use to see what other information can be done to validate the ^{36}Cl field studies.

The first dataset I want to discuss has to do with the UZ-1 drilling samples. This slide doesn't show up very well, but this is the UZ-1 drill pad at the northern corner of Drill Hole Wash. This is a view to the southeast. The data package says southwest, but that's an error. Down Drill Hole Wash in this area right here is the G-1 drill pad, and in the talk that Dr. Weeks will give following this one, he is going to discuss the evidence that water that ended up stopping this dry drilling at 1260-some feet most likely came from this particular location here.

The UZ-1 cuttings were kindly made available to me through the USGS and the good offices of Rick Whitfield and Mike Chornack, and one of the problems that I have is that the technique I've been using for my sample analyses requires a large amount of cuttings because I'm using a low efficiency extraction technique, so I take as many as 20 kilograms of cuttings, leach them over--usually a 72-hour period is found to be adequate--and then precipitate silver chloride from the lechate by adding silver nitrate. Actually, the experimental part of this work has all been done in a hydrogeochemical consulting firm at Tucson called HydroGeoChem, Incorporated, under contract to Los Alamos Laboratory. All of the samples have been measured, the ^{36}Cl content has been measured at the University of Rochester's tandem accelerator facility, which is the only place where sensitivities of 1 atom of ^{36}Cl in 10^{13th} atoms of chlorine can be done in this country on a routine basis.

What I was looking for was to see the decay of ^{36}Cl, which is produced as fallout from cosmic ray interactions with argon in the stratosphere and it's over geologic time, so there's ^{36}Cl there which decays with a 300,000-year half life. Water that falls to the surface carries the ^{36}Cl down, as well as the other chlorine that has fallen out from aerosol deposition from sea sprays, and so there is a source of ^{36}Cl over geologic times and I was looking for long decays. And here I did find them. In other work I have measured the amount of ^{36}Cl to chlorine ratio at the surface to be of the order of 519 x 10^{-15}, so I was looking for indications that decay had occurred, so this value is roughly half the value at the surface and I interpret that as an indication that the ^{36}Cl component here has taken about 300,000 years to get to the thousand foot level in UZ-1.

The next value down here at 1200 feet indicates another half life, and these numbers right here are indicative of another 300,000 years to here and in terms of water travel times, a rough calculation would say that this is of the order of .2 of a mm per year to .4, which is what was originally calculated from observations and documented in a report by Montezar & Wilson in 1984. So this is consistent with that, and although I cannot interpret what mechanism of flow there was to result in these things, hydrologists would say that that would be evidence for matrix flow.

The picture is not consistent. This would be a contemporary water flow there, and I should go on to say that this is preliminary work and that the results depend on the degree of pulverization of the particles that I leach, and the next page--

DR. DOMENICO: Wait, wait. What are the red numbers?

DR. NORRIS: Okay. I was going to come back to that. If you'll excuse me, I'll go to the next one first to finish up this subject; then come back.

These are the data where we took the cuttings which have--which are finely ground from the drill bit and have a median particle size of about 10 microns. We ran them through a shatterbox for the times that are indicated here, re-leached them, got more chloride out, and measured the ^{36}Cl to chlorine ratio and found that the more we shattered in a shatterbox, the lower the ratio is, and we think we're reaching a plateau value here which most likely results from the uranium and thorium content of the tuffs that are here. If this tuff was laid down about 11 million years ago, all ^{36}Cl in at that time should have decayed, but we think there's a residual amount of chlorine. There's approximately a few parts per million of chlorine and there's 20 parts per million of uranium, and not quite that much thorium, as I recall, and the neutrons from spontaneous fission of those two isotopes interacting with the inactive ^{35}Cl can result in ^{36}Cl, and so we think that we're coming down to it.

So we're hypothesizing that the sample, as we leach it, has two components; a meteoric source that we're interested in with the chlorine coming down from the surface, plus an underground source. And so we're looking for ways to separate out these two components so that we can investigate the groundwater travel time, or the travel time in the unsaturated zone only through the meteoric component. And we're in the process of that. We're looking at methods for that and we don't have a final method yet.

Going back to the previous slide with the red numbers now, there is another source of ^{36}Cl besides cosmogenic chlorine, and that is chlorine that is resulted as global fallout from high yield nuclear weapons in the Pacific Ocean in the time period between 1952 and 1962, and at that time the megaton bombs that were exploded at sea level, the large neutron component there in those thermonuclear devices irradiated this chlorine in the sea water, and particularly activated the ^{35}Cl. Because it was such a forceful explosion, the chlorine was taken up into the stratosphere and distributed globally with a residence time of about one year, and this is distinctive with respect to cosmogenic ^{36}Cl because of its magnitude. The bomb pulse resulted in up to three orders of magnitude higher ^{36}Cl to chlorine ratios than we've seen from cosmogenic fallout.

So I interpret the data in terms of anything larger than the roughly 500×10^{-15} values. Those are fairly hard to come by by anything other than from carrying the bomb pulse at these locations. And so I see the bomb pulse in the UZ-1 data at the 97-foot level, which was in the Yucca Mountain member of the Topopah Spring tuff, here at the 170 foot level in the Pah Canyon member, and the rest of this is all Topopah Spring member down here, and I see it here, too. This hole was drilled with water for the first 50 feet. There was a pond of 400 gallons at that point after 50 feet that interfered with the drilling they were doing. They pumped it out and they went dry from that point on, but in a paper by Rick Whitfield that was published on this, he mentioned that they saw hydrologic effects of the drilling water down to depths of at least 250 feet.

I can't say where this bomb pulse came from, but I can say that this is indicative of it. I can't say, for example, if it was a result of the drilling, if the drilling had been drilled all completely dry would I have seen it? I don't know. So I'm saying that there is some way that--the only reasonable interpretation I can see of this is that it is a bomb pulse at this depth. I can also say this is--looks like it's reasonably contemporary. The drilling water that was used here came from Well J-13, and that water was sampled in 1983 about the time of the drilling, and a ^{36}Cl measurement was made by HydroGeoChem at the University of Rochester and it's contemporary, so that part is contemporary water on there.

DR. DOMENICO: The last three blue numbers, those are equivalent to something? I note they're less than one millimeter per year as an estimated inflow.

DR. NORRIS: Yes. Excuse me, two of the three are.

DR. DOMENICO: Two of the three. Is that basically where the bandied about infiltration rate of less than one millimeter per year comes from, on the basis of those three points; more or less?

DR. NORRIS: No.

DR. DOMENICO: No. Okay.

DR. NORRIS: I believe the one millimeter per year result came from the Montezar & Wilson paper of 1984, which estimated for the Topopah Spring, it said that the net water flow may be upward, but the downward component was most likely in the range of .2 to .4 of a millimeter, and I remember Sandia did a study--and I'm sorry I forget the author's name now--in which they doubled that to be conservative, and came out to roughly one millimeter. So my guess is that number is from there.

DR. DOMENICO: Those numbers are consistent with that, though?

DR. NORRIS: But these are consistent with those sorts of numbers, yes. But those numbers arose before this. These data I didn't get until January of this year, and in fact, I know some of you were at the September meeting, like Dr. Deere, and this is a new datum that I did not have at that time and I consider it important because it looks as if, in the cuttings from the 495-500 level, it looks like it's just contemporary value, and in the 500-502 it comes up very rapidly here. So it's not a distribution--if this were a distribution coming down, you know, from water flowing down, I would expect, say, some of these to be higher, particularly in here. I don't see it. I can guess that this is some sort of lateral flow, but I don't know from how far it is, and I think that the fact that this is here, contemporary, this is bomb pulse right here. Unfortunately, I have no more samples until 590 feet is the next sample that happened to have been saved, and so I'm not able to do a profile on there and I'm sorry about that, so I'd like another hole drilled to complete those studies.

I'll leave that subject, then, of the site characterization and go to G-Tunnel work. Again, this is a photograph of G-Tunnel. Only part of my time on this project is spent on ^{36}Cl work. The other part was spent on an in situ diffusion test that I was running in G-Tunnel, and this is the instrument panel for it, and I had two holes drilled here. This, the depth below surface is about 1300 feet and I wanted to drill it with air and it occurred to me that since I was having so much problem getting deep samples drilled with air from Yucca Mountain, that this would be a good location to just measure some of the cuttings here and see what it was like at 1300 feet, and I was expecting some--hoping for some nice values well below the 500 x 10^{-15} that I knew was at the surface.

The values are shown on this next viewgraph in red, and I started off with two values, just two cutting samples because I didn't know what I would get, and this one here from a dry-drilled hole labeled AC-1 was below the 500, or at least a little bit, and the next page has the exact figures on there with the one sigma results, the following page for those who wanted to see what the statistics are, and those are the one sigma from the counting results.

Over here, I was very much surprised to see bomb pulse at this location, so I continued on and saw a bomb pulse here at the adjacent location at two depths. This is at 15 to 25 below the invert of the drift. This is at 25 to 37 feet below that drift. There was a horizontal hole here from which the investigators, the principal investigators were kind enough to make the cuttings available to me. There was a nuclear bomb exploded--nuclear device, I should say--exploded about 900 feet to the east in this direction, so one of the geologists suggested that I take a sample here to see if it was increasing out this direction, if the nuclear device might have been the source. That was unlikely, and I did get a nice low value here and I see something that looked to me--it was a higher value here than contemporary, and although various people can look at these data and come up with different conclusions, I looked at them and said, "Well, this looks to me like kind of a breakthrough front. I don't know why there is a breakthrough front there for the ^{36}Cl bomb pulse. I assume it may have come from the surface, and there is a fault that's visible right here." And I said, "If I see this value to the east, then maybe I should see it to the west," so I took two samples here and I do see it to the west here, so at least that's consistent with it. It's all a one-dimensional array.

Now, these data, I have talked to various scientists about these and their use in validating water travel in the unsaturated zone. As I say, this is--experimental data are hard to come by. I should have mentioned, by the way, back at the UZ-1 hole with the red numbers that were there, that while I see these things there, the bomb pulse at depths, I should have mentioned that Al Yang has some isotopic evidence from Oxygen-18 as I recall, that he sees somewhat similar sorts of things in some of the UZ-1 samples. So it's a new field.

This offers the potential of a three-dimensional model, or a three-dimensional data because one can do drilling in three dimensions here and trace out where the bomb pulse is and where it isn't, and I should mention that there is a seep at this location, so having water coming down here, the oxygen isotope data indicate that the seep is very recent, so having water there is not the big thing in G-Tunnel, but being able to determine if you can trace out where the source of the water bearing the bomb pulse was and what its dimensions are at 1300 feet underground, which is repository dimensions, even though it's not repository material there, it's still--it's an unsaturated tuff but a different one. Still, that would be useful for modeling and for setting bounds on where you would expect to see this, the solute transport. So those are the two sets of data that I have.

The applicability I've at least touched on here, mentioning that it's--there are different geologic structures, lithology and mineralogy, and that there is also large-scale fracturing of G-Tunnel tuffs by the explosives tests, but that it does seem to me to offer a real opportunity for an in situ study of where solute transported, because we do have this and it's the only place that I know of that does offer this potential.

I'll conclude, then, by saying since this is a relatively new study as far as using ^{36}Cl for water transport in the unsaturated zone, then the additional work that I would recommend for validating the--this experimental study, if you would like to do it, is on more surface-based air cores there, and I'm particularly interested in one or two air-cored holes between UZ-1 and the west slope of Yucca Mountain to see if I can pick up the ^{36}Cl that I see at 500 feet in another hole and see if it might possibly come from the western face and coming through in a lateral flow. That's a testable hypothesis that one can do, so I'm just pointing out what one can do there.

Also, I would like a hole that does not have water there, that--at the surface, to see what the--whether I can determine water flow over times that are comparable to or longer than the 300,000 year half life. I feel that's very important for the site characterization there and for groundwater transport times.

I'd like to delineate the bomb pulse in three dimensions in G-Tunnel. The USDOE is trying out some drilling equipment, as I understand, at Apache Leap, Arizona, and I've requested cuttings from that location. Again, it's an unsaturated tuff location where they hope to drill to 1700 feet and, again, since the concept here of matrix flow and long times is one that needs validation from the ^{36}Cl viewpoint, I've requested cuttings from that to see if I can do that to do a good scientific study of the cuttings from there as a function of depth and see what the results are.

The bomb pulse that I see at 500 feet should also have ^{99}Tc in it as a fallout product and if the technetium is traveling as the pertechnetate anion, I should be able to analyze this using some of the mass spectrometry facilities at Los Alamos Laboratory and see if technetium validates the ^{36}Cl, which I think is also important. Some day an exploratory shaft is supposed to be built and it would offer additional possibilities there for validating this sort of work.

So I conclude with--

DR. DOMENICO: Have you checked for technetium on the work you've done?

DR. NORRIS: Have I checked for it? No, I have not.

So I'll just summarize, then, by saying that the UZ-1 data showed the potential as far as I'm concerned for detecting flows over long times that I think would be described as matrix flow. They also show the potential for using bomb pulse when one sees bomb pulse, for detecting what would probably be non-matrix flow. The G-Tunnel data show the potential for a solute distribution study at a repository depth in unsaturated tuffs. So I'll conclude at that point.

DR. CARTER: Ted, could I ask you a couple of questions? You might want to put up, if you can readily retrieve it, the information that you had or the data you had for UZ-1.

DR. NORRIS: I'll be happy to.

DR. CARTER: Not the G-Tunnel.

DR. NORRIS: Oh, I'm sorry; saw the first one with red figures on it.

DR. CARTER: Thank you, sir. As I look at those data, it would appear to me that you've really got two samples that bear on long flow times; is that not correct?

DR. NORRIS: That's correct.

DR. CARTER: The one at 245, plus the one at 102?

DR. NORRIS: That's correct.

DR. CARTER: So those are two samples out of that group that really, you say, track the cosmogenic ^{36}Cl?

DR. NORRIS: I had hoped that I would say that they showed the potential for it.

DR. CARTER: And obviously it would have been very desirable if you had had samples from greater depth to see if that would--

DR. NORRIS: There are samples--well, the hole terminated at 1260 feet and there are more samples here that have not been analyzed.

DR. CARTER: But some at deeper depths?

DR. NORRIS: Down to 1260 feet, one or two, and plus two or three more in here. So there's the possibility of getting more samples in here.

DR. CARTER: Well, the point is you have two samples at least thus far, so this was part of the question. What kind of turnaround time do you have in the use of the tandem accelerator in Rochester? In other words, when you send them a sample or several samples, how long is it before you can get the data?

DR. NORRIS: The turnaround time is--they are trying to have one ^{36}Cl run for all users in the country every three to four months. Occasionally the tandem has been down for periods of up to 18 months, I think it was, so for my studies, it takes a long time to get the samples. The turnaround time is--I would expect to be, say, from the time I get drilling cuttings to the time I get them--the chloride leached, a sample prepared and the results from the accelerator, once they run on there it's fairly rapid. I mean, I was standing at the console there when these data were coming out and there are some refinements that are in there, but it's of the order of a year from the time of cuttings to getting a ^{36}Cl to chlorine ratio out.

DR. CARTER: Now, these--the error term you show there I presume is an analytical error term, 2 sigma or something like that?

DR. NORRIS: That's one sigma based on the counting data from the tandem accelerator only in the ^{36}Cl to chlorine ratio, so it does not include other potential sources of error.

DR. CARTER: I was going to ask you, do you have--having done some of this, or you've done it over a period of time--do you have any sort of estimate on what the sampling error may be, or errors other than the analytical error?

DR. NORRIS: Yes, I do. In a paper that I published in the infiltration measurement at Yucca Mountain on using ^{36}Cl bomb pulse measurements, I did a propagation of errors technique on the samples there and found it was of the order of 50 per cent for some of those.

DR. CARTER: The other question I had is that I presume until you get samples that are lower, considerably lower than a hundred, for example, in your ^{36}Cl:Cl ratio, that you're not really all that concerned in pinning down the underground background in ^{36}Cl, if it's of the order of 25?

DR. NORRIS: That's correct.

DR. CARTER: That's not going to affect these results until you get down, presumably, another few hundred feet?

DR. NORRIS: Well, it does affect it insofar as it would be a component of the media here, and in resolving the two components. But you're correct, it's a small one.

DR. CARTER: Well, I'd read the 100 as plus or minus 25.

DR. NORRIS: Sure. Within the error uncertainty, that's entirely correct.

DR. CARTER: Thank you, sir.

DR. NORRIS: Are there other questions?

(No audible response.)

MR. WEEKS: Good afternoon. I'm going to be discussing some observations concerning air flow and water flow in fractures. I might mention that in regards to observations concerning water flow, I've been kind of an interested bystander. The data I'm going to describe aren't really due to my own efforts, but I have followed these and maintain an interest in them.

We'll start off discussing water flow in fractures because I'm really into the air flow and want to finish on that. But basically we're going to talk about two sets of observations; one of them involving drilling fluid from Well G1 found its way into Well UZ1, and second, we're going to talk about water observed in neutron holes. Alan Flint discussed some of that this morning, but we'll give the full suite of what we know and include a situation where we found some water during drilling.

Okay. First we're going to talk about flow in--okay. North is in this direction. For those of you that have been to the test site, here's the subdock, Drill Hole Wash, Well G1, Well UZ1, for point of reference the exploratory shaft facility will be in this general area and Pagany Wash is here. First, keep in mind G1 is 1000 feet downwash and also downdip from Well UZ1.

During the drilling of Well G1, they lost 58,000 barrels of polymer drilling fluid. Well G1 was cored to a depth of about 5,000 feet during the period March-August, 1980, and they had continual problems of being unable to recover their mud and the drilling report shows that once they hit the--reached the water table, the sandline mud cut, which would be an indication of how high the mud was getting in the hole, was typically at a depth of 1200 to 1400 feet.

As I mentioned, Well UZ1 is located 1,000 feet upwash and updip and it is 75 feet higher in altitude than Well G1. Standing water was encountered in Well UZ1 at a depth of 1267 feet. Now, one thing about the reverse vacuum drilling method is that when you hit perched water, you stop. The water doesn't get lifted, the cuttings quit coming up and you're dead in the water, literally, so we know the depth very well. According to the drilling report, they weren't able to bail that level down, but they did recover samples and found a trace of the polymer drilling fluid used in Well G1.

The conclusion that we can arrive at from this is that some of the drilling fluid from Well G1 migrated upwash to Well UZ1, and this was an--I should use "possibly"--from fluid lost before Well G1 reached the water table. We had to have a mud level higher than 1200 feet to get it to a depth of 1267 feet; moreover, the last time I presented these data, the Geologic Division people pointed out that there had been hydrofracing, or they felt that they were hydrofracing the formation with the drilling mud at greater locations and that the preferred direction was not upwash, so I think probably we got that early on.

Okay. Alan Flint discussed water in one of the neutron logging access holes. There are about 90 of them that have been installed in the vicinity of Yucca Mountain, all drilled using an ODEX system, and a typical construction is they're basically drilled about 50 feet deep, or commonly. This one shows one all the way in rock; steel casing down to about one foot below the--above the bottom of the hole drilling this out, and when they shut off the air, which is used as a drilling fluid, the cuttings tend to settle out, forming a seal around the casing.

Now, we don't know for sure how good that seal is, but we have been able to obtain gas samples that do not seem to be contaminated by air and in addition, they put a cement plug around the casing here at land surface to prevent water from going straight down the hole.

The first instance where we saw any water was in Well N24, which is a 75-foot deep well drilled in the bedrock channel of Wren Wash, and it's not shown or labeled here, but I believe it would be right here, this little pad right here. As they were drilling down, they encountered perched water in the columnar unit, which is the basal welded unit of the Tiva Canyon member. They drilled the hole on down into non-welded unit, failed to get a sample that day, and the next morning the water had drained.

We've also observed water in neutron holes following snow melt and rain, and we've seen this in four neutron holes, two of which Alan Flint discussed in some detail this morning. As you recall, Alan showed that Well N2 is in a minor bedrock channel on the hillslope coming into Pagany Wash. Well N7 is in what I'm calling a raised braid in an alluvial channel. I'm not a geomorphologist. They may cringe at that term, but anyway it's in the active channel which is braided, but it's elevated above the lowest part.

Then the N26 and N44 are both in Wren and Coyote Washes, which are two washes coming off the side of Drill Hole Wash. N44 is in this bedrock channel and N26 is up here in this bedrock channel. Water was found in these holes, or entered these holes in February, 1988. It occurred in Wells N2, or was found in Wells N2 to quite a height. This is just to give you an idea that there was a lot of water here, and N7, there was only a very small amount of water. In April there was a rainfall event. N2 and N44 collected a lot of water. N26 collected only a very small amount. Then in August of this year we once again got a lot of water in Well N2 and they kept checking the other wells, and finally enough water got in the bottom of N26 to cover about half the bottom of the borehole.

We were unable to get samples for N7 or N26, but we do have samples and analyses available for Well N2 for February and April, for Well N44 in April, and we have analyses pending for the water collect in August of this year. Now, I want to emphasize that I'm a water quality amateur and so I may be saying things that you won't believe, and that'd be justified.

But first of all, tritium activities for all the analyses range from 25, plus or minus 4 to 30 plus or minus 4 tritium units, strongly supporting the assumption that the waters are from very recent precipitation. The Del Oxygen-18 and Del Deuterium data indicate that, indeed, we had snow melt in Well N2 in February and that for Well N44 indicated rain, but the water when we collected in April of '88 in Well N2 was very light, only a little heavier than that in February, making one wonder if possibly there was some residual water that was pushed on into the well with that more recent rain.

Now, next, here's where I am kind of getting out of my depth, looking at major anions and major cations, basically what I did was to take eleven analyses of deep groundwater from various wells drilled basically depth of water to 2,000 feet or more, took the minimum median and maximum of those samples from those eleven wells for the bar with the X's, and then plotted the various analyses, adjusted to keep on the scale here, and what amazes me is the similarity between the water in these neutron holes that collected in just the week or two following a precipitation event with that deep groundwater that's been there for many years, is dead relative to tritium and had to have a very long flowpath. We see that the chlorides really show more scatter, but are quite similar. This point got left off on the handout but basically sulfate is higher to significantly higher. Nitrate is a little higher to quite a lot higher, and the alkalinity is basically a little lower.

There's a couple of thoughts there. One, the water was sitting in an open borehole, open to the atmosphere. It might have degassed some CO_2 and changed its alkalinity. On the other hand, if we assume carbonate equilibrium, we really--and having a great number of soil gas CO_2 samples, we really can't get it much higher than that and still be consistent with what we know about soil gas CO_2, so--

DR. LANGMUIR: Ed, where are these groundwater samples from; what wells? Is this from J13?

MR. WEEKS: J13, H3, G3, G4, they're all the ones listed in the site characterization plan by Los Alamos except H5, which was in altered tuffs and it was tending to be one of the maximum or minimum for every well, so I took it out to cut down on the span, and I didn't put the carbonate wells or the VH-1 samples. So it's basically all of the samples that we have for the various wells drilled on Yucca Mountain. Unfortunately, we don't have any water table. The wells that were drilled to just below the water table have never been sampled, so we don't have these, so these are basically deep samples from well below the water table.

DR. LANGMUIR: They're not likely to have the same origin anyway as the waters you're looking at.

MR. WEEKS: No, I wouldn't think so. Right. But I'm just amazed at how--maybe naively--that the major ions look so similar.

A similar thing with cations. Calcium's right on. Sodium is lower, which is certainly what one would expect considering that silicate weathering, these glasses should increase the sodium concentration. Magnesium is higher and, unfortunately, the circle is right here for potassium so it doesn't show up, but basically the waters both have quite a few dissolved solids and are somewhat--at least similar to the groundwater. I might mention that the silica is very much different. It's very much in the neutron holes than in the deep groundwaters, which is also what one--a naive person would expect.

In conclusion, we found that water is collected in four neutron holes ranging in depth from 35 to 50 feet within days of precipitation, and we take this as a strong indication of rapid fracture flow, and that despite the short contact time with the rock materials, the water has become similar to deep groundwater in many respects. So basically this is just recounting some observations with a minimum of interpretation.

Then the next topic I want to talk about is gas flow through fractures. This is basically my own and Don Thorstenson's and many colleagues' research, and so this is something I like to talk about.

The first thing is that most wells that have a section of hole above the water table will exhibit substantial air flow when the barometric pressure changes. People that go out to measure the water levels will note that in any crack or gap in the cover, air might be whistling out or whistling in. Everybody that goes out there on a day when the barometer's changing a lot notices this, just because the noise will attract their attention. These flow rates are so great that we can only explain them as a fracture flow mechanism, and actually there are two phenomena that produce this gas flow. One is changes in barometric pressure, and the other is topographically affected density-driven flow.

As I said, this has been a long-term effort over the last three-plus years. All that we admit to is in this report, which is appended to the back of your handout, so if there's any discrepancies between these overheads and the report, at the back of my presentation handout, it'll be in this preprint.

First of all, let's explain the barometric effect. If we have a well that's open above, say, an impermeable layer, the water table, we get a step change in barometric pressure, as that barometric pressure changes at land surface its progress is attenuated and lagged in phase with depth, so that within the rock itself, say, at this fractured rock confining bed interface, the pressure change has been much less than here. Whereas, the pressure change can be translated instantaneously down the well so that we have a pressure imbalance here. So if we have an increase in pressure, air will move into the well and into the fractured rock. Then conversely, when the barometric pressure falls, the air comes out of the well and blows up the hole.

Now, a lot of times people tend to be out on a field trip in the afternoon when the barometric pressure is dropping, and it's quite common that they'll see the well exhausting. But the inflows and outflows tend to balance. Even though most of the wells around the mountain exhibit this effect, the inflows and outflows balance over time so that we have essentially a zero net flux.

Now, I should elaborate a little bit on the phenomenon in the absence of drill holes. Edgar Buckingham, back in 1904, was concerned about this phenomenon and its relationship to the aeration of soils, and he showed that we wouldn't expect barometric exchange to a depth of more than about one per cent of the thickness of the unsaturated zone, which for Yucca Mountain would be on the order of 20-30 feet or ten meters. He didn't take into account any kind of mixing, but we might double that and say that for depths greater than 50-60 feet, we would probably not expect that we'd have a lot of exchange and it shouldn't be an important mechanism for the transport of radionuclides.

DR. LANGMUIR: Ed, how do you know at Yucca Mountain that, in fact, the inflows and outflows exactly balance? What measuring techniques have you used to prove that exactness of balance?

MR. WEEKS: Well, I'll get to a correlation analysis we did later. It's basically a physical argument, though, that without some opportunity for it to come from some other boundary, what goes in has to come out. But that seems to be verified by--

DR. LANGMUIR: We're talking about large, large volumes. A one or two per cent change might be quite important, however.

MR. WEEKS: Let's wait until we get into the topographic effect and some of the measurements we've made and see if we still think that.

This is a reference that I just recently found to the barometric effect. You can't read on the back. It says: "Relax, Worthington. As the warm moist air from the jungle enters the cave, the cool denser air inside forces it to rise, resulting in turbulence that sounds not unlike heavy breathing." Okay, here's a different cartoon to explain that phenomenon. If we think--let's think about wintertime situation and think of a U-tube, our atmospheric column extending from a hillside outcrop up to the hillcrest would be cold, dry and, hence, relatively dense. As we come through the fractured rock, the moisture, or the air picks up both moisture and heat, and assuming initially static conditions, we'd have the same pressure here, but a higher temperature. Moreover, since the rock gas will be essentially saturated with water vapor, we have to adjust its density and the typical way or a way that's been used by meteorologists to account for this is through the virtual temperature, which is defined as the temperature that a packet of dry air must have to have the same density as the wet air. And since water has a molecular weight of 18 and air a mean molecular weight of 29, the virtual temperature is always higher than the actual, or the air temperature. In Nevada, the air is usually dry enough that the atmospheric air temperature is close to its virtual temperature and we can more or less ignore that effect in the atmosphere.

For this part of our U-tube, then, we have a warm, moist light air, heavy air here so that the air gets forced out the well. Now, this process should and does reverse, at least to some extent, in the summertime with this column of air being cooler and denser than the atmospheric air, so we should have air entering the well and draining out the outcrop.

We first actually really heard about this at a presentation at a workshop in Tucson, Arizona, where we heard about a well that John Gary had put in the basalts of the Snake River plain overlooking the Snake River gorge, built a greenhouse over this well and the well blew warm air into his greenhouse all winter long. So as soon as we heard this, we knew that there were a couple of holes, Well UZ6 and Well UZ6S that had been drilled and left open waiting, at that time, funds to stem them, and so we were sure that we should see that effect at least to some extent in the wells up here.

So this is a view looking from the northwest. This is Solitario Canyon. This is the Tiva Canyon welded unit, the Paintbrush non-welded unit, and the Topopah Spring welded unit, and then this is basically a dip slope proceeding to the east.

To look at this in cross-section, we have Well UZ6, which penetrates down 1850 feet into the Calico Hills non-welded unit, but it was cased to a depth of 320 feet. Finally, I was told that UZ6S was installed to provide additional access for instrumentation.

Our first reaction, we found that the wells were, indeed, blowing like crazy when we got there and our first reaction was that we would emphasize Well UZ6 because it was bigger and better than Well UZ6S. However, much of this hole is below the floor of Solitario Canyon. It's quite deep so that any barometric pressure changes take a long time to translate through the mountain and equilibrate. This well is very strongly dominated by barometric effects. Well UZ6S, on the other hand, can equilibrate quite rapidly both laterally and vertically, so it's much more nearly a topographic effect place, so we switched our emphasis to it.

Well, this looked better as a slide than it does as a transparency. This is flagging hanging on--or that we just tied to a hammer here, that is, and it's really being held up by the blowing air. We don't have a nylon string or anything to actually hold it up, and this turns out to be a, due to a flow rate of about three meters a second. There's snow on the ground. That's why the background's so white, and that turns out to be a very typical flow rate. Here's for a couple of days this last winter. I've left off the barometer for lack of clutter, but it explains a lot of the flow on a daily basis. But the flow is about three meters a second. The air temperature ranged from about -2 up to about 12 degrees Celsius, and the whole time the well blew out air with a temperature of about 17 degrees Celsius.

Here are some of those typical values. Relative humidity is 100 per cent, water condensing all the time because it's cooling as it comes right through the top of the casing. At that temperature, the vapor density of the air is 14.5 grams per cubic meter. We measure the CO_2 concentration at about 0.12 per cent by volume. This actually should be about 1.8 grams per cubic meter. The air has a density of about a thousand, but I forget to correct from volume to weight.

DR. LANGMUIR: Are you going to explain to us at some point how that much CO_2 gets in the system, Ed?

MR. WEEKS: No, because I don't know. I wish I knew. I wish I could.

For a typical winter, daily winter fluxes, that comes to about 10,000 cubic meters of rock gas, 145 liters of water vapor discharged. If we assume the air entering the outcrop has a vapor density of 4 1/2 grams per cubic meter, then our net water vapor discharge is 100 liters. Discharging 3.3 kilograms of carbon as CO_2, and assuming a .035 per cent CO_2 in the atmosphere, that comes to 2.3 kilograms per day net carbon discharge.

DR. LANGMUIR: What's the Del-13 of that CO_2, Ed?

MR. WEEKS: -17. There's a long table in the back that--or in the paper that shows that.

Okay. In the summer, as I said, it did reverse to some extent, but it certainly doesn't reverse to the extent that we might think. Here is zero flow. Positive is up the well, out of the hole; negative into the hole. Even though on average there's flow into the hole, there are periods each day when the flow reverses and the well exhausts. Moreover, the temperature fluctuates a lot and the chemistry becomes a hodge-podge mixture of air and rock gas so that, as I'll mention later, we don't try to do anything with summer gases in terms of chemistry.

In addition, we are obviously not explaining anywhere near all the fluctuations by temperature alone. We have a lot of missing record, or a certain amount of missing record plus a period before flow measurements were taken that we want to extrapolate, so we want to try to make some sort of extrapolation based on weather records alone.

DR. LANGMUIR: Ed, what are your flow units?

MR. WEEKS: Okay, these are in meters per second. This is just a flow velocity in this case, and then the well radius is about .03. The area of the well bore is about .03 m^2.

I tried a number of fancy filtering techniques and didn't ever seem to be getting anywhere, so finally, in desperation, I went to ten-day block averages for barometric pressure, flow rate and temperature, and when I went to these ten-day averages, the correlation between barometric pressure and average flow rate went to zero, but our correlation coefficient for average flow rate versus average temperature became quite good.

DR. CANTLON: This is ambient temperature at the wellhead?

MR. WEEKS: This is air temperature, right, at the well.

DR. CANTLON: At the wellhead?

MR. WEEKS: Yeah, or close by. The weather station's a couple hundred yards away.

So we feel this is a good enough relationship we can extrapolate the data. There is a negative correlation as the temperature increases. The flow becomes increasingly downhole. Here is one of our mysteries, is that if everything I've said were true, we should have zero flow at an air temperature, ambient temperature of 19.5 degrees Celsius. The virtual temperature of the rock gas was determined by monitoring and temperature logging. But instead, the zero intercept is about 24 degrees Centigrade. In other words, we exhaust--we would expect some net exhaust, but it is, in fact, quite a lot larger than we would anticipate. Somehow or another I had hoped that the X would be more dramatic than it is, but nonetheless, it is significant.

Based then on temperature records, we've come up with a volumetric flow. Now, this is cubic meters per month times 100,000. Note that most months it is positive out of the hole. We only have about three months in the summer when it reverses and net flux into the well, and now to come up with annual net fluxes, our net flux out is about a million cubic meters a year. Our water vapor discharge is about 10,000 kilograms, and carbon as CO_2 is a net discharge of about 380 kilograms. So that really does raise the question, where does all the carbon come from? I'll move on and then maybe speculate a little on it.

We have collected a lot of gas chemistry that I'm going to discuss very briefly. There's a great deal more in the paper by Thorstenson, et al. He is the gas chemist and so he talks more about chemistry and I talk more about the flow.

Two reasons for studying the gas chemistry is, one, to understand gas flow patterns; and the second is to determine potential for gaseous radionuclide transport.

We've analyzed for a large variety of gases. I'm going to, for the sake of time, emphasize just carbon dioxide and Carbon-14, but as you can see, we've analyzed for quite a suite of gases. One of the interesting things about CO_2 concentration is that there has been a relatively small shift in the CO_2 concentration over these three March periods. As I mentioned earlier, we get a lot of mixing in the summer, but about November the well starts blowing continuously, so we've taken the philosophy that by March all that mixing should have been discharged and we should be getting pure rock gas, so we sample on purpose in March.

And even though we've discharged three million cubic meters, or between each of these, from this to this to this, we've discharged a million cubic meters of air, there's been only a relatively small change in gas chemistry with maybe something coming in below a hundred feet here. Now, one thing that's important to emphasize--and I've never known quite when is the best time to bring it out--is the fraction of total flow coming in at various depths in the hole.

This is based on composite flow logs, including a sweep in which we logged every four hours for 28 hours, and found that no matter how much the total flow was, the fractional flow rate from each depth was about the same. That wasn't what we were hoping for, and we were a little disappointed and it shot down one of our hypotheses, but at least it makes it easier to graph.

The very top 25 feet are in the upper cliff unit, which has very few fractures and, in fact, we're not--this is within our measurement error, but that's kind of the average and we think well, maybe that's right. At 43 feet, we're already picking up about 25 per cent of the flow; a little over 60 feet we get about half the flow, and a little deeper than 100 feet we only have about 20 per cent of the flow coming in from a lower depth. We don't have measurements lower than that because we're reluctant to lower our hot-wire anemometer down and our prop anemometer begins to stall out at deeper depths.

But basically what this says is that most of the air comes in above 100 feet, and seems to be consistent no matter what the flow rate. So going back to this, most of our flow's coming in above this and that's why in this, this is being sampled in the blowing air stream. It can come back to its original height from the--or concentration from contributions above 100 feet, but we do have, perhaps, a big fracture or something down in here in which we're getting possibly some atmospheric air.

The Carbon-14 is also quite interesting. We also sampled for Carbon-14 in March, at the same time those measurements are being shown. These two surface measurements are made on soil gas. They're collected by putting out trays of potassium hydroxide underneath a stock tank, letting them set awhile, and then analyze for the activity. These, then, are the results of carbon or CO_2 sampling and radiocarbon analyses. Note that we have modern carbon or post-bomb carbon down to a depth of 400 feet or so, and relatively little shift in these three years despite the fact that we've been blowing a--we uncapped the well in September of 1986, and by March of '89 we'd had a cumulative exhaust of about three million cubic meters of air, and yet we didn't change this very much.

On the other hand, this is post-bomb CO_2, which we've never seen at depth at any other site in the northern great plains or other places where we've worked. I will show some results for comparing UZ6--that should be UZ6S versus Well UZ1. Now, this very high activity in Well UZ1 is probably an artifact pad construction. We get about one and a half per cent CO_2 in this probe, which is totally anomalous with anything we've measured anywhere else around Yucca Mountain. We put a one-meter depth hand-augered probe in the pad and then a few yards downstream in the wash, the probe in the wash showed about a .12 per cent CO_2 was exactly analogous over a season of sampling to everywhere else, whereas the pad was totally anomalous. So this is probably an artifact that perhaps bulldozing some organic matter, modern organic matter vegetation in, asphalting the top and capturing that gas, but this is much more typical of what we see at depth everywhere where we've sampled it; 75 to 80 per cent of modern as opposed to 110 per cent of modern.

What we have to argue from that is that, first of all, we can surmise that natural flow through Yucca Mountain in absence of the boreholes is almost certainly less than with the well bore there. If it can flow so freely through the cap, then we couldn't get a lot of flow concentrated just by putting a well in. On the other hand, the fact that we're getting post-bomb Carbon-14 at depths greater than 400 feet in contrast to Well UZ1 indicates that possibly even though on a time scale of three years we didn't change the radiocarbon chemistry, maybe on a time frame of 40 to 50 years, the length of time that we've had a lot of post-bomb carbon, that we have changed it. So perhaps this is telling us something about time scales, that in fact we are changing gas chemistry on a scale of 30-40 years even though it seems imperceptible on a three-year period.

As a consequence of being able to transport significant quantities of Carbon-14, this may be an important mechanism for the release of gaseous radiocarbon that Ben Ross will address. This next consequence that I'm showing here, I feel a little shakier about after seeing Alan Flint's results on tritium data this morning. We've only been able to sample Well UZ6 near its bottom because of all the mixing due to atmospheric air, so we've had to go only sample in the non-welded portion near the bottom. This conclusion that little gsa circulation occurs across the Paintbrush non-welded tuff, that may be a little extreme. I'm still a little bit in shock of seeing very high tritium levels in the Topopah Spring beneath the non-welded tuff, and all of a sudden feel more agnostic about saying anything about that. But we nonetheless would feel that the non-welded tuff is acting as a significant barrier to flow.

So I guess, Don, in terms of the barometric pressures changes, I think that they are small relative to the topographic effect. There might be an effect of temperature difference and moisture difference so that there would be a small net up flux, but compared to our topographic effect and our hundreds of kilograms of carbon and so forth, and let's see, one thing we do speculate on in terms of a source of the carbon, and I think Ben Ross's results will tend to suggest or support that, is that this is so close to Well UZ6S that it's hard to imagine that root zone respiration could produce it, so we're anticipating that possibly we have updip migration from, particularly from these various washes that have more vegetation and more area, but we still don't--that's just speculation.

DR. LANGMUIR: Is that Del-17, though, a little bit too heavy for that? Del-17 versus -25?

MR. WEEKS: Don, do you want to--I'll rely on Don Thorstenson to answer that. I never feel very comfortable talking about Carbon-13 and it seems to be something that people always ask me.

DR. CANTLON: Has anybody done any microbiology of the fractures?

MR. WEEKS: No, although we have seen blowing fractures on two limestone ridges to the south of Yucca Mountain, and they do develop slimes, molds, and so forth. They do get a healthy biological growth on them.

MR. THORSTENSON: I'd just throw a couple of things in here very quickly. Carbon-13, like the Carbon-14, is essentially constant down UZ6 as with time and depth within a half per mil. It goes from about -17 per mil near surface to -16 1/2 at depth. The numbers are in the tables in the paper. If you compare the C-13 and C-14 signature of UZ6S gas with groundwaters, it doesn't fit. That is CO_2 and the groundwaters. With deep unsaturated zone gas, it doesn't fit. With fracture flowing carbonates it doesn't fit. If you look at the statistical average of the data that we've got from the neutron holes, which Ed hasn't put here, it fits like a glove. I mean, it doesn't seem feasible for there to be any other source for the gas than the soil zone, however it's getting transported, which is a different question. And the chemistry, both gas and isotopic, that we see in UZ6S in the neutron holes is basically consistent with a whole pot full of soil gas stuff that we and other people have done in the southwest great basin; I mean, chemically and isotopically.

DR. CANTLON: So it doesn't have to be soil in the normal sense of soil. It can be biologically active fractures at almost any depth, as long as you get a gas flow--

MR. THORSTENSON: Exactly.

DR. CANTLON: --and water and organic driving the system.

MR. THORSTENSON: Yeah. I mean, soil very likely is well into the fractures, not in--

MR. WEEKS: In fact, I didn't present any methane data, but they certainly suggest that we have biologic activity. The methane gets consumed, and it's consumed deep in the rock.

DR. DOMENICO: Ed, have you calculated any elastic properties based on the barometric fluctuations to air, you know, the moduli or anything of that sort?

MR. WEEKS: We always assume that the elasticity of the air is great relative to the rock.

DR. DOMENICO: They fall apart?

MR. WEEKS: Yeah.

DR. DOMENICO: But you don't have--you do have earthtide effects in some of your deep water wells?

MR. WEEKS: Right. Right, and from that they--

DR. DEERE: Ed, I wanted to make a comment which I made in Las Vegas in one of the earlier meetings, probably last April or last June, about a case in ignimbrite or almost a welded tuff down on the Mexca Volcano in Peru. We have a tunnel about 10 kilometers long, and part of it goes through this particular welded tuff unit. When they drill the borehole out into the welded tuff, they're amazed to find that the smoke that they were smoking from the cigarettes was sucked in, but on the night shift it was blowing out. Now, the interesting this is, that couldn't have been a topographic effect. It had to have been barometric because it changed every day, and the temperature change was considerable in that area; in summertime from maybe 22 degrees Celsius to about 5 degrees at night, but it would reverse itself twice a day, and the tunneled portal was at the same elevation as the drillhole that was drilled horizontally, and so it simply shows, I think, the difference in travel time in the open tunnel which only had to come in about maybe 100 meters, versus passing through the fractured ignimbrite which was overlain, incidentally, by a capping layer of lower permeability.

So I think that was very direct evidence where we had no topographic effect involved.

MR. WEEKS: And yet a very large barometric effect.

DR. DEERE: Yes.

MR. DOMENICO: With all that Carbon-14 blowing out, Ed, you might be in violation already.

MR. BLANCHARD: Well, now we're staying with the same topic and moving from gas flow in the mountain to radionuclide gas releases that potentially could occur from the repository, and so we're shifting gears away from the mountain itself into more modeling and empirical information. The first speaker is from Lawrence Livermore, Richard Van Konynenburg, who will talk about reviewing the gaseous isotope releases from the waste package.

MR. VAN KONYNENBURG: Well, it's my pleasure to respond to the Board's request on a discussion of gaseous radionuclide transport to the accessible environment. Before I start with my viewgraphs, I want to make a couple of general comments which are probably obvious, but I'll say them anyway.

First of all, there are several things necessary in order to have radionuclide gaseous transport to the accessible environment. First off, we have to have a long enough half-life. It has to be long enough that the particular nuclide is in the spent fuel and then it's there long enough to undergo the transport processes, however long those take.

Then we have to have a high enough vapor pressure. Vapor pressure, of course, depends on a number of things. First of all, the chemical form of the species. Some chemical forms of a particular radionuclide have a high vapor pressure, others have a low one. We have to know which one we have. Secondly, we need to know whether the particular chemical form is in the pure state for that compound or element, or whether it's diluted by some phase that it's dissolved in. That, of course, will lower the vapor pressure by Raoul's Law or Henry's Law or something of that sort.

And then finally and most importantly is the temperature. Vapor pressure depends exponentially on the temperature by a relationship and we, of course, have a range of temperature. The highest temperature we anticipate in the waste package is the designed temperature for the cladding, which is around 350 Celsius. By the time the containment period of 300 to 1,000 years is finished, the temperatures, maximum of the spent fuel, will be under 200 Celsius. And then, of course, from there up to the surface, the temperature falls to the ambient temperatures up at the top. So we're concerned about a range of temperatures and it's important what the vapor pressure is throughout that range.

Another thing that's important is that the radioactive species has to escape from the waste package. Now, as was mentioned earlier and as everybody here, I think, is aware, we'll have a much more comprehensive discussion of the waste package in Livermore in the middle of January, so I won't say so much about that, but it's implicit that it has to get out of the package to get to the surface. And then finally, we have to have transport through the geologic environment. Anything that gets in the way of that will prevent release to the accessible environment.

With that, then, I'd like to look at some of these radionuclides and compare them and see which ones can get through that screening. This slide has a listing of radionuclides which have been found in various circumstances, either in the elemental state or in the oxide states, to have a significant vapor pressure, and I've put them on regardless of half-life, and you can see some of these are quite short.

Now, the species marked by the asterisks--there are five of them--those are nuclides that have sufficiently long half-life that they could be present in significant amounts after the containment period of 300 to 1,000 years. That eliminates a lot of things. Now, you may or may not want to take that as a criterion, depending on whether you believe the waste package will work, but the regulations say that it must, and so I'm going to assume that it does. If you want to argue about that, come in January.

So then I'll restrict my consideration to the five that were marked there, and first I want to look at the regulations in regard to those nuclides. What I've plotted here is, or listed here is the inventory to thousand years in curies per metric ton of uranium in the spent fuel, and then the release limit, cumulative 10,000 year release limit established by the most recent version of the EPA regulation, which of course, as we know, is under review and these numbers could change. But this is what we have as of now.

You'll notice here that I've got two of them marked again with an asterisk, and those are the two for which the inventory is larger than the cumulative 10,000 year release limit. For the others it's less. Now, just because it's less doesn't mean you can ignore it, because in the EPA regulation when release is less, the allowable release, what you're supposed to do is set up a ratio, a fraction of the release to the allowable release and add up those fractions for all the radionuclides that are controlled in the regulation, and that fraction has to be less than one. So we have to think about it even though it might be below the release limit.

The release rate limits from the NRC for the same five radionuclides. Now, everybody will recall that, in general, it's one part in 10^{-5} per year and that's what holds for technetium. However, for the others it's larger and the reason for that is because they have to rise to that value in order to have a release rate that would constitute more than a tenth per cent of the calculated total release rate. So that's the limitation in 10 C.F.R. 60 for those that aren't present in very large amounts. So all of these can take advantage of that, except for technetium, which is present in large enough amount that it is fixed at the one part in 10^{-5}.

Now, what about the chemistry? And I'm asking the question there at the top, would these species really be present in their volatile forms? So I'd like to kind of progress from the left through this and see if we can learn something about it. Again, I've listed the five radionuclides we've talked about, and now notice here I say, what is their probable location and their probable form in spent fuel? Some of these are known fairly well; others not so well, so I've put the word "probable" there.

^{14}Carbon we know quite a bit about. We know that it's present at the surfaces of the fuel rods, and by that I mean the zircolloy cladding on the outside of the zircolloy cladding. There's ^{14}Carbon there. We know it's within the bulk zircolloy. We know it's within the bulk UO_2. We know that at least some of it is present as the element, and probably some is present as a carbide.

Now I'll just continue on down here and then we'll come this way. For the ^{79}Se, the best knowledge we have is that it's in the bulk UO_2. Our problem with selenium is it has a very low fission yield. It's out on the shoulder of the fission yield curve, so it's hard to find it. There isn't much of it, but the best information we have is that it's still in the bulk, not out in a separate phase.

The ^{99}Tc, again in the bulk UO_2, and then here at ^{129}I we know is in the--partly in the fuel clad gap and partly in UO_2. Now, the portion that's in the fuel clad gap is known to be cesium iodide. Now, this has been studied a lot because of the other isotope of iodine, the eight-day Iodine-131 which is important in reactor accident situation and in response to Three Mile Island there was a lot of study of iodine because there was much less release of iodine that was predicted by the code, and it was because of the chemistry of the iodine.

And then ^{135}Cs we know is present in the fuel clad gap, also in UO_2, and it's present in several forms; the oxide, uranate, molybdate, cesium iodide, which takes up essentially all the iodine in the gap, and then as cesium metal probably, too.

Okay. Now, the high pressure, vapor pressure forms are given here and in the case of cesium you can see it's the metal. Iodine it's the element, and we have oxides here. Now, this question is very important, I think, and that is: Under the oxidizing conditions that we expect with air present in Yucca Mountain, could those species exist?

Well, first of all, for cesium, the answer is clearly no. Alkaline metal loves oxygen and it's not going to be there as the metal, and so that one gets wiped out. Carbon dioxide, it's clearly yes, as we've been hearing this afternoon. Iodine, only in small amounts because we'll see a little later, but iodine has been found in an equilibrium with an air and water environment to be partitioned primarily into the liquid phase so long as we have low concentrations and a neutral to alkaline pH. A lot of studies have been done of that for Three Mile Island, and that was the key there, that most of the iodine doesn't go into the I_2 form, it partitions into the water.

And then on the other two I've said yes in terms of could. Now, that's not the same as would, because if we also have water present, these two will hydrate, forming a selenius acid and pertechnetic acid. Those can ionize and do, and there can be reactions with the geochemistry there. So yes, they can exist in oxidizing conditions, but if they were able to get out and move up into the rock where it's cooler and there's liquid water present, then it's most likely that they would hydrate, ionize, and would no longer be transported very much in the gaseous phase.

So in conclusion, then, cesium is knocked out; iodine to only a small extent; and the others, if they escape the spent fuel with the provisos that I've just given you.

Now, finally, I'm going to give you some numbers for vapor pressures, and what I mean here is equilibrium vapor pressure for the pure species, either the pure element or the pure compound, and I've plotted just two temperatures, 100 and 200 Celsius. Remember, I said that we should be substantially below 200 for most of the fuel after the containment period.

Well, this is no surprise. CO_2, of course, is a gas at the conditions we're used to. I_2 has a fairly substantial vapor pressure, 3.7 atmospheres at 200, and you can see the others here. And of course, as the temperatures move on down to ambient as you move up to the top of the mountain, these things continue to drop exponentially.

Now, I want to look individually and review just a little. First of all, Carbon-14. This is the one we've done the most work on. The first two points here I've already discussed--actually, the first--this one and this one I've discussed. I also mention here that DOE has established a performance goal for the containment period and defined that as 1×10^{-6} of the current inventory for Carbon-14. That is what DOE has proposed as a definition of substantially complete containment during the containment period. Then we have the EPA limit, the NRC limit, and this is some data that we've gathered within the Yucca Mountain project, done by Harry Smith and Dave Baldwin up at Pacific Northwest Lab, on contract, and what they have found--and I think this is an important observation--that it's released, Carbon-14 is released as CO_2 from the outside surface of spent fuel cladding when heated in air, and up to 2 per cent of the total inventory of the spent fuel is released at 350 within eight hours. Now, 350, as you can see, is above the temperatures that are significant at the end of the containment period, but it gives you an idea of what the maximum potential release is from the external part of the clad.

DR. LANGMUIR: Could they take those tests longer to see how much more came out?

MR. KONYNENBURG: They flattened out there. They did a series of temperatures. That was the maximum temperature they ran, and they did run longer, but the release essentially stopped at that--I mean, it reached sort of a limit.

Some more information here about Carbon-14. In some work done at Oak Ridge several years ago, called a veloxidation process, spent fuel was roasted in air at 480 C. Now, that's a much higher temperature than we're dealing with, but that's a piece of data at least, and in that case 50 per cent of the Carbon-14 present in the UO_2 was released within four hours. So we can get Carbon-14 out of UO_2, as well as off of the clad.

Now, aqueous release of Carbon-14 would also contribute to the gaseous Carbon-14 dioxide release because of isotope exchange in the pore space, and then this last topic here is going to be discussed by the next speaker, Ben Ross, but it is believed that transport is rapid compared to radioactive decay, and by that I mean transport times seem to be not large compared to half-life for Carbon-14.

Now, Iodine-129, we haven't actually done work on this in the project, and so what I'm going to give here is really what we've gleaned from the literature. We've talked about these before. We know it occurs as cesium iodide in spent fuel and that has a very low vapor pressure at the temperatures we're interested in. In order to get much transport, that has to oxidize to I_2, and we don't have a lot of data on the kinetics for that reaction. What little data we have indicates that for that to happen at significant rates, you have to have a temperature close to 500 C. Now, this next data point is at 480 C. This again is from the Oak Ridge work, and again, roasting in that same circumstance, they found that 44 per cent of the iodine in the spent fuel adhered to the walls of the vessel they were roasting in, stainless steel roasting vessel, and it came out of the spent fuel and adsorbed or reacted, was occluded on the walls of the vessel, and only one per cent was released to the gas stream that was flowing through the roaster. The rest remained in the spent fuel which, by the way, under these conditions, was converted to U_3O_8 from UO_2 and turned into a powder, so this is a very brutal treatment in comparison to what we anticipate, and they only released one per cent.

During transport, as I said, we expect that the partitioning would be favored into the aqueous phase and that's by a factor of a thousand or more. But nevertheless, we are dealing with a half-life like 16 million years, and so even though this will give retardation, that long half-life can still lead to entry into the global circulation at some time.

Okay, now technetium, you've seen these two before. In LWR fuels, it appears to be present mostly in bulk UO_2. Now, some is present in noble metal precipitates with these other metals, and in high burnup fuels, high temperature, high burnup--for example, UO_2 fuel studied for the breeder reactor program, the technetium essentially all goes into this noble metal phase, but in LWR fuels it doesn't have a chance to do that, so most of it appears still to be in the bulk.

And this is what I think is going to happen, that primarily the limit on release of that will be--if we're talking about gaseous release, it will be the oxidation rate of the UO_2, and after it cools down and liquid water is in contact, then aqueous release is going to be limited by dissolution of UO_2, so that one is primarily trapped within the phases that it's found in.

Selenium, we've talked about these two points. Again, it's probably in bulk UO_2. It would have to be oxidized to SeO_2 for gaseous release, and that release is probably, again, going to be limited by the oxidation of UO_2, and then if it ever did get out, we have the hydration and the lowering vapor pressure as the temperature goes down.

Now, for cesium, I think the most important point is the third point here. It simply will not be present in a volatile form when we've got air there, and to confirm that, again from the Oak Ridge work, same conditions we talked about, only 10^{-2} per cent adhered to the burner walls. 10^{-3} per cent, which is, coincidentally, one part in 10^{-5}, escaped the burner through a sintered metal filter with this pore size, so part of that material could have been very small particulates, not necessarily even gaseous. But nevertheless, a very small amount of cesium gets out under those conditions with air in there.

So finally, then, to conclude, the potential gaseous release of $^{14}CO_2$ from the conceptual design waste package would likely exceed the current NRC and EPA release limits. DOE is currently considering what approach to take on waste package strategy.

Iodine would likely undergo some gaseous release from the waste packages, but the inventory is less than the current EPA limit and we also have the fact that partitioning is primarily into liquid water during transport. For these species I expect the problem would be less severe than for the earlier ones because we have lower vapor pressures, because it's diluted in spent fuel, and because of the hydration. And then, finally, cesium, I think, is a non-problem because it's not volatile under these conditions.

Any questions?

(No audible response.)

DR. ROSS: I'll be talking about work that's ongoing on the subject of modeling Carbon-14 transport in the gas phase. It'll be mostly work that I've been doing with my colleagues at Disposal Safety, but I will also mention some other work.

Let me start by talking about a conceptual model. I'm referring to it as the baseline because I'm not going to try to defend it as true, although it might very well be true, but I'm more presenting it as a starting point for discussion and a starting point for analysis, and that's the following:

First, the gas transport, the dominant mechanism is advection. It's carried along with the gas flow, as opposed to molecular diffusion or some other mixing mechanism. Second, the gas flow is driven solely by buoyancy, that we can neglect the barometric pressure effects. We're also neglecting the effects of molecular diffusion of the different air components, which I'll talk about that somewhat later because at higher temperatures they do become important.

Next, there is an isotopic equilibrium within the fluid phases, and really, the point goes beyond that. That is the principal mechanism of retardation, the main mechanism by which the Carbon-14 moves faster than the air--it moves slower than the air itself moves, and it has two aspects. First, you have an isotopic equilibrium between CO_2 and dissolved carbonate species; and second, you have an isotopic equilibrium between the fractures and the matrix pores. Finally, we have a system in which there's calcite in fractures all over the mountain and you have a chemical equilibrium and this isn't an assumption, but rather the result of some calculations that calcite buffers the pH pretty well.

Well, if you start with this model, you basically need--with this conceptual model you need three kinds of models, numerical models to do your calculations. You need a model of the chemistry telling you how much carbon is in the gas phase and how much in the liquid phase. You need a model of gas flow, and then you need to do some kind of calculation of Carbon-14 transport, and the nice thing about this setup is that none of these models are coupled to each other. The Carbon-14 is trivial compared to the other carbon in the system. The effects of CO_2 on the gas density are very small compared to the temperature and water vapor effects, so that these two models can operate on their own and then you get the output of those two and feed it into this.

Now, I've sketched this very nice and attractive model and let me first, right up front, mention two problems that we have in the way of discrepancies with real data. First of all, we have a question: Is there a mechanism driving gas flow that we haven't thought of? I think Ed Weeks mentioned that the predicted temperature--well, the predicted gas flows at Yucca Mountain are considerably greater in the upward direction than what the buoyancy model predicts, and as far as I know, this isn't a question of alternative conceptual models because I haven't heard of any--it's not that we can't choose between the explanations of this. It's that I think we don't have any explanations, but I'm sure at some point we'll understand it and we'll probably think that we were being very stupid not to have thought of the explanation, and that may raise some questions. We may have to change our conceptual model.

Second question is that we have left out sort of old style sorption as a retardation mechanism. We're only letting stuff be held up in the liquid phase, not on solid surfaces. Now, at the Sheffield, Illinois Low Level Radioactive Waste Site, there's been some work by Rob Striegl of the USGS, where he finds that adsorption of Carbon-14 on--I think he thinks on oxyhydroxide surfaces--is a major factor in holding up the Carbon-14, and the experimental work has not--the geologic environment is very different at Yucca Mountain, so I think it's fair to say, you know, maybe the geochemists can guess, but I can't even guess whether the same thing would happen here. But whether or not we guess, we ought to do the experiments.

Now I'll talk about each of these three kinds of modeling that I mentioned, the chemical modeling first. Then I'll talk about the gas flow modeling, and finally, a little bit about Carbon-14 transport.

Well, our objective here, given our conceptual model, is to determine the ratios of the dissolved carbonate species to the gaseous CO_2, and that will be a function of temperature and CO_2 partial pressure, and once we have those we can use them by a very simple formula along with the saturation to get a retardation factor for carbon. All this, as I said, assumes isotopic equilibrium.

What are the assumptions we make to do this modeling? First, we assume that there is secondary calcite all over the mountain in sufficient amount that you can assume that there's equilibrium of the solid phase, and that that's the source of the calcium in the water, so we ignore the possible source of calcium from silicate weathering. And finally, we assume chemical equilibrium on carbon species and that, no kinetics. That's a real good assumption. It turns out that's the reaction that's used to calibrate instruments, so somebody did a computer search and got a printout like that.

We took the data that was--I shouldn't say the data that is available, because it was the data that was available to us about a year and a half ago, and tried to do some modeling with this. The data, due to the difficulties of squeezing water out of the unsaturated tuffs and, as we heard this morning, I think the methods have gotten better since this data that we used, but this is some of Al Yang's data from a few years ago. We didn't have a CO_2 concentration or a pH, and also the data didn't have a very good charge balance, so we took two different approaches. Either we fixed it to--we adjusted the calcium to get a charge balance or we didn't, and in either case we let it equilibrate with the measured partial pressure of CO_2, which is probably a much better number than the liquid concentrations--and if Al Yang doesn't agree, he should jump on me--but we then equilibrated that until we had something that reached equilibrium and we did it, as I said, either with or without adjusting the charge balance.

Then once we had that, we allowed it to equilibrate with calcite and cristobalite as we changed temperature, and we got the total dissolved carbon as a function of temperature. Now, when all this was done, we found that if we had known the answer we wouldn't have had to do so much work because the pH is pretty well buffered, always seemed to be around 7.7, 7.8 no matter what we tried to do, and when we put this in with the measured saturations in the different units, we got a retardation factor. This is the factor by which the Carbon-14 moves slower than the rock gas, and the numbers are different for different units. That's simply a function of these units having different fractional saturations, and as temperature goes up, the retardation factor goes down, and that's simply a function of the fact that calcite solubility goes down with temperature.

That's what we did on chemistry modeling, and it's one of these things that seems a lot less interesting after you've done it than it might have seemed before you started. Now I'll talk about gas flow modeling, and this work has been done at a number of different places.

At USGS, Ken Kipp has done some modeling of gas flow under current subsurface temperatures and for atmospheric temperatures of various seasons. This is essentially in an effort to explain the field observations, and there's certainly some more work going on at the USGS. I'm not sure it's as elaborate as what Ken did a couple years ago.

Next, there is work at moderate subsurface temperatures which I define as up to about 60 or 70 degrees C. that we've been doing, and I'll talk about that in some detail. Finally, there is high temperature work. Lawrence Livermore has done high temperature modeling, focusing on the immediate vicinity of the waste package. I won't talk about that. LBL, Karston Pruess, Yvonne Tsang, Joe Wang and Christine Doughty have done quite a bit of modeling looking at both the waste package vicinity and the mountain scale, and that's of so much importance that even though it's not my work and I'm basically only familiar with it from the published literature, I think I have to talk about it some.

The basic mechanism we're talking about for buoyant gas flow, I think Ed Weeks has already explained, so I won't go into it any more, and as he said, the temperature differences comes from several factors. There's a geothermal gradient. There is the heat contributed by the repository, and finally, of course, seasonal changes in atmospheric temperature cause a difference between the subsurface and the atmosphere and this causes expansion of the gas and increased water vapor content.

Now, in our modeling at Disposal Safety, we made several assumptions which allowed us to simplify the model a great deal compared to what you have to do to get up to higher temperatures. First, we assume ideal gas behavior. We assume 100 per cent humidity, which is a very good assumption as long as you're below 95 degrees C. We neglect molecular diffusion. This is the key assumption that limits how high we can go in temperature. We assume quasi-steady-state flow, that the pressure changes in the atmosphere have equilibrated through the mountain. For the modeling that we are doing, which is trying to get an annual average gas flow, that's a very good assumption and the time which it takes for barometric pressure changes to propagate into the mountain is very well measured from the barometric studies and it's more than a few days and less than a year.

We assume a single porosity medium. We assume that there's a pressure equilibrium between the matrix pores and the fractures. We went to some length to validate that, and it turns out to be very good. Again, on the short time scale of barometric effects, you do have to look at dual porosity effects, but not on the scales we're interested in. Finally, we assume that the saturation is constant in time, but of course, this can be dependent on space. What this means physically is that if gas is flowing through a temperature gradient, it's getting hotter, water will evaporate. What we're assuming is that water, enough water is able to flow back in the liquid phase to keep that area wet where the water is evaporating.

The approach we used to the modeling was we formulated our equation in terms of fresh-water head. Basically, this is an approach that's used a lot in modeling saline waters; has not been used before, as far as I know, in gas problems. Essentially you're just cancelling out the big terms in the equation, they cancel; pressure versus the weight of the air.

We solved it by node-centered finite differences. When you use this fresh-water head approach, you have a problem dealing with zone boundaries. We had to play around quite a bit with our governing equation and finally came up with something that when you go back to regular water flow problems, it reduces to the usual refraction method of handling them, and we implemented this--as a matter of some interest--on a spreadsheet program, on Symphony, actually, which is essentially the same as Lotus 1, 2, 3, and were able to get all sorts of results from this and only lately have finished translating this into Fortran.

Now, for the simulations we did, we used this geometry. We have beds that dip six degrees to the east. As you can see, this is very similar to Ed Weeks' cross-section of Yucca Mountain, because it was copied from it. The repository is located here. We have a no-flow boundary at the bottom of the Topopah Spring welded unit. The gas permeability of the Calico Hills unit is at least an order of magnitude down, and given that you have assumed no-flow boundaries here and here, this just won't contribute to the flow. So the water table actually is down here someplace.

We have a non-welded unit here, and then the welded unit on top. This is a--

DR. DEERE: Excuse me. Where's the Ghost Dance Fault there?

DR. ROSS: The Ghost Dance Fault is somewhere over here. Well, actually I'm not sure because this--it may actually be off the cross-section. The location of the section is shown in the paper that's attached to your notes, and this is actually across the southern end of the mountain. We were actually limited in this by computer memory. The problem with doing it on a spreadsheet is it stores the governing equation over again in every cell.

Some of the assumptions we used in these simulations, we simply had a surface temperature equal to the mean annual temperature. We put the repository at several different temperatures, corresponding to heating; one, measure current temperatures and we increased the temperature by 3 degrees, 14 degrees and 30 degrees. The temperature decreased linearly to the surface from the repository. We had a sharp temperature cutoff at the edge of the repository, which was unfortunate, but a matter of time, and we assumed that the non-welded tuff unit is ten times less permeable than the welded units. Now, the welded unit permeability has been measured by backing it out of the barometric pressure effects on downhole pressures. The non-welded unit air permeability is much less well known. This is probably the high end of a reasonable range. It could be a hundred or a thousand times less. On the other hand, one factor that we haven't taken into account is that the heating of the repository and the thermal expansion of the rock may cause fracturing in that unit, and that might increase the permeability over the current conditions. So this is certainly a subject of sensitivity analysis in the future.

The gas flow vectors that we get for current conditions, surface and subsurface, annual average looks like this. The temperature is approximately 30 degrees C. at repository depth; varies a little bit in the simulation because of the geothermal gradient, and you'll see that there where the Topopah Spring unit is open to the atmosphere, there is an inflow, and then you get about what everyone always sketched. And as Ed mentioned, you do get a very substantial flow updip, both below and above the semi-confining bed.

This next slide is mislabeled considerably. This should be repository at approximately 33 degrees C., and you'll see we have the same gas velocity scale and what's interesting is that by heating the repository only three degrees, which is an amount of heat that will probably be there for quite a long time in the future, my guess--and it's only a guess--is that that'll go past, well past 10,000 years--you still get a very considerable increase in the gas flow, two or three times maybe, something like that.

And to see the same thing again--and this is not in the paper because we've just generated them--here are path lines for six gas particles starting from different parts of the repository under current conditions, and then if you heat the repository by three degrees, you get a very substantial change.

Well, that's interesting, and I should mention, as you'll see in the paper, if you heat the repository by 30 degrees, you will only get a gas flow velocity increase of another factor of two or five, but anyway, less than an order of magnitude over the three degrees of heating. So the time the repository returns to its--that the temperature effects are all gone as far as gas flow is concerned, is very far in the future.

DR. LANGMUIR: Is there any obvious reason why just three degrees has that much of an effect on the flow?

DR. ROSS: Yeah, because the natural driving force is only a few degrees different, so when you increase the subsurface temperature by three degrees, you're increasing the difference between subsurface and atmosphere, I don't know, 50 per cent, or doubling it.

DR. LANGMUIR: But three more degrees has nothing like that effect?

DR. ROSS: Well, three more degrees would presumably increase it, you know, instead of doubling it you're adding another 50 per cent or however it comes out.

Now, the work that's been done at LBL that's been published falls in two main categories. Yvonne Tsang and Karston Pruess have a paper on water resources research where they've done numerical modeling of gas flow with a pretty realistic repository geometry and geometry of Yucca Mountain.

Also, Christine Doughty and Karston Pruess have published some papers where they have semi-analytic solutions for the heat transfer around a line heat source, so it's a very unrealistic geometry and they have no gravity in it, but since it's semi-analytic, you're able to get a lot of--an awful lot of good intuition out of it.

In the latter work, they distinguish four heat transfer regimes, and we'll go from the outside in. Furthest away from a waste package or a repository, we have an undisturbed zone, and as I've indicated, that may be so far away that it's not really of interest.

Next in we have an outer conduction zone. In this zone, the temperature is below the boiling point of water, which is 95 degrees at the repository elevation, and conduction is the main heat transfer mechanism, and I should add that this is without gravity. With gravity, you'll get this buoyant flow and you may get a much greater contribution of latent heat transfer due to the buoyant flow, so that conduction may become secondary to convection of latent heat, even in this zone. But that's something we really don't know.

Next you get what's called the heat pipe region. In this region, the temperature is pinned at the boiling point of water. You will have gaseous water vapor flowing out from the repository or the waste package, condensing here; liquid water flowing back in under the influence of the suction gradient, it evaporates here, comes back out again. This is a very efficient heat transfer mechanism and, therefore, if the permeability is high enough, you can get a very large region in which the temperature goes no higher than 95 degrees.

And then finally, depending on the assumptions you make about what the parameters of the system are, you get an inner conduction zone, which is dry, so you don't have a heat pipe effect, and again, the ordinary conduction and whatever convection of substantial heat there is will be the heat transfer mechanism. And I should only add that this is without gravity. When you throw in gravity, the effects may be to blur the distinction between these two zones somewhat, but again, that's something that I don't think we've got a real good handle on.

Now, since it's not my work I'm not going to go into the details, but I do want to talk a little bit about what I think the implications of their work may be for the Carbon-14 transport problem. First of all, in the inner conduction zone and maybe in the heat pipe region, Carbon-14 is going to move away from the repository so quickly compared to how fast it's going to move in the outer conduction zone, that it probably won't be worth our while to think too hard about the details of how it moves, that the time delays in the inner conduction zone are going to be so much shorter than in the outer conduction zone, it's not worth calculating what they are.

However, we need very much to know what the heat transfer is in that zone because, clearly, to be able to model this gas flow, we've got to have our temperatures, and the effectiveness of heat transfer away from the repository is going to be what determines the temperature field. And, of course, Rich Van Konynenburg and I are not the only ones who are interested in temperatures. Lots of other people in this project need to know what temperature is.

Now, a third point is that there can be another need for this modeling in the Carbon-14 problem when the partial pressure of water becomes large compared to, you know, a large fraction of one. The diffusion, two-phase gas diffusion becomes important in the overall mass flow, and at that point the simpler modeling approach that we've used stops working. We don't know where that is. I'm sure our model is good up to 60 degrees, probably 70, maybe 80, but at some temperature you're going to have to come in with the much more complicated LBL model.

Let me talk very briefly about one possible approach to transport modeling, and that's the following: That we would assume instantaneous travel through the high temperature region, basically ignore the near-field; then simply calculate travel times along the path lines using temperature dependent retardation factors like the ones I showed earlier, and ignore all the mixing processes. It would be nice to be able to do this. I hope we are going to be able to do it, but we may not, and I'll give you one possible reason that we may not.

Going back to this geometry, we've assumed only ten times permeability contrast here in the semi-confining bed. If you raise the permeability contrast to a factor of 100 or a factor of 1,000, you're clearly going to get much less flow through that bed, and you're likely to get some kind of circulating cell here, and then an independent flow up there.

Now, this bed has plenty of porosity, so gases will be able to diffuse through them even if they don't flow, so what you will have to worry about is that Carbon-14 will be advected up to here, then diffused through this confining bed, and then get advected out the top, and we'll have to go to a more complex approach if we do that, if that turns out to be the physics of the problem.

So let me just summarize some conclusions. I guess I did about as well as people in the morning, coming up with my conclusions at the right time.

The Carbon-14 travel time is quite uncertain. I've deliberately not thrown up any calculated numbers, but if you take all the assumptions that we've worked through, you get travel times in thousands of years under ambient conditions, and maybe even down into hundreds if you heat up the repository enough. How it's finally going to come out, I don't know. It's very possible that when you know more you will find that the travel time is more than 10,000 years, but it's also very possible that when you know the situation it will be less than 10,000 years, maybe a good bit less than 10,000 years, and I don't think you can rely on it being either way.

The second point to make is that the subsurface temperature at the time the Carbon-14 is released is going to be a key variable, because the gas flow velocity is very sensitive to that parameter.

Any questions?

DR. DEERE: In this last statement that you have in your conclusion, this is released at your 300 to 1,000-year period; is that right?

DR. ROSS: Yeah, when it comes out of the package, when it starts to come--well, when it keeps coming out of the package, for that matter.

DR. DEERE: Would this be the same conclusion if the fuel went in at 40 years aging rather than five years, or does time catch up with you so, say, it could be 10,000 years, that it doesn't look like it could be as great as 10,000 years, but at higher temperatures it would be in the hundreds of years.

DR. ROSS: Yeah. Well, I don't know what the temperature is going to be under any aging. I mean, I'm sort of agnostic.

DR. DEERE: So it's obvious that time is a variable?

DR. ROSS: Yeah. Time is important, but I think--

DR. DEERE: It's very much dependent on the temperature?

DR. ROSS: Well, the temperature depends on two things. One is, it depends on--I'd list three things it depends on. It depends on the cooling of the waste before it goes in. It depends on the lifetime of the canisters, and it depends on how efficient the heat transfer is away from the repository.

DR. LANGMUIR: Does the problem go away, though, if the temperature at the start is 100 degrees, as they intend to do in the European situations?

DR. ROSS: No.

DR. LANGMUIR: You still could have a release problem?

DR. ROSS: You could, yeah. I'm not saying that there will be one. I mean, you know, once we know more we could come out with a number greater than 10,000 years.

DR. DOMENICO: Is there an engineering fix for all of these, for not only this one, but the rest? Is there a fix? It seems like you need a fix.

DR. ROSS: Well, first of all, I think you've got to ask a couple questions. Is there a problem? I mean, you know, what is the health effect of--

DR. DOMENICO: Well, there could be a problem.

DR. ROSS: Well, there's a regulatory problem, but is there a health effects problem is a very different question. You know, yeah, if you built a 10,000-year canister, you'd solve the problem.

DR. DOMENICO: I don't think that fix is permitted.

MR. BLANCHARD: I think that this topic will be pursued in greater detail, Pat, at the January meeting on waste package. I hope you come and ask similar questions.

Don, Dwight Hoxie's talk will be less than the time allowed, so if you want to give the group here an extra ten-minute break, that's perfectly fine with us. Come back at four o'clock?

DR. LANGMUIR: Okay, fine.

(Whereupon, a brief recess was taken.)

MR. BLANCHARD: The next speaker is Dwight Hoxie from the Geological Survey, and he will be discussing the validation strategy and how to build reasonable assurance with modeling.

DR. HOXIE: Well, today we've had a lot of talks on very technical matters, and now I think we're going to change pace for the last talk of the afternoon, and wax philosophical, I'm afraid. We're going to talk about model validation.

And as you probably realize from the talks earlier on today, that models and modeling are going to be a very important aspect and took that is going to be used throughout the Yucca Mountain project, and one of the problems that we are facing right now is that model validation is something that, I think, is in the eyes of the beholder and it's a very contentious issue and, like I say, we're going to be getting a little bit philosophical, and that's the kind of note that I want to put us in right now.

One of the first things I'm going to want to do is to try to talk about some of the vernacular that we use in models with regard to model validation. The kinds of modeling that we're going to be doing, I might just mention at the outset, with regard to the Yucca Mountain project, is we're going to be using models as tools to understand things that are going on. We're also going to be using models to predict the performance of the potential repository, and the physical processes that will be occurring within the Yucca Mountain site. And so what we really need to do is to talk about model validation in the context of building confidence or reasonable assurance that our models are providing us with essentially--maybe not correct answers, but reasonable answers; answers that we can believe in, that we can defend.

So the overview of my talk is going to be this idea of model validation strategy with regard to building reasonable assurance or confidence in our models, and I'm going to break it into four different topics. First of all, I want to get rid of semantical difficulties and talk about terms and definitions. Then I want to talk about what we mean by a model development kind of methodology in itself, and then I want to talk about the model validation methodology that we have been attempting to develop within the project, and where I'm coming from in that regard is that we have within the project that was formed about three years ago, a group known as the validation oversight group, and this group consists of personnel from the participants, from DOE headquarters, from DOE Yucca Mountain Project Office in Las Vegas, and from contractors to the various participants and DOE. And we have developed essentially a model validation methodology, and we have prepared a document, a proposed draft model validation methodology which was dated October, 1989, and has been submitted to DOE headquarters for review. So we are still in the process of developing our methodology, as it were.

And then what I would like to talk about are some model validations, experiments essentially, with respect to the Yucca Mountain project itself.

So the terms that I really want to talk about are going to be model, and I want to talk about code verification; that is, the term, verification, as distinct from model validation. I want to talk about model calibration and again, distinguish between calibration, verification and validation, and then finally, I will offer a definition of model validation.

Trying to move right along, I will submit that the kinds of models that we're talking about are quantitative, numerical sorts of models that essentially predict things. So I'm saying that a model, from our point of view, is a mathematically-based representation of a physical system by which the state, the particular state of the system or a succession of states in time--that is, evolution of the system--can be quantitatively predicted. So we're talking about essentially predictive models. And I want to make it very clear that a model is a representation of a system. A model is not the computer code or the analytic technique or formula or whatever that you use to make the computations.

Verification we distinguish from model validation, or from validation, and verification is a term that we apply to computer codes, and any computer code that we are going to be using we are going to verify, and all we wanted to indicate there is that verification is the demonstration that the computer code is performing all of its numerical and logical operations correctly.

There's another term that often comes in when we're talking about models and modeling, and that's model calibration, and generally, what that refers to is that we have a system that we are trying to model. We know the state of that system at a particular, say, point in time if it's an evolving system, but we have incomplete data. And so what we do is fit the model to the system in order to fill in data gaps, and this is what I really mean by model calibration. And oftentimes this involves solving what we call the inverse problem in order to get parameter data or this kind of thing by using the model in reverse, essentially. We know the solution to the system because we know the system, so now we back out the data from that solution.

I just want to distinguish all these different kinds of things, again with respect to our model validation methodology, and I will again emphasize that, to my knowledge anyway, we have no consensus terminology within the modeling community as to what these terms might mean. People have even suggested throwing out things, throwing out terms like validation, verification, calibration just because they are imprecise.

So now we get to the nitty-gritty, and that is model validation itself, and all I really want to say--again restricting concern to predictive models--is that model validation is the assurance that a predictive model provides an adequate representation of the system that the model is intended to simulate. And the real problem that we're going to have here is what constitutes an adequate representation, and that's where we get into this rather gray area of building confidence and providing reasonable assurance.

That's enough of a glossary. Now I would like to talk just very, very briefly about what I regard as model development and the steps of model development.

First of all, we can regard this as sort of a model in itself. We have an input, and that is the system that we are intending to simulate by means of whatever model we develop; and we have an output, which is going to be the simulation results. So the first thing that we have to do if we're given a physical system that we're going to try to simulate, is devise a conceptual model for that system, so another kind of model, a conceptual model. And that's a set of hypotheses, and so forth. I'll run through these in more detail step-wise in just a moment.

Then we want to take the conceptual model, we want to translate that into some kind of mathematical model. Then we transform that into a numerical model by which we have performed the computations, and then finally, in general, if we're dealing with complicated systems we're going to have to rely upon a computer program to actually perform the numerical operations and calculations, and then to provide the output, which is the simulation results.

The first thing is our conceptual model that we may develop for the system, and that is going to have to do with the system geometry and structure, generally the internal structure. We have to identify the controlling mechanisms and physical processes that are operating within the system. We have to identify the boundary conditions which essentially represent a summary of what is going on external to the system that affects the internal configuration or processes within the system, and then we have to identify the initial conditions which are a summary of everything that's happened to the system prior to some initial point in time. So the conceptual model generally will consist of a set of hypotheses, for example, and we may have alternative hypotheses that may be valid that we may have to test, and so we have to validate our conceptual model within the context of the overall model validation strategy.

Given the conceptual model, we transfer that into a mathematical model, and in order to make the problem mathematically tractable, we are going to have to make some kinds of simplifying assumptions, approximations, and we're going to probably have to idealize the system in some way or another; for example, like assuming the validity of the ideal gas law. Generally, for our predictive models that are quantitative in nature, we are going to end up with some set of coupled integro-differential equations that we are, therefore, going to want to solve. In order to solve those equations and obtain a unique solution, we need to quantify a set of boundary conditions and initial conditions at some initial point in time, and finally, we also will need to identify and quantify any kind of constitutive relations that entered into the model; for example, the equations of state that are appropriate to the materials that compose the system, and for hydrologic modeling, for example, this would be the function of relative permeability as a function of matric potential for example. So these kinds of relationships we need to identify and quantify.

Finally, we end up with a numerical model which is actually going to be the set of equations that we are going to solve and the methodology for solving those equations. We first are going to have to decide what kind of approach we're going to take; that is, are we going to adopt a purely deterministic kind of approach and simply solve the differential or integral equations with their associated boundary and initial conditions, or are we going to try some kind of stochastic approach, which is becoming more and more in vogue as we realize that the systems that we're dealing with are so highly heterogeneous, that we really need to invoke some kind of stochastic methodology, and this stochastic methodology, I'm thinking of the kinds of things that Lynn Gelhar is doing at MIT and that others are doing using essentially Brownian motion diffusion kind of techniques.

Once we decide on our approach, then we can actually formulate some kind of solution algorithm for solving our either deterministic or stochastic equations, and then we have to worry about just practical matters that will affect how we are really going to go about solving the equations.

Oftentimes we're going to be dealing with non-linear systems, non-linear mathematical systems, so that we have to use some kind of iterative approach in order to solve the equations, and then we have to worry about the convergence of our solutions and establishing appropriate convergence criteria. We also have to be concerned with the stability of our solutions. This means, first of all, is our mathematical problem sufficiently well-posed that the solution is going to depend continuously on the boundary and initial data so that the solution will be stable. The other problem that we are going to confront is that when we are dealing with non-linear systems, the non-linearities themselves may induce a instability, an inherent instability in the solutions, and they may grow exponentially, for example, with time. We had an example of a non-linear growth this morning when the sound system went berserk, as you may recall.

The other thing that we need to worry about is the efficiency of our algorithms in terms of computer resources and money to purchase those resources. So we may have to involve some tradeoffs here in order to develop a numerical model that is both tractable and practical as far as solving.

And then now turning to the model validation issues, the first thing I would like to point out is that the models or numerical predictive models cannot be validated in any kind of generic or global sense, and we really have to validate or attempt to validate our models with respect to the specific system that we are trying to simulate, and this is going to pose great difficulties for those of us who are going to be using performance assessment models and site characterization models, for example, that are going to be trying to predict effects and events and behavior of the Yucca Mountain system for periods of 10,000 to 100,000 years, for example, because we are going to have difficulty validating our predictions.

The classical, or what I would contend is the classical model validation methodology is that we are going to compare the predicted state or response of our simulate system against an actual observed state or response of that particular system, and this brings me to another point with regard to our predictive models.

We can use our models in two different modes. First of all, we could, for example, be predicting a particular state of a Yucca Mountain system for which we have data; that is, a state, say, occurring at the present time, and we might be then using the model to interpolate spatially between points at which, say, for example, wells, boreholes where we actually have physical data. So we want to simply use the model as a device for interpolation.

The other aspect of modeling is that in the sense of this--well, again, they're both predictive models. One is predicting the spatial relationship, but we want to extrapolate the behavior of the Yucca Mountain system, for example, in time. So we can use models either to interpolate at a particular point in time, say with regard to the spatial variables, or we can use models to try to extrapolate into the future, and of course, if we are predicting a known state of a system where we actually have observations, we have better hope of actually validating the model against our observations. It's when we're trying to extrapolate forward into time, for example, that the real issue of validation becomes much more contentious.

So in the case of trying to predict the future with our models, where we can't really go through this classical methodology, we have to rely on some kind of indirect model validation methodology, and again, this whole idea of building confidence or reasonable assurance in our model predictions. And so the model validation strategy that is being proposed has been drafted by the validation oversight group is outlined here, and the first step is that we want to maintain a very good documented record of the model development; that is, the conceptual model formulation, the mathematical model formulation, the translation into the numerical model, and finally, the implementation in a computer code.

And essentially what we're saying here by maintaining this record of model development, is simply standard quality assurance kind of practices. Once we do that, then the next thing that--or another thing that we really want to do--these are not necessarily going to be done in sequence--is first of all, is to design and conduct field and/or laboratory experiments by which we can test particular model hypotheses; for example, particular physical processes that we want to incorporate into the model--and I will give an example of that here shortly. The other thing that we want to do with our experiments, laboratory and field experiments, is to provide data that's input to our models; for example, hydrologic relationship like the relationship between relative permeability and matric potential for a hydrologic model in the unsaturated zone, and then again is to use the experimental data, laboratory data perhaps in essentially an analog sense to assess the accuracy and adequacy of our model and its ability to simulate some actual physical system.

Another aspect that I incorporated that we incorporate in our model validation strategy--although it may not be exactly what we would normally think of as model validation--is to perform model uncertainty and sensitivity analyses, and that is simply to get some idea of what effects our hypotheses or errors or inaccuracies in our hypotheses in our data may--how these may impact the numerical results we obtained from the model. So essentially, again, that goes back to this idea of building confidence or reasonable assurance, a kind of a warm, fuzzy feeling, essentially, if you will, about our model data and its relevant parameters.

And then finally, and I think essentially for our performance assessment models where we're going to be extrapolating for long periods of time, we really have got to rely on formal technical reviews to be conducted by our technical peers out there in the community, and to assure that our model is really adequate for its intended application. And I'm hoping that we can find enough people out in the technical community that will stay independent of the Yucca Mountain project so that we can rely on you to provide us with these much-needed reviews.

I would like now to turn to the topic of how we might go about validating a model with respect to some experiments, and I want to relate these to the Yucca Mountain project, so these are not supposed to be generic. They are supposed to be kinds of experiments that have some bearing with physical processes at the Yucca Mountain site.

First of all, I'm going to actually be talking in some detail about an experiment that involved two-phase concurrent flow of steam and liquid water in an unconsolidated porous medium. This is an experiment that was done at LDL. I'll talk more about that in detail in just a moment.

Some other opportunities that we have for laboratory scale kinds of experimental model validation is the work that you heard Alan Flint talk about earlier today, and this is the liquid water imbibition and moisture release studies on tuff cores. We can try to model those, and we could take some cores where we use the core data to calibrate a model, and then try to predict the results on other cores to see if we can actually transfer from core to core, as it were.

And I might mention that we're going to have data from these kinds of experiments from USGS, from Sandia National Laboratory, and also from Lawrence Livermore National Laboratory. We're all doing those kinds of experiments.

Another set of experiments involves liquid water percolation through large blocks or large cores of fractured tuff and the United States Geological Survey is involved with those kinds of experiments, both in the laboratory and potentially in the exploratory shaft facility, and we have another, shall I say, repository of experimental data, a wealth of data that's coming out of the Apache Leap tuff site that's being operated by the University of Arizona in cooperation with the Nuclear Regulatory Commission, and the Apache Leap site was mentioned earlier today by Rob Trautz, for example.

And the other thing is the Los Alamos National Laboratory has proposed to conduct some solute transport experiments involving crushed tuff columns in some caissons that they have on the Los Alamos site, and then finally, we also need to look at the chemical modeling that is being done, and so we have rock water interaction and resultant chemical reaction products at elevated temperatures, a series of experiments that's being conducted at Lawrence Livermore National Laboratory, and these will be described tomorrow in tomorrow morning's presentations.

I just want to very, very briefly here talk about how do we go about setting up this experiment by which we are going to try to validate our model, and once again, as input we have the physical system. We then develop the conceptual model. On the basis of the conceptual model, we now take two paths. One of them is to go through the model development scenarios, which I show here on the left-hand side of the slide, so that we have our mathematical model, our predictive model, and our model predictions. But from the conceptual model, we can also use our concepts to design an experiment by which to validate or test the hypotheses in our conceptual model, for example, and so we have an experimental design. We set up the experiment. We measure system variables, and we get our experimental results. And so the classical model validation methodology we'd have is, therefore, trying to compare our model predictions against the experimental results and trying to make some assessment of how adequate the model is.

DR. PRICE: Could I ask a question?

DR. HOXIE: By all means.

DR. PRICE: This morning Dr. Flint presented models and data and different extraction methods of getting the water out of the core samples, and so forth. It looked like one model would fit one kind of extraction method, another model would fit the other kind of extraction method, and you've got experimental design here and math model. How do you know which one is the one to go by?

DR. HOXIE: Okay. I can answer the question. Okay, the first thing I really need to do is to point out that what Alan Flint was really talking about was not models in the sense that I'm using--not in the sense of being predictive models. Those were essentially curve-fitting relationships for experimental data, and so, again, since we have no consensus vernacular, we really would call those, I guess, empirical kinds of models. They're empirical relations. So they're not really the kinds of things that apply here, but I suspect the kind of thing that we would have to do with his work is to take a series of tuff cores, for example, from a--what we would define as a hydrogeologic unit--that is, something that is relatively statistically homogeneous. We would then come up with, from a series of experiments on a series of cores, a series of these curves, and hopefully we could define some kind of mean and standard deviation with how that data fit the various curves. And then if we wanted to validate that, we would have to apply that to other cores to see how well they transfer. And so this would be the kind of experiment that I would perform, anyway, with that kind of system.

DR. PRICE: But in working in the unsaturated core, wouldn't the experiment itself that you're carving out be a relatively new area in and of itself, and so, for example, getting the dependent variables out of the experiment would involve a particular methodology; in this case, getting a certain amount of moisture out of the rock and evidently you can get it in one kind of configuration from one kind of methodology, so you'd have to run through, maybe, a family of experiments somewhere.

DR. HOXIE: I think one would have to do that. Of course, what we're really trying to do there is to validate a technique that is appropriate for the tuff cores. And I think our problem is, is that some of those techniques are well-suited for high porosity, high interconnectivity kinds of material with high saturated conductivities, essentially. Some of them are not really appropriate, for example, to the very tight--especially the welded tuff matrix, so we have different kinds of methodologies for getting these relative saturation curves that--or moisture release curves that Alan was talking about.

But the actual experimental techniques are standard within the soil physics repertoire, essentially. The other thing about getting water out of the cores, I'm not sure what you're referring to there. I mean, that's to get chemical samples and we have, you know, one technique that is being pioneered essentially by Al Yang, with USGS, is this idea of squeezing the rocks to get the water out for water sampling.

DR. PRICE: Well, this obviously isn't my area, but I think there was a centrifuge method and a gas.

DR. HOXIE: Right, okay. That's for getting the moisture release curve--well, the hydraulic conductivity curves, also, but those are standard techniques. We're not developing those. We're applying them. What we're doing is applying them in a new area; that is, to indurated rock and ultimately to fractured indurated rock. We have to develop methodologies for that.

I don't know if I'm addressing your question or not.

DR. PRICE: That's fine.

DR. HOXIE: What I would like to describe is an experiment that was conducted in 1985. The report was published in March of 1986, and it was done by a Ph.D. student at Lawrence Berkeley Laboratory, and what the experiment concerned was trying to get relative permeability curves for liquid water and steam in a high temperature system where that's the only fluid phases that were present. So it's a one-component system; that is, just H_2O essentially, but two phases, steam and liquid water. And the experiment consisted of a column that was 7.62 cm in diameter and it was essentially one meter tall. It was arranged vertically as I show it here, and the column was filled with 100 micron diameter glass beads. I don't know how the packing was, because he didn't explain that, but presumably it was simply just filled up.

I don't show it here in the diagram, but there were pressure transducers essentially at two different points on the vertical column. I should mention this. Preliminary modeling was performed in order to determine what kind of end effects might be expected within this experiment, and I should also point out that the whole idea of the experiment was to cause a constant mass flux of fluid to move vertically upward through the system, with a parameter for different experimental runs being essentially the temperature gradient within the system. I'll explain more about that in just a minute, what that actual parameter was.

But anyway, the pressure within the system--both the vapor pressure and the liquid water pressure--was measured by pressure transducers. There was a set located at about a third of the way from the top, and also another one about a third of the way from the bottom. The preliminary modeling indicated that that would be the right spacing in order to get away from the boundary effects at the bottom and the top of the column where the fluid was entering and the fluid was discharging.

The basis of the experiment is that by causing a fixed flux of fluid to move through this system at a fixed, essentially, temperature gradient, that one would establish unit hydraulic gradient vertically over the central portion of the column, so that there would be essentially no capillary pressure gradient over some portion of the column; therefore, the saturation within the column, the liquid water saturation within the column would also be constant over a fixed, about a 50 cm vertical length. And so by measuring the flux through the system, then, for different ratios of steam to liquid water within the system, so different liquid water saturations, one could then develop the relative permeability curve for both liquid water and the steam.

Using those permeability curves, then, one could go back and use simply the boundary condition data and what one thinks the physics of flow is going to be to model the system, and see if one can predict the experimental outcome, and that's what we're going to do.

So the way that the system was set up is that we have a liquid water reservoir down here. The water is deionized and degassed, and then it's pumped into the system at the base of the column at a constant mass flux. Then we have a power supply over here with a certain amount of power output which will flash some of that liquid water into steam. So then steam and liquid water will move up through the column, establish a temperature gradient, appropriate temperature gradient. The walls of the column are completely insulated so there is essentially no heat flow out of the walls of the column, and then the water and steam are discharged from the upper end of the system and condensed, and so forth.

Saturation was measured within the system by means of a gamma ray densitometer, and again, that was also measured at two different points within the zone in which unit hydraulic gradient presumably would be established within the experiment. And just to remind you what the concept here is, is that we have the 100 micron diameter glass beads in the system. Surrounding the beads is going to be pore space.

Some of the pore space is going to be filled with liquid water, shown in the shaded areas here, the blue areas, which is going to be held in place by capillary forces, by surface tension, and then in the gas-filled void space is going to be steam. So we just simply have a one-component two-phase system.

Now, the model that we are going to attempt to validate, our conceptual model essentially, involves the two-phase flow of the steam and water through a non-isothermal system, and the code that we're going to use--and this is where we run into another problem of semantics, and so what I have done--and we have not done this in our model validation methodology, but I think I'm going to suggest that we do something similar to this--we have to distinguish between the computer code that we use to simulate a physical system, and the model of that particular system. But the code that we use may involve the physics of the system as well, so I'm referring to that code with all of its physics to be a simulator, and the code that was used to model this particular experiment, was the code, TOUGH, which Rob Trautz mentioned this morning, and may become a workhorse for many of our hydrologic modeling since it will handle multi-component, multi-phase flow, non-isothermal flow at that, so it's a TOUGH hydrologic simulator, and the physics that's involved in this is that, first of all, it does allow for coupled heat and multiphase fluid flow in, however, nonreactive porous media. So it does not include chemistry.

It assumes that the flow is taking place under Darcian continuum conditions so it doesn't allow for turbulence or for the inertial turns in the flow equation, for example, and it assumes that both the gas and liquid phases are flowing under Darcian type of flow. So we have to deal with small fluxes, that's one restriction.

We also assume that there is always local thermodynamic phase equilibrium within the system, although that phase equilibrium is a function of saturation and temperature. And we also realize, true to the unsaturated zone hydrology, that the capillary pressure and hydraulic conductivity are functions of liquid water saturation within the system, and then again going back to the same kind of assumptions that Rob Trautz and Ben Ross have been making that seem to be perfectly valid, is that air and water vapor are both ideal gases.

The experiment was conducted. The relative permeability curves were obtained, and then the model was used to try to go back and predict the experiment. There were six different experimental runs. Each one was parameterized by the power input at the base of the column; that is, the power that was required or used to flash the water into the steam, and I will show you what these power levels were in just a moment.

I have only one simulation that was reported in Dr. Verma's thesis that showed a comparison of, for example, the temperature profile, the vertical temperature profile, so plotting temperature in degrees Celsius on the left with height in the column starting at the base to the top of the column in centimeters along the horizontal axis. The solid line indicates the model-predicted temperature profile, and the dots represent the temperature measurements as measured by thermocouples placed along the column with their experimental error bars. And I don't know what these error bars refer to. Unfortunately, Dr. Verma does not really indicate whether they're 95 per cent confidence intervals or sigmas or whatever they might be, but the thing that I've been curious about this, we don't look like we have especially good agreement, but I think what is most important with regard to the experiment is not so much the absolute values of the temperature, but the temperature gradient itself, which the model seems to predict quite well.

We can get a much better handle, I think, on the degree to which the tuff simulator was able to reproduce the experimental results by looking at the mean saturation in that zone where the saturation was constant as a consequence of the unit hydraulic gradient regime, essentially. And what we show here is for one experimental run, the computed profile of liquid water saturation compared with the two measurements by the gamma ray densitometer of liquid water saturation within the system.

Now, the triangles represent the six different experimental runs, and over here to the right, I show the parameters for each of these runs, which is the power input at the base of the column by which the water was flashed into steam, so for the top measurement up here, for saturation of .75, so high liquid saturation corresponds to essentially a low power input. Finally, we get down here to essentially the irreducible saturation of about, let's see, just a little over about 37 per cent actually, corresponding to power inputs--these two overlap--of 327 and 500 watts, so much lower saturation because most of the fluid flux is being transported by steam. But the point is, is that this triangle here shows the mean value of the saturation in the unit hydraulic gradient zone for a power input of 151 watts to the system, and these are the two experimental measurements. This is the predicted value, and I would argue that we have quite good agreement and that there is quite a room for discrimination between the different experimental runs. Unfortunately, I have only this one computed profile, so I don't have a good statistical sample of independent validation experiments essentially.

The other parameter that we would like to predict, aside from the temperature and the liquid saturation, is going to be the pressure in the system, and in this case we used vapor pressure. So I show two--this is for the same run with a power input of 151 watts to the base of the system. Again, the solid line represents the predicted vapor pressure profile and the curves up here on all of these predicted profiles show you what the predicted end effects due to the boundary conditions were.

So we have the vapor pressure that's actually measured in the system by the pressure transducers at two different heights within the unit hydraulic gradient zone, and again we can compare that with the model simulation and, again, we get, I would argue, quite good agreement. Again, I only have that one example from his thesis.

So this is the kind of classical model validation experiment that we can conduct, but this goes a little bit beyond just that experiment. We have more transfer value because what that experiment is doing is allowing us to test the TOUGH simulator and its physics for actual cases of steam and water vapor, I mean, steam and liquid water transport within a non-isothermal system. This has application, for example, to the near-field environment in the repository in the sense of validating that the TOUGH simulator does have the proper physics to accomplish that kind of task. So we are involved in this kind of indirect model validation sort of study by analog, if you will. But all of this kind of information can be input to enhance our confidence in the model and, therefore, hopefully, to build reasonable assurance.

Now, I've just been talking about a laboratory scale experiment, but we also need to look at field scale kinds of experiments, and again, with reference to situations that are analogous to the Yucca Mountain site. Well, one of the experiments that has been conducted, for example, in G-Tunnel, was the wet versus dry-drilling prototype test conducted by USGS that Alan Flint talked about this morning. We have a great deal of information on moisture content and changes of moisture content as a result of drilling boreholes using either air or liquid water as the corresponding drilling fluids. And we have a lot of data there, and what we could do is take the core data that Alan is analyzing in the laboratory, calibrate our models using that data with respect to the hydrologic properties of the non-welded and welded tuffs in the G-Tunnel environment, and then we can try to predict the outcome of the wet versus dry-drilling prototype tests for both kinds of drilling fluid, and then try to compare those results with the monitoring that was conducted after the tests so that we could essentially put our blinders on and try to predict the results of the experiment and see how well we do. That would be a model validation field-scale kind of experiment.

There is another suite of experiments that have even more extensive data that involve non-isothermal flow of water and liquid water and water vapor and air, and that's the heater experiments conducted in G-Tunnel by Lawrence Livermore National Laboratory. You will hear much more about those tomorrow in great detail, and I also understand at your January meeting.

Another site that we've talked about already is the Apache Leap site that Rob Trautz mentioned this morning. This is a site that was established purposely for model validation of modeling liquid water flow and gas flow and heater experiments--heat flow, essentially--in a fractured welded tuff, partially a welded tuff, and this is the Apache Leap tuff site near Superior, Arizona that's, again, being administered by University of Arizona.

And finally, we have a site that is not really appropriate or germane, perhaps directly, to Yucca Mountain conditions, but it's the Jornada Del Muerto trench site that is being instrumented by the New Mexico State University, again in cooperation with the Nuclear Regulatory Commission, and this is a site where they're looking at solute transport and moisture flow in a valley fill alluvium under desert conditions--so very, very dry conditions--but what they are looking at is a very heterogeneous alluvium type of environment, and they are trying to validate, essentially, their stochastic approaches. And so this might have transfer value to us in terms of whether or not we are going to, or how well we can hope to do modeling in a deterministic versus a stochastic kind of approach. So we may have some transfer value there.

And finally, what I would like to do just very, very briefly, is to go back to this issue of sensitivity and uncertainty analyses. These are not direct model validation, I will agree with that; but again, these kinds of analyses can enhance our confidence in our models, and the whole idea here is that--and I'm going to do this very, very schematic, so what I'm saying is that from a quasi-mathematical point of view, formulation, if you will, is that let U be a solution to our integral differential equation that we're solving in the mathematical model, and it is going to be a function--if we are dealing with a non-linear system in particular--but it's going to be a function of the solution itself and its derivatives, for example, plus a set B of the boundary condition data, the initial condition data, material properties, and so forth.

Now, let P be some prediction that is based on the model solution; for example, the prediction might be groundwater travel time, and let C be some kind of model parameter that we define the sensitivity of the prediction to the particular parameter, C, as the derivative of the prediction with respect to that parameter. And this can be expressed, then, in terms of--by just simply expanding the derivatives--of looking at the sensitivity of the prediction to the model solution, and essentially the model solution to the parameter, and the boundary data to the parameter.

But what we want to do by sensitivity analysis, therefore, is to identify those parameters to which our model are particular sensitive. So this is a parameter identification kind of process. Now, what we would like to do then is to extend this to get some kind of idea of what--if we're going to be dealing with a prediction--what kind of uncertainty attaches to that prediction. So again, let P(U) be some prediction, P, based on a model solution, U; again represented here schematically as function of itself plus the boundary data.

And then suppose that we now have a set of different model parameters up to some limit; N, for example, and we want to define the uncertainty. Well, we define the uncertainty in our prediction as essentially a particular prediction from a model run, for example, minus the expectation of that, or expected value of that prediction. Then the expected uncertainty of P is simply going to be the variance of P; that is, the variance around the expected value of P, and again, we can define this mathematically with respect to these different parameters that enter the model.

And again, looking at this times the covariance of the set of individual, pair-wise, set of parameters, and what we're really saying here is trying to examine how these parameters are correlated and how they affect each other, and we can even take this very schematically, if you will, in terms of information theory and replace these parameters by hypotheses, and look at the sensitivity or uncertainty induced in our model by the introduction of erroneous or alternative hypotheses, for example. So this is, indeed, to be very schematic. But the whole idea here is to get some kind of quantitative measure of our expected uncertainty. Since we, particularly for these models that we are going to be extrapolating, making predictions sometime in the future, 10,000 years hence, we cannot validate directly. So all we can do is to try to come up with some kind of expected uncertainty. We can't come up with an absolute measure of accuracy of our model predictions.

And I would like to conclude with a parting thought, if I may, and we have been doing an awful lot of preliminary modeling within this project, and so I would like to emphasize that the models of the Yucca Mountain system, our system models do not substitute for actual facts and data, and Alan Flint made, in a special appeal today, that we need to get out, we need to get that data. We need to get real facts, real data as input to our models, and we need to get some measure of expected uncertainty in our various parameters, like saturated hydraulic conductivities of our various hydrogeologic units, however many hydrogeologic units we may end up with.

But I would also like to emphasize that this idea of preliminary modeling provides us with very important tools for promoting understanding of our system, its state, and the processes acting within that system. The experiment that I talked about at LBL with the column and the steam and the liquid water gave us a very good handle on the physical processes that were involved with the transport of steam and liquid water through the system.

Just to give you an example, it turns out that the relative hydraulic conductivity of the steam is greatly enhanced just because there is a phase transition occurring lengthward along the column due to the decreasing temperature from bottom to top. So that's what models can be used for, for one thing. They can be heuristic tools, and the other thing is, is that we can use our models to define specific data needs. And so as far as I'm concerned, this is where we stand right now.

The one thing I do want to point out, however, is that we're going to have a problem because we are doing right now what I consider to be preliminary modeling and will we ever get to the stage where we're going to say that we have a final model? And I would submit to you that we probably will not, but we'll probably drop the word "preliminary" as a modifier to our models, and simply say that we have a model for the system. But we will not say, or ever say, probably could ever say that it's truly a final model.

And so one of the very important parts of the performance assessment process or program at the Yucca Mountain site is going to be the performance confirmation period, the 50 to 100-year period after closure of the Yucca Mountain site where monitoring will be continuing and where we will have an opportunity to gain more data and perhaps better refine our models and maybe approach in some kind of asymptotic sense some kind of final system model, performance assessment model.

And with that, my tale is done.

DR. PRICE: Isn't there some kind of strong sense of humility that is attached to having to approach 10,000 years and you're talking about 50 years of grace period to tweak your models to make them better, but what's 50 years in the light of 10,000 years? And you're talking about getting a peer review in order to provide reasonable assurance, but we're all threescore and ten bound. I mean, isn't there a strong sense of humility attached with modeling against 10,000 years?

DR. HOXIE: Well, I feel a very strong sense of humility. I assure you of that, and a little trepidation, as a matter of fact. But I think that we can handle the task, but I think that we are just going to have to perform sufficient bounding and scoping calculations and uncertainty analyses and these kinds of things so that we can make very conservative kinds of predictions so that we can try to put the system in its worst light, is what we really want to try to do. We don't want to make it better than it actually is. We want to look for its failings. I think the modeling can help us do that, and I think that should be our goal.

MR. WILLIAMS: Dwight, don't you think you can validate the isothermal portions of any one of the models every time you drill a new drill hole, test hole, and don't you think you can validate when you put down the shaft, and don't you think you can validate from when you extend the drift?

DR. HOXIE: I think we can validate, you know, with respect to that particular point in time. We can do the same kind of validation that we do--we're kind of doing sort of a partial validation. I don't like to use that word.

MR. WILLIAMS: Well, it gets back down to the problem that Dennis brought up. You'll never, you know, you can't--I don't think you could justify this project on a validated model for the end product.

DR. HOXIE: That's what I'm saying.

MR. WILLIAMS: But you can do it in steps, and that's what you're going to have to do.

DR. HOXIE: That's right. That's exactly what I'm trying to say. That's exactly the point I'm trying to make and I didn't say that. One of the very important things is that we can validate especially our concepts with regard to processes by the exploratory shaft facility and experiments that'll be conducted there. That's going to involve testing our models, especially our conceptual models, so you're right. But all of these things--and I think that's the important point, is that all of these little things are going to have to fit into the bigger picture of trying to provide reasonable assurance and confidence.

MR. BLANCHARD: Don, do you have questions from any of our previous speakers that you'd like to have the Board make inquiries to? We'll hold the speakers here if that's the case.

DR. LANGMUIR: I don't think we need to. I don't see any urgency to that right now.

MR. BLANCHARD: Scott Ford, the Court Reporter, had requested some of the speakers help him out with some of his notes for matching his tape. Two people he wanted to talk with was Alan Flint, or someone who works with Alan, as well as Dwight. And how about Rob Trautz, did you get enough?

MR. FORD: Yes.

MR. BLANCHARD: Then that rests our presentations for this afternoon. We turn the floor over to you.

DR. LANGMUIR: We've had a change in the schedule for tomorrow for the morning, and there will be a reversal of presentations so that Bill Glassley and David Hobart will precede the eight-thirty presentations that are currently set up for Meijer and Rundberg, so we'll be seeing Glassley and Hobart first, and after the break, Meijer and Rundberg's presentations tomorrow morning.

And with that, we'll look for you all at eight-thirty on Tuesday morning, here.

(Whereupon, the meeting was adjourned, to reconvene at 8:30 a.m. on December 12, 1989.)

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