UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

JOINT MEETING

PANEL ON HYDROGEOLOGY & GEOCHEMISTRY

AND

PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

June 25, 1991

The Registry Hotel
3203 Quebec Street
Denver, Colorado 80207
(303) 321-3333

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman,
Nuclear Waste Technical Review Board
Dr. Patrick Domenico, Chair,
Hydrogeology & Geochemistry Panel
Dr. Donald Langmuir, Co-Chair,
Hydrogeology & Geochemistry Panel
Dr. Clarence Allen, Chair,
Structural Geology & Geoengineering

Also Present

Dr. William D. Barnard, Executive Director
Nuclear Waste Technical Review Board
Dr. Leon Reiter, Senior Professional Staff
Dr. Edward J. Cording, Consultant
Dr. Bridget Scanlon, Consultant
Dr. Tim Jones, Consultant
Dr. Roy E. Williams, Consultant

I N D E X

P R O C E E D I N G S

8:35 a.m.

DR. DEERE: Good morning, ladies and gentlemen. Welcome to the joint meeting of the Panel on Hydrogeology and Geochemistry and the Panel on Structural Geology and Geoengineering. We are going to be talking about almost all of those topics. The rock mechanics studies will be on the third day.

I am Don Deere, Chairman of the Nuclear Waste Technical Review Board. I see a number of you in the audience that met last week again at the meeting in downtown Denver, and quite a number of board members that I had been together with the week before in Canada.

I wonder how many of those in the audience have had a chance to visit in Pinawa, Manitoba the underground rock laboratory. Are there any here that have had a chance to visit that? It was certainly a very worthwhile experience for us.

The thing that was impressive to me was the work that they were doing in the ground water hydrology and the work that they were doing in rock mechanics. And the relationships that were developing that they had not really quite counted on to the degree that things are developing. This is mainly the permeability, the number of joints and the in situ state of stress. They have been doing a great number of measurements because this shaft was sunk to a depth of around 230 meters and then extended with the aid of DOE to a depth of 420 meters, with a shaft stationed at about 400 meters.

The vertical stress is going great, increasing with the depth below the surface almost equal to the weight of the rock until they get near a low angle fault. This fault is a regional fault, dipping at around 15 to 18 degrees, something like that, and it is not a heck of a fault. As a matter of fact it has only moved about four or five feet according to most of their meetings, but it just does all kinds of things to the in situ state of stress and to the permeability and to the amount of fracturing.

Above that depth, and they hit it I guess at a little over 200 meters, but being at a dip depending exactly where you are it would be shallower or deeper. Above it the granite is more or less normal with joints and normal joint permeability and the state of stress is rather normal. As you get near it, it changes dramatically, and below it the vertical stress suddenly is no longer equal to the overburden but much lower, but the horizontal stress is building up. So at a depth of 400 meters, the vertical stress is not equal to 400 meters of overburden material, but much less. But the horizontal stress is much higher than you could calculate. So what they have is a very great stress difference with the horizontal stress.

Now, if you take an opening and you put a very high horizontal stress and not much vertical stress, you have tensions, pure tensions, and there are some tension cracks that appear to have opened up. You have a stress so high that spalling is developing. The jointing is just fantastic. I've never seen a better example of variation in jointing relating to the structural setting.

At the level 400 or 405 meter level, they have opened up a station and they have tunnels going out in two directions and one spiraling down very nicely to a lower level. They have yet in probably several hundred feet of workings found a joint. When you have heard of monumental type granite, that certainly is. It is absolutely unjointed, except all of the stress fractures that are taking place.

It reminds me of some of the tunnels in Nevada Test Site, when you get below a certain depth, where you start getting the stress slabbing around it.

Another great difference is in the fault zone itself, they have done a number of borings into it and have piezometers and have done permeability tests, and there are zones where the rock within the fault zone is quite tight with very, very little flow. And there are other zones, just 20 to 30 meters away within the same zone where when they drill into it it is throwing water across the tunnel with very great pressure and great flow.

So, in one intersection of that fault you wouldn't have the slightest idea what the true permeability characteristics were. It took a number of intersections of the fault, a number of piezometers into the fault, a number of permeability tests into the fault to see the terrific range of values.

The instrumentation was impressive. They are looking at the regional water table and they are looking at the close-in water table which is affected by all of the workings. And they have had a chance to make predictions of draw downs before they deepen the shaft. So they actually have been validating some of their local modeling.

They are using the Westbay from West Vancouver, Canada, multiport piezometers that allows them to get a great number of measurements. And what we are finding of course is a lot of different piezometric levels in the same hole depending on which set of joints they happen to intersect. They are putting a great number of those in farther and farther away from the site, and deeper and deeper up to 200 meters depth. They found this to be a very, very worthwhile effort. They again are making some modifications to even get some improvements in that particular series of devices.

I thought it was perhaps worthwhile to simply point out that until you get underground, until you get the instrumentation, you just can't sit at the surface with the very nice outcrops that they have and the good satellite maps and the structural mapping and the air photos and the number of surface borings, really get to how that mountain is breathing and acting and working. But once they get underground and get the additional information, it has been I think for them, extremely helpful. Remember, they are not trying to characterize that site. That is not the site for the waste. That is only the site to work out their techniques that they would use in siting.

So my recommendation to everybody in this room would be to visit that site. I'm not sure that the Canadians would agree with that recommendation. But, I think for every board member and every staff member that had the chance to go two weeks ago, it was extremely worthwhile.

Both of the topics we are going to be discussing, the hydrology of the unsaturated and saturated zone, and the rock mechanics are topics I think of great importance which the Board has not given enough attention as yet. The first year, primarily because we did not have geohydrologists on the Board, although we had two consultants helping us, but we sort of wanted to wait until we got the appointment. And then we were interested in hearing the presentations by DOE and their contractors of where they are at the moment and what their plans are, and any modifications they may be thinking about.

I will probably be speaking a little bit more on Wednesday morning about the rock mechanics part of the program, but I have a terrific interest in hearing this geohydrology portion. So I am going to turn it over to one of the co-chairman of the Panel on Hydrogeology and Geochemistry, Pat Domenico, to introduce the program and the other people and then he will turn it back over to Dave Dobson of DOE and Dave can go ahead and introduce his program.

So, Pat Domenico.

DR. DOMENICO: Thank you, Don.

First, let me welcome all of you here and thank you in advance for your attentiveness. As you may surmise from the agenda, the presentations today will focus on the unsaturated zone and in particular surface base testing in the unsaturated zone.

A similar agenda was the focal point of a meeting with DOE contractors in 1989, so we view this particular gathering as one of more or less updating. In many cases, the same people who talked to us in 1989 are here again. Some others have disappeared from the program. It's like a black hole sometimes, but the program hopefully remains intact.

Before we get started, I would like to make first one announcement. We are on record here, so if anybody is going to ask any questions of the presenters please identify yourself and speak into the microphone so we can pick you up over there.

The other thing I would like to do at this time is introduce some of the members associated with the Board. At least here there are some old faces; there are some new faces. Amongst the old faces, of course Dr. Allen on my right here; Dr. Langmuir on my left; Dr. Deere and myself are members of the Board; Dr. Barnard, the Executive Director; and Dr. Reiter, Staff Professional. Also here at the table two consultants that you may member, Roy Williams, University of Idaho--Dr. Williams, University of Idaho, we have to keep all this formal here and Dr. Cording, University of Illinois. Dr. Cording of course is the expert in geoengineering; Dr. Williams in hydrogeology. And the two new faces, Dr. Scanlon, Bridget Scanlon from the Bureau of Economic Geology, University of Texas at Austin; Dr. Tim Jones who is with the University of New Mexico State. Their expertise lies in the flow in the unsaturated zone and presumably monitoring in the unsaturated zone. So things that are of pertinence to us today is something that they will be listening to with great interest.

So, with that I'll turn it over to Dave.

DR. DOBSON: Thank you, Pat.

On behalf of John Bartlett and Carl Gertz and the technical project office from the U.S. Geological Survey in Los Alamos, Lawrence Livermore and Sandia, I guess I would like to say that we are extremely pleased to be presenting a rather extensive series of presentations on unsaturated/saturated zone testing and rock mechanics testing that are planned for Yucca Mountain over the next eight or ten years or so.

You'll be seeing a whole series of presentations as Pat mentioned. They will be by all of the participants, all of the major scientific participants in the program. We hope that you perceive a large but generally well integrated program, and it is as you will note fairly large. The one remark I wanted to make is that I think the timing of this meeting is particularly propitious right now, in that we were recently a few weeks granted the first of the major permits that we will need to conduct the surface disturbing activities at Yucca Mountain, that is the air quality permit for surface to serving activities.

We have two more that we are going to have hearings on this summer regarding water or water injection permit and water appropriations permit that we will be having hearings on later this summer. But I think there is considerable reason for hope that in the very near future we will be able to get out there with some drill rigs and some real new site characterization work. So we are real happy and I think this meeting is well timed in that any input that the Board has would be appropriate right now as we really start going with the site characterization program.

The last thing I guess I'd like to do is to introduce Claudia Newbury who works for the department in the Regulatory and Site Evaluation Division with me. Claudia was the principal person in our office responsible for coordinating all the presentations in this meeting and producing this big set of documentation that you all have in front of you. I'd also like to introduce the other two scientists sitting at the front table, Alan Flint from the U.S. Geological Survey, who will be much in your presence for the next several hours, to be followed by Joe Rousseau, also of the U.S. Geological Survey. Alan and Joe of course, are two of our primary principal investigators for the unsaturated zone investigation program.

And with that, I would like to turn it over to Claudia who will be giving a short introduction and briefing on the setting of all of the studies.

DR. DEERE: Thank you, Dave. Don Deere here again.

I told Scott Ford of the Federal Reporting Service, that I would remind everybody to stay close to the mike and to speak into it. I failed to do that, so I am doing it now.

MS. NEWBURY: I am Claudia Newbury. I work for the Department of Energy and I am going to tell you a little bit about what we are going to be doing in the next few days.

What we are going to talk about today is the unsaturated zone program, and we'll get into the saturated zone tomorrow.

First I wanted to kind of put things in perspective. The hydrology program for the project is really in four different areas: The saturated zone hydrology, the regional hydrology, the UZ hydrology program and the waste package environment which takes care of the near-field program. All four of those areas relate back to the issues that are being resolved to determine whether or not we will receive a license application.

As you can see they are pretty well spread across the issues and there is a lot of integration between what is going on.

We are going to start today with the UZ hydrology program, but before we get into the program itself, I want to tell you a little bit about the peer review that we conducted in the last year. The DOE decided that it would be a good idea to take a look at our unsaturated program and to do that we wanted to have some people who were independent of the project. We asked Alan Freeze to be the chairman of the group and asked him to find six other people who would be good contributors to this program. Those were as you can see Lorne Everett, Gerald Grisak, Jim Mercer, Bill Nelson, Stavros Papadopulos and Rein Van Genuchten. We limited their scope to the flow in unsaturated zone as it is now. We asked them not to look at transport; not to look at the saturated zone; not to look at any kind of radionuclide migration. Just, what does the mountain look like now, and what's our program look like?

Well we didn't want to just give them a bunch of books to look at and review and read and comment on. We wanted them to really think. First we sent them some background information on the hydrology program and the hydrologic setting at Yucca Mountain. And then we brought them up to Las Vegas and stuck them in a room for four days and said, "Okay, if you could characterize this mountain, what would you do?" And we took them out and showed them the mountain and let them walk around on it and kick some rocks. We didn't let them pour any water on it.

Based on what they determined in those four days they came up with a bunch of questions on our program. What we did then was send out these questions to the participants and have them come up with reams and reams of literature, which we sent back to the peer review team, and they divided it up among themselves to see what we had done.

Then we brought them back to Las Vegas and brought in the different participants, the different PIs and spent another four days reviewing everything, discussing, doing presentations very similar to what you are going to see today, and discussing the whole program. Based on those reviews, the peer review team produced a report which was finished in late August or early September of last year on what they thought about our basic program.

What we did in response was sent those comments and there was a number of pages of them out to our participants and they responded to the comments and based on those responses we proposed 14 actions that we would follow up on as a result of the peer review. The team evaluated our responses and finally signed off on it last month, and we should have it out in publication form in the next couple of weeks.

Okay, what did they say? Most of their comments were very complimentary of the program. They thought we were doing an excellent job. But, they were disappointed that we weren't out actually pouring water on the rocks. They thought it was imperative that we get some permits and go out and actually test our theories. And we agree.

Some of their key points was that--they were very concerned we lost G Tunnel. We had to close it for economic concerns. They were concerned that we'd get out there and find another place where we can do our testing. They felt that it was important that we find some sites and do some testing prior to getting into the ESF. Well, with any luck we'll get into the ESF. So, I don't know what we'll do on that one.

They encouraged some early field experimentation in the nonwelded units. They felt that the nonwelded units would attenuate a lot of the pulses of infiltration and that we would be looking particularly at the Paintbrush nonwelded unit.

They were concerned that we have a lot of models out there that have never been validated because we haven't gone out and poured water on the rocks, that is a real concern. They felt that it was very important to go out and validate some of our models before they are taken as truth. And they said for as long as some of those models have been out there they are beginning to be accepted as true even though they have never been validated for the mountain.

One of their other positive comments was that we are doing a lot with stochastic modeling approaches. You will hear some of that when Alan Flint talks about some of his geostatistics. They were very impressed with some of the work he's been doing.

They were concerned with some of the isotopic age dating that's been done, that it was not really adequate by itself to represent what is going on at the site, and that it is very important to resolve some of the differences between the calculated isotopic ages and the travel times. June Fabryka-Martin will be talking a little bit about that tomorrow, and I think you'll find what she has to say pretty interesting.

Stavros Papadopulos in particular was concerned that our saturated zone travel time, even though they weren't supposed to be looking at the saturated zone, was too low and that maybe we should go back and reevaluate that. And that is one of the actions that we have been recommending that we do.

This is not a direct response to the peer review, but it addresses one of their concerns, and this is the north access ramp as proposed at this time. What they have done is put in 9,000 feet ramp and you'll see all these test locations and most of them correspond to changes in the lithologies. We will be testing in the Paintbrush tuff and in a lot of other locations along the way. It should give us a pretty good idea of what will be there before we even get to the repository block.

DR. DOMENICO: Is this a ramp?

MS. NEWBURY: This is a ramp.

DR. DOMENICO: In addition to other things that we have been hearing about in terms of access?

DR. DOBSON: Pat, this is the preliminary design for the new north access ramp and this is where we are in what we call the preliminary designing phase. I might add one thing, and that is that where most of those numbers are, where test locations are, there will be an alcove. The test will not be done necessarily in the drift. We will put in alcoves for testing.

Specifically, there are several of those alcoves will be on either side of the contacts between the welded, the Tiva Canyon, the Paintbrush unwelded, and the Topopah below it. As I say it is a preliminary design and we haven't got detailed configurations for how long the alcoves will be and precisely where they will go, but the testing locations have been developed by interacting between the principal investigators with the considerable assistance in this case with Los Alamos who does our underground RESF test coordination for us. So in an attempt to be responsive to recommendations not only from the peer review panel but from the Technical Review Board and others.

MS. NEWBURY: You don't have this in your presentation; I just put it together yesterday.

Today we are going to start looking at the geohydrology program and look at the regional hydrology and characterization of the meteorological systems. Alan Flint will be talking about that.

As I said, the hydrology program has a lot of parts to it, and when Alan is done talking about the meteorology, he is going to become a part of the unsaturated zone hydrology program, and talk about first, the unsaturated zone infiltration and then about matrix hydrologic properties which falls under the unsaturated zone percolation studies surface-based.

Joe Rousseau is then going to take over and talk about some of his deep bore hole testing. Gary LeCain will be talking on the section on unsaturated zone gaseous-phase movement. He'll be talking about air permeability testing. From there we are going to switch over to a different program entirely, which is the waste package environment. You will hear presentations from three people. You'll hear about some work in the near-field mineralogy and chemistry. I don't have a name up there. U-Sun Park will be talking about gaseous and semi-volatile radionuclides. And that sort of falls under that WBS, but not really.

Then we'll switch back to the UZ program and at that point we will talk about the physical and chemical characteristics of air circulation at Yucca Mountain with Ed Weeks and Don Thorstenson. Then we will go back to the waste package environment again, the near-field, and Dale Wilder and Tom Buscheck will give you some information on the effects of the repository design on the heating of the waste canister on the hydrology in the near-field. And on some modeling work that Tom has been doing on fracture and matrix flow. That will take care of today.

Tomorrow we will finish up with the rest of the unsaturated zone hydrology program. We'll listen to Al Yang talk about the geochemical and isotopic methods. We'll listen to June Fabryka-Martin talking about Chlorine 36.

So with that, I'll give it over to Al and he'll entertain us for the morning.

DR. FLINT: When I was listening to Dr. Deere talk, I started to think that maybe he looked at my slides and saw how much emphasis I had placed on surface-based testing, so he put some torpedoes in there, but since I am on the surface of the Yucca Mountain I get to do that.

I have three talks to give today. They are tied together to a certain extent. There is sort of an underlying current that goes through those three talks. I'm going to try to give you an idea of what that is up front. When I spoke about two years ago I had two study plans I was working on, infiltration and matrix properties and I had a staff of about 20. Now I have three study plans which includes the characterization and regional meteorology and a staff of less than ten. So we have increased our workload and decreased our staff so we could get more done that way is what the philosophy was, I was told.

What we have tried to do is to look at the program from the perspective that, can we get things out of our different studies with the staff that we have, the budget that we have to address some questions up front that need to be addressed? So what you'll see through these three talks is our attempt to try to pull out information that can be used by modeling, both in the performance assessment side. We have our in the USGS a three dimensional model that we are working on with the Lawrence Berkeley Labs. And I am trying to go through my different studies, pull out the basic information I can, feed it into these models and hopefully they can look at and evaluate that data and tell me whether I need to concentrate in one area or another and try to pull more and more information. What you are going to look at in here and what you'll see through these talks is us trying to pull out information that can be used for the modeling effort.

When I started doing this work, I asked myself the question, what is controlling unsaturated zone hydrology? What is controlling the state variables in particular, water potential, water content? Why is the unsaturated zone the way it is? I came to the conclusion that it was a combination of climatic change and the rock properties themselves. I asked the question, does the saturated zone have control over the unsaturated zone through capillary rise, through drawing at the surface? And came to the conclusion that most likely not, and most likely it is climatic changes.   Why is there a disequilibrium of water potential? Is there a disequilibrium in the unsaturated zone? And I think there is. And I think I have some answers that may address that question in terms of climate. So what I am going to try to do is show you that, one, I think we need to look at cyclic variations, not necessarily in some cycle, but some variation in the input to the unsaturated zone. I think that is very important. We have been using constant fluxes at some point in the repository level and moving down. I think we have to look at variable inputs to the system. I think that is very important and I am going to show some information that we might be able to obtain.

I am also going to show the importance of doing surface-based testing and some of the things that we can do without actually having the permits. I have some data on that, but I am going to start in meteorology. The infiltration program last time had the meteorology imbedded in it from the two year ago meeting. I've pulled it out, although I have about a half an hour for that, I am going to extend it a little bit longer and cut time out of the infiltration program. I want to spend a little more time with meteorology. So, if someone could turn the slides on for me in the back.

DR. DEERE: Alan, while you are doing that, this is Don Deere, I'll make you feel a little bit more comfortable. They did a fair amount of surface work. They knew all about the fault. They had it well instrumented. And this was the beauty of going underground. You could instrument it. You had an idea of what you were going to find. And then you were able to go down and find how closely you predicted the behavior, etc. And I am sure that is what you have in mind. This was the project in Canada.

DR. FLINT: Good. Did you get that in the record? They did do surface testing.

Okay. The objective is to characterize the meteorological conditions around Yucca Mountain, particular emphasis on precipitation.

I am going to have an outline set up and we are going to go through these four areas and I'll come back to each one when we've covered one. I want to show you where we are and what we are doing and why we made some of the decisions. The first one is the study area.

We have broken the region up into four study areas. From Yucca Mountain which is in the center, about a 150 kilometer radius which includes Las Vegas, this is our largest area of interest. We are going to do meteorological characterization within that area, mostly based on supporting information. I'll show that later.

The next area is the Upper Amargosa Watershed. This is a surface watershed basin. It is just an area we picked because we could characterize any rainfall in here with measurements at the base; the outflow at the bottom. So it was an area which we could bound the watershed.

Inside of that is the Fortymile wash area, perhaps major recharge site for the saturated zone system and the sub-basins, and then the smallest area we are trying to characterize is an area around the repository. There are three small watersheds that cover some part of the repository. We have broken those out, put some boundaries around it to get away from edge effects in some of our modeling and that is our third area of interest. This is the hydrologic research facility where the DOE offices are and the USGS offices on the Nevada Test Site. That was the study area. I'll come back to that later and show you where we have some measurements that we are collecting.

I want to talk a little bit about our current understanding of meteorological conditions and I think those are important. We can get some information from those that's quite useful to us. The Yucca Mountain is in the northern Mohave. We have two distinct weather patterns, more than half of the precipitation comes in the wintertime. We do believe that bimodal distribution of rainfall exists and I'll show you that data later.

There are five main weather types in the winter. Three bring precipitation, one of those fairly insignificant; two types bring dry conditions. The summer weather is dominated by the Southwest Monsoons as far as rainfall goes.

The Great Basin, most of its precipitation is snow in the wintertime; the Mohave, rain and snow most in the wintertime --more than half in the wintertime; the Sonoran Desert gets about half and half, winter and summer mostly is rain; and the Chihauhuan Desert mostly rainfall. And these four deserts are different in the way that the storm patterns develop over these. And they become important in our understanding, and it shows something about the unique character of where Yucca Mountain is in the northern Mohave. I'll show some weather patterns that deal with these.

This is the storm Type A. We get no precipitation from this storm type. What we have is a Pacific high and a high fairly far to the north keeping the storm track, the steering winds, jet stream to the north. We get a lot of storms up to the north through Oregon and Washington into Colorado and Utah. No precipitation from this storm type. This is an important one in keeping moisture away from southern Nevada.

Storm Type B, we have a high pressure dominating the southern United States, again keeping the storm tracks fairly high to the north. No precipitation from this storm type.

Storm Type C, we have the belt of high pressures that were down to the south way up north. The lows and the storms that come in from the south have moved considerably up from the equator and bring in several storms. These are time sequence of storm fronts as they move across. This type of storm--we get several storms coming in from the southwest. This brings the greatest winter precipitation that we see. The only other high precipitation events we see are from the Southeast Monsoons. But this is a very important storm type to southern Nevada. I'll talk a little bit about why we are looking at storm types now a little bit later.

This is Storm Type D, the high pressure to the north. We have the steering winds that come along and keeping this fairly high to the north, we don't get much precipitation from this, if ever. Sometimes we get polar outbreaks of cold air that move down in--you see these in Colorado a lot where it gets real cold fairly fast. We do get some precipitation of snow at about 4,000 feet. This is that one insignificant type, but we do get some precipitation.

And the last storm type, Storm Type E, the high pressure moves along the upper part of the continent forcing the low pressure as it comes in along the coast. This is where we get a lot of the northern California, Oregon, Washington, they get a lot of their precipitation from. But the storms move across the Sierras down into southern Nevada. This is an important storm because there is a large influence of the Sierras as a rain shadow from this type of storm event. This is our second major storm event in the wintertime. The availability of moisture and the rain shadow control the amount of moisture that we are going to get from this particular storm type.

The Southwest Monsoons, there was a lot of argument of weather that moisture source is from the Gulf of Mexico or the Gulf of California, but actually we believe it is a combination of the two. The lower elevation moisture and the upper air moisture combine over southern Nevada and Utah and bring us a lot of our major thunderstorms. You need both of those. This is where we get a lot of thunderstorms coming into the Chihuahuan Desert, then the Sonoran Desert and then into the Mohave, but they miss the Great Basin. We don't get much precipitation from these kind of events in the Great Basin. But these become important as these storm types might change with future climatic conditions.

This is a compilation of that information with Yucca Mountain located--the rain shadow influence mostly from that one storm type that comes down from the northwest. The Southeast Monsoons, this is real interesting if you are out at the test site, you get a lot of thunderstorms building up in Mercury, but they don't move as far west as we are at Yucca Mountain. We can see this. It will be a nice sunny day and we will come into Mercury and they will have rain everywhere and water running down the roads because of the way the storm track moves. It is a nice boundary. We have a deficit zone in here. Excess precipitation on this side, excess on this side and a deficit zone in the center. It is important to Yucca Mountain and this may change with future climatic conditions.

This is the bimodal distribution that I talked about. It is bimodal. Of course if you were to take these bars and stick them on the other side and do the graph differently you would say it wasn't bimodal. But since I made the graph, it is bimodal.

I want to show you a little bit about the character of the precipitation. This is a kriged map of one storm on February 2nd. In the wintertime we have more of a strata form cloud type, large areas covered by these storm clouds. And you can see the nice smooth heights, iso-heights made. It moves to the northeast; we get increasing precipitation. This is a summer storm. A storm cell sitting in this area (indicating), 25 millimeters of rain, 30 millimeters of rain in this area. So we have two large storm cells and then we get fairly good deficit zones. So there is a difference between the kinds of storm we get. We are going to need this to do some modeling of precipitation to know this kind of information.

A variogram, standardized by taking the variance and dividing by the mean, the total precipitation divided by the mean to compare these. Again, the winter storm, this is in thousands of feet, low variability, large storm type. Summer storm, high variability because these are smaller cells. So we can characterize the variability of these different storm types. That's important for our modeling spatially for rainfall.

This is to show with the variance--these are just simply experimental variograms and distance in thousands of feet, so this is 400,000 feet. This is the average summer precipitation, not just for one storm now. Summer variogram, winter variogram, the average annual rainfall is controlled by the winter in this particular year because the winter dominated the storm type. If you use variography to locate new sampling sites based on spatial correlations, you are going to miss the summer if you use the average annual precipitation. That is what we used before. We are working with both of these systems, but one can dominate if you get more precipitation from it. That is what this is to show.

Two different storms; two different variograms. The spatial correlation of the storms is different. But there is different amounts of rain from these two different storms. If we standardize these storms by dividing by the mean, we find that we basically have the same variogram. What that means is, that the variance of summer storms is about the same, it is just the amount of rain that we get differs. So we can use the same variogram for characterizing summer thunderstorms as they move through our system which makes modeling quite a bit easier.

These are our estimates. This is from kriging for the site. Kriging just uses the spacial correlation in this case and I showed some of this information before. High rainfall in the Spring Mountains, and the Sheep Range through north Las Vegas down to this area. The kriged map shows about 150 millimeters a year for Yucca Mountain, average annual rainfall. It doesn't say anything about the distribution: winter; summer; the intensities of the storms. We added to that cokriging, the elevation relationship. There is a strong relationship in increasing elevation and increasing precipitation. Now our estimate at Yucca Mountain is about 175 millimeters a year, average annual precipitation.

This is just a summary of the rainfall data that we have to-date.

That's our current understanding. I'll talk a little bit more about the storm types later on. I want to talk about the data collection we are doing now and how we are putting this information together.

The parameters of interest, precipitation, air temperature, humidity, barometric pressure, short wave radiation and wind speed and wind direction. Those are the main parameters that we are after from our small MET stations. Additional information that we are looking at is cloud to ground lightning. We will be looking at cloud-to-cloud lightning; cloud top temperatures. We have some video tapes set up, a small camera that takes a picture every two minutes to get conditions as they go through on a day-to-day basis. We can look back and get some interesting information from that. We are also looking at satellite imagery. This is both to do our statistical analysis, geostatistical analysis and some of our mesoscale analysis.

DR. LANGMUIR: Alan, what about potential evaporation?

DR. FLINT: We are not looking at potential evaporation in this program. The infiltration program is doing a lot of work in evapotranspiration, potential evapotranspiration from evap-pans, and then John Czarnecki's work on regional evapotranspiration. But we do do a lot of work in the infiltration program and I'll show some data from that a little bit later. I'll show you later how this information can be used to make those calculations too, since these are standard Penman weather stations that we use for Penman calculations.

This is the outer region again. What we have done is compiled all of the weather stations that are currently active. Most of these collect precipitation. Some collect in addition to that rainfall. These are all the stations in the outer areas from Department of Defense, from the National Weather Service from Co-Operators. That is everything that is outside of the other regions. So these are supporting stations and we think there are enough stations out there for characterization of this Region IV.

Excluding all of those now, these are all the additional stations that are in Site Area III, excluding anything in this zone. We don't have as many stations out here as we would like. We have to put more stations out here.

We go to the next slide, now we are looking at Area II. This is Fortymile Wash. These stations are run by the Nevada Test Site and there was also some Desert Research Institute has some stations out there. We are getting information from them. We are going to put some additional stations in here ourselves. Then finally within the area that we are most concerned about, we have right now about a hundred rain gauges. Most of those are non-recording rain gauges, but that is what we had access to and that is what we use. I'll show you some photographs of that later. Mostly centered at all of the neutron holes with a few stations at higher and lower elevations to get some areal coverages. But those are where we have instruments located; where we are going to collect the data. So all told there are about 160 stations around that we are using for collecting rainfall information and air temperature.

More detailed information as we get closer to the repository; more information on the air temperature; solar radiation and things like that.

The platforms that we are dealing with for collecting the data, one, the standard Penman Weather station which I'll show you; we have rain gauges, both automated and storage; we have our lightning detection network on the Test Site; we have GOES/POLAR orbiting satellite information; our time-lapse photography. We also get supporting data from the micrometeorolgy studies. That is where be get the evapotranspiration information. There are stream gauging stations that have some rainfall collection platforms and we'll use that information, plus the environmental compliance MET stations, the SAIC tower, 60 meter tower and 10 meter tower. We combine all that into our statistical and our deterministic analysis mostly looking at precipitation, but we will consider all the other parameters of interest.

This is the standard Penman Weather Station, the solar radiation, wind speed and wind direction, temperature, humidity and then there is a rain gauge on this also. This is the kind of data that we can collect and will put out on a daily basis. Actually we collect it every 15 minutes. But this is just a summary of information from one year. We have our monthly means and temp in degrees of C. A maximum mean, this is for reference later on, for instance the average temperature is 23.9 in June. All 30 days or how ever many there are in June, the average of the highs was 30.1 and the highest day was 35.8. So this is the highest any day, the average of all the highs in the month, the average of all temperatures collected within the month, and then we go with the minimums. So you can see that in six months we have at least one day go below zero, so half the year we have at least one day do that.

Wind direction, this is kind of an interesting plot. As you notice the winds come from the east/northeast and then from the southeast. It never comes from the west at Yucca Mountain. It never comes from the north or the west. This station was near UZ-6 just over the edge. And this is an interesting phenomena, and Ed Weeks I think has some slides that he'll show you later that show that this is probably correct for the location of this station and he'll show why that's the case. This station has since been moved because of this particular problem we are facing. Anyway we collect the rest of the information--fairly good storm in April. But this data is the kind of data we collect from our weather stations, that we'll put out on a year-to-year weather station by weather station basis.

Snow gauge, heated snow gauge, a data logger, we'll use these data loggers for other purposes later on. We are going to do some evapotranspiration measurements using those in a new technique, fast response psychrometers that we are going to hook up to these locations. We will have quite a few of those out.

These are the rain wedges; ten dollars for the wedge; we've got the wood on the test site. And since we didn't have any permits we strapped them on the bottoms of the neutron poles so they are not touching the ground and no surface disturbing activities. So we've got 80 rain gauges out on the site using this technique and it was real cheap to do.

See we had a BPA, a blanket purchase agreement with a company and we just kept telling them we broke the one they sent us and so we got 80 of them that way.

The kind of data that we get from that analysis, just standard statistical analysis and some geostatistical analysis, just for information, so you can see this later, 2 millimeters was the average for that storm; 7 and again 2 millimeters. You can look through the statistics anytime you want. What is interesting is what we see down in here. Negative correlations between precipitation elevation, the higher up you go the more rain you get, but the higher up you go the less rain you get in our measurement technique because those wedges are just too small for the wind effects.

But the further north you go which is also the higher in elevation you go, you get a good correlation. This correlation overrides this particular one because measurements at the same elevation going further to the north get more precipitation because as you go further north, we have a mass of land building up which brings these large storms and the convective cells in, other kinds of storms even in the winter we get more precipitation, although it doesn't show up here. But we are fixing that now with different kind of gauges.

Lightning detection information is also useful for us for estimating rainfall in the region. This is hooked up to the lightning detection network on the test site for the weapons program. We get this information real time. Each one of these spots is a strike; it's a different time of day on this particular storm on May 21st. We want to use the correlation between rainfall and lightning to estimate rainfall in areas where we don't have gauges. The way we do this is we have in this case one lightning strike in this squares, two lightning strikes, these are ten by ten kilometers. Throughout all these lightning strike areas there are about maybe ten or fifteen rain gauge stations that collected some precipitation. When we get a better storm than this we'll count the number of lightning strikes, in this case seven, and the amount of rain. We'll just use a simple correlation. We don't have information up in here, but we would imagine there was rain up in there because of the lightning information. So that is useful to us in cokriging or other estimates of rainfall. So this gives us a better regional picture, additional information, it's free, it comes from the test site whenever we get a lightning strike.

We also have a polar orbiting antenna on our building and this is a GOES satellite antenna. We can get some useful information from here. This is a photo taken on June 15th of this year showing cyclonic storm coming into the north coast, probably the kind that will move across to the north and leave southern Nevada alone. This is one that is the beginning of a Southwest Monsoon. I have three series in here to show you the development. We have storm clouds across here, these three if you remember the arrows I drew, went up in this direction from the Gulf of Mexico, Gulf of California, added up into here. Now watch these storm types. Now it has built quite a bit, but it hasn't moved, it is fairly stationary as the air is moving up. And then finally it dissipated and we can look at this analysis. This is an infrared image; this is just using the Gray scales. We go back and look at cloud top temperatures; the higher in elevation the cloud tops the colder they get. And we see that on the IR imagery. The large cloud tops give us the more intense rainfall. So we can use this when we calibrate it to estimate where we did get large areas of rainfall for our bigger region that we are dealing with in part of the southwest United States. So we do see the cold cloud top, probably highest storm top. We add that information with the lightning information with all our gauges and we get a better estimate of rainfall for the region in different scales.

Now I want to talk a little bit about past/future conditions and some simulations we are doing and where we try to tie all this information together. Again the application is what can we put into some models now? What kind of information do we have?

I think there is variable nature of climate that we have to account for and I am going to go through those and talk about the past conditions. We are currently in an interglacial/interpluvial period. There isn't any drift or there doesn't appear to be drift, but there is high variation in historical climate.

Future conditions, is it going to be colder and wetter based on these Milankovich cycles? Or, is it going to be warmer and dryer based on greenhouse gases or greenhouse effect? What about Yucca Mountain? Are these two conditions necessarily, if it is for the earth in general, is it that way for Yucca Mountain? We have to answer that question.

We are going to build a rainfall simulator based on some of this information. It is probability based and it is temporally and spatially variable. We have to take into account those informations.

These are just information on ocean core, the Oxygen 18 in terms of depth. When you age these, then you can put that in terms of time. The last 700,000 years, these are changes in climate; not a lot of drift, but there is a good cycle. We can add this information together from the marine core, from ice core or from the work that Ike Winograd has done in the Great Basin. Looking at climate change just in general, colder temperatures, warmer temperatures; here we are now at the higher temperatures, warmer temperatures, the current conditions we are in. If we look at the Milankovich cycle for orbital input of the earth and the way it moves, we run that in a model and we get some insulation information out, total radiation load. Again these Milankovich cycles may be the trigger for climate change, not necessarily a running climate change, but it may be the trigger.

Then these are some of the information from the past. We see where we are now. This cycle says we should be going down which indicates that the climate should be getting colder and wetter over the next 10,000 years. Those cycles may or may not override the global warming trend that we've seen. These are just global mean temperature change over the last 90 years. This is the mean from about 1951 to 1980, but we may see this change. Doubling of greenhouse gases if it gives a five degree temperature change, this is what we would expect; a two degree temperature change this is what we would expect. So the estimates may be more two degree temperature change. It may be overridden easily by the Milankovich cycles.

So we put this together and try to look at the conditions we might expect in terms of how is this going to influence, in this case infiltration at Yucca Mountain. Where we are today, temperature, precipitation, we are looking at that right now with our natural infiltration studies. If we do get wetter conditions, we expect the same temperatures to get more winter and more summer precipitation. Our artificial infiltration program is going to look at that.

Dryer conditions, I don't think it can get any dryer than it is now. We had 50 millimeters of rain last year and everything about died, so we think we have seen the lowest. But, if we did go down, we'd see less precipitation.

Colder and wetter, future climate--winter precip goes up. If winter precip is a major source of recharge we are going to see an increase in recharge at Yucca Mountain. Warmer and dryer, winter precip goes down, we will see a decrease in recharge at Yucca Mountain. And you can go through and there are some uncertainties in our analysis, but we expect that warmer/dryer, wetter/colder these are the two main areas we are concerned about in future climates. But we want to take this into account when we do our model.

This is on a probability of precipitation. Now, from what we have today in comparison with other models for our model input, we want to try to put down what we are seeing currently. This is the EPRI model. EPRI is doing analysis on climate and infiltration right now, and this is their model based on Yucca Flat data.

This is Desert Rock data and 4JA. 4JA is a weather station out in Jackass Flats. If we look at this on a probability scale, we are looking at about a 14 percent to 15 percent chance of rain; say 15 percent on any one day it could rain. I think Joe Hevesi who did these calculations said there was 20 percent chance we would have a tour on any given day. Which means there is a three percent chance the tour is going to get rained on.

If we look at it on a log scale, this is a standard modeling technique that's used for rainfall probability estimation and you can pick from that distribution. What we see is this model underestimates the probability of getting a high intensity rain storm. And we see these and those may be very important for runoff. But their model is not going to get us three orders of magnitude less. This is a 90 millimeter storm which we've seen.

And, we have less probability of getting a lot of the smaller storms and then again a higher probability of getting the very small storms. So, quite a bit of difference in these. So we want to base this on a better data set and not using the standard climate models for picking.

If we have rain on one particular day, this is the EPRI model, this is our model for the Desert Rock data. If it rains today at Yucca Mountain, what are the chances it is going to rain tomorrow? And that is what this shows. Almost a 40 percent chance of rain the next day. So it is a rain following a rain event. That is a very important for infiltration processes. We have data that help us to model that. Again I'm trying to show you all the information we are trying to collect in an attempt to put together some modeling efforts to get some infiltration and rainfall inputs for our larger modeling. So this is important. Of course if it rains the second day what are the chances it is going to rain the third? It starts going back down again.

Important information that we are looking at for infiltration processes in rainfall from a historical perspective on a storm, one-half inch of rain, followed by another half-inch of rain in another storm. How often does that occur? Those are the kind of events we think give us the major runoff in the washes and a fairly good infiltration rate and recharge in a particular wash.

In Las Vegas, we had only two events where it happened within seven days. After seven days, a rain storm of half an inch after seven days in the summer, it is gone; it is pretty much dissipated by that amount of time. So we need to get these more frequently than that. In Beatty we had two of those. So we get one every 12 years or so. At 4JA at Yucca Mountain, we get one every five years. So every five years we get two major storms one following another. We think we had that in 1984 and 1985 and saw some major runoff in the washes. I'll show that in the infiltration program. We have compiled this information for 14 days and 21 days, so you can look at it some other time if you are interested.

One of the questions we asked in terms of storm events, the model that EPRI used and the approach that they are taking, is we assume we are going to have so many storms a year, 17 they picked. And all we are going to do is increase the intensity of the storms. But it maybe more important to look at how many storms you get, because, it is storm following storm that may be more important than one big storm. This shows a strong correlation with the number of days it rained in the amount of precipitation we got. So what brings increase precipitation? More storms. More days of rain. This is a day-by-day basis and this is a storm-by-storm. The storm may be two or three days. This is important for our modeling. It is the number of storms that we get that determine how much rain we get, not how much we got in a particular storm. It makes it a lot easier when we start looking at global climate models because we can see storm development and the probability to estimate how much more rain we would get just based on storm developments for large regions.

So our rainfall simulator that we are going to build, we use stochastic models. We have to be consistent with the current mechanisms that exist. These different storm type that I talked about, the Southwest Monsoons, the rain shadow effect, we want to couple that information with infiltration. Since I get to talk about infiltration next I threw that in there, and the global climate model. Now here is where I want to tie those storm types together.

The global climate model uses one measurement point around Yucca Mountain, but points over the globe fairly good coverage. Although they have a mesoscale model within their global climate model, they can get a little more detail around Yucca Mountain, but rather than trying to take one of their grid points and predicting rainfall, what we want to look at instead is to look at the types of storms that we have. Storm Type C. That's the one that brings the clouds in from the southwest. In a global climate model, you can see that develop fairly easily. So rather than looking at how much rain fell on the eight point, you look at how many times has that happened compared to what is happening today, and you can estimate rainfall fairly easily, I think.

How often does Storm Type A occur in the wintertime? If you get that a lot more often, you are going to get less rainfall because of that or the Southwest Monsoons. So I think we can use large-scale variation for that. Large-scale modeling and you look at storm types instead of rain-by-rain analysis.

In summary, we are looking at 70,000 square kilometers centered on Yucca Mountain; more detail the closer we get. Strong summer and winter storm types, the differences that we see. We have a lot of ongoing activities from weather stations, our precip stations, lightning, satellite. We are doing both statistical and deterministic analysis to see how these storms develop. And we are using existing information to build a rainfall simulator, a numerical rainfall simulator. And these are the conditions we want to use it for: one, infiltration program; regional saturated zone; also for the flooding/runoff analysis to see what the probability is of these kind of storms occurring.

So that covers the meteorology program.

DR. DOMENICO: Did I hear you say that the Yucca Mountain on your regional simulator model is represented by a point?

MR. FLINT: One point.

DR. DOMENICO: One point.

MR. FLINT: One point for the global climate model. NCAR is now using what they call the MMR mesoscale model which is embedded into the global climate model. That embedded model within our 150 kilometers at Yucca Mountain, we have about seven of their points. So seven data points from their mesoscale model which gives us a little bit more detail, but no one is particularly good for a point estimate. I think you have to use them in total. And I am trying to get around that by using these weather types, storm types to look at that information. But we are feeding them data on evapotranspiration, on rainfall, on solar radiation, wind speed, wind direction and things like that for calibrating the model. They are doing '84, '85 and '86 right now for us.

DR. DOMENICO: When you link that to infiltration models on the Yucca Mountain site, you will use a spatially distributed network, you will have more than one point.

DR. FLINT: Right. We are going to use a spatially distributed model when we do our simulations and for those we'll have to do that. And we want to make that model consistent with the variograms that we see for summer precipitation so that we do have small storm cells and for the winter that we have the stratoform cloud types. But we will have a number of points.

In our case, I think we have in our 3D model which I'll show infiltration, our surface covers about 250 points over Yucca Mountain for our modeling.

DR. DOMENICO: Does anybody up here have any questions?

DR. ALLEN: Just out of naive curiosity, with only 30 or 40 years of records here, meteorologic records, how well can you really estimate the catastrophic events, extreme unlikely events that may have fantastic geologic importance. Having worked out of Antofagasta, Chile, here a number of years ago with an annual rainfall of less than a millimeter, I am impressed that what, two weeks ago they had three inches of rain, killed a whole bunch of people and clearly a kind of event that 30 years of record, there would have been no basis for expecting, and yet that is where all the geological effects are is that one extreme event.

DR. FLINT: Oh, absolutely. It is a particular problem and we've noticed that. That is a major issue and a concern of ours is the length of the record. And that is why we think we are going to have to do a lot of modeling to try to make some of that up.

What we see is some stability information on the site itself; desert varnish, how long it's been there; some of the washout zones that have washed out some of the desert varnish to get an idea of how often that may occur where we get those catastrophic type events. We already see from our model versus the EPRI approach, that we do have more probability of getting these large storms that come in. But that is a very difficult question. And I think EPRI suggested that you needed at least a millinea of study to know that. I asked if they would support a proposal for that, and they said they would if they got a percentage of it up front.

DR. ALLEN: Well to some degree it is the same problem we have on the seismology. How do you use a short record for extrapolating the only important events that you really worry about?

DR. FLINT: Well one of the things that is important here, I think, is that we understand the relation between the kinds of rain storms that we do get and the processes. We can estimate what we would expect to see of a major rain storm. You can bring in as much rain as you want; I know the conductivity of the washes; we know we are going to have some down-cutting of the channels; we know a little bit about what is under the channels; and we know how much or we can make an estimate if we have two or three hours of runoff in a channel, how much water can we move into the system? We can make those kinds of estimates now. There is a physical length to the stream channel that we know about that we can measure. And there are properties that we can measure and some estimates we can make. And because of the way Yucca Mountain is situated, we may be able to shed most of the water from major storm types away.

I believe that it is these long duration winter storms that bring us most of the recharge in unsaturated zone system. And it is the frequency of occurrence of those that may have a major influence. Although the summer storms are more interesting, they may not be as important because once you get more than so much rain, it all runs off. It becomes important for Fortymile Wash in the regional recharge, and John Czarnecki can answer that question when we get to that point.

Are we ready for the next talk?

DR. JONES: Are you building a rainfall simulator or a weather simulator?

DR. FLINT: The NCAR group is building a weather simulator; we are building a rainfall simulator. What our rainfall simulator will do, is using these probabilities and the bimodal distribution of storm types, we will control it statistically so that we can pick out rain. If it rains on a particular day, the next day the chances are more that it will rain and we'll do those storm types.

We'll do this for 100 years on a day-to-day basis. We expect in that 100 year period of simulations that we can go back and find that we are statistically, that we are showing the same structure, the same bimodal distribution, the same average annual precip and that information. We'll take those 100 years day-to-day and feed that in to an infiltration model. Then we'll look at the impacts of those different storms. We expect to get the variety of storms that we would see in a 100 year true event, and use that information day-to-day for 100 years to estimate infiltration processes given then an average annual precipitation. Then we can take 100,000 year or million year model rainfall simulations and see how often we expect to see these kinds of infiltration processes occur, we can add to that drift so we can increase rainfall, so we can double the rainfall and see what that influence has or we can decrease it, keep it the same, add the probability using our model which is different from the EPRI one to have these high probability three and four inch storms that we see, that we may see or five or six inch storms. But ours is a numerical rainfall simulator.

We can do it now at a point. Once we start doing it at a point, now we have to start looking at two dimensions and making the summer storm cells one or two kilometers and then have several storm cells. And we don't have enough information yet to do that, but we are working on it on that. And we are not sure quite how to put all that together. But we do want to build these numerical rainfall simulators to bring the storms in, just rainfall, not the clouds or anything else at certain locations, and limit them so that they are consistent with our spatial correlations.

DR. JONES: Do you have an estimate or some kind of a feel for how large of an area controls the water balance at the repository site? I mean, do you need to simulate the climate and rain over one square foot on top of the repository or 100 square miles?

DR. FLINT: We are not sure how big an area we have to do. That area that I showed, Region I, around the repository covers three watersheds that cover the repository. We feel that that is enough now to reduce the edge effects around the repository. We feel that we have some boundaries, no flow boundaries within that zone.

If that is the case then we can make it a little bit smaller and go into more detail. But I'll show in the infiltration talk where our current boundaries are now for modeling. We think it may be big enough, but we'll do the model and see what the edge effects are and then we'll go back and increase it or decrease it depending on the results.

DR. JONES: Could you give just a synopsis of the kinds of data that the model needs? I've tried to pick out some things. You need some data on the spatial correlation of the rainfall; you need some data on the temporal correlation of the storms. You need some data that correlates macroscopic storm types with local precipitation. Could you kind of summarize those kinds of things and whether your weather network is supplying that data to the model or whether your local weather network is providing data for validating the model? Maybe we can talk later about it more.

DR. FLINT: We think that--well actually your question was a pretty good summary of what we are trying to do. That's the information we think we need

DR. JONES: So, I guess what I'm getting at is some of your model is based on a couple of years of data or maybe a couple of storms. I don't know how much of this spatial correlation you give in a couple of storms. I assume you have few years. Some of the stuff is from bore holes or ancient data. I am trying to get a feel for which parts of these models are built on short-term data; which parts are built on long-term data.

DR. FLINT: Spatial structure that we have for average annual precip is based on about a ten to fifty year record in total. The close base storm-by-storm analysis is based on about two years of information. So our bimodal distribution, probably 40 years. The average annual precipitation, on average about 25 years and the storm-to-storm analysis about two years. And hopefully we'll collect another year or two of information.

We think that the storm types and the processes that we see which seem to be fairly consistent or in the literature may support our using the kinds of storm cells, the thunderstorms that we see now. The spatial correlation of a two kilometer storm cell is probably correct. It is probably the same size that we have had for the last 100 years or more. So we think that being consistent with that--one of the things we are trying to do is say whether or not we need that kind of information. If I take the two kilometer storm cell and I put it into a model and I get "X" result and then I make it four, and eight and sixteen and thirty-two and sixty-four and one hundred and twenty-eight and get the same result, that by the time it gets to the bedded unit or through the Tiva, nothing happens anymore and it doesn't matter, then that tells me that I am okay with what I'm working on. If it makes a major change in the modeling results, then it tells me I have to go to work.

What I want to do is take the best information I can get as quickly as I can, feed it into the models, run the models, look at the results and then iterate this as many times as I can, to see if it does make a difference. A lot of what we are doing, I can collect I think ten times or 20 times more data than a modeler can use. In fact, any data collected is probably mor ethan a modeler wants to use, be we can collect it.

DR. JONES: Just one last question. You've got very site specific kind of real time data collection all the way up to trying to answer the greenhouse question for the world. How much of this is your program; how much of it are you relying on other programs? I mean, you are not going to answer the greenhouse effect as part of your USGS study, but--

DR. FLINT: Is there funding for that? No, you are right, I am not.

DR. JONES: But you've said that what we think will happen with the greenhouse effect needs to be accounted for and you do.

DR. FLINT: Right. There is a program within the USGS that looks at future climates; there is a program in the USGS that looks at past climates. Although I am looking at regional meteorology and my program is to look at what is happening today and today includes the historical data that we can get from weather stations, not the pollen information--I am sort of moving out of my area a little bit but I wanted to show where we are today with where we were in the past and where we expect to be in the future to get a feel for what we are measuring right now, that these conditions are probably not going to stay this way very long.

When you talk about average annual precipitation, there isn't such a thing, unless you look at that 700,000 year record. Then you might be able to estimate average annual precipitation, but that is an inconsequential number. What you really need to know is what is happening over the next 10,000 years or so, depending on the repository. You have to tie cyclic variations in climate in with waste package design. What is going to happen during the life of the waste package when it is intact? What is going to happen after it is decayed? Are the climatic conditions going to change fast enough?

I asked Austin Long who wrote a lot of the work that EPRI did, when the next pluvial period would be here and he said, any day now. But I am trying to tie together, the USGS has a lot of programs on that. And maybe during a summary section, Dan Gillies who will speak later or one of the group from that program can talk a little bit about that or add a sentence or two in.

Infiltration. Now we are on to infiltration. This one will be shorter. We want to collect the necessary information to characterize the upper boundary conditions. That is what the purpose is, with enough resolution for adequate use in hydrologic models. This is something that is undefined, but this is sort of our goal.

We want to characterize the surficial materials is one of our objectives; characterize the natural infiltration process, what is happening today in infiltration. The changeable variables, these are more static. And what is going to happen under weather conditions, our artificial infiltration program.

This is the outline that we are going to go through and I'll bring you back to these areas. The first thing I am going to do is talk about the outline from the last meeting that we had two years ago and sort of a review of the conceptual model that we have set up so you can follow what we are trying to do now. Again, keeping in mind what I am trying to do is to extract out of this program information to feed to this models. We have a very good effort now with Sandia National Labs and with Lawrence Berkeley Labs and their performance assessment model, our own 3-D model to try to put infiltration into the program for the modeling effort. So I will talk a little bit about that.

This was the outline from before, as you recall. We talked about surficial materials, the physical and hydrologic properties, our geophysics program, our mapping in GIS, we went through in detail on that. Natural infiltration we talked about precip, we talked a lot about evapotranspiration, our neutron logging program. Remember the geochemistry, the tritium, Oxygen 18, the modern carbon in UZ 4 and 5 and UZ 6.

Our infiltration program we talked about the infiltrometer study, the small plot rainfall simulation, the large plot rainfall simulation and ponding, all this information is pretty much the same. We haven't made a lot of changes in that. I am going to try to show you what we've extracted new and how we are trying to apply that to some of this modeling approach.

I want to talk about the conceptual model. This is I think a useful quote. Sensible philosophy controlled by a relavant set of concepts. There is so much research time that it can nearly act as a substitute for genius. They didn't tell you that it takes genius to get a sensible philosophy through QA though. But I guess the saying is, good science without good QA is only good science.

This is our cartoon, our schematic that we have for Yucca Mountain. Not a real cross-section but a schematic to point out part of our conceptual model of infiltration. We have a series of different materials, alluvium, bedrock either welded or nonwelded that is exposed at the surface or covered by alluvium. We have channels with alluvium in the channels. We also have bedrock in the channel. Side slopes with some cover and without cover and then the ridge tops. Again the channels make up about one percent or less than one percent of the surface are of Yucca Mountain. How significant are the channels? How much water can you get down the channel? The alluvial valley itself, much larger percentage, maybe 30 percent of Yucca Mountain. Then the side slopes and the ridge tops make up the rest.

We feel that winter precipitation on shallow soils in fractured bedrock maybe a major source of water into the unsaturated zone. From snow melt, the water sits there for a longer period of time, low ET rates, the ability to move one or two meters in the fractured rock through the fractures, and one or two meters may be below the zone of evapotranspiration for the bedrock material. That water becomes net recharge.

Side slopes are important. Remember I showed neutron hole in two where we had water in the bottom of it, 50 feet 24 hours after a rain storm, a rock channel. If we do get a runoff event we see pulses of water moving through here. So the concept was that these side slopes and ridge tops may be very important. One, they get more precipitation; two, they have a very thin soil cover; water can get to the rock; move into the fractures and move down. So we want to study all of those different component parts. Again the channels maybe important. Upslope particularly we see a lot of water in these channels in the rock themselves. But as we get further down the channel and they get more alluvial cover they may be less important and I have some data that shows that. But this is sort of in general of the conceptual model to tie it altogether.

I'm going to talk about the surficial materials now, an overview of Yucca Mountain. What Dr. Domenico said early is that what we are trying to do in this talk is to give you an update. What have we done in the last two years since the last presentation. I came across this in ready Arrowsmith. It is probably one of the best books about scientific discovery that has been written. Leora, whose husband just went to work for the government as a researcher, she said, "I get it", said Leora, "Your job will take only about 28 hours a day, the rest of the time you are perfectly welcome to spend on research, unless, of course, somebody interrupts you." So, that is why there aren't many slides in this talk.

I want to talk about Yucca Mountain now. I am going to come back to this a couple of times. This is the surface of Yucca Mountain, colored in Scott and Bonk photo. We collected some information again for the modeling inputs and I'll show you where some of that shows up. One of the things that you may want to do later during the break is take a look at this, but what you'll see which is very important, I think, is that there are two different types of washes at Yucca Mountain. These are fault controlled washes. If you look at the character of the colors in here you'll see different exposures of different units in the Tiva Canyon. Over the repository itself, these are erosional washes, not fault controlled necessarily, although, there may be some faulting. Erosional controlled washes; fault controlled washes; different character.

A lot of the information that we are collecting, some of our geochemistry, in fact a lot of our geochemistry data comes from Pagany Wash and Drill Hole Wash. Fault controlled washes. And maybe giving different characteristics to infiltration processes and percolation processes. They may be different over Yucca Mountain. That is something we have to consider. So in our program, we have broken up the repository. On the surface of Yucca Mountain, from here down is one type, and from here up is another. We have to be careful that we don't take information from fault controlled washes where we get tritium at depth and Chlorine 36 at depth and apply it to an area that may not be faulted as much.

DR. LANGMUIR: But isn't there a tendency for the fault controlled washes to be eroded substantially?

DR. FLINT: The fault controlled washes are eroded. I am going to get back to that in just a second.

The fault controlled washes are eroded. There's more bare rock; steeper slopes. But these are eroded but not down as deep. We don't get into the same units that we see on these washes. We don't get down into the nonwelded Paintbrush units. But I'll tell you a little bit about what we had done. I want to get rid of the alluvium first so we can deal with the rock.

This was a slide from last time looking at some of the physical properties, simple physical properties of sand, silt clay, bulk density. We added to that an estimate of saturated conductivity for the surface flow property, based on some work that Gaylon Campbell had done. The important thing is this low variability. Coefficient of variation is fairly low. This is for about 200 samples all over Yucca Mountain.

The alluvium may be fairly uniform; it seems to be. Very high sand content and the measurements we've taken so far indicate that we may have some small range of numbers for the conductivity. We need to add to this conductivity before we dismiss the surficial materials, some characteristic curves. This is one approach that we are using right now to get characteristic curves. What we have done is gone out into the field after a storm, collected samples, measured the water potential, water content, and then several days later we keep doing this for a long period of time. We put all this together where we get volumetric water content versus water potential. In this case we fit a Brooks and Corey model to it and it fits fairly well. We can use this characteristic curve from this simple measurement technique, the conductivity to give some property to alluvium.

Now we can change that around these properties and look at the modeling and see how much of a difference it makes depending on the bedrock you have underneath it. It may not be that important. Getting one set of curves like this maybe all you need to know and it may be all the models can handle. We want to know that. Well we put in a little bit of effort and now we have some information so we can go on from there and start looking at the rock properties, and a little bit more about the cover.

Pagany Wash, this is a transect that we ran. We went from the top, Mike Chornack, and I and others went out there and ran up and down the hill looking at the different units. The reason I did this is that I was told that some of the modeling groups were using Tiva Canyon caprock as the overlying rock for the entire repository and that was the answer and that is what they were going from.

Well Pagany Wash is 12 percent of the area. The upper lithophysal covers more. Of the upper lithophysal, 27 percent covering the unit, 24 percent of that is exposed. So a quarter of it is exposed, rainfall, runoff right onto the bare rock. Then, we have some fracture information. This transect was done to Pagany Wash, one of these northern watersheds; 18 percent of it was alluvium in this particular case.

Split Wash, another wash to the south over the repository, three percent of the area is caprock. Well it turns out that the caprock measurement that was used by the modeling groups was they went in the first borehole G-4, drilled down to the first sample, came from about 40 to 50 feet, well it is close to the top, it must be caprock. No one told them that G-4 was in the bottom of the wash and it was down in here somewhere in the unit. So they were actually probably using the wrong number. So then it turns out that there isn't any caprock on Yucca Mountain anyway. It is way far to the north and all we have is upper cliff which is quite a bit different. And I'll show you some of that information in the matrix property talk. That is real important, the fact that the caprock is not there and that we are dealing with upper cliff which has really different characteristics from the caprock.

At any rate, we have these estimates from the transect, percentages and the exposure. The next thing we did once we had two different watersheds is that we set up in this watershed a point count system, how much of this is alluvium? How much is upper lithophysal? How much is caprock? How much are these different units? We put that altogether into this analysis. I am not going to go through all the numbers, but I'll show you how it works so you can look at it later if you are interested.

We have Drill Hole Wash Watershed and then the area right around the repository itself. If you look at the percent of the area, let's look at the repository. Thirty-one percent of the area is upper lithophysal. Ninety percent of the total area, it's the sum of all these, is bedrock, not the alluvial channels. Only ten percent in the alluvium. The whole Drill Hole Wash, 50 percent is alluvium and 50 percent is bedrock. We are back to here, (indicating). Thirty-one percent upper lithophysal; thirty-five percent of that is exposed bedrock. So over the repository itself, 11 percent of the repository is exposed lithophysal unit from the Tiva. You can go through and look at all the different ones. We also have some Yucca Mountain and Pah Canyon exposed--well not Yucca Mountain but Pah Canyon.

Seventeen percent of the area is exposed. The last time I made an estimate, I was looking over the transcript from the last one and I said 80 to 85 percent was covered. Well, it turned out to be pretty close to that. So we have some estimates now and we deal in a major property program on what are the properties of these rock that we are dealing with that are not exposed? The surficial materials may be something we can characterize fairly easily. Again, we are trying to feed this information to the modeling group. So now we have some numbers for them that we can use fairly easily. We can vary these a little bit, they can run the models and tell us how much detail we have to get at to get this information.

DR. WILLIAMS: I didn't understand what parameters you were basing your statement on that makes these percentage by outcrop very important. You made the statement that these numbers are in the next to the last column are extremely important with respect to, I guess you met infiltration.

But, what properties are you basing that statement on that you've measured?

DR. FLINT: One of the things that I want to bring up is that the modeling to date has used the caprock of the Tiva as the main unit for modeling infiltration. One point I want to make is that it is about 82 percent of the unit that we are dealing with has some kind of surficial material; six inches or more covering. Most of Yucca Mountain is covered by this nice sandy loam that has storage properties, so we can store a lot of water there in the near surface.

If we are dealing with rainfall on exposed bare rock, which is what the modeling efforts using constant flux may do, you have to deal with more of the upper lithophysal units. The kind of properties I mean are its fractures; how many fractures do you see in it? What are the matrix properties? Saturated conductivities, what are the characteristic curves? What are the relative saturations now? And if we put rainfall on it, how much water can this take in under a typical rain storm? How much of this area is actually going to just move water down into the alluvium? I think the storage properties of the material, the colluvium and soils on top are very important. This is important because most of Yucca Mountain is covered with some kind of soil material. I think that may be the point I was getting at. But the properties we are looking for and trying to get some idea of fracture density--go ahead.

DR. WILLIAMS: Do you have enough data to preclude that or is this a hypothesis?

DR. FLINT: This is a hypothesis. What I am trying to do as I sort of said earlier is that I am trying to pull out the information that I can get up front fairly quickly to modeling groups. Modeling groups have been working for years without this information. I am trying to draw this out as quickly as I can with these less than ten people I have, actually I have 9.5 people, including myself. I'm the .5.

With that I want to pull this information out. Somebody can go back and do a much more detailed study. They are not going to get a lot different from this. What I want to know is can the modelers put this information to use and tell me what is happening by just taking these percentages in their models. If they are going to model only one unit of vertical, one dimensional model, do they want to start with caprock, or do they want to start with this, because there is quite a difference in these rocks and how you deal with that. Quite a difference as I'll show you later. It's kind of some neat stuff actually. I've saved the best for my matrix talk, I hope.

DR. JONES: Could we go back a little bit to your representative soil properties and could you explain very quickly, your Ksat estimate was precisely down how?

DR. FLINT: It is a particle size analysis of the old Shirazi and Boersman paper on using geometric mean, particle size and particle diameter is what Gaylon Campbell used in his soil physics book. We just took those calculations. We did some measurements like that in some other areas where we had better data in Oregon and put that together. It seemed to work fairly well, so we just applied that. I needed a number; this was a technique. We actually took this number, put it into a flow code that we developed at Oregon State and made some calculations to try to match neutron log data that we saw. It worked fairly well, although it is very sensitive to this. This is probably within an order of magnitude of the right answer. But it was a simple estimate from particle size analysis. And a small correction for cobbles which was done by Brakensiek and Rawls back a couple of years ago.

DR. JONES: And your water retention curve, your equation you have there is just water content. Is that correct? Was it Theta over Theta S?

DR. FLINT: This is water content in this equation, but I don't have a Theta S for this. I don't have this measurement. I could make an estimate of it and say 35 to 40 percent, somewhere in there or maybe 50 percent. You can use a Van Genuchten if you want, you can use a Brooks and Corey. Van Genuchten I am not real happy with at the dry end. It doesn't work very well at the dry end. Brooks and Corey seems to work a lot better at the dry end water contents. We also suggest that you may not want to use it at the real dry end for soils. So we used Brooks and Corey in this case, but I could fit either one to it in this case.

DR. SCANLON: How did you measure water potentials for that curve?

DR. FLINT: With the psychrometer.

DR. SCANLON: In situ psychrometer?

DR. FLINT: No the psychrometer in the laboratory. We just took samples into the lab.

DR. DOMENICO: Bridget please identify yourself.

DR. SCANLON: I'm sorry, Scanlon.

DR. FLINT: We have some in situ psychrometers. We are not real happy with them right now, so much so that I am not going to show you the data, I'm just gong to show you the instrument string itself. But this is our first approach. This is a field measured water release curve. This accounts for hysteresis only in that it has a water content/water potential relationship. The points will vary because of historetical facts. But we think we can get a curve through here.

Hysteresis for some processes maybe very important. It may not be that important once you get a ball park estimate of the curve. At least you know that you are somewhere around there and you can account for that numerically by just adding some hysteresis point in there if you want.

DR. SCANLON: Did you also measure the moisture content in the lab?

DR. FLINT: We measured the moisture content in the lab and the in-field using time domain reflectometry. We also have neutron probes in the area. We are also using crosshole gamma to do changes in water content with time. We haven't compiled all of that data yet. The person that was doing the time domain reflectometry left and we haven't replaced him yet. So we are sort of waiting on that one. But we have a lot of instruments in the field to do some of these measurements. This is just our real quick and dirty to get some characteristic curves. And we'll just apply the Brooks and Corey to the conductivity function and use that for some estimation modeling.

DR. JONES: But where did this data come from?

DR. FLINT: Which data?

DR. JONES: That's in this plot. That was the same sample you took back for the psychometry that you did--

DR. FLINT: Pardon?

DR. JONES: The water contents on this plot?

DR. FLINT: This plot we just did water content/water potential. That just came from one field measurement. These were 200 measurements that were collected all over Yucca

Mountain. A lot of this data is a part of a master thesis that came out of Colorado School of Mines. There was a graduate student that we had working for us.

DR. JONES: But this particular curve, you took a sample back to the lab for the psychometry and you measured gravimetric on that sample or is this neutron data?

DR. FLINT: No. That is water content, that sample versus that water potential in the laboratory. Measured in the laboratory.

DR. SCANLON: It is just sort of confusing when you say it is field measured. You really measured this stuff in the lab.

DR. FLINT: Right. Collected in the field.

DR. JONES: Field samples.

DR. FLINT: We didn't make it up. I guess that was the whole the point. We didn't make up the data.

We have these measurements of water content which are consistent with these in the same point in space in the field. So when we say in the lab it is 15 percent on that day, it was also 15 percent in the field. Although we measure it in the lab we get a volumetric measurement in the field. And that is how these are volumetrics. It is consistent with what we have in the field because we calibrated from it.

DR. SCANLON: But it is also you have a number of different soil textures.

DR. FLINT: No, all of this was the same site. You are right there are some variation textures. It's just a big plot on the ground. We went out there and took one here, and then one over here and then one over there.

DR. SCANLON: The texture was the same--did you measure texture in all the samples?

DR. FLINT: No. But the texture is pretty much the same in the whole area in this particular location. The spatial correlation which we've done variography on texture which is in the last handout from the TRD meeting, we showed sand, bulk density, silt variograms and show that we do have a good correlation at close spacing. We feel pretty comfortable. It accounts for changes in texture of the area, so this would apply to the area that we measured.

DR. JONES: An extension of the particle size correlations to the K sat, there is also correlations for the water retention curves, did you use that and compare it with what your field measured?

DR. FLINT: No. That is the one thing we haven't done yet. But then again we just got this out a couple of days ago. We'll do that this afternoon.

DR. JONES: I mean it might be interesting--

DR. FLINT: You are absolutely right. It would be good to check that to see if we can take that texture analysis and predict that characteristic curve and see how close it matches. This is a half a bar air entry potential, which seems pretty dry to me for that soil, it's mega pascals; so you can figure in mega pascals 0.05.

DR. JONES: Yeah, but only if you've got a saturated water content hidden in that number 2 which will change.

DR. FLINT: Yeah.

DR. JONES: So you don't read that directly as an air entry potential in that particular way so it maybe more reasonable.

DR. FLINT: Potential, but it is in the Brooks and Corey function and is used as the air entry potential in the aggression analysis.

DR. JONES: If it is multiplying Theta over Theta S.

DR. FLINT: Right.

DR. JONES: If it is multiplying a number going from zero to one, not zero to Theta S. That is really air entry potential divided by Theta S to your B value. So if you did all that out, you might get a more reasonable air entry potential.

DR. SCANLON: I just have one last question, did the 10 mega pascals, does that indicate the natural water potentials in the system during dry times?

DR. FLINT: It's fairly dry. I think it gets much drier than that. Some of the data that you showed in your talk a couple of weeks ago was--

DR. SCANLON: Was a 15. Actually Glendon Gee is up to 250.

DR. FLINT: That's right. He has a different way of measuring.

Let's see now, I'll try to catch us up a little bit. That was sort of an overview of the different regions, how we are getting at some of the quick approximations again. These are quick. We are trying to get as much information as we can out, as fast as we can so people can start using some of that.

Our current future work, what are we currently doing? The sampling, testing and mapping of the physical an and hydrological properties, this current work, we are doing that right now. We are still working on the alluvium like you saw. We are going to do that on more locations. The soil cover, we are still working on, and surface and subsurface bedrock. We are working on the properties of those and that I'll show you in the matrix property program.

Estimating the surficial units for our 3-D flow model; we are doing that right now. We have the people to do that. Vertical variability; we have looked at surface. What about the vertical variability particularly in the alluvium? We are using inverse modeling for hydrologic properties. That's current work. We are going to try to do that in a symposium in air/land recharge in Denver, I think it is in October in The Soil Science Society of America in the section and show how we use inverse modeling to estimate these properties; inverse modeling from our neutron data. Hopefully, that will work out. It better, we already did the abstract.

Measurement from the neutron core holes, that is future work. With the air quality permits we have, we may be able to get some core data fairly soon. We won't have enough information to test our inverse modeling on these. This data we have six years of record; this data we will have it when we get it. We'll try to make those calculations later to see how we do.

We will take some measurements of core holes near where we did some of our analysis and see how well we did too. Soil thickness map, very important to see how much alluvial cover we have. Borehole contacts, we have that information, now we are trying to make some maps today. Surface geophysics, although we had done that and I presented that information last time, my two geophysicists have left and I haven't replaced them as yet. So that is future work again.

I want to talk a little bit about natural infiltration. Again, I have shown you most of the work last time and since this is an update, I want to talk about how we are doing some of our estimates of recharge, and then I'll go into a little detail on Pagany Wash later on and some of the things we have learned since then.

Recharge, we want to get an idea of the regional estimate because we want to see what is the potential at Yucca Mountain for recharge. This is a rainfall analysis, the krig you saw earlier. You can apply this to the Maxey-Eakin technique for estimating recharge where they put it together for several watersheds in the desert region. And this is the map you see for the region based on the kriging. A lot of recharge west of Las Vegas in the Spring Mountains and the Sheep Range, some higher elevations to the northeast and then the mesas thought to be the major source of recharge.

This estimate says no recharge of Yucca Mountain on an average annual basis under the current climatic regime we have. We look at our cokriged map and get a better estimate of rainfall, and we can make the same Maxey-Eakin calculations. We get a different estimate, not much different; Spring Mountains, fairly high. The mesas are the major source. We do get some estimates of recharge based on that analysis although that is mainly from gridding. It stops at about six millimeters. This is up in north of Yucca Mountain in the Calderon Complex. The repository down here again from this estimate of cokriging, no recharge. Although this would indicate you do get some at Bear Mountain across the way. That is sort of a region. We don't think we have any based on that analysis.

Local estimates of recharge. How were we doing those local estimates? Again that is based on our 3-D model. I'll show you how that is put together right now. This is the region we are working with on the 3-D model. We are bounding our model at Yucca Wash to the north, Bowridge fault, Solitario Canyon fault, and then down near Busted Butte on the south. These are the three main surface types that we have broken out for Yucca Mountain that we think have different infiltration characteristics: ridge tops, the side slopes in white, the alluvial fill in the blue.

For our first estimate we are going to give those some different properties of infiltration for the modeling exercise. And this is about 250 elements in the model. We add to that a rainfall estimate. Again you've seen in past modeling people start with a half a millimeter a year recharge making it uniform over the site. One thing we've proposed is if you take rainfall--if you assume that the properties are uniform over the site which the modelers do, and we know that we get an increase in rainfall to the northwest, we should get a different recharge to the northwest. And because of the way the beds are dipping in some directions to the east, a little to the northeast, some to the southeast, we want to see in 3-D what happens if you put a lot of water up here. Does it move across the repository in this direction? But we can take a half a millimeter of rainfall and scale it according to this map.

This is an exaggerated rainfall map based on a regional elevation rainfall relationship. If this exaggerated map doesn't show us much, then we are pretty good. If it shows us a lot then we have to get more detail. This also is average annual, not storm-by-storm, which I think is more important. But, we use this modeling technique to try to look at local recharge at the site for each of these different units that we are dealing with.

DR. JONES: Alan, could you define--you have used recharge and infiltration sort of interchangeably.

DR. FLINT: That's true.

DR. JONES: Could you define them and tell me what lower boundary you are talking about?

DR. FLINT: Okay. Net infiltration as we define it and it is in our study plan that way, as water that has moved below the zone where it cannot be brought back up to the surface by evapotranspiration processes. The depth in the alluvium, we don't know, maybe six, seven, eight, ten meters; in the bedrock it may be one or two meters. You can get air with moisture which Ed Weeks will talk about later, brought back up to the surface, even excluding the boreholes. That process of bringing water vapor back up to the surface, not part of the ET process that we normally think about in agriculture, is not part of the net infiltration.

The water that we move down below the zone of ET may never make it as recharge. It may all come back up to the surface again, but it is not part of that infiltration. When I talk about recharge in this case, I am talking about an input to the system that will make it through the model to the saturated zone and recharge. So recharge is what makes the unsaturated zone that infiltration of getting below the zone of ET and what happens in the middle is what Joe Rousseau does. So, I'll let him talk.

DR. JONES: What is your depth of simulation?

DR. FLINT: The depth of simulation is to the water tables, about 2,000 feet. We have 20 elements down there. The faults are modeled individually as fault. We give those whatever properties we want. We are just putting it together now, so hopefully we will get it running fairly soon.

We think that we can do this 3-D simulation with whatever inputs we have and some real physical properties which I'll show you from the matrix program. We think we can do this or Bodvarsson thinks he can in a three day simulation--three days on the computer for a million year simulation to study state of this 3-D model.

DR. DOMENICO: Do you have the spatial variation in rainfall which you measure? From that I see that you are estimating a spatial variation in what I would call infiltration.

DR. FLINT: Right.

DR. DOMENICO: It seems to me you are skipping a lot. What about the matrix potential? Shouldn't there be a relationship between variations in rainfall and what you are observing in the rock in terms of the moisture content or matrix potential?

DR. FLINT: What we will do here is we'll put in the physical properties as we can best estimate them. We'll set up some infiltration process with these physical properties and we'll let the system run the steady state and find out what those properties are, what the potentials are. Then hopefully we can look at the data that we have, which is very limited at this point and see how well did the model match.

I think the model will not match. I don't think that there is anything that is steady state at Yucca Mountain. I think there are disequilibrium in potentials. I think the disequilibrium is caused by climatic changes, cycles. I think that there is an integrator at the site and that is the Paintbrush unit. And we may see some variation in that, but it is in disequilibrium with the Tiva, it is integrating the system over maybe thousands of years or longer. We hope to get that information from the model and this cyclic input of rainfall. I'll talk about that at the very end, how we are going to try to do that.

DR. DOMENICO: Do you measure any change in matrix potential in response to changes in precipitation? Has that been measured in the field at all?

DR. FLINT: No.

DR. DOMENICO: No. You've looked for it though?

DR. FLINT: No.

DR. DOMENICO: No, you haven't looked for it.

DR. FLINT: We have no access to the site for those measurements at this time. We have access to another site we've got, unfortunately. And unfortunately, the instruments aren't as well as we had hoped for. But, I'll show you that in a little bit. The only access we have to the site for those kind of measurements now is some surface measurements which we've done, and neutron logging to get volumetric water contents. There is no way right now that we can measure water potentials. It's a very important measurements. It is one of the most important and that is what the borehole program is going to go after. I'll talk in matrix about that, but I think that is a very important next step for those potentials.

DR. SCANLON: Sorry, what model are you using?

DR. FLINT: TOUGH.

DR. SCANLON: Okay.

DR. FLINT: The TOUGH Code that was developed at LDL was designed and has been used successfully on large geothermal sites in three dimensions. I think it is an excellent code for what we are trying to do.

DR. SCANLON: And you think that model shows sensitivity to initial water potentials?

DR. FLINT: It will come up with some water potential for the system. I don't know how sensitive it is to initial water potentials.

DR. SCANLON: But then you don't know how important water potential measurements are, do you?

DR. FLINT: I think they are very important.

DR. SCANLON: But I mean, if the model says it is not very sensitive to it, I mean, we don't know.

DR. FLINT: We don't know that. We don't know that. If the model says it is not very sensitive, I think it will say that because you are looking at steady state conditions. I mean if you are doing steady state flow, you know the porosity is not important under steady state. Yet porosity may be one of the most important variables we have for ameliorating these large rainfall events.

DR. SCANLON: But they are basically running an transient simulation until they reach steady states.

DR. FLINT: Right. They just start out with some conditions and run it for a million years until we get steady state. But, I want to get beyond that. Right now that is just to test the model to make sure everything is working. Then we want to start looking at non-steady state by changing the input.

DR. WILLIAMS: Alan, I think I delayed this question. You are going to discuss UZ-1 and UZ-7 in chapter 3, right? The data from UZ-1 and UZ-7?

DR. FLINT: No. The data from--I don't have any data from UZ-1. Joe Rousseau has the data from UZ-1.

DR. WILLIAMS: So he is going to discuss it?

DR. FLINT: He may discuss UZ-1. The UZ-7 data that we have, UZ-4 and 5 data we have we showed last time and I am not going to talk about that unless we have some questions about it. But we showed all the data that we had at the last meeting.

DR. WILLIAMS: I think the data from UZ-7 maybe pertinent to instrumentation--validity of instrumentation. That probably needs to be brought out. I don't know who is going to do it.

DR. FLINT: Joe Rousseau is going to talk about the deep borehole measurements. I might talk a little bit in matrix about matrix water measurements potentials, but I hadn't intended to talk about UZ-7.

DR. WILLIAMS: But he is?

DR. FLINT: I don't know.

DR. ROUSSEAU: I'll try to answer that question. I did not bring information on UZ-1, so I will not be presenting any of that. I did touch UZ-1 in our last meeting. But, I am going to be talking about the types of sensors that we are going to be using in the unsaturated zone instrumentation program, deep borehole.

DR. DOMENICO: On unsaturated zone? You said unsaturated zone.

DR. FLINT: Unsaturated zone he said.

DR. ROUSSEAU: Correct.

DR. DOMENICO: Thank you.

DR. JONES: Alan, in your discussion of the meteorological program, one of the objectives that I thought I heard was to try to figure out how--if there was a depth at which these surface variations, long-term climatic changes are on some time scale were to damped out and it got a lot simpler, is that objective in part of this modeling program? You said you are starting to put it together and you are already going right to the water tables.

DR. FLINT: That is part of it. There are several programs going on now, and I think the Board met sometime ago with the performance assessment group and heard from Maureen McGraw, I'm not sure if she talked about it in detail. The USGS and Sandia are working very closely on looking at boundary conditions. One of those is what is the influence of the bedded tuffs on ameliorating cyclic changes in input from rainfall events. They are looking in one and two dimensions. So we are working with them on that to try to see if these bedded tuffs can dampen out the input.

We are going to take some of our information, our rainfall simulator and a 1-D model to start with and run it through using tuff to see what the cycle will be. I'll talk a little bit about that when we talk about variable infiltration input and try to tie everything together if I can, at that point and how we are looking at that. But there are a variety of different models going on that we are using now.

One is a site scale model, 3-D to the water table, or some one dimensional models that just go to the Paintbrush tuff and some that go beyond that.

DR. DOMENICO: Does tuff have a functional relationship between recharge and moisture content? In the workings of tuff.

DR. FLINT: I am not sure--I am not that familiar--

DR. DOMENICO: Or is recharge just a number put it?

DR. FLINT: You can put in an input, a recharge, you can put in a boundary and just start flow occurring and then see what potential gradients build. Or, you can put in a cyclic input, any kind of input you want and then look at what comes out the bottom of the model or some zone where you would say that anything that gets to that point is recharge and make that calculations.

DR. DOMENICO: Yeah, but we have to have some physics involved. I am asking, the physics, does it incorporate a relationship between let's say how much infiltration is permitted given a certain up moisture content?

DR. FLINT: It uses--the physics that it uses, it's Richards' equation base flow based on characteristic curves, initial water contents, water potentials, etc., saturated and unsaturated conductivity curves.

DR. DOMENICO: So tuff is the solution to Richards' equation?

DR. FLINT: Solution to Richards' equation.

DR. DOMENICO: All right. That's fine.

DR. FLINT: It's an integrated finite difference solution to Richards' equation. So it accounts for all of those properties. And the more information we can feed into it, hopefully the better our results will be.

I want to talk a little bit about our Pagany Wash study and what we've learned since then. I'll talk a little bit about evapotranspiration, too.

As you recall from the last talk, I showed you this borehole and I showed you how in days since January 1, 1984, Yucca Mountain has been drying out from our wet year. We dry down in the summer, winter, down in the summer, winter--and then we had that really low precipitation year of last year of 50 millimeters. Then this year we had more rain by March of this year than we had all of last year. So we are seeing at the surface, this is the top meter, we are seeing this increase in water content, although it hasn't recovered, we do see the increase. From one to five meters steadily drying down. It is constantly drying down. And even five to ten meters fairly deep in the system it is drying up. Is it drying out because the water is moving down below it or is it because it is an active process that allows roots or evapotranspiration to occur, which I think is the case in this system.

This isn't a channel. This is next to the channel in a terrace. The same phenomenon at the surface, but initially lower water contents. So the terrace is drier at depth than is the channel, indicating to me that we did have the major event that infiltration, possibly recharged later on, in the channel itself. Just my guess based on these higher water contents. Although now, the water content at one to five meters is about the same as it is at the terrace.

If we go down the wash, and I'll put all this data together hopefully in a minute. If we go down the wash, again in the channel, the same phenomenon you saw, and at the terrace wetter in the channel at different depths, the surface; one to five meters, five to ten meters. But, still it's declining and you can see at a depth of one to five meters. And in 14, quite a bit drier in a terrace. I want to put this together, the top meter, all of those sites. That's the data you can look at later in detail if you want, but we see very consistent results.

This is the top one to five meters, the channel upstream and downstream about the same in general. The terrace upstream is wetter than the terrace downstream. We notice that and I showed you correlation is that the less alluvium you have the wetter the bedrock seem to be. Now the further upstream you go, the thinner the alluvium in total, even though we are only looking at one to five, it seems to be wetter. So we are getting more water into the alluvium, the further upstream we go. And N-14 downstream terrace is fairly dry.

One to five meters under the channel it seems to be fairly wet. If the water feeding this was side slope flow, it would pass through this zone to get here. And since this is quite a bit dryer, we feel that the water that is in that zone came down from above or from up channel. That is a question we are asking ourselves. Can water from the side slopes move down subsurface out to the bottom of the channel and then down? Can that occur?

We know that sometimes it does along the bedrock interface, the contact, but in general it seems like most of the water moving from the channel came down from the channel and not side slope flow. If we look at five to ten meters upstream the wettest, the channel downstream is next, then the terrace upstream is wetter. So, these two holes are side-by-side and these two holes are side-by-side. So it is wetter at depth upstream and it is wetter in the channel, indicating that the channel may be a very important source of recharge or net infiltration because of the higher water contents.

DR. LANGMUIR: Alan, do these recharge events relate? Are they all at the same times or can you use isotopian chemistry to help you out?

DR. FLINT: Well we don't have any information on isotopes on this--this is all from neutron logging data. The isotopes that we collected, the tritium data came at this point in time. I think that these differences that we are seeing may be the result of the '84/'85 recharge or storm event that moved through and got a lot of recharge into the system. They may be coming back to equilibrium. How long does it take before they'll come back, I am not sure. But we do see pulses at depth, five to ten meters. So we are getting some flux into these, although it shows up and then it sort of goes along and then comes back down again. There is some noise in here. The noise I think is due to the--not the best quality calibrations that we could have and using different meters to make the measurements.

One of our first proposals is to recalibrate and redo all of this analysis with the first couple of holes we get to drill on Yucca Mountain to try to fix some of this stuff. But the point that I was trying to draw from this was that the further up the channel we go the wetter it is. And, we know when we get up to the bedrock that is exposed, some of the nonwelded units, those are the wettest we have on Yucca Mountain. In Pagany Wash, they are exposed directly. And we see tritium data that has moved down quite a ways in there and from this data we know that we get pulses that move in the channel at about thirty centimeters a year. So it is moving down there and we can actually see those pulses. This was sort of part of our conceptual moving, the further up channel we go, the more moisture we have, meaning that the upstream side may be more important for recharge because of the larger volumes of water we get in.

The flow we had in the channel that made these high water contents probably never made it to Fortymile Wash. It probably was sucked into the alluvium on its way down. The water that moved in here, there may not have been as much available and the flow may not have occurred as long down here, and I'm sure most of you have seen in desert hydrology, you can be standing ten feet away and watch water coming down the channel. It disappears before it gets to you. We've seen that in Mercury where we have all these big thunderstorms. But I think that same thing is occurring here. So, one, we just don't have the support in the same amount of moisture. But that is just some information that shows the direction we are trying to go with this and to do more analysis on this later.

DR. SCANLON: Alan, I have a question. When you are comparing moisture contents in different environments like the slope or the channel or whatever, you assume that the soil texture is the same. Do you have information on that?

DR. FLINT: No, we don't. We have--we are using the inverse solution to modeling some of the flow to try and estimate what those properties are. Moisture content is not the best comparison, but it is the only comparison we have right now. We don't have any other way to make any measurements. We feel that the transects that we've done and some of the surface measurements that we've done are consistent enough in the alluvial materials consistent enough that they are the same.

If you look at a water profile for instance of N-13 and N-14, which are only about 30 feet apart, you see the same property, the same profile. The profile just shifted because of the water content. You see the same layering. So whatever laid up alluvium, even though the channel is in one location and now it is probably somewhere else, but there are layering, distinct layering, and those layerings show up and are consistent across numbered boreholes. N-7 and N-9 are within about 20 feet or each other, 20 to 25 feet of each other. So they are fairly close in the channel, although 7 and 9 are quite a bit different from 13 and 14. And those comparisons are a little harder to make. It is the data we have available now and it does show some consistencies and it seems to show up in different washes too. But, you are right, we need to get more information on this alluvium and that is why I talked about using inverse solution to estimate the new drilling program.

We are going to locate some more holes next to where we have a tremendous amount of water content, historical data.

DR. SCANLON: How about using water potentials?

DR. FLINT: We don't have any water potentials.

DR. SCANLON: I know, but you are going to do more stuff, so why not put in--

DR. FLINT: We are going to measure water potentials. I'll show you in a little bit the technique that we are going to use to add water potential measurements. I talked about that last time at the other meeting and this was sort of an update. I was trying to through some things in here for you actually, to cover some of that.

DR. SCANLON: I understand that, but you know the water potentials are not going to change across different lithologies or soil texture is going to be independent. So in order to check these differences, you need maybe some water potential data.

DR. FLINT: Right. We need water potential data. We intend to collect water potential data. We have a whole series of experiments. Joe Rousseau's work goes from--I'm not sure how near the surface, to depth with water potentials. Ours will go from the very near surface as best you can do with water potential measurements with these large thermal gradients downward to about where his start. But I'll show you just sort of schematically how we are doing some of that.

DR. JONES: Alan, over here, Tim Jones again.

DR. FLINT: I hear you from up above. That is why they hired you. Just kidding.

DR. JONES: I don't know what to say about that. I think I forgot my question now.

DR. DOMENICO: Just step down from your cross.

DR. JONES: I need you to help me understand what you are saying here. You've got higher water contents at fairly significant depths at the top of the channel, then the bottom of the channel.

DR. FLINT: Well, not at the top, but the closer we get toward the top of the channel--I don't have a photograph of it. I'm using water content as a surrogate.

DR. JONES: That's close enough. And you are hypothesizing that the reason is is that there is so much more water coming into the alluvium in those regions that they are sort of artificially maintaining these high water contents. Could you compare that hypothesis with another hypothesis that there are soil differences that this rather consistent difference between the long-term average water contents, if you will forgive the qualitative phrase, just a field capacity phenomenon and that your little noise is really your signal that you've got these little discreet recharge events that are oscillating around this mean?

DR. FLINT: We want to look at this signal. We want to look at this in detail because of this information.

DR. JONES: Have you done any back of the envelope calculations to see how much water it would take to come into the top or that channel to maintain that large difference in water content?

DR. FLINT: No. We haven't done that yet. Well 7 and 9 are close together; we assume they are the same. The point here was that the channel had more water because of the inflow. Simply water was running off and it was in the channel, here it was not. That is the difference, we think. The same with these two measurements. Why would the terrace which is subject to the same amount of rainfall have such a different water content at that depth? That question I don't know. It may be due to the textural changes, field capacity of that material. I think it is different when you get down to those alluvial channels you get a lot more coarser materials or finer materials. There are differences in the channels we are pretty sure of that. We don't know just what they are yet. So it is hard to make the comparison, we can make it between these two and between those two.

DR. JONES: If your hypothesis is correct and the water content differences are due to extra sources of water, but that water is disappearing before it gets downslope, what kind of recharge rates would that give you for those areas? If you've got a certain volume and a difference in water content and all that water went down--

DR. FLINT: We could make those calculations--what I did to make the calculations, I did do a back of the envelope calculation on this. Actually I did it in the car so it is back of the steering wheel calculation. But looking at the amount of water that we saw moving through the system, a pulse that we think existed, the amount of time that there was runoff in this system, on an average areal basis for Yucca Mountain, it consisted of .04 millimeters per year based on the frequency occurrence of these kind of events.

Quite a bit at that.

I think it turned out to be about two or three centimeters of water in the wash itself. But because it occurred in the wash and not in the terrace and the terrace and the rest of the hill made up so much of the mountain, when you start to distribute that it becomes inconsequential. But that is a question we want to talk about and we are concerned about when we look at the cyclic input.

If you take a half a millimeter a year, what does that do to your system? If you put in three centimeters in one year in this channel and then don't do anything for five years, is that different? At the near surface it is. When it gets to the bedded unit is it any different, and that is the modeling exercise we are doing right now. We are taking these large pulses and I'll make the back of the envelope calculations, give some numbers to the modeling groups, they'll move it through the system and tell me how frequently do I have to have this three or four centimeter pulse in a wash before it becomes significant? Before the assumption of a uniform infiltration rate below the Paintbrush is important. And I want to answer that question.

Again, this analysis was done last week. You know, I get it--Leora said, 28 hours a day and the rest of the time, and this is what we did the rest of the time.

DR. DOMENICO: I was just going to suggest we move along. We are running about 15 minutes behind.

DR. FLINT: Okay. I'll go faster. I've only have 100 slides left so we'll be okay.

Evaporation pan, again we are looking at evapotranspiration processes in the wash. One of the things that we want to know that is important is what is happening with evapotranspiration in these washes and particularly Pagany Wash. I'm showing a little bit of detail on that.

This is 1990 Class A evaporation pan. Joe Hevesi was clever enough to go around to all these other stations that he could find where they had evap pan data, put it altogether to compare it with ours. A lot of people don't like standard evap pans, and we don't either, but we have one. What we notice here, there are two stations of Pahrump and Logandale which seem to be quite a bit less than potential ET. Logandale near the lake; Pahrump near some golf courses. So we think we have an oasis effect. Boulder city, in the summertime seems to be a little bit less. These might be the high potential ET rates.

The one thing we notice when we look at daily rates of potential ET from our pan, we get more evaporation from the pan than there is solar radiation at the pan. The increased rate, we believe is due to large advective conditions; hot dry air blowing across our pan. A huge energy balance calculation that we have to make and those are important for these channels, we think, in the kinds of storms that we see. So, we are trying to do some energy budget calculations to see how much potential invective energy we may have in the system. The reason, and why we are doing some ET studies is that if you have in this case a rainfall event, let's say a fairly large storm that comes through, hits Pagany Wash and runs down, you basically have a larger dry area, a storm that may be one or two kilometers in size, a large dry area, big advective conditions, so you can't just do a simple energy balance. You have to account for that.

We saw this 15 centimeters of water move into the system and down to 15 feet in about 24 hours. In about 48 hours later there was only about 4 centimeters left and it didn't move below that. That 4 centimeters eventually got down to two or three. It got down below the zone maybe of ET. But we moved most of that water back up fairly quickly, higher than what you would estimate from a standard energy balance calculation.

So we are looking at ET for that reason. One of the things we want to do--this is a Bowen ratio station set up in Pagany Wash to measure evapotranspiration. And if we look back up the wash, it is kind of light in here, but this is a station looking back up in the wash. This is where N-13 and N-14 were, the down channel, and then up with this small trailer is UZ-4 and 5. This is where the other two holes were.

A lot of people argue you cannot make an estimate of recharge based on water balance calculations. And I agree you can't. But, what we are trying to do with this and why we think these measurements are important is this. If you have one inch rainfall average distributed over the site, let's say for instance, and you measure two inches of ET down here and half an inch up here within a couple of days after the storm, it tells you a lot about the movement of water down the system.

We think that we see in the near surface and maybe the subsurface, water moving from these areas where we get rainfall down into these areas and evaporating down here. We want to know that. So, we are using ET, not as a way to get recharge, but as ET, as a way to look at the spatial evapotranspiration process to know if it is going on in this zone. We will make some estimates using that information, but we are looking at where the ET is occurring. And we are using these ET measurements for that.

The other thing we are interested in, is if you get a summer storm and you get one inch of rain, if you get an inch of ET in three days, that tells you something about the system. Although you can't take the difference and say here's how much recharge we got, you can measure the inch pretty easily within some error bars, but you can get a good idea how fast it disappeared. In the wintertime if you get an inch of rain, it takes a long time. Long contact time with the soil for infiltration.

I wanted to talk a little bit about vegetation, about evapotranspiration processes again, and about the influence of the climate we are currently working on. This is a 1984 picture in Drill Hole Wash. Here is a borehole, pretty good vegetation in the site, then we had five years of drought. This is was the site looks like after five years of drought, which makes me believe that we are probably as dry as we are going to see out there, less than 50 millimeters a year. This is that same borehole. Most of the vegetation or a lot of it is dead. Then after three months of a pretty good rainfall in the springtime, this is what the site looked like again, the same borehole. It is a lot greener, but it is mostly annual. Those are all gone now, so we are still back to those conditions.

The point being here, how fast can the system recover to account for increased rainfall? What about the periodicity? What does four years of drought do to you? If you dump a lot of water on the system, does that mean it is all going to infiltrate because there is no plan? Well the annuals can pick up a lot of the load and get a fairly high ET rate out of there. So, we have to account for that.

You get a future climate change; increased precipitation, you are going to get change in vegetation, which may account for a lot of your ET and still maintain a fairly low net infiltration of recharge if we use the other term.

The control plot studies is the way we are using to get at water potentials. Our artificial infiltration control plots, and there is a lot of information in the study plan on these. This is one of the sites--this is where we will do ponding infiltration. We have a couple of neutron holes; we'll also have psychrometers, heat dissipation probes, TDR cross hole gamma. Lawrence Livermore has expressed an interest in doing some geotopography here if we can get rid of the steel casings.

On another side, which is not really shown is our control plot will do the simulations here and on the other side, we'll measure all the same properties under natural conditions. That is one type of study.

This is a wash. It was a neutron hole near test cell C. These are not part of the neutron logging program, had surface casing, about five foot of surface casing and about 35 foot of hole. It was open hole. We took the drill string, the instrument string out of G-Tunnel when they closed G-Tunnel down and just put all the instruments back in the borehole at depth down to 30 feet, and we are measuring water potentials temperatures and pressures. We are collecting that data now trying to get an idea if we can use this. We have a few locations on Yucca Mountain where we want to do just this.

The work that Joe Rousseau has done has really advanced our understanding of tensiometers and how we can get them to work to collect the kind of data we want and we hope to apply that in these locations. We'll have neutron holes, these are some small diameter holes that we are testing out and some water potential measurements. Hopefully we can get some information on that. But, we don't have time to go into too much detail, but we are trying to collect that data now.

I want to talk about our variable input to infiltration model, then I am done. And, I think I do most of that in this one slide. This is kind of a complicated slide, but it is actually quite simple too.

That is not millimeters per day, it is millimeters per any time that you want. It could be a lot of rain; it could be like Oregon in time.

This is a cycle which I am proposing that we use some kind of a cycle for input to some of our models. At least we try it out. Right now we have an average annual precipitation of 172 millimeters a year over the repository. If for instance what Dwight Hoxie said is right, and for all practical purposes, let's start off with the idea that there is zero recharge. Zero net infiltration. If that is the case and we know we have some variation and whatever you want to do with this cycle, what it takes to get net infiltration, has to be exactly 172 millimeters per year. So that in our wet years in '84 and '85 we got some infiltration into the system, and then in our dry years we took it all back out again. So, we are averaging out. What are the chances of that occurring, that the amount of rain you need is exactly 172 for zero recharge? Pretty slim. So, we know that there is something different from there.

Let's assume for a moment that at 100 millimeters a year or less, you lose water from the system, which we know the plants are dying, we are drying out the system. So during--if we had on annual average precipitation or what we need to get recharged is let's say 100 millimeters or less, during this time we are losing water from our system. During this time we are adding water to our system. So, we are going to get a net infiltration.

If on the other hand we need 200 millimeters a year, just an example, we know that when we get this much rain, we do get infiltration. Here we lose water. Here we know we gain water to the system. If this is the case, and this is the average, then during this wet climatic condition, we get net infiltration. During this climatic condition when we have rain less than that, we are losing water. Do these balance out? Is this a net loss from the system?

Well it is easy to put in a lot of water; it's hard to take it back out again. So, if you were to do an infiltration slide, you might show big infiltration and then you might show a small water loss. But, if these kind of events are on the order of 20,000 years you put a lot of water into the system, you have maybe 10,000 years or 20,000 years to get it back out again. These Milankovich cycles may be important for this. But, we want to try to try to look at this input to see whether or not this variable input, if you were to have one could get through the bedded unit. What would the frequency have to be to get through the bedded unit? What would the amplitude have to be to get through the bedded unit to test that?

This is a simple model. We can make this anything we want. We can do 1-D flow models and test it fairly easily and I want to do that. But the idea is is that we do know that under some conditions we get recharge and under some conditions we don't. So, right now where we currently are, some days it is going to get some through the system and some it is not. I want to try to characterize that a little more, but I want to start looking more at this cyclic nature of recharge.

In summary, then we need a current understanding of the processes, we need to know what they are; fracture flow, matrix flow, thin or thick alluvium, things like that. We need to define these upper boundary conditions, develop our conceptual model, develop our sampling measurement scheme, collect and analyze the data and then we have to iterate, go back and retest and answer some of these questions that have been asked and find out where we are missing water potential is really important. And we want to design some models for current and future climatic conditions. We think those are real important. I think we have got to get away from a constant input until we can show that below the Paintbrush unit, no matter what we do on the surface, we get a constant unit.

Again, I believe that the conditions that exist at Yucca Mountain today, are controlled by past climatic conditions. Water movement through the Topopah Spring unit today may have been water that was input to the surface 20,000 years ago, or 10,000 years ago. I'd like to know that information. And I would like to know if this drying effect that we've seen for the last couple of thousand years can recover if it is a certain condition today and we start this new pluvial condition tomorrow, it may take 3,000 years or 2,000 years to get through the system.

So even increased precipitation, doubling the rainfall rate right now, may not get to the waste packages while they are real hot. So, we need to tie the whole system together, cyclic input as we expect to see it. Plus waste package design, the heat in the canisters and how that system is going to move along. Anyway that is it.

DR. DOMENICO: I think that seeing as you are going to be our next speaker again, we'll hold off any questions we have at this time and we'll have a ten minute break instead of a fifteen and then you can finish up on matrix properties.

(Whereupon, a recess was had off the record.)

DR. FLINT: Again, I'm going to try to show from the matrix property program where we don't have access to boreholes right now, how we are going to get some information out to modeling groups, and how that information can be used for determining the methodology and models' sensitivity to what we measure. So I'll spend a little bit time on that, so look for that in the talk where we try to pull information out again for modeling.

The purpose of the matrix hydrologic property program is to collect the necessary information to determine the character of the hydrologic properties, both the physical and the state variables with enough resolution for adequate use in hydrologic models.

The objectives, one, is to characterize these flux-related properties in the major unsaturated zone units; and two, to estimate what these properties would be for larger volumes of rock. How do we take core samples and put them into large volume of rock using the statistics and geo-statistics.

We use the Richards' equation to help us out in determining what properties we want to measure. Water content as a function would change with time, as a function of mainly the conductivity, the major potential with depth and we also have gravity with depth. You can simplify that down into a conductivity with water content, a water characteristic curve. We have a unigradient for the gravitational potential and we have our change in water potential. It is easier to measure the water content values.

One thing that is important, I think, is to look at conductivity as a function of water content. If you look at hysteresis curves, you'll find that there is--there may be if you use the conductivity as a function of water potential, you are not accounting correctly for hysteresis. Now, the water content makes the conductivity function not as sensitive to the hysteresis problem. We actually have the data that supports it. So I prefer the conductivity and I'll show that a little bit later. We use these Richards' base equation properties and this is what we are going to try to measure and there are a lot of supporting data that go along with this.

What we measure, water content, water potential, permeability both saturated and unsaturated to gas and to liquid. We also use models or equations to fit the data. We can't put the data in these large models. We have to use some kind of functional relationship, Brooks and Corey or Van

Genuchten or something like that.

Moisture characteristic curves, we also measure. We have to account for hysteresis particularly in the bedded units again with models or some kind of equation to describe that characteristic curve. Related properties we measure and I'll show where these are important, bulk density, particle density, porosity and then the capacity and thermal conductivity we will also measure. These are the main properties. There are a lot of different techniques we can use to measure these and we have to evaluate each technique and each equation that we use to describe that relationship.

The outline that we are going to go through, our sampling program, our testing program, the analysis and then a summary. I am going to talk a little bit about surface outcrop samplings. We have access to the surface of the site. We have the ability to go out and collect hand samples, and I have some here that I'll talk about in a little bit.

The outcrop sampling, we can look at deterministic processes, although Claudia said that the peer review liked the work we did in geostatistics or probabilistic or stochastic models. I am sort of leaning more toward deterministic models now. If possible, that will help us in our stochastic modeling. Stochastic models, I am not sure where the term came from, but I am not real comfortable with it. It is just a probability estimation technique. We can get preliminary characterization and special relationships from outcrop sampling. And we can help to determine the number and location of samples for testing within each unit using these outcrops.

This is Yucca Mountain and from Solitario Canyon. I am going to show you some data we had on some transects. The transects were collected from UZ-6--well at any rate it is up in this location, going from the top down to the Solitario Canyon fault, right in here. We can get access to all the units. We have the Tiva, the bedded units, the PTN, and also we have a lot of the Topopah flows at this location. In detail we are looking at the columnar unit of the Tiva. We have a basal litrophere in this location. Then we get to the bedded units and the caprock and the rounded unit of the Topopah Spring.

We can look in detail at units. We can do laterally invertical measurements. We can look at fairly thin units. One borehole, you are going to have a fairly small sample. For hydrologic properties, we can go across contacts fairly easily and get more measurements from surface outcrop samplings and we can do it right now. This is the top of the Topopah.

This is an interesting unit that we found, and I'll show you some data on it later. This unit is a very thin unit on Yucca Mountain. It seems to be fairly continuous; very low porosity. I'll show you the data on that. But, you are not going to get many samples from this in coring. Once a borehole goes through it it is only about a foot thick, but may be very important. That's a question we are going to ask the modelers later.

We can also get far to the north, we can get to the Calico Hills and take samples from the Calico Hills and do outcrop studies on those to look again at the same properties I was talking about earlier. This is to the north of Yucca Mountain.

We also have borehole cores we are going to collect. The borehole samples from--we have a feature based drilling program to locate boreholes. We are looking at a lot of cases at faults or faulted areas. We have a systematic drilling program that looks at areal coverage. We also have additional drilling for phase 2 if we need to add additional holes to look at properties or look at features. And, we also have our sample selection program we have to go through.

This is the area of the repository. Although these holes have moved, we have a lot of the UZ holes, 7 and 8 across faults, plus the Sandia holes or the systematic drilling hose for areal coverage. And these were based on some geostatistical analysis that Chris Rautman had done on the outcrop sampling of the Calico Hills unit.

For the matrix property program, what we've proposed is that we get eight inches of core out of every 3.3 feet. That is roughly 20 percent of the core. Not that we get it that it is preserved in its state condition. The way that we proposed this is that one sample will be in a LEXAN liner capped and one sample will be in a hermetically sealed can. We'll process these differently. These we'll process right away; these we'll process a lot slower in time, but we want these to be preserved, because we feel that a lot of the measurements we have to make, we need to know what the initial water contents were, and we may want to do some measurements right at the initial water contents. We have some ideas on this, how we can tell whether the unit is wetting up or drying down.

This is a picture of the LEXAN liner. I brought that one, this is a little bigger than the sample we expect to get, but we will do some measurements. The clear LEXAN lets us see through. We can see fractures, we can see if we are going to make measurements in pumice, because we'll make some measurements right through here. We also have these hermetically sealed cans, it is just a small sample, we will break it off immediately at the drill rake, put it in the can and seal it up.

DR. LANGMUIR: Alan, which would you use for sampling water?

DR. FLINT: Okay, I'll get to the testing program. I'm sorry, I thought maybe the next slide might be it. But, I will tell you, these will measure--well, I'll go through this in a little bit when I get to the testing and how these are broken up. Well, I'll tell you now.

These are for water potential, water content, porosity, bulk density and particle density. We'll also do some imbibition measurements on these. These preserved samples will be for the more long-term measurements such as the unsaturated hydraulic conductivities, the water characteristic curves and also for wetter samples. If we have tensiometry, we have to use a heat dissipation probes. We want to do it on these; we want to see where we are going to put our instrument, drill it through the plastic and take a measurement.

These samples, because we have so many things to measure on them, we are not going to be able to measure water potentials that are fairly wet with tensiometers. We are going to just do psychrometer samples on these. But, I do believe I show this in another slide.

Testing of the surface outcrop samples. We can collect hand samples. We have a small core saw that we got from surplus on the test site, a small trim saw, some other equipment we got surplus on the test site and we set up a laboratory in the back of our building where we could take hand samples that we pick up on Yucca Mountain, this is the top of the Topopah, take small core plugs out of it, we have some down here. We can also go out with a small chain saw motor that has a small drill on it and collect a few samples at Yucca Mountain itself.

These are some of the kind of core that we get. I brought some up here. These are all of the units from the top of Yucca Mountain, down to the repository level. At any rate, we don't have the rest of it below that at this point. We'll get that later on, and I'll show you some--there are some interesting things in here you might want to see.

What we are going to measure on the outcrop samples, bulk density, effective porosity, effective particle density and sorptivity. What I mean by effective porosity and particle density is we use oven dry weights, but those oven dry weights are from a relative humidity over, 60 degrees C, 40 percent relative humidity. That is important. That is not the water content you want if you are calibrating a geophysical tool like a neutron probe. But those are the water contents you may want, and it may be that these are not real particle densities because of entrapped gas bubbles, but these are effective. And I'll show you where this becomes important a little bit later.

I'll talk about the testing of the core samples that we get from these sealed containers. The hermetically sealed cans, gravimetric water contents, water potential that are drier than one and a half bars, bulk density, particle density, porosity and characteristic curves on those samples immediately. Again, the particle density and the bulk density, porosity measurements and water contents will be measured using a relative humidity oven and the harder oven drying, for calibration in neutron logs. We will use both oven drying techniques on all the samples, but I wanted to point out that one change.

We think we can get some characteristic curves on these samples. By simply taking a sample and taking a measurement and letting it dry out a little bit, take another measurement and look at least at the desorption phase using evaporation or microwave. Probably evaporation would be the easiest.

On the LEXAN liner, those samples, we want water potentials that are fairly wet, that are wetter than a bar and a half. We use tensiometry, heat dissipation probes. We want to make sure that we don't drill into a pumice fragment or something like that. That is why we use the clear LEXAN, and we don't want to open it up to take the measurement because it may take some time.

We are working on under coring samples now, and using pressure plate, SPOC cells, which I'll show you a little bit later, get hysteresis information for our characteristic curves, centrifuge. Mercury porosimetry we don't really like very much, but we are doing some tests on it now. Pore size distribution by gas injection. This is a technique that I've seen from Micromeritics and we are going to go look at their device. And I think it may be really interesting, even if you don't get a real pore size distribution, you may get some property of the rock which is correlated to the other properties that you want to know. And if it is correlated you can use that information; stick a sample in get the answer out in 24 hours; use the correlation in geostatistical analysis to better estimate where you don't have measurements until you can get them.

Hydraulic conductivity, we are just using a permeameter. Unsaturated centrifuge technique, a steady state or non-steady state; multi-step outflow which we get from the SPOC cell. The gas drive technique, Hassler or simple imbibition. Again, we want to try to use the samples, maybe starting them out at their initial conditions as they were in the field and taking some measurements on that that may be really useful to us.

Simple lab measurements we get from just a wet weight. We use the standard ASTM procedure for measuring water content at saturation and just disperse it or weigh it in water to get the volumetric water content and dry it in relative humidity ovens. And these are part of a transect, a lateral transect we did from the base of the Tiva unit.

We have some concerns over sample handling that I just want to address for a minute. Trying to preserve the in situ water contents, I know that there are some problems in making measurements. There are some questions in my mind over historical data that we have on water contents, and I'll talk a little bit about sample drying and outcrop versus borehole.

This is some data that we got from D Tunnel when we were doing some work. This is depth in a borehole up to ten meters, volumetric water content. The red dots are core samples. We took them out of the LEXAN liners. What we did is we took a LEXAN water, capped it in the borehole, brought it outside, opened it up, broke off a piece, crushed it with a hammer, put it in jars, took it to the lab, took this sample back to the lab and did a volumetric water content measurement. Took the jar sample, took a sample out and measured water potential and measured water potential and put the rest of it in a moisture can and weighed it.

And the difference you get is water content, or psychrometer samples versus our core samples. We have a lot of drying occurring. Quite a bit of change, and those water potentials may be very significant. We did a very good job, we thought of preserving the moisture. Not as good as we wanted to do. That is why we have gone to hermetically sealed cans now in the field trailers rather than pre-processing the samples. We bring them back to the lab; we open these up; we take tremendous amount of care now, we are developing the procedures to try to keep the water potential samples at the same water content. But we are going to make these same two measurements again and hopefully we'll get a lot closer. If we do some prototype drilling at Yucca Mountain we will be able to take some measurements. I think the next prototype probe that was scheduled is not going to take any cores so we won't be able to do this analysis. But, after that we should be able to make sure that we are getting fairly good numbers.

DR. LANGMUIR: Alan, before you move on, by simply weighing the sample in the field immediately on taking it, do you presumably correlate that with the weight loss from evaporation. You can do that fast and you can measure the moisture probably.

DR. FLINT: You can weigh it in the field. We are not processing the samples ourselves. DOE has a contractor SAIC that is going to process the samples for us, and we would prefer to have them rather than to take anytime at all to weigh them to simply get them in these moisture cans, because one, the drillsite is not a really good place to keep analytical balances. And the time it takes for them to do the measurements, a lot of errors can creep into it. So we are real concerned about that. We want to try to preserve them as quickly as possible and we think we can do this. And besides, if we don't weigh it at the drillsite, then there aren't any errors. You see, because when we get it we get the original first weight. We learned that. Never measure the same thing twice.

Relative humidity drying; a very important process, we think in sampling. There are some consequences of doing this and I'll show you some data from relative humidity oven drying. We have several units, you can look at these later, but I want to point out one in particular, Tunnel Bed #5, nonwelded zeolitized, 13 micro darcies permeability in a relative humidity oven drying. I'm sorry, 0.13, 0.15 micro darcies. About the same permeability; 14 percent porosity in the relative humidity over; 37 percent porosity if you measure it in a hard oven drying, a vacuum oven. The water that you take out, the difference between this porosity and this porosity is water that is in the minerals, it is in the zeolites, it is in the clays, it is not part of the real porosity, it's part of the flow in this particular case. This porosity may be correlated to the flow characteristics much more than this one would. In some cases, we don't see the porosity change very much, but we do see a change in permeability. These samples were--I think they were the same sample that we used in these two analyses. What happened was was he increased the permeability, maybe we dried out some of the clays, got them out of the necks of the pores.

In some cases we see a change, the nonwelded unit from 11 millidarcies to 3.4. Increase in porosity; decrease in permeability. The decrease in permeability most likely due to the clays breaking up in the sample and falling down into the pores and plugging some of them up with the permeabilities. These are air permeabilities, not water permeabilities.

So we do see some changes. Now if you use a relative humidity oven, your initial measurements, I think you get a better understanding of the flow system. Around the repository itself, around the waste canisters, you are going to get drying out at temperatures approaching this. So we take the measurements here, but when the water comes back in three or four or five hundred years later, you have to know what these properties are going to be like around the canisters, I think. So we want to take this into account when we do the measurements, the fact that for the welded units, anyway, it may make a difference. So we want to know both of those things.

DR. DOMENICO: Back to that--can you go into that again?

DR. FLINT: Sure.

DR. DOMENICO: Your zeolitized units, you say you attribute that 37 percent to water in the minerals?

DR. FLINT: Right.

DR. DOMENICO: But the Calico Hills is zeolite as well and you don't have a significant change in the porosity.

DR. FLINT: Well, one thing is is that we don't know quantitatively how much zeolitized--how many zeolites are there; how much clay is there. The total quantity, although we see in the zeolitized we see some effect of this one, 22 to 27, there may be a lot less zeolites and a lot less clays than there are in the Tunnel Bed 5. So maybe just a total quantity in which we don't know that yet. We are doing some thin section work now, and we are doing some x-ray work on these to try to get an idea of how many zeolites we see and how many clays we see. And really, we are looking in detail at the influence of changing the porosity. We are pretty comfortable, and also if you measure these porosities with gas versus water, you can get water back into the clays and don't change the total porosity, but you can't get the gas molecules into the clays. So we get different measurements if we use a gas porosity measurement versus water. But I think it is just the different amounts.

DR. WILLIAMS: Are there any other explanations?

DR. FLINT: I'm sure. I am sure there are.

DR. WILLIAMS: What about multiple working hypotheses? What are the other possibilities?

DR. FLINT: The other possibilities, I don't--I don't really--I guess I don't have any other at this point. We have done a lot more measurements with gas and liquid and have a pretty good feel for the gas movement into the system. I am not really sure. I think this is--well this is a start anyway. So, if you have any, let me know.

DR. LANGMUIR: That 23 percent that you show there, the zeolite tuff as being related to minerals seems a bit much.

DR. FLINT: Pardon?

DR. LANGMUIR: The 14 to 37 percent. It sounds more like it is going to be capillary or sorbed. That's an awful lot of water.

DR. FLINT: Well sorbed water onto the clays, onto the surfaces of the clays, in the clays, the surfaces--we are pretty sure there is a lot more clay in this area. But it is attributed--the difference in here, we don't think is water that contributes to the flow porosity. So it is water that is either sorbed or--I don't think it is capillary water. These potentials that you are measuring are--I don't know seven or eight hundred bars or 80 mega pascals. So you are dealing with maybe four or five molecular layers of water at those ranges. But we are doing a lot of the x-ray work now trying to get an idea in looking at what happens to the clay, and how much clay is in there, but we are not very far along on that.

Do outcrop samples represent borehole samples? I don't know the answer to that yet. I think that they can for a lot of properties, and I'll show you the data later on which makes me believe that, but we want to try to make some use of it. What if they don't? Then I can go over to the infiltration program and say, well, I need that information at the surface anyway, so I can still do it. So, it is fortunate that we have to do those measurements in both cases.

Method selection. I want to talk about how we are going to select the method we are going to use. How are we going to measure water characteristic curves? Which equation are we going to choose? We have to consider whether the method is repeatable; how accurate is it? Can we use multiple measurements to get several things out of the same measurement? That is what we really want to do because those are a lot faster. We have to consider how fast it is and the cost versus the error. You may be within ten percent of the answer for one cost, you may have to double the cost and still be with only five percent of the answer. We feel that that ten percent may be okay. It depends on what some of the modeling results show.

Is it conceptually adequate? Are what we are measuring conceptually adequate? This question of porosity; which way are we measuring it? Which is the best to use?

Desorption curves, characteristic curves or sorption curves; which will we want to use?

I've listed some of the techniques and some of the performance criteria, whether they are indirect, whether they ar fast, slow, what the ranges are. We have tried to consider all of these things. This is just for your information, I am not going to go through them all. But, we have for instance pressure plate. We think we can do a fairly good job in hysteresis, so we like that technique. But the rest of this information you can read at your leisure.

The approach that we are going to use is whether we are dealing with the wet or the dry region. We have to use different instruments; whether we are looking at hysteresis or ball park numbers. There are one or more methods and then we are going to verify the accuracy with modeling. This is I think an important step that we've made to help us to understand which properties, which methods are right. I'll show some results of that.

Matrix potential versus relative saturation. This is the SPOC desorption curve and then the sorption end of the curve. Pretty good idea of hysteresis. Centrifuge in this case followed along fairly well to the desorption end, and another pressure plate technique elevated a little bit higher than the SPOC. Which of these three techniques is correct?

That is something we are going to evaluate. I'll tell you in a minute or so.

All right, one technique that we are looking into is using composite curves. This is the SPOC desorption curve and we talked about the SPOC, so that is that submersible pressurized outflow cell that we use, so we can have access to the core, water flowing in or out.

The centrifuge which we are not comfortable with at the wet end, but may be fairly good at the dry end so we can look at adding those extra data points on to get a better feel for what is happening at the dry end. And we believe we are past the point where hysteresis is a problem, so we can do that.

One core, all the different measurements to be made: desorption, sorption, centrifuge, pressure plate, gas drive or centrifuge permeabilities. Which of these is correct? We have to figure that out and that is how we use this inverse modeling and some simple one dimensional modeling to give us a better estimate of what is going on and I'll show the results of that.

We want to fit curves to those equations that I just showed you, moisture characteristic curves. Brooks and Corey with air entry and water potential, this is the water content we are using as a relative water content, or the Van Genuchten. These are both empirical equations. They are sort of loosely based on theory, but the empirical equation meant to fit the data sets. So there is nothing magical about either of these two equations in my mind.

You can use the water characteristic curve to predict the relative permeability equations using these simple relationships. If you have measured them, then you can fit them simultaneously, or you can predict one if you have the other.

Well we start off with a data set, centrifuge and SPOC sorption data and we fit a cure to it; Van Genuchten, Brooks and Corey, just fit to the data. They fit fairly well. Van Genuchten has this nice little tail on it trying to get down to this point which we like. Brooks and Corey doesn't in this particular case. So we use these data points and these three to fit this particular example.

Now we predict the relative permeability curve from those equations. Relative permeability versus water content. Brooks and Corey goes through the gas drive data but does not match the centrifuge data. We don't know which is right yet. The Van Genuchten, that little tail dropped us down, carries us out at a lower level than what the gas drive data says. So which of those is correct?

We use a simple imbibition measurement. The balance a Marriotte system and a rock core, one dimensional flow up the rock core; we collect the data on the computer.

We do it in a glove box to minimize evaporation and you can see water moving up, this is a core and the balance, several chambers, we can do more at one time.

Time versus the amount of water imbibed in the core; one dimension. This process we think is governed by the Richards' equation and we can use Richards' equation to try to predict this data. Different initial saturations, 20 percent, 56 percent, what ever we want to put in there--or, 19 and 56, and take the measurements.

DR. JONES: Alan, excuse me, this is Tim. Could we go back to a couple or three slides where you were taking the water retention and predicting the conductivity?

DR. FLINT: Sure.

This is the characteristic curve. We use the standard Van Genuchten--

DR. JONES: Yeah, but what happens when you fit your water retention curve to the centrifuge data and compare it to the centrifuge hydraulic conductivity data?

DR. FLINT: There is a NUREG publication that is coming out that was done in Arizona, that describes in detail all the different combinations of these and how it tests out in predicting the imbibition. It doesn't particularly work. What we found, I'll give you the bottom line, is that you need desorption data from the water characteristic curve, and the gas drive relative permeability data seem to be the best.

But there is a paper, Dan Evans is the editor, we have director's approval on those papers, I don't know what that means in terms of releasing that information right now, but that is going to him and is going to come out in the next month or two, I hope. But it describes in detail this process of model verification.

DR. JONES: But if you use the square symbols there to get a water retention curve and then predict conductivity, it does not explain the difference in the next slide between the gas drive and the centrifuge conductivity?

DR. FLINT: No. The centrifuge characteristic curve data and the centrifuge gas drive don't seem to be correct, either one, and they don't match each other. If you were to fit the data to here, you could not fit this. It wouldn't predict that. And it may be that part of the centrifuge data is correct, the dry end; and, the part that is not correct, the wet end. This stop at 40 percent saturation and this gets down--we have just a couple of data points below that. So this data is where we have our range for the permeability and it doesn't match it in that particular case.

DR. JONES: Have you tried comparing on the next slide, if you fit the M-parameter and the Van Genuchten as an independent parameter, doesn't that bring your Brooks and Corey and Van Genuchten conductivities together?

DR. FLINT: We can bring them together by simultaneously fitting the gas drive data if we choose to do that. Unfortunately, the Van Genuchten equation when you use something like V-fit, that one that came out of Blacksburg, causes this last data point you measured to have very good control, or a lot of control over the curve. The Van Genuchten drops off quickly. You have to be very careful because what the fitting does, simultaneously, it puts a lot of weighting on these last numbers when you do the simultaneous fit and drops this curve off very quickly and that is due to the residual water content.

DR. JONES: I was just suggesting instead of the assumption that M = 1 - 1/M, if you fit that M as an independent parameter.

DR. FLINT: You can fit that as an independent parameter and we did that do. We did M as an independent parameter but now that is just a fitting technique and that takes away--I mean you can do anything. What we found was that a simple equation that we came up with, the guy that used to work for me, Ken Richards, we call it the Richards' equation, fits these really well, and we can do that. That is just another independent equation. But do you want to fit this data?

DR. JONES: No. I am not suggesting you fit that data. I am simply saying that if you do not constrain the Van Genuchten on your water retention curve with that relationship M = 1 - 1/M.

DR. FLINT: Right.

DR. JONES: Then you get a predicted conductivity curve that does not have that big dip at the front end.

DR. FLINT: No, you can make this--you can change those relationships and make this whatever you want it to be pretty much. You can change that M. You can let it be a fit parameter. You can get rid of this. We've had some fairly straight numbers down through here. We've tried using M independent and did measurements on that and did some predictions on that. We put a whole series together, which a lot of that is in that paper.

Okay, we got back to this point, the imbibition data. Now this is the measured imbibition data. This is the centrifuge characteristic curve and the gas drive relative permeability curve and the gas drive saturated conductivity value. And this is the result we get. We can try the pressure plate, characteristic curve in the gas drive conductivity. You get this blue curve. The SPOC desorption and gas drive, and the SPOC sorption and gas drive. This SPOC sorption seem to fit the best. This is using Brooks and Corey. One of the things that we see is that we continue on taking water up in the model, but the core stops.

The Brooks and Corey equation, the Van Genuchten equation, at this point don't account for air entrapment in the system. They don't account for the hysteresis effects, we have to account for that. There is part of the TOUGH code that now can count for hysteresis. What is happening is that this core reaches about 85 percent saturation, stops taking on water. The model keeps going until it reaches full saturation. We have to stop the model from taking on water. This is an important point if you are dealing with some initial saturation. You say it is 70 percent saturation in a fracture media and water starts moving down the fracture, by the time the rock gets to 85 percent saturation, it is done taking up water. So your fracture flow may be more significant because of this influence of hysteresis in the bedded units. And at 85 percent it is done taking up water which is what we see in this case. And, the model says it is going to keep taking it up. So fracture flow may be more important than modeling, but you have to count for hysteresis. This is Brooks and Corey.

Van Genuchten does the same thing. When you hit this saturated water content--although the sorption curve seem to work the best, it underestimated per point. Even though we seem to be at the same location at the end of our experiment this model continues on up to 40 percent.

This is a technique. We can try all the different combinations like Dr. Jones was talking about about fitting M as an independent parameter, looking at characteristic curves, simultaneously fitting the two, fitting only the conductivity function, fitting only the characteristic function or whatever combinations we want to use to look at this. And we think it is a useful technique.

A simple one dimensional measurement; we can repeat it; it is real easy to do. We can test Van Genuchten, Brooks and Corey, the Muallem or the Burdine assumptions to Van Genuchten or any of the other formulations we want to use.

Then we do this at different water contents and we try to fit it at different water contents. One formulation, one characteristic curve fits at one water content. Like Van Genuchten, I can get Van Genuchten to fit great at 50 percent saturation, or, at ten percent saturation, initial saturation imbibition. But the function and the formulation for those two are different. We want to find one that fits both of them best, and right now Van Genuchten doesn't work as well at the dry end, but we are working on that.

Some simplifying relationships that we can use to help us out a little bit, inverse modeling and I'll talk about that in a second, its sorptivity is it a function of water content and porosity where you use this formulation, sorptivity, infiltration over time to the one-half. You do need mechanistic models, even if you do simplifications. You need to know how the system is set up, fractures, fracture networks and things like that. Is conductivity a function of sorptivity? And is the water characteristic curve a function of some forced structure. Can you get thin section analysis? Can we use some simplifying relationships to help us out?

Again, trying to get as much information to the modeling groups that as we can with what data we have, and if we can make some simplifying assumptions, we can at least get started and get them better information than they have today, I think.

Brooks and Corey, analytic solution that Zimmerman and Bodvarsson did, they can predict sorptivity if they know the conductivity. This is porosity, viscosity, Brooks and Corey parameter function alpha, and N plus the water content. They can calculate sorptivity. We use this in an inverse solution. This is the inverse solution, the log of sorptivity. This is the saturated hydraulic conductivity in blocks. This is not a linear scale. I'll explain what is happening in our inverse solution.

We start out at some conductivity this times 10^{-13}, N meter squared. And we run--we set an N parameter and we set an alpha. Those are the only three variables. We know porosity; we know sorptivity; we know viscosity. So we run through all the alphas, so we are here at one conductivity, one end, we run through all the alphas. We change N and run through all the alphas; change N, run through all the alphas. We get done, and all the alphas are the huge range that we expect to see, bigger than we expect to see, more Ns than we expect to see, maybe.

Then we change the conductivity. We do it again. We change it and we change it and we change it. Now, with the large estimation of N and alpha that we have, we believe that at this point we are right now down at 10^{-13}. The conductivity is not faster than that, if this inverse solution is correct.

We go to the other side, we never hit this sorptivity, this is the real sorptivity line. We know that the conductivity is not different than 10^{-15}. It is not slower than that. If you were to pick in the middle, you could say 10^{-14} is within one order of magnitude of the right conductivity. We measure porosity and sorptivity. And we have an estimate within an order of magnitude of the right conductivity; one technique.

We could start looking at alphas and Ns and seeing which are the most realistic alphas and Ns. We can add any data we have on characteristic curves and I have a way to get two measurements to get a whole characteristic curve now, but I don't have time to explain that, but it might work out, to get the right answer.

This triangle is the analytical solution using sorptivity and the measured core parameters. The measured core parameters, our best estimate based on our modeling, the modeling that I just showed a minute ago. So you plug in our best estimate of the core properties, put it in the analytical solution and you still don't get the right sorptivity, but this is where you are. This means that the conductivity was somewhere around 10^{-14} and we have N and alpha parameters that fit right in this location.

This maybe very usual, because we get sorptivity measurements in four hours and we may be able to get estimates of conductivity which may be real useful fairly quickly, but this is just the first approximation, the first time we've run through this model. This is one sorptivity. If you do another sorptivity measurement at a different initial water content, this all changes. And the overlying curve doesn't quite overlie this. So you might start to get a unique solution to where the next set of curves where these red lines are, might come through a different way. And pretty soon you can eliminate this side of the screen or you can eliminate part of that side of the screen and do it at a different water content. This is the technique we are working on.

Sorptivity; total porosity. If you know the total porosity, this is from G-Tunnel, welded and nonwelded core, you know the porosity, you know the initial saturations, this is the relationship in predicting sorbtivity. So there seems to be a correlation. A lot of people don't like using porosity as a surrogate, and I think that is because they don't use relative humidity oven drying, they use oven drying that gets water out of the minerals. And I'll show you how the correlation improves later on and whether or not we can come up with an alternative hypothesis I am still not sure of yet.

Twenty percent saturation, initial for your sorption. If you know the porosity and you set it at 20 percent saturated, you can do a pretty good job of predicting what the sorptivity will be, we use that information for some other purposes.

This is a thin section, pore structure. This is a welded unit, fairly large pores but segregated. This is using fluorescent dye forced into the pore under pressure and then done at thin section so we can see where the pore structure is. This is a welded unit. This is nonwelded; much more massive pores. We have the small pore sets that we see in the welded unit and it is fairly consistent. In the nonwelded we see just large pore structure. Can we use this information as a surrogate? We are not sure. We are still doing work on it, but this is very fast, very easy to do to take these measurements, and with a small computer and some analytical equipment, you can do a computer analysis of this and make some calculations. If it is correlated, we can use it in our geostatistical analysis, fast simple measurement.

This is an interesting photo. Here is a pore that can contain water. These are small fractures that are connecting the water with the pore. This you'll measure probably in your relative humidity oven and in your vacuum oven, but it probably does not contribute at all to the flow, but it is storage capacity. In a steady state model, this isn't important. But in a non-steady state model, this may be important if you have a lot of these kind of systems built up. But this may be one of the reasons why porosity doesn't correlate all the time with the flow properties. But, we are looking at these kind of phenomena.

I'll talk a little bit now about statistics; some basic information. Then I am going to talk about the preliminary data we have on rock outcrops. There is some interesting stuff there.

Classical statisticals, you can make the calculations, mean, variance, distributions, regressions; you can do all that on all the properties you can collect either from boreholes or from rock outcrops.

Our geostatistical analysis: 3-dimensional, multivariate, we do structural analysis, we can do predictions kriging and cokriging. Simulations are very important though. A lot of the criticism you see about geostatistics is that it seems to be not a realistic representation of what could be out there. You lose what some consider the structure of the system. You make that back up in a simulation. One realization, the kriging estimate or cokriging estimate like those rainfall, that is not what you would see in an average year on that rainfall map. That, is what the average is. If you were going to take any point or all of the points combined and estimate the properties for the water content or the rainfall, the kriging estimate is your best estimate for all of those points. But the simulation gives you a better realization of what it might look like at one point in time. It is more realistic, so you have to go to the step of simulations. I think we've looked seriously at the complaints about kriging and cokriging and I think simulation is the next step which we do and will use.

This is just some data that shows classical statistic. In this case we are just using a mean, a standard deviation and a coefficient of variation from boreholes. Not a lot of samples. Sample of one, we don't get enough data for that. But this is Topopah Springs nonwelded unit, fairly high mean in terms of permeability, but some data. So we can make calculations, put together tables and this is the kind of information you can use for preliminary modeling. It's our best guess at this point for the nonwelded units.

I want to talk about geostatistics for just a second. We have categories of data so that we can get more information from what is available. We have exact data, inequality data, that means we know what the minimum might be, the minimum porosity zero, maximum is 100. We can do that. Or we have interval data where we know and can make an estimate of the minimum and the maximum, so we have these measurements. The "i" is missing here. You know the "i" is after the "t", and before the "m", you don't know where it goes exactly, but you can--but you do know it does go in here somewhere. That is interval data.

Hard data, we have; measurement, a number. Soft data we guess. We guess the expected value. We guess the minimum or we guess the maximum. Geologic inference becomes important. So these are the categories of data we are dealing with. What do we do with those in terms of making estimates? We have our kriging methods that we can use.

This data you can look at whenever you want. I'll just show a few points to give an idea of what is going on. Simple kriging, exact data, yes, you can use exact data. It is required, and you cannot use any of these other data. Some ordinary kriging, yes, you can use exact data. All of these you can use exact data. Dual kriging, yes, it's optional. You can use hard data and inequality data. You can use interval data. But, you cannot use soft data. If you use soft kriging or bayesian kriging, you can use soft data but it has to be exact. So you can pick what data you have.

What if all you have is soft, exact data? You can sort of guess at the answer. You can come down here and do bayesian kriging. That's required. I know that these all say optional, but you have to have at least one. Or, you can use soft kriging with kriging techniques. So, we have a way to try to put all of this together. But what we try to do is to incorporate soft data; geologic inference.

Now I want to talk about the preliminary data on the outcrop samples and some of the things that we've learned that we think will be useful for modeling. This is a vertical transect that we ran down from UZ-6S. This is Mike Chornack's work and Chris, Robin and mine and Maureen McGraw from PNL and Chris from Sandia. It's a cooperative effort trying to get some information for some performance assessment modeling and for some site modeling from the Solitario Canyon Fault up to the top of Yucca Mountain, particle densities and porosities.

Some interesting things that we see are, one, look at the increase in particle densities you get toward the top of Yucca Mountain. As Mike Chornack explains to me that that is the last of the eruption, it is lower down in the magma chamber, more phenocrysts, more matrix minerals, you expect a higher particle density. That's a deterministic process. This relationship which we measure from outcrop samples every five or ten feet down the mountain, may be uniform, may be consistent all over the mountain, that fact that it is a deterministic process and we expect to see this in other locations. This gives us a tremendous amount of information. Now, on our sampling scheme, we can go through the clinkstone unit, lower lithophysal, quite a bit of variation in the clinkstone. We are not sure why, lithophysal fairly uniform. As we get to the base of the columnar we are starting to get more glassy materials. This is vitrophyre. The particle density of the glasses may be around 2 to 2.1.

DR. LANGMUIR: Now, Alan, might that upper density relate to some of the caliche filling of pores with carbonates and secondary minerals?

DR. FLINT: No, these are not. I don't think these are. These are down where we think that they are unweathered and are realistic.

DR. LANGMUIR: The upper cliff would not be like that?

DR. FLINT: We don't think so, from surface filings, no. I think this is real mineralogy from the phenocrysts which you can see in the samples and are consistent with volcanics.

The porosity numbers are higher. The porosity numbers may be due to some case to weathering. But that is different from the particle density measurements and we see these high porosities. This is an important point. The modeling that's been done for the caprock, they put in about 6 percent porosity. I have some samples up here. These are all the units that we have. This is the Tiva caprock; it has about 6 or 7 percent porosity. This is the upper cliff unit; it has about 30 percent porosity. This unit which is mapped by Scott and Bonk, the whole surface of the Yucca Mountain is mapped as caprock. It is not; it's upper cliff.

We have done a vertical transect from the far south end to the far north end and find no lower than about 22 percent of porosity. So, first of all if we are dealing over the repository with the material on top with fractures, it's not 6 percent, it's 30 percent high porosity. I think that is real important to know that information, because if you are going to move water through the fractures, you have to realize you are going to imbibed a lot of water in this high porosity material.

It drops off, goes through the different units, but we get some measurements and it starts to pick up again at the base of the columnar unit. If we look at the bedded tuffs we see the high porosities at the base, this is the shardy base of the Tiva, the lower porosities as we go through the top; this material laid down first and cooling without a lot of pressure on it enough to keep the porosity fairly high. This material is more welded; a lower porosity. We see the porosity go way up, up to 60 percent porosity in the nonwelded unit. This is the top of the Topopah Spring.

Remember from that slide I showed earlier we had that huge conductivity measurement, that huge permeability measurement. We are dealing with high porosities here in the top of the Topopah. But these measurements we also have some at the same location we did vertical and horizontal, we can get about every five feet from outcrop samplings. We think we can provide information. Again, the porosity starts to drop off as we get to the base and we had more material sitting on top of it from that erupted phase.

Finally if we look at the Topopah Spring welded unit, we see similar characteristics of increased particle densities near the top of the unit, at the last of the magma chamber more matrix materials, more phenocrysts in that particular location.

The one thing that we do get is this measurement at the top of the caprock, 2 percent porosity. A unit that is about 12 inches thick all over Yucca Mountain, top of the Topopah Spring, and that is this unit, is 2 percent. Now what is this going to do in a flow model if you have 2 percent porosity sitting right underneath this 30 meter thick nonwelded unit that is supposed to be taking on water from these climatic changes; perching of water, this is the kind of rock that I would expect and it is not as fractured as we thought it would be.

This kind of sample you cannot get in detail from borehole samples, because you are going to pass through it at six inches thick and I am sure Al Yang will grab it before anybody else can. So, we can get it from outcrop samples. But this may be fairly important information and I want to know if we put characteristic curves to this and feed it into the modeling, how this is going to have an impact.

At the top, this is the unit that sits on top of the bedded unit. This gets down to 2 or 3 percent porosity, lower than we have measured before on the unit. I think fairly useful information. I put this altogether so that you can get an idea of the kind of information that we can get. We have porosity, particle density, bulk density, we are going to get saturated permeabilities on these, we are going to be imbibition and characteristic curves all from outcrop sampling. We think this is real useful information.

This is the particle densities, starting fairly low, or starting high and they get lower and then they start to get high again. You can see the trend a lot easier here. Then the bedded units getting more towards the real glassy materials around 2 to 2.1 for particle densities and then back up again. Again we get this cap up at the upper cliff.

It's very important the fact that we have high porosities near the surface. Again it's real low density and it is very thin six inch or 12 inch layer of very low porosities.

We did some variograms. I am not going to spend a lot of time on these, but we can make some estimates of vertical variability to use in our modeling to help us estimate how many samples we are going to have to test. The samples that I'm talking about and the testing we did is fairly simple, fairly easy. It's not the more complicated, unsaturated permeability measurement. Those are going to be harder to do and we don't want to test every sample. We want to know which samples to test and how often we need can test them. So we can look at some experimental variograms to see the range. Bulk density has the highest variability.

You might want to keep in mind the numbers for the Tiva Canyon. We are looking at around 0.01. The bedded unit we are looking at 0.1; quite a bit more variability as you saw from that diagram in there. Quite a bit more variability in the bedded unit. Again, the variograms we can calculate some ranges, get an idea of how often we would have to sample.

This is vertically, the Topopah Spring, we are back down to the 0.01 now, very similar to the Tiva in the range and the kind of modeling we would want to do in using our geostatistical techniques on these experimental variograms. So we might be looking at ranges in the Topopah maybe 40 meters.

This is the transect of the Shardy base, this is horizontal transect. We already know that in the Shardy base we have this variability from the Shardy base to the top. This is porosity. This variability in porosity maybe real, but there is a variability horizontally or maybe the way Mike Chornack hikes up and down the hills and he gets a sample near the top, near the bottom and things like that. We haven't figured that out quite yet. But, we look at this information and look at horizontal variability. We also use the conductivity measurement at those same points and look at that information and apply this to the geostatistical analysis and we find that the range in this case for hydraulic conductivity, nice variograms from there, we are looking at about 100 meters or 200 meters of 300 to 600 feet. The borehole is centered at 3,000 feet, so we are probably beyond the range of the variogram for conductivity from our borehole sampling. So we are going to be using estimates in between borehole samples of the variance of the population, unless we can add rock outcrop sampling to this, which we can get samples in numerous places to try to make this improvement and also bulk density and porosity both have very, very small ranges.

Chris Rautman in his analysis of Calico Hills came up with 3,000 feet or 1,500 feet for a range. We come up with 500 to 600 feet for conductivity. His is air permeability; ours is water. So there may be some difference in that. But we can use this information.

We also did a transect at the top of Yucca Mountain to see if there really was caprock up there and found there was not. We have not done the whole analysis but there is some nice trends in it. The trend in the data on the caprock is not because there is a trend in porosity, but because at one end of the mountain you start low-end in the unit and as you go further to the north, you get higher up and closer to the real caprock.

This is some interesting information that came out. I want to stick this slide in in terms of the relative humidity oven and the 105 degree oven. Saturated conductivity versus porosity. Is porosity important? Can it be used as a surrogate? Unfortunately the red dots apply to the 105 degree oven and the blue squares to the relative humidity oven. The red dots, there is a correlation there. You can use porosity as an estimate of saturated conductivity from this transect. The r^2 was 0.4, using a 105 degree oven. If you use a relative humidity oven instead of the 105 degree oven, you get a correlation of 0.6. Anything over a correlation coefficient of 0.5 or an r^2 of 0.25 is good enough to use to improve your estimate using cokriging.

So this kind of information, either of these would be useful, but this is a fairly good correlation and we improved it considerably by using the relative humidity oven to predict conductivity. And this is over four orders of magnitude. So that may be a fairly good surrogate using this technique. So we think there is a lot to be learned from porosity. In a steady state model, porosity doesn't matter. But in a transient model it does become important and as a surrogate, estimating saturated conductivity becomes important.

Preliminary data on the borehole core samples, there is not a whole lot of information there. We put all of it together to give you some ranges to know some of things that we see. In particular, I want to point out that the Tiva Canyon 1.5E-10-9.7E-10, fairly small range in saturated conductivities.

The bedded unit we have five orders of magnitude change in conductivity. That makes this bedded unit an important number and the conductivity is approaching the same conductivity of the Tiva in some cases. The Topopah Spring, a couple of order of magnitude change. The Calico Hills, fairly large, five orders of magnitude again, and Crater Flats, two orders of magnitude.

So we have our largest variability in the Paintbrush and Calico Hills, which may make the biggest difference in terms of whether we might find perched water or how we are going to do flow modeling and the importance of getting more samples in here. We may not need to take as many samples in the Tiva. And if the deterministic analysis is correct, we may be able to do a better job with fewer samples. I think we can get enough information out to look at modeling and to see how much more information we need. I'm hoping that these surface outcrop samples will provide enough information to put us a long way ahead in modeling than using the five or six data points that we have used up until now. And I am hoping to add conductivity data and characteristic curve data from estimates using my inverse solution modeling or whatever for the modeling effort.

DR. DOMENICO: The Paintbrush tuff and the Calico Hills have been identified as potential barriers, but they are certainly not barriers at that high end of hydraulic conductivity.

DR. FLINT: No. Yeah, with this high end of the conductivity--but there are units that may be continuous that are the low porosity, so you may not be able to break the whole Calico Hills unit up into one unit. You may have to look more at microunits. Microunits are more and more important. So you can't consider just the Paintbrush nonwelded tuff as a unit, you have to consider the smaller microunits. And even though that whole unit may be thinner where it acts as a barrier, it may still be very effective as a barrier.

And I think that Tom Buscheck will talk a little bit more on some of this when he gives some of his talk. So he might be able to answer that question. He is actually raising his hand now.

DR. BUSCHECK: An important clarification to make is that we have to consider the fracture, the bulk permeability due to fracturing. The fact is is that the high matrix permeability units actually act as an attenuater and in fact will give you a better barrier. You'll see this later this afternoon. But, you just can't look at the raw matrix permeability data and consider that to be bulk.

DR. FLINT: No, I think what he was talking about was that these low permeability numbers. He was talking about the low permeabilities. The high permeabilities is right.

DR. BUSCHECK: They facilitate a barrier.

DR. FLINT: The low permeabilities don't facilitate a barrier because they are the same as the welded units above and below.

DR. DOMENICO: Well, I said that the Paintbrush and the Calico Hills have been identified as potential barriers, but they do not appear to be potential barriers at that high end of permeability.

DR. FLINT: The high end being the flow?

DR. DOMENICO: 10^{-4} or 10^{-5} is not low permeability material.

DR. FLINT: That's high permeability, but it can act as a capillary barrier.

DR. BUSCHECK: It is very low relative to the bulk fracture permeabilities. It is essentially quite low, orders of magnitude lower.

DR. FLINT: To the bulk fracture permeability if fracture flow is current, if fracture flow is not current, then those are high permeabilities. But, the high porosity under the current situation we have today if we put a climatic cycle on there, we may find that a long-term climate cycle, a long-term climate change we can take up a lot of that moisture in storage and not approach these saturated conductivity values. And we have to understand what the relative conductivity looks like. It may be several orders of magnitude lower than that. It looks to me from the inverse modeling and the other work and from other results, the conductivity actually may be the most important parameter. It may be very useful. You're right we need to look in more detail at that, but the high porosities, especially when we are looking now at maybe 60 percent porosity, it may be very important in the storage capacity.

I wanted to show the difference between core samples and the outcrop samples to give you an idea if we are measuring weathering or not. Outcrop in the Tiva, porosity 0.02-30. In the core, 0.08 to 0.12. This two percent I think is real. And the 30 percent I think is real in this case.

DR. LANGMUIR: What about your numbers of samples in each case that you are basing that on?

DR. FLINT: There are considerably more I think in the outcrop samples, and that is one of the reasons why I believe the core is a limitation to our ability to model. There are two points to make here I think. One, is that because we bracket the core data that reduces my concern about the outcrops being weathered a tremendous amount that would cause us not to use that data, and because these ranges are so large, with the exception of the Topopah Spring, and I think what happened here is that we added a nonwelded unit in this data set. I think that the core are not enough; we don't have enough core, enough information. I think this is a better set of data to use for the Tiva, the Paintbrush and the Topopah.

Fairly large--in this case, the Paintbrush we did a fairly good job, although we are looking at maybe 59 percent versus 54. But, this is real important this high porosity at the top of the Tiva. Again that may only make up 15 percent of the total unit though covering the repository, but still useful. And I think the outcrop data provides more information because we can get to the samples a lot easier. When we get drilling core we will be able to do a better job. But I still think there are unlimited number of samples; these 2 percent values. We went around Yucca Mountain and collected at about four locations these kind of samples and got that same two or three percent porosity, which we can do and we think is fairly realistic and useful information.

DR. CORDING: Alan, wouldn't your best information ultimately be from the ramps, because you would be able to collect continuous samples. We are back to the underground again, but you wouldn't be subjected to the weathering effect. So is the plan to extend this to the ramps? You are going to be doing the same sorts of tests and collecting continuous data that way.

DR. FLINT: Yes. I think the ramps are a very useful idea and hopefully someday they will go in.

DR. CORDING: I understand.

DR. FLINT: I'd like to get the effort out, the modeling information out as soon as possible, but I agree. And the reason I don't talk about, I remember two years ago we talked about the exploratory shaft facility and how we were going to do sampling. Dave Dobson asked me to rewrite my matrix property study plan to account for the ramp, and I said, which one and where is it going to be. He said, well there are 30 choices, write 30 study plans, we'll pick the right one when you are done. So I opted not to do that or talk about that. But we will have a program looking all the way through the ramps and making those same kind of measurements. We can do the horizontal variability I think a lot better down there. We can get away from the weathering effects. If it turns out that we don't have any or very much, we can use all the data we've collected from outcrop samplings which we have to do for the infiltration program. I think we are well ahead of the game. If the deterministic processes are correct, we will see similar things in the system. And I think that the deterministic processes are real useful.

What is nice is that Chris Rautman has been doing a lot of work in probabilities and using that as a technique to get model data. He is now becoming a believer in the microunits which are one, identifiable, mappable, and from those cross-sections I showed you where we saw a major shift in particle density or porosity, those also occurred right at the contacts, which we can note from borehole. So if we can start to look at the processes and know where we are in the unit, we can make some really good estimates of what is out there.

By knowing contacts and geostatistical techniques is a wonderful tool to look at contact locations. You can make great estimates of contacts from that. I think we are well ahead of the game. I am sort of hoping that we can get 90 percent of what we need to know about matrix properties through three or four boreholes and a lot of outcrop sampling and some models to get started. We need the boreholes to get the matrix water potentials and water contents and to get a better idea of fractures. But you can also do a lot of fracture mapping. Betsy Irvin and Mike Chornack have been doing a lot of work in fracture mapping and some outcrops and a lot of people in geologic division have been doing outcrop samplings. They'd be real useful.

DR. LANGMUIR: Alan, to what extent have the outcrop samples been looked at petrographically to identify weathering effects, secondary mineral effects relative to the core samples?

DR. FLINT: Vaniman at Los Alamos is doing some on the Calico Hills. We have a joint study going on between us, Los Alamos, Sandia and Oregon State University looking at the influence of weathering. Right now we start on the Calico Hills. We have samples from boreholes and from outcrops. They have done petrographic analysis of it; we are doing matrix property analysis of it, conductivities and characteristic curves, and trying to find that relationship. We are addressing that one unit at a time right now. We are working on that particular one, but we are not making real fast progress because of all the other things--the 28 hours a day business. But, we are looking at that. We want to know more about that.

We do know that in the top of the Tiva that you do get some time of a weathering line that seems to reduce the permeability. It is very thin, maybe a couple of millimeters, but it is there, it is not continuous, but we are looking into that to see how much of an effect that has. So we increase the porosity, but then we decrease the surface infiltration processes and we have a lot more fracturing, but then we have high porosities below that.

DR. JONES: Alan, it's Tim again. I'm trying to assimilate all this stuff pretty quickly and I seem to be getting some mixed signals.

DR. FLINT: That's good. That means I'm being inconsistent as I am supposed to be.

DR. JONES: Yeah. Quite a bit of your talk has been on comparing methodologies for making some of these measurements with conductivity and permeability and porosity and I got a lot of the different--there are three or four different ways to do this, they all disagree, depending on how you dry it, how you fit your curves, how you predict these things. And now we are going through several slides of these are what the measurements are, these are the properties, this is how many samples we need. That seems to--how can we have so much controversy in how to make the matrix, but yet know so much information about what these properties are already before we solve those things.

DR. FLINT: All right. Porosity and bulk density, the two measurements here. Porosity is a possible surrogate. Again the overlying idea that I had in putting this stuff together was, can we get some fast information out to modeling groups. I am using this as a first approximation as a surrogate to having the actual measurements of conductivity and water characteristic curves. I am saying that with this information, these outcrop samples, I can get a fairly rough, but maybe extensive data set together for the modeling group. Now the modeling group can come in and this is another point I want to make strongly, I think, is what difference does Van Genuchten and Brooks and Corey make? Maybe it doesn't make any difference. Maybe all you need to know is within an order of magnitude of the conductivity and to know that it decreases with decreasing water content, you can put a linear function in there, because the models that we use, the numerical models may not be sophisticated enough. We may deal with 100 meter thick blocks of rock and that that kind of information is inconsequential to the overall program.

I think it is consequential because that is what I get paid to think, but I am convincing myself that worrying about the difference between Brooks and Corey and Van Genuchten, which I will do and I think is important, may not turn out to be useful in the modeling arena. And I can show how they may differ. The modelers can test that for me. They can test Van Genuchten and Brooks and Corey, and they might find they don't make any difference, then I am further along.

I wanted to show you the detail we went to to get the information. A lot of detail; a lot of different tests. But, what does all that data use? I don't want to collect a lot more data and do a lot more work than what can be used by anybody but myself. I can get processes down and some ideas down, but we are working as a group. We are trying to come up with a big picture, a big program to answer a lot of the questions. And if I can go out in a week or two and get 100 measurements of porosities in bulk density and estimate conductivities and get that to the modelers, I think that does more good for the overall program in performance assessment in the international model validation group we are working on, on our own 3-D model, than it would for me to spend that same week or two which is all I can devote to one or the other than really working it closely with Van Genuchten, Brooks and Corey, which I would really like to do. That's where I have more interest. But I feel that this information is so important, because the modeling has been going on for years and years and years, I'd like to have them have now a vertical transect. And the reason we are doing this is because they asked for it; Sandia, PNL and even the USGS wants to know some of that information.

So, yeah, you are right, I am doing two things at once. I think they are both important. When I get this information out I want to go back and do the detail. We'll take one core at a time, be very careful and take the measurements in the lab, do the imbibition, do the modeling and try all the different techniques we can. I am just working at a bunch of different scales.

DR. WILLIAMS: Do we have time for one more?

DR. DOMENICO: Well we are getting pretty late here, but make it quick.

DR. WILLIAMS: This is Roy Williams. I want to ask you a question about the statement or the conclusion, not the hypothesis that the Paintbrush and the Calico Hills are hydrologically different than the Topopah Spring with respect to a barrier or a non-barrier. The numbers that you have listed here for core porosity and for those two units, you don't have the Calico Hills on there. You have to go back--I am talking about the core numbers.

DR. FLINT: Okay.

DR. WILLIAMS: You have to go back one slide to get the Calico Hills. Let's just assume all these numbers are valid and we don't have any problem with this question that Tim brought up. Okay. The Calico Hills has given their--that's probably the most important one--there is sufficient overlap among all three of those units that I can't see why you, on the basis of porosity would say that they should behave differently with respect to their properties as a barrier. There is so much overlap that you could hardly tell the difference as far as porosity is concerned. I know that is not true of permeability.

DR. FLINT: I want to go back to this picture. First of all we are dealing with units that were defined by different people. When we say the Paintbrush nonwelded tuff, that includes the top of the Topopah nonwelded unit and the base of the Tiva non-welded units. Don't think in terms of units. Think in terms of what is up here. Let's assume that this is real. You do have high porosity unit right here, and although--look at this porosity from here to here or from here to here. Granted, we do have some nonwelded units in there but that is where they are, they are right here. As we go further on down, and although they overlap, maybe they are not that important, because this happens to be in one unit, it is not as important as the fact that we have this large block of high porosity material.

Also, we see some high porosities in here which also overlap with these porosities. But, in general, if you look at this picture and don't think of Yucca Mountain as a series of units, although you know that you can make or break here and you see how this makes a jump across the unit and so does this one, you know that those units are useful to you in defining the property, but really, I think you should forget about the unit boundaries now that we have maybe some more information from here and look at how does this system behave in a model. This is what we are going to do next.

DR. WILLIAMS: You want to change your conceptual model based on the numbers rather than the names.

DR. FLINT: Not the conceptual model, but I want to change--well I want to plug in the real data and improve the conceptual model by saying that the PTN unit looks this way. And you can't just take the range and say well this range overlaps, because here is where the overlap occurs in the PTN unit, these low porosities, one of the reasons the overlap occurs. But that occurs at this location. How does that influence the fact that this is a high porosity unit?

We have to be careful about the way we put names on there. One, we know the microunits are important; but, two, we want to look at the whole system together and the fact that you have these transitions.

I think this should be fairly useful information. The microunits help us to know where we are. This thing doesn't stay--you get changes in thickness, it gets thinner. This is important, 2 percent, it seems to be everywhere over Yucca Mountain. A very important unit. But a lot of it is exposed. A lot of this material is exposed. It doesn't go through that.

I am going to flip real quick to the last slide which I don't think really says anything that we haven't already said, but this way it will be official.

The idea is we take the samples either outcrops or boreholes. Right now outcrops are easy. We test, analyze and we put them in some models, we look at the results of the models, and we look at the sensitivity analysis of the models and we iterate. The models are not one big model. The performance assessment; the UZ; they are one dimensional models. They are very small models. They are near surface models, and more detail than others and we iterate through this process. That's just the general idea.

What I want to do is I want to get this sampling and analysis done so that the modelers can be working on it right now. I think it is real important. So I am putting in some extra effort trying to get that information out as soon as I can to the modeling groups. And hopefully that will be of some use and that can maybe tell me more about what I need to do and what is more important. Maybe they will say if you can give us conductivity, that is all we need to know, right up front, and then improve it as we go.

DR. DOMENICO: Alan, I agree with you that you are probably getting a lot more detail than the modelers can use. But you are getting values of saturated hydraulic conductivity for these units and there are a lot of them, and I know they are crude estimates. But, how do they compare with the estimates, at least one of the estimates for recharge to the Yucca Mountain? Do we have the potential for fracture flow to take over because you certainly have got some estimates now of saturated hydraulic conductivity and you have an estimate of recharge. How do they compare?

DR. FLINT: Well some of the units would indicate that if you were to pick half a millimeter a year as a recharge, that it could not go at that rate through the matrix.

DR. DOMENICO: In otherwords, the matrix would reject it?

DR. FLINT: The matrix would reject it. It cannot handle that fast of flow, especially when you are dealing with that two percent porosity unit.

DR. DOMENICO: The Tiva Canyon on top has a very, very low hydraulic conductivity, I noticed, 10^{-10}, 10^{-9}.

DR. FLINT: It's fairly low for this unit, although this unit doesn't show up on most of Yucca Mountain. But you get down into the columnar unit, it has too low a conductivity to support that kind of flux. This is 2 percent, and so is this one. But, it has a lot of vertical fracturing. So if you are going to have that kind of flux, you are going to have to have fracture flow going on.

DR. DOMENICO: One of the concerns of your review panel was the discrepancy between calculated travel times and the observed constituents in the subsurface.

DR. FLINT: One of the reasons I think was because some of the observations were up into these faulted washes. And those may be different. I think it is just accounting for the mechanisms in there. I think that the modeling does account for the mechanism. We know the fracture flow occurs. We have measured fracture flow; we've seen it happen. The modeling in my opinion hasn't handled it quite the way that it should, but it is just because of lack of information.

DR. DOMENICO: Well I would hope that the models that they are considering would now be concerned with that because it starts to become very, very important now, especially since we are in a site evaluation stage as opposed to a licensing stage because we are now talking about the potential--

DR. FLINT: I think we are considering those things. One thing that Claudia didn't mention when she talked about the peer review, although they said, well we are concerned about this inconsistency or this or that, they listed those things. Those were things that we actually told them. We told them that we were having problems with inconsistencies and that we had their better account for the mechanisms and we are trying to do that. So, we were aware of these things and knew that they were a problem, and are trying to deal with that. But, changing your whole modeling around for one Chlorine 36 data, you have to be very careful. Because other data, tritium data or carbon data which Don Thorstenson can talk about may disagree with that particular sample of that conclusion. You have to account for the mechanism in there and then you have to run a lot of simulations, and the simulations should allow for that to happen if it is a mechanism.

DR. DOMENICO: Okay. Joe Rousseau has been kind of enough to postpone his presentation until the first thing this afternoon, so I think this may be a good time to adjourn for lunch, and let's probably return here in 45 minutes, if possible.

DR. FLINT: I am going to pick up these samples. If anybody wanted to look at them for any reason, they'll be here for a couple of minutes.

(Whereupon, a lunch recess was had.)

AFTERNOON SESSION

DR. DOMENICO: This may be a good time to gather the wagons in a circle over here and get started.

MS. NEWBURY: Our first speaker this afternoon is Joe Rousseau, who will be discussing the deep borehole instrumentation.

Joe?

MR. ROUSSEAU: I guess we have everybody here. My name is Joe Rousseau, and I'm project chief for the deep unsaturated zone hydrology project investigations, U.S. Geological Survey. The topic I'm going to talk about this afternoon is the deep borehole testing for flow processes.

A year ago, I gave a presentation that dealt with in situ instrumentation and monitoring. I wanted to change the focus a little bit this time around and start talking a little bit about the sort of flow processes that we want to measure in the deep unsaturated zone.

Last year, the presentation centered around these items, which I identified the purpose and scope and measurements, and gave a general overview of the drill instrumentation program exclusively. I want to back up a little bit and look at the other sorts of things that we're going to be doing as part of the surface-based borehole investigations. You saw one of the activities early this morning, Alan Flint's matrix hydrologic properties testing program, and there are two other activities; one called the site vertical boreholes investigations, the other one called the Solitario Canyon horizontal borehole investigation, and within those groups there are many, many sub-activities.

Gary LeCain, who will follow me, will be talking specifically about one of those, which is the air permeability testing program. Last year I did go over and highlight--now, last year I only had 15 minutes. I have the same number of slides I want to present in the hour that I have right now, so I want to go quite a bit slower, but you won't see the same type of information.

Last year, I did go over, in general, what was some of our UZ-1 experience. I think I came to about three conclusions on the UZ-1 prototype borehole monitoring program. One was that we did not have the unit hydraulic grading concept--at least from the data that we had--with the Topopah Spring. Second, we had a significant amount of thermal activity, at least if you believe the data coming out of the sensors; and third, that the concept of permanent installation of sensors was probably viable, providing we provide some backup mechanism to verify essential performance.

I also showed you some data, about half of the data that we collected from the G-Tunnel instrumentation program, which lasted about 13 months. We considered that to be an analog site. It might give us equivalent sort of information that we might see in the Paintbrush non-welded bedded units, and/or the Calico Hills units. And again, that was a prototype investigation just to look at instrumentation, but we came out with rather significant results in terms of hydrologic interpretation.

And lastly, I summarized with what I consider to be the benefits of in situ monitoring. I will do that again today, and I also will go back and revisit the purpose/scope. I'll also concentrate on two other things that we've done in the past two years, or year and a half. One is: Where are we with our psychometric evaluations? And two, where are we with our gas sampling program? So I'm going to save some of that towards the end. I'm also going to show you a couple of examples of what came out of G-Tunnel in terms of what you can do with quality measurement.

The site vertical boreholes investigation and the Solitario Canyon investigation have these two purposes: One, to define the fluid flow potential field within the unsaturated zone; and two, determine the in situ bulk permeability and bulk hydraulic properties of the unsaturated media. Here we are distinguishing data that we can collect at a borehole environment that is going to be distinctly different from what you can get from a core sample, and tested in a laboratory.

I will go over the various components of this program, but before I do that, I thought it might be important to kind of list out the related interfaces and, just in a generic sense, some of the information that we'll be collecting by drilling the boreholes alone and providing access to the unsaturated zone will feed a number of other investigative programs; one being the systematic drilling program, the second being the saturated zone hydrologic investigations. Our discharge area becomes their recharged boundary. We will also provide an opportunity to measure water levels within 17 vertical boreholes and 12 of the Sandia systematic, which will give us another 29 measurements of water table levels.

Matrix and physical rock properties testing programs, of course, you drill a hole, you get the core, you provide the material on which to run those programs. Hydrochemistry studies, primarily within the unsaturated zone, Chlorine 36 and the work that Al Yang is doing, and the work that Don Thorstenson is doing right now.

Exploratory studies facility investigations, site integration modeling programs, performance assessment, and engineering design programs, I just tried to do this in a very generic sort of sense, so the data feed to other programs is rather significant.

I'm going to be talking about the features based drilling program almost exclusively this morning, or this afternoon, and we distinguish it from the systematic drilling program. Alan highlighted a little bit of that in an earlier discussion, showing you these circles with these dots on there and how they tried to locate additional drill holes to provide infill-type information. Our scoping or our siting strategy was distinctly different from that of a systematic drilling program.

The general scope of this one is 19 vertical boreholes, about 32,000 feet of hole, one horizontal borehole nominally right now, about 1,000 feet. We have planned to instrument hydro instruments, 17 vertical boreholes plus one horizontal hole, conduct a passive in situ monitoring program for about three to five years, and during that passive monitoring program, we will have active in situ testing going on intermittently and/or at the conclusion of the monitoring program.

Geophone--this is all part of the surface-based investigation program, site vertical boreholes. I'm not going to talk about this component of the program. I have a professor, Dr. Balch, School of Mines, is working on it for me; geophone instrumentation of two vertical boreholes for cross hole tomography and vertical seismic profiling investigations. I'll show you the locations of these a little later.

Continuing on the scope, we plan to have 15 to 17 geophysical logs right in each borehole. There is a geologic and lithologic logging program which formally the Geologic Division was responsible for. I'm not sure that--how that's going to sort out. There's a matrix and physical rock properties testing program. Alan discussed a component of that earlier. There are other people doing physical rock properties testing, too. There's an air permeability testing program which Gary LeCain will cover as soon as I'm done. There's a gas tracer diffusion testing program which we are now beginning to get started with. This is kind of a new exercise that we intend to run in the instrumented boreholes, plus a water injection testing program that we plan to run inside the instrumented boreholes upon termination of monitoring. All vertical boreholes will penetrate to the water table. In some cases, they will penetrate through the Calico Hills unit into the Crater Flat unit, but all boreholes will go to the water table and provide penetration of 10 to 50 feet so that we can actually monitor the position of the water table.

Siting strategy for these holes: "Target those areas of interest with the greatest potential to provide the evidence needed to assess the suitability of Yucca Mountain as a repository." Basically, we're going after show-stoppers. So we've looked for features of the mountain, faults, surface drainage features, large scale structural features in Ghost Dance, in Solitario Canyon fault systems, the topographic features, which are your washes to, and your minor washes coming off the ridge of Yucca Mountain.

We also want to provide some sense of aerial coverage--and this is where the systematic drill program will help out--for fracture system continuity, develop a broad definition of the potential field. Discussions we had out at Berkeley about two weeks ago was to include a minimum of six of the Sandia holes as part of the instrumentation program, simply to provide us more information about what the background potential field might look like in the absence of thoroughgoing features like Ghost Dance Fault, Solitario Canyon Fault, and things like that.

Lithologic variations, permeability characteristics, specialized testing requirements that we had. We had to co-locate holes so that we had a near-hole proximity to do cross hole air k testing to conduct vertical seismic profiling investigations. So there are some constraints related to the testing. We were also constrained by minimizing disturbance to the integrity of the repository, so if you look at the site locations--which I'll show you in a minute--of these holes, we've only had two locations where we actually penetrate with inside the perimeter drift boundary, and we also had to be concerned about adverse influences of prior activities; that is, boreholes that were drilled with water fluid and/or foam.

Returning to this slide, on the left, this is the borehole locations. Our first borehole complex that we wanted to do as part of the project when we officially get into site characterization work--I should point out that we are not now actively monitoring any boreholes. UZ-1, we terminated 1988, I believe; October, 1988. So there was no more activity going on, and after G-Tunnel was shut down, December--11-12, the same time we had this meeting the first time--we are no longer doing any active monitoring in any holes.

The UZ-9, 9a, 9b and VSP-1 borehole complex is located in the imbricate fault zone over in this location here. It's now being referred to--is that focused good? Can you see the right-hand slide?

DR. DOMENICO: Joe, what's proposed and what's actual?

MR. ROUSSEAU: Okay. This comes from a document that was done, or a study plan that was actually written several years ago, so this may be the actual proposed perimeter drift boundary, this boundary in here. So our UZ-9 complex, that's the first one we wanted to get into it. It's basically virgin territory right now. There are no penetrations in the near vicinity. Three boreholes, one to be--and a fourth borehole, one to be instrumented with geophones, and all three of these boreholes will be air k tested, geophysical log instruments run on them, and the whole--everything that we're going to do with the other holes, too.

UZ-4, UZ-6, 6S, 2 and 3 on the ridge of Yucca Mountain. Investigations are ongoing with 6 and 6S as part of Ed Weeks' gaseous phase movement investigations. These holes will be about 2600 feet deep to penetrate to water table. UZ-7 and 8, the pair of boreholes that straddle the Ghost Dance Fault. Some of these have been drilled. Some are partially drilled. All of them would have to be reamed out or redrilled to accommodate the instrumentation. UZ-4 and 5 in Pagany Wash, UZ-1 and 14, we're going back to revisit UZ-1 by drilling UZ-14, take another look at that steep hydraulic gradient, and also re-instrument it to confirm our measurements at UZ-1.

We've done a lot of work in designing the center packages and that sort of thing since UZ-1 went in. UZ-10 and UZ-13, Yucca Mountain Ridge and the thickest section of the Tiva Canyon unit, those were the objects here, and we have the Solitario Canyon horizontal borehole, which would be located somewhere in this vicinity. So we went after targets with features that you could easily identify on the ground or in maps. No statistical basis for siting here at all.

I'd like to now concentrate the discussion primarily on the drill hole instrumentation and monitoring. I do want to do a little bit of a diversion here, restate something that came out of the peer review team comments. During that four-day process, there was lots of discussions about how you simplify things, and I just kind of wanted to revisit this rather quickly. The second slide I'm going to show kind of gives you an idea where my thinking's coming from, especially with respect to the instrumentation requirements.

But ideally, we like to assume things to make life a lot simpler. Reduce the number of measurements so we can --less data to handle, less overhead and everything else; make the analysis a lot simpler, but they have to be appropriate, they have to be realistic, and they have to be able to represent the system. At any rate, I think everyone would want to tend to simplify to the maximum degree possible, so I took a look at what did that mean with respect to Yucca Mountain and what we know today, and what sort of systems we're dealing with, and tried to put in my own mind where do I think we're going with my program.

If we look at the left-hand column here, and the degree of simplification, the least complex, the highest in terms of degree of simplification, and work downward, or transition down to the least amount of sophistication in the most complex systems, if we have to deal in that arena. From a hydrologic point of view, what would that system look like?

The least complex systems would be isothermal flow, homogenous porous media, very simple. As you work through there, you can transition down to a non-isothermal flow, heterogeneous fractured, porous media. The title of this is flow processes in the unsaturated zone, and that was the motivation for developing this chart. So we look at that from a system description point of view, come under the column--the third column in--and then we start to say, okay, what does that mean in terms of the flow processes and geometries we'd be working with?

The very simplest is a single phase, liquid flow, very simple boundaries; in fact, infinite boundaries in most cases, and that's how it's dealt with. And as you work through this--I don't want to give a lecture here or a class, but I want to jump right down to the bottom and the most complex process and geometry you deal with. You have liquid and gas flow, discrete fractures, internal and external boundaries within the context of, let's say, the repository, the perimeter drift. You have very complex circulation systems, perched water that has to be dealt with, and you have to deal with lots of transients.

Now, each one of these particular units that we have--there's a mistake here. This vitric should be sitting up here, and the zeolitized should be sitting in here--naturally lend themselves to some level of simplification, I think, as we see it today, and here is where the PRT was trying to drive us. It was drive up in the right-hand corner, see if we can't solve the problem right up in here. I can't presume that that's the answer, so when I look at what we're dealing with the features-based drilling program and the three to five-year monitored program, we're actually working down here. We've basically made a minimal number of assumptions and are working with the candidate which is no longer a hydrogeologic unit anymore, the secondary features. So that's where I think we are right now, and ultimately, we'd like to be able to step up and maybe we can end up up there.

But I think, in my own philosophy, if we can understand local processes, then we'll gain a lot more confidence about how the system really behaves.

Here are some fast facts about the instrumentation program. We'll be working in 12¼-inch diameter boreholes. There is an active prototype drilling program going on now for dry drilling to this diameter. Our maximum depth will be 2500 feet. As a minimum, we can probably accommodate 16 instrument stations per borehole. That could go to 20. We have not yet attempted to instrument with the sensors and apparatuses that we have in hand today with this diameter.

For the features-based hole, that counts out to about 300 instrument stations total. If you add in the Sandia holes, we're talking about 600. We've adopted a solid stemming design that uses grout for isolation and structural support of the column. The cavities themselves where the instruments are located would be infilled with polyethylene beads. I wanted to provide a fairly inert environment in there so I could accelerate the water potential equilibrium process and start seeing that quicker, introducing some other type of material.

This should read--okay, inert filler material between instrument stations. We may drop this altogether. We're now working with a calcium sulfate-based grout design, the idea being here that, one, we could bring it down to about 1,000 psi compressive strength, and probably not have to introduce short circuits in the pneumatic pressure wave. So we may grout up the borehole right between stations. I anticipate each station being about ten feet in length.

There's a hollow stemming tube that supports all the electrical cable, the tubing, and the down hole instrument station apparatuses. You can see a picture of portions of this in a minute.

The sensors--this is probably very important. The sensors are not recoverable in any useable form. We won't be able to go in the hole, put the sensors in, get the type of measurements we want, take them out, take them back to the lab and determine the end point calibration. So we've had to do some work in terms of what can we do to calibrate sensors in situ, and I think we've been successful in a couple of areas.

We anticipate that the monitoring program would last from three to five years, and that's based on my review of the UZ-1 data and the experience in the G-Tunnel underground facility. Five years is sitting here because we want to get temporal continuity for all boreholes. It'll take us about two to three years to get them in place.

Sampling frequency, each sensor that we'll use, we'll want to read once every five hours. There is a scope for high frequency measurements. This is some of the intermittent testing I talked about earlier, where if we have a pressure front coming through, we've been monitoring for a year, year and a half, and we have a pretty good idea how things look, and we want to run some very fine, high-resolution tests, we have those capabilities. The interactive testing program that follows, a gas tracer diffusion and water injection testing that I mentioned earlier.

These are the types of measurements we'll be taking. These are the accuracies that we are--we're there now in terms of our capabilities in the laboratory and what we did in G-Tunnel. Our pneumatic pressure, we're dealing with about 1 psi absolute pressure differential between the top of the mountain and our bottom measurement. We're going

--our target right now for maintenance is 0.005 psia at 2 sigma significance. Right now, we can calibrate to about 0.003 of a psia at 2 sigma, so our pneumatic pressure measurement is in hand.

Temperature, we're shooting for 0.005°C and right now we're at about 0.003. We are at the limits of primary standards with these measurements.

Water potential. I'll have more information on this in a minute, but right now we can calibrate psychrometers to about .9 of a bar. That's the lowest limit that we've tested. We get repeatable results. We can calibrate, on occasion, psychrometers up to about -100 bars. We don't anticipate that we will be successful on every device, so we're going to drop back down to about 75 bars. We have been successful on isolated cases. Our relative error of our psychometric measurement is nominally about 1 per cent in the very dry range, or at least dry range for the Peltier-type devices, and about -70 bars, 75 bars to about 10 per cent in the -1 bar range.

Each of the instrument stations that we'll put in place will carry a gas sampling system designed to carry dry carrier gas down and bring out source gas, lower the dew point temperature of the source gas, because each--well, the rock gases at depth will be at nearly 100 per cent level of saturation--bring those gases up in a controlled process, and, one, prevent condensation and loss of heavy isotopes from the hydrochemistry sampling program, and secondly, to give us an independent means of verifying psychometric output. We've actually tested a mockup of this system--I have more talk about it--in the laboratory, and I want to defer right now for that.

Hydraulic testing is taking it a little bit far, but we feel--especially some of the work that Gary has already done--that at the rates that we'll be flowing gas, we'll be actually conducting many single borehole hydraulic tests, pneumatic tests, and there's other variations of that, too, that one can go into, but it's not important at this stage.

DR. LANGMUIR: Joe?

MR. ROUSSEAU: Yes.

DR. LANGMUIR: I have a question. Have you been working with geochemists who were concerned about the reactivity, possibly, of the gypsum grouting, which has a pH_2 influence, H_2O influence and could conceivably dissolve and move around?

MR. ROUSSEAU: I've sent a plug of that sample. We got samples in about three or four weeks ago, and I gave Al Yang a core of that and he has a student that he has working on it right now to see if we've got compatibility between that material and the hydrochemistry requirements.

DR. LANGMUIR: It's a pretty reactive phase in terms of things moving by you might want to know about.

MR. ROUSSEAU: Yeah. We've, you know, we have to work it step-wise. We've completely discounted calcium carbonate as a grout material for reasons of CO_2 invasion.

System reliability. In terms of monitoring, long-term monitoring, we've provided capability to do in situ re-cal of pressure transducers. We duplicate sensors at each station and one sensor provides confirmation of sensor accuracy, is not excited as often. There's a sensor backup at each station in case of failure, and the backup sensor also provides us some ability to look at relative drift. I mean, we ought to get the same reading back out of both sensors all the time, and some sensors will get more duty cycles than other sensors will, so that's kind of a backup.

We also have a central stemming tube that's hollow inside that gives us longer long-term backup. It doesn't give us the capability of conducting very high frequency measurement. The role of that thing is to provide us access for the water injection testing, gas tracer diffusion testing in the event that we need it and we lose our tubes down hole. It's a backup to gas sampling--again, in the event that we lose tubes. It's a backup to thermistor measurements. We can always run up a column of thermistors. The measurements aren't going to be as good, but in terms of long-term measurement, if we want to carry instrumentation out ten, 15 or 20 years and we've lost the sensors in hole, this is a way of doing it. Access to the unsaturated zone for--or saturated zone for water level measurements.

Pressure measurements--and this is based on what we saw in G-Tunnel. We can expect that, as a general rule, the characteristics of these measurements, fairly high frequency, relatively high amplitude, damped, of course, with depth. Equilibration time could probably be within hours to days. Pneumatic pressure doesn't take a long time to equilibrate. We can expect, depending upon depth, strong diurnal and seasonal signatures. Now, this could be--also occur at depth if you've got faults that are open and interconnected, fractures that are interconnected, that you don't see.

So one of the advantages you get from this kind of monitoring program is you get to see a very large region, very large region. You're not isolated to that single thing. If you are in a fracture or if you are in a fault and that fault is open and conductive, let's say, to air pressure, you ought to be able to see something, detect something, and I think with the accuracy of our measurement, we've taken it just above primary standards for pressure measurements. We can't--the primary standard for pressure measurement is about 0.001 psia, 3 sigma. We are about .003 right now.

One of the things it will be used for will be vertical and horizontal pressure distribution to determine whether or not we have convective gas circulation processes outside of the influence of an open borehole. We expect to see phase lagging and damping effects that could be used to compute permeability to air in the vertical sense and treat that as a model; hydraulic conductivity, provided a Klinkenberg effect is minimal, so this is the sort of thing that this data could be used for in its own right, in addition to providing a measure of the pneumatic pressure potential.

I've included some of this information because the last time I gave the talk there were lots of questions about, what are you using and how are you doing it? So it's here. I want to hit it real quick, just so that I can answer those questions. I did review the transcripts and I thought this would be important.

We're using the Druck PDCR 930, non-thermally compensated unit, manufacturer quotes 0.06 per cent full scale accuracy. We are now at 0.03 per cent full scale. The resolution of the device is better than about 0.001 of a PSIA. From the control limit of electronics, we're looking at about 0.0005 psia. We detected that level of movement in G-Tunnel. This is resolution. It's not a statement of the absolute pressure.

Stability. It's a silicon diaphragm-type transducer. It's basically an industry standard for long-term stability. We are currently completing our evaluations of the effect. I feel that we have completed it. We have had no apparent problems to date with this particular device. We went through some false starts with the earlier models that Druck made for us. We got back with their engineers, talked to them. They went back and made some modifications and we got a device that we could work with.

We tested long wires. These instruments or sensors are going to be installed at depths up to 2,500 feet, so I wanted to take a look at what are we going to lose by putting this sort of device that maybe has a nominal 100-200 millivolt signal output, what we're going to lose in terms of deploying them in that manner, and our tests--because we are running the devices in current mode--indicated we lose nothing in terms of accuracy, nothing in terms of resolution over 2,500 foot wires. That's also a very strong function of the electronics that support this measurement.

Continuing on, without spending a long time here, this is just for your reference. I provided you with the support equipment that we're using to take this measurement, the number of measurements that we take in order to produce a value, and a general summary of what sort of a calibration mode we go through. I don't want to spend any time there. I don't want to run out of time.

Temperature. The characteristics of this particular measurement; moderate to low frequency variations, variable amplitude signal. A lot of this is going to depend upon how deep we are, and again, on whether or not we are working in areas where we are getting convective transfer of gases, primarily gases that would have a tendency to want to change the thermal regime. Equilibration time, weeks to months. The equilibration time will more likely go up in very, very tight units. Where you've got to accommodate latent heat of vaporization processes, it may take a long time to stabilize. We also have to accommodate the heat of hydration that goes on with setting up the grout packages on the upper and lower end of the instrument station.

In the near field, the near shallow environment, we could expect to see strong diurnal and seasonal characterization at the near surface. At depth, we should expect it to be dominated by the geothermal gradient. In G-Tunnel, the UZ-1 data did indicate quite a bit of thermal activity at depth, so either the sensors are right and there's something going on, or the sensors are wrong. We need to confirm that.

The thermal profile that we'll be looking at is going to be influenced by the basic thermal conductivity of the media, measurements that Alan's program is going to be taking from matrix properties; the volumetric heat capacity of the matrix; and the latent heat of vaporization processes.

I want to show you some of this stuff from G-Tunnel, because we actually used this instrumentation process to look at this sort of thing.

The liquid water level influences the thermal diffusivity, or the form of the--and the location of the geothermal profile that we've been looking at. The thermal profile can be used as an indicator of liquid and vapor flux processes. I gave a paper on that at the IAH convention. I think we saw that happen in the G-Tunnel environment, using the psychometric data and information we collected from core or pressure data, and our temperature data.

Heat flow could be used by itself, if you will, to compute ambient saturations, vapor flux, liquid flux, and establish the validity of the water potential measurements. If you have a temperature change, you better be having a change with your psychrometer.

These are the fast facts on the thermistor. I mentioned earlier, our accuracy is better than about 0.005°C. Our resolution is 0.0005°C and that's the limit of our electronics. We were able to resolve those kinds of limits in the G-Tunnel. We are using a device that we're going to rely on the manufacturer's specification for stability, where he quotes less than 0.01°C over 100 months, and the effects of long wire. Again, we tested this. Again, we are operating our sensors in current mode, and we tested short wire, long wire, and came to the conclusion we had no degradation in any form with accuracy, quality of the signal coming out of the long wires. We tested to 2,600 feet.

I'm not going to spend a long time here. Again, this is provided for background information. This is the support equipment package that we use to take measurement.

Now, I am going to spend a little bit more time with water potential. We measure water potential in the vapor phase. For it to be a true measure of water potential, we have to have an isothermal state. It has to be there. If you don't have it, you've got movement, so you have a harmonic equilibrium plateau that you're working with. The difficulty of trying to measure in a shallow environment is you've got to deal with very, very severe temperature changes that can really mess up your measurement. If you think that's the true water potential, it probably is not.

Equilibration time, I anticipate anywhere from months to years. It'll be strongly influenced by the drilling methodology. We are dry-drilling. It's my feeling that was a proper choice. One, if we introduce water into the media, it takes a lot of energy to get it back out. If we dry drill, all we've got to do is change vapor into convective flow process. We're likely to get things closer to equilibrium a lot quicker.

It may exhibit seasonal characteristics, depending, again, on depth, and dependent upon whether or not we've got convective exchange of gases, or whether we have any recharge events. True measurement does require isothermal conditions. Though you can get a pretty good idea of what's going on even if you don't have that, you've got to be able to track your temperature. If you know precisely your temperature, start to see movements in your water potential measurement, they should go hand in hand.

They are used to compute liquid flux. We will reference these to the measurements of the matrix hydrological property test. There are some units--especially that glassy vitrophyre that I think Alan referred to--that are likely to come up very hot, and if it's sitting at 2 per cent porosity to begin with and there's any water in it, it will probably be registering, you know, -1,000 bars or better, and we know that's not true. So the only way to get that measurement in place would be to do it in place, to capture that sample in place.

Presence of open, interconnected fractures may produce high frequency, high amplitude signals, and we'll see this in the G-Tunnel thing that I'll show you in a minute.

Summary fact sheet, not going to spend a lot of time with this one. Background information. We do have a redesign of the device. We did that about three years ago when we first started testing. I do want to show you the diagram of that. It's--we don't have anything out on the street right now with respect to it. Our range right now is better than -1 bar to -75. I already talked about accuracies. Here is your sensitivity statements for the device.

We don't know yet what the stability characteristic is. While we were operating in G-Tunnel, we took another measurement with a six-wire psychrometer, and basically turned it into a four-wire resistor, and in doing so, we think we may be able to track psychometric drift. So what happens on the bulb or the bead of the device is it tends to corrode, get pitted, so the resistance is only 8 ohms to begin with, and will degrade with time. But the way we design it, we're able to take a simultaneous measurement and treating it just like a four-wire resistor, and I'll show you the design of this in a minute. So we think we have a handle on being able to track that.

All devices that we used in G-Tunnel all had the same characteristic output curve over the 13-month period by looking at that measurement. That is not the water potential measurement. It's another one that we take in advance of that. We tested the effects of long wires. We will calibrate these devices on their wires, because the signals that are coming out aren't greater than about 25 micro volts. Many of the signals will be down in the half-microvolt range.

We have tested over long wires to 2500 feet with this design--that electronic package I showed you--and found no degradation in the quality of signal again. So those were big unknowns that I couldn't tell you about or sell you on when we had the meeting last time. They are solved now. I'm very happy with those kind of results.

This is a quick and dirty on the calibration. I don't want to spend a lot of time. This was the setup that we used; this sort of background. The support equipment package is the next one. Let's flip past that one, but here is the design of the device that we're currently working with.

The basic Peltier psychrometer is a three-wire system. The voltage measurement is taken across this bridge component here to meet the EMF off of two types of thermocouple to give you your dry bulb reference temperature. The current that has driven into the device--this is the standard one. The one over here is the one we've modified. Current is driven into the device to cool down a chromel/constantan junction up here, which is a welded junction, and condense vapor onto the bead, and then you monitor the time it takes for that vapor to evaporate.

This--we changed the design for a couple reasons. One, we didn't have a balanced electrical signal--we had an unbalanced electrical signal problem in here. I looked at data that had been collected years earlier, and there's no way we're ever going to get those kinds of signals that we need over 2,500 feet with this type of thing. The noise would kill you, so we immediately dropped--second, we wanted to drop interfering influences of this thermocouple component.

So we just added three more wires, and separated the dry bulb measurement from the wet bulb measurement, and now have a circuit dedicated to current voltage, and a separate circuit dedicated to dry bulb reference, and got the manufacturer to put all that in the same size screen and everything. He had to add three more wires. He didn't have a lot of room to work with in here, but he managed somehow to do that for us.

About 10 per cent of the devices that we have been calibrating from him, we have to reject because they don't meet--they're good psychrometers, but they don't meet our model, and I'll show you what the model looks like in a minute.

This gives us the capability to do something like this with the measuring sequence. One, we can get our--read our dry bulb first; takes about 15 seconds. Then we can read the wet bulb zero voltage. We try to find out what the zero voltage level is immediately prior to any measurements that we're taking because there could be some slight offsets. We're really working with a differential measurement now, not an absolute. We're taking off the base reading of the voltmeter.

We learn excitation current for 5 mA for 30 seconds. Simultaneously, we read the voltage during current excitation, okay? This is giving us our four-wire resistance measurement. So we're attempting to read the resistance across the wet bulb while it's being excited, and we did that for 13 months in G-Tunnel and we're now evaluating, can that tell us what's going on with drift? Are we changing resistance and getting a long-term drifting sensor?

About four seconds prior to the time that the excitation signal terminates, we switch meters and bring in the 181 nano voltmeter to get it conditioned, ready to read that signal as soon as it comes in. There's no signal settling time involved right now, so we're basically washing out four seconds of our measurement, and then we read the wet bulb for about 150 seconds. The six-wire design gives you this capability.

This is what the data looked like, assimilating, or making a large population out of ten psychrometers. This is 900 data points from ten psychrometers all on one plot, and you can see the distribution. What we're trying to do here is just find a calibration model for our psychrometers. I don't want to carry massive amounts of tape. We'll look up data in the computer, so I wanted to lock in on a single model that we'd be comfortable with that wouldn't degrade the quality of the data up front. So that's the reason of putting this thing together.

We have measurements down to .02 molal solution, which is the equivalent of about .9 bars at the temperatures we were running, at 10, 15, 20, 25, and 30, and 0.1, and that's information that's sitting over here that we had to blow up and bring up to this scale to look at.

When I assemble the data for all ten psychrometers' 900 data points, we lose our resolution power in here, which we're losing by doing our regression through here, but it gives me some estimate of the confidence I can put in the coefficients we're going to use in the regression.

When I take a single psychrometer, I preserve this information. I can resolve or separate about .05 bars down at the .02 molal or .9 bar equivalent. And things are getting real close to going through zero. We've fought that for a long, long time. This is our model that we ended up. It turned out to be a cubic form equation, six coefficients, standard error, the estimate, and that's for all 900 data points for ten psychrometers all lumped together. I wanted to study the qualify of the coefficients and this is what we ended up with. Probably someone can do better, or come up with a new variable package for this particular device. We will work on that some more, but I needed something now, so over the bar range of 1 to -75, we have a standard error estimate of 0.815.

I thought it might be pertinent for those that don't have much experience with this device to kind of see what the data graphs look like as you're going through a calibration. The one on the upper left there represents a .02 molal. This is done at 20°C. This is about .9 of a bar section. The full scale left-hand side range here, this is the data right up in here. All this noisy looking stuff is about 28 nano volts a single--standard deviation. The full scale here is about .4 of a microvolt, so we're looking at 400 nano volts, 40 nano volts in each one of the little blocks in here; about 28 nano volts. That's about what it holds steady at. That's our noise level. So practical limitation, bar any other physical problems, would be about .2 bars. I don't believe we'll ever get there, because I think we're running across some physical hurdles we never can get past.

As you move to .1 molal, our signal gets a little stronger; .8 micro volts. Okay, we're looking at 9 micro volts, 20 at .8, 30 and, yeah, about--okay, 30 here, too, but intercepted at a little different level. This is actually where we're taking our data. This is the cooling part of the wet bulb when this evaporating water--this will take about 24 hours before it gets back to the zero voltage reading, if you ever could get back to it to begin with.

Here is where we're pulling our intercept or our data points. All this data represents one single data point and one single data realization to put together a calibration curve. Each time we operate the sensor in the field, we collect exactly the same type of data that we did in the laboratory environment.

I thought I'd throw up a plot here that would show you what the results are from a calibration equation of a single psychrometer. This is not ten now. Everything you saw previous was ten psychrometers, or data points. So our residuals at about .9 of a bar in here are running ±.8 bars. Now we're over here about -73 bars and holding pretty steady. I think most of that is the resultant stability of electronics, to be able to hold that.

Our motivation for taking this thing wetter was to try to get out of having to work with the heat dissipation probe. We needed something. They are very difficult to install. They are nominally rated out about 1 to-- minus one to about minus six or seven bars. Previous literature will tell you psychrometers are good maybe down to a -3 bars or -5. The other option would have been a pressure transducer tensiometer. I had to look at the complications of trying to stick that in the borehole, and I think we've been able to solve the problem with the psychrometer now.

Here is a plot of the predicted observed value for that same psychrometer, so you can get some idea. If you had a perfect match between your predicted and observed, everything would fall on this line. So for all the datas, for all the isotherms that I showed you, this is the plot of predicted versus observed using our candidate model equation. We can do better, but we lose our estimate of error by interpolation. We can do better, but there's a lot of baggage to carry if we had incurred all those data points to do that kind of work.

The next thing I'd like to talk about quickly is the gas sampling program. One of our requirements is to bring gas up from these deep instrument stations. Their temperatures could go as high as about 37°C. Our surface environment temperature could drop down to zero. We could get to freezing up there, and while the gas is coming up, is getting cooler, as it gets cooler it wants to condense, so we had to come up with a scheme to lower the dew point temperature of the gas downstairs and ensure that it wouldn't condense as it's moving from deep environment to the surface.

There is some other ancillary information I suspect we're going to find as we start to run these gas sampling tests in each one of these sites. We can monitor the pressure and temperature inside the cavity while this is going on. If the flow is predominantly from the matrix, things should look pretty stable. If we start to get flow introduced from a fracture, or air coming from a distance, then we should start to see some different behavior occur. So there are some other things that may come out; no guarantee that that'll happen. We're pretty comfortable that we have the type of measurement and that we can actually do it.

This is what the scheme looks like. We did a mockup of this in the laboratory, which I'll tell you about in a minute, but this represents an instrument station at some depth in a borehole. We have one tube coming in which represents a dry gas carrier tube. In this case, it'll be dry nitrogen. It passes through a mass flow meter, a mass flow controller to measure the mass of flow into the system. This side of the system is under vacuum--the vacuum pump here. The assembly down here is blown up and I'll go to that in a minute, but basically, that gas mix is this vacuum withdrawal here, pressure under here, mixes together and comes back out.

We ran this in a laboratory, created our own saturator outside, assumed that we were at 100 per cent saturation in the saturator, ran this thing under controlled inflow and outflow, hung a thermistor inside of the saturator and say, "Well, how good can we do this?". We use the data to calculate the temperature in the saturator. We calculate it to ±.1°C inside the saturator, using the flow data, dew point temperatures that we're measuring, and we are able to control the condensation in the tube. Now, we were dealing with gas that was at 53°C dew point temperature, and it dropped it down to 23 over an almost infinite boundary. Our interest was making sure this would work in a very passive mode.

The pressure transducer in here, that served a dual function; one to measure pneumatic pressure in the cavity, and also to measure the pressure drop that's occurring here through a solenoid switch. Another solenoid valve here allows us to bring the wet gas in, bring the dry gas in, mix it in line--and these are teflon tubes, or impolen; we're looking at the two types of material--bring it up, go through a set of mass flow controllers and dew point hygrometer, then into a bank of traps for the hydrochemistry program, take out CO_2, take out water vapor, through the vacuum pump, discharge to atmosphere. All this is done in a controlled temperature environment, 23 ±4°C.

We are now building our first multiple station gas sampling rack, and probably we'll test that--run initial tests of that sometime in October. Right now, I want to show you where all this equipment is going. We've built a thing we call the down-hole instrument station apparatus. This is where all the sensors, electrical cable, and tubing will go. We had to keep it fairly compact. We had to make it, size it so that we could carry all our sensors in. It's all made out of plastic. All the plumbing is self-contained.

This is the component here where the dry gas tube and the wet gas tube, this is the little mixing chamber right in here. These are pressed together. This is a very high tolerance machine. These are pressed together and torqued together to seal that completely off, and this block is assembled to this block and holds one of the two or three-way solenoid valves. This block holds another one. There isn't much room in here, because you've got all this plumbing now internally drilled through all of this. This little groove in here carries all the electrical cable. Your first pressure transducer is housed in here, your second on this side, and they communicate through here so there's our double-pressure transducers. Psychrometers and thermistors are all carried in through here and are located down at the bottom of this thing. This is about one foot. Then the thing will be strapped onto a fiberglass stemming tube that I've talked about earlier.

This is just a little blow-up, again, of that front-end component of it. These are clean solenoids, so the solenoids will not contact any of the moving parts in there so we won't contaminate the gas with any hydrocarbons or some such thing like that, and that's the size of the assembly when it's all put together, so 16--approximately 16, a minimum of 16 per instrument station or borehole would go in, and that's what we call our downhole instrument station apparatus. It does all the work that I talked about; carries and houses all the sensors.

I'm getting pretty close to the end of one hour. I've got four more slides. I promised you to show you a couple of things that happened in G-Tunnel.

This is a snapshot of a record of water potential measurement and temperature, and the reason I'm showing you this is that we also--we have in situ recalibration. We have to turn a solenoid on to regulate the access of the pressure transducers to the gas stream. We turn that on over a weekend. These are two--the ones we used in G-Tunnel were 2-watt valves. We've dropped down to .65 watts now, but that little bit of thermal perturbation in here over a weekend caused a severe perturbation in the water potential measurement. This happened to be a very, very tight station. It's 120 feet in. There was no evidence of any fractures. It was isolated with packers, and you can see the amount of time. Here we're talking greater than 30+ days.

So our temperature before we turned the solenoid was 16.705. After turning it on, we spiked to 16.780. Our water potential went drier, which it should. We've raised the temperature, the ambient temperature of the environment and it should look drier. Relative humidity is getting lower. We went from 4.31 bars to 5.25 bars, so for a temperature change of .075°C, we saw almost a one bar change. It took almost a month to re-equilibrate, and we--this particular station was on a long-term seasonal cycle at 120 feet, and that seasonal cycle was about .01°C in a 12-month period. So it was on that cycle already. We hit it. We wanted to recalibrate the pressure transducers, and that's the response. Very, very tight station.

I think we're going to find stations in Yucca Mountain that are going to behave like that. I also think we're going to find stations that'll do something like this. This happened, also, to be in G-Tunnel. This was a station at ten-foot depth of vertical orientation, sandwiched with a station at five feet and a station at 15 feet. The five foot station and the 15 foot station did not behave like this at all.

I showed this last time, last time I gave the talk. This is the psychometric record, a very noisy, high frequency record that at first glance would say your psychrometer is wrong, something's wrong, but when you start to patch the data together, put temperature and pneumatic pressure together on the same graph--I have a snapshot of a window of 500 hours in here, 4400 to 4900--here's the psychometric up and down occurring over that period, but what we also have is we have some pressure waves coming in, and we have some temperature changes occurring that coincide exactly with introduction or exhaust or breathing of the station.

There's one high-angle vertical fracture through it. Other stations had fractures apparently closed; did not behave or respond in this manner at all. This station took about a week to re-equilibrate. That's how it's going to behave forever. It's going to see everything, and I believe that some of the sites that we're going to instrument at Yucca Mountain are going to show similar sorts of things. We went after the show-stoppers. So this system is wide open. It doesn't take long to equilibrate.

To wrap up, my view of the benefits of in situ monitoring: A chance to observe the dynamics of the system, look at episodic events, impact of diurnal, seasonal, and annual harmonics; obtain pneumatic measurements and pressure, or pneumatic pressure and temperature measurements. You can't get that from core. Look at equilibrium. We would hope that the repository or the units that we hope to protect the repository would be very dull, we would not see lots of changes. We wouldn't see much activity going on, all right? So negative information is positive information from that point of view and we're not going to get all excited. We're going to say it's tight, it's 10^{-15}, it's 10^{-16}. There is not anything happening there.

Evaluate equilibrium process. What I showed you about that little, you know, 2-watt solenoid going on for two days, that sort of thing. That's a very, very tight station. Other stations, immediate response.

Isolate discrete intervals of interest; here, fracture zones, stratigraphic and structural contacts, hydrogeologic boundaries; and lastly, provide a platform for isolation of rock gases for geochemical sampling.

Our immediate plans for the future, actually, I had hoped they would already be way underway, that we would have another three boreholes instrumented. That did not happen. I propose drilling three augered boreholes right next to the hydrologic research facility, Area 25, and set up a program to conduct a long term evaluation of the sensor drift characteristics. Here we're talking--I'm talking five to ten years, using the sensors that we've selected to use at Yucca Mountain.

This test bed facility would also provide us a training vehicle for gas sampling, in situ pressure transducer recalibration, water injection testing, and gas tracer testing program that we were going to do in the out years. The primary object here is evaluation of long term sensor drift characteristics. This particular scheme allows us to remove the sensors. We will not be able to characterize the real estate--we're not too interested in that--the real estate right next to it; more that we can isolate and put it into an environment as near like the environment that will be at Yucca Mountain.

In this particular case, three 40-foot deep augered holes, four instrument stations per borehole, solid stemming design, and removable sensor packages, and that concludes what I have to say.

DR. DOMENICO: Any questions, comments?

DR. SCANLON: I have some questions. Bridget Scanlon.

Do you--will you be getting--will you have some water in the grout? Will you be adding water to the system?

MR. ROUSSEAU: Yes, we will be. The grout will have to go under a very heavy slurry. Our preliminary tests that the people working on this have done is that the water that's available right now is all going into hydration. There is no loose water available to invade the matrix, at least that's what they're telling me at this stage. The stuff has got to flow pretty good. They will put retarders and things like that in it, but it's going to have water in it; absolutely.

DR. SCANLON: You didn't think--you changed your mind about epoxy, or--

MR. ROUSSEAU: Oh, we had some tests on epoxy. Epoxy dissolves.

DR. SCANLON: Pardon?

MR. ROUSSEAU: It dissolves. We put some epoxy in a container and put water in the container and let it sit there, and over a period of about eight months, it dissolves --the vapors dissolve the epoxy. So, yes, we've changed our mind about epoxies. Bad experience with epoxies?

DR. SCANLON: Pardon?

MR. ROUSSEAU: Did you have a bad experience?

DR. SCANLON: Well, I don't know. We haven't--

MR. ROUSSEAU: Oh, you haven't used it either, okay. No, we looked at it, and decided against it. And the other aspect of it was high heat, very high heat. The grout we're looking at, I think, will add an additional heat level to about 35°F for a short period of time, and then it'll come back down.

DR. SCANLON: What's the shallowest depth you are going to monitor?

MR. ROUSSEAU: Oh, I'll probably go something like five meters, something like that; ten. We're going to carry some temperature sensors all the way up the hole. We also have these other holes that have already been punched that if we can get them to open up, we'll go ahead and use them up. We'll use up the space and instrument. We've got plenty of data acquisition system capacity to take care of that sort of thing.

DR. SCANLON: Are you going to compare the results of your in situ monitoring with the lab measurements of water potential, when you take cores when you are drilling these holes?

MR. ROUSSEAU: Yes, we will, and the cores that are fairly wet we'd probably get pretty good agreement, and in cores that are going to come out dry, that have low porosity to begin with, are probably going to dry out significantly as a part of drilling. So I don't expect to get good corroboration with those types of cores. These would be your--

DR. SCANLON: But you're going to do it anyway?

MR. ROUSSEAU: Excuse me?

DR. SCANLON: You are going to do it anyway.

MR. ROUSSEAU: Yes. We will make the comparison, certainly.

DR. SCANLON: And Al Flint this morning talked a lot about going back and forth between modeling and his monitoring program, and have you looked at all the modeling results to see how sensitive models are to ranges in initial water potentials or how accurate you have to have these measurements?

MR. ROUSSEAU: Not to any degree. As I pointed out earlier, my primary focus or interest--again, I believe probably the quality of the data, the statement of accuracy is more than what the modeling program needs, but I also feel very strongly that if you can't explain what's happening locally, just from the physical concepts of what's happening locally, then you're probably never going to develop any confidence that the model that you've got on the other end of it is telling what's going on, either. I really feel strongly that you've got to be able to explain local phenomena, and in order to do that, you need this quality measurement.

In terms of integrating that measurement into a bigger picture model, you probably don't need it. It's probably not necessary, but the physical processes are what are important. Should we be modeling this thing as this kind of process; yes or no? And I think we get our yes's and no's by seeing if this data matched what we observe.

DR. SCANLON: Thanks.

DR. DOMENICO: This will give you the wherewithal to calculate a flux through the system, too; is that not true?

MR. ROUSSEAU: Well, you need the permeability, you need the moisture characteristic curves, which put together all the potentials, along with all the permeability data, then in a sense--and the infiltration data, and the other data--in a sense, you can do that, yes.

DR. DOMENICO: No, but this would be a necessary ingredient.

MR. ROUSSEAU: I would think so. I think you need to know what the fluid flow potential looks like.

DR. LANGMUIR: Langmuir. Let me come back to your grout thing.

I have a suspicion your calcite grout might be better than the gypsum grout, because the gypsum grout, you're already at saturation of calcite in the pore water. When you add gypsum, it's much more soluble, so you're driving calcium high, which is going to tend to precipitate more calcite, and therefore, scavenge Al Yang's CO_2 out. If you put in calcite grout, you're already at saturation with calcite grout. Not as much is going to happen.

MR. ROUSSEAU: I'm not sure I follow all that, because I'm not a hydrochemist, okay. Anything we put in there, they'll have a chance to hack at: I like it, I don't like it, we tested it for this, we tested it for that.

MS. NEWBURY: As Joe mentioned, Gary LeCain's going to be following his presentation with a presentation on air permeability testing.

MR. LeCAIN: As I've been introduced, I'm Gary LeCain. I'm going to talk to you about air permeability testing. I'm the PI of various testing that is all dealing with air permeability and the air permeability testing program. The program is basically broken into two parts. The first is the surface-based testing, which goes along with the talk Joe has been giving. Before he instruments his holes, I will go down with packer assemblies and inject air in single-hole and cross-hole tests, and basically measure the permeability.

This will be done with air and nitrogen in 12.25 inch diameter vertical holes. The primary goal is to characterize the geological units at Yucca Mountain. The second part is the exploratory studies borehole program. Again, we're going to measure permeability and anisotropy, single and cross-hole, air and/or nitrogen testing, only in this case we're using 4.25 inch diameter horizontal holes, and the primary goal of the exploratory studies borehole program is to test the contacts and the faults.

Just recently, we had Option 30 introduced into our study program, and I'm quite pleased with it. The ramps give access to more of the formations, contacts, and faults, and we have increased our proposed testing.

The air permeability test goals are to measure the Yucca Mountain volcanic tuff in situ matrix and fracture air permeability. We can talk about what I mean by matrix maybe later on that; to quantify the volcanic tuff heterogeneity and anisotropy, measure the air permeability of the Yucca Mountain faults, quantify the anisotropy of the faults, estimate the matrix and fracture in situ effective porosities, and overall, in summary, is to provide effective permeabilities and porosities that will aid in estimating water vapor and gas storage and transport at Yucca Mountain.

To start out our program, first we had to develop some methods, so we came up with the prototype air permeability test goals, and these goals are to develop and/or modify pneumatic equipment equipment and test procedures for conducting single and cross-hole tests; packers, pressure transducers, thermistors, thermocouple psychrometers, data loggers. We wanted to develop and/or modify existing procedures for pneumatic test analysis, single and cross-hole. We wanted to compare saturated air injection testing versus nitrogen injection to evaluate the possible drying influences of nitrogen.

We wanted to determine if the isothermal assumptions in our equations are reasonable. We needed to determine if the calculated permeabilities are independent of the injection rates and pressures. We also wanted to test along and across a geologic contact, along and across a fault, and conduct cross-hole gas tracer testing. These three items are temporarily postponed due to the closure of G-Tunnel, at least until we find another site to conduct the tests at.

The prototype single hole injection testing--now, this is just single hole testing--was to inject with saturated air and dry nitrogen, inject at variable flow rates, and use a test interval that is 3.1 meters long in the borehole. The borehole's about five inches in diameter.

We wanted to compare the calculated permeabilities using saturated air versus dry nitrogen. We wanted to determine if the calculated permeabilities are dependent on the injection rate, and to monitor the injection and monitor intervals to see if the system is isothermal.

Now, if I could have the slide projector--I don't think I want the fan to go off, do I?

(Pause.)

MR. LeCAIN: Okay. Can everybody see that? Is that focused, Alan? For your eyes, anyway?

Okay. This is the University of Arizona's Apache Leap tuff site where they have done some work for the DOE in unsaturated flow and volcanic tuff. This is their test site right here. It's an unsaturated zone, and they've covered it with a black plastic to prevent any moisture from getting down in there, because this area does get about 12 to 14 inches of rainfall a year. This is just a shot of the volcanic tuff that we're working in. It's a moderately welded tuff with fractures spaced anywhere from, oh, probably averaging around one major fracture every three meters in the boreholes.

This is the packer assembly we used for our prototype testing. This is an individual packer right here, with an inflatable gland. This is a test interval with all the little connecting tubes that go through it. One of our technicians, Chuck Warren, is putting this unit together to go down the hole. This is another shot of it; again, two packers with a monitoring interval that connects the two, and in this interval we've put our monitoring instruments, our pressure transducers, our thermistors, our thermocouple psychrometers.

This is some of the surface equipment we used for testing. You can see over here we've got compressed nitrogen for injecting. These are reels that contain the electrical wire and the inflation tubing for the individual packers. This is a control panel right here with eight wires coming out of it. Each one of them individually controls a packer, so we can inflate or deflate and change our test interval lengths.

This is actually four mass flow controllers inside one box here. We've got an inlet line insulated here where we're taking air from a compressor. We can set the mass flow controllers and that's basically what keeps hold--keeps account of our Q rate, whatever we want to inject in. This individual box is able to go from about one standard liter per minute up to 750 standard liters per minute.

This is the control box, again, for the packer inflation. What we're able to do here is inflate the packers downhole, keep an eye on the individual pressures in each packer, and then seal off each packer and then watch the pressure. This ensures that we don't have any leakage into our test intervals. I've been out in the field before and you thought you were getting a test, only to run out here and find one of your packers was leaking into one of your monitoring intervals.

These are two packer strings down two holes. There's Hole 1 that we're injecting into. This is Hole 2 that we're monitoring in. They're approximately ten meters apart, and as I said, we've got two packer strings, one down each hole.

This is the instrument bundle that we use in the monitoring zones. The large item is a 930 Druck pressure transducer, a thermistor, and a thermocouple psychrometer.

This is our data loggers we use to program and activate the downhole instruments and store the data. This is a little personal computer we take in the field with us to program the data loggers and down-load all the data onto. This is a current and a voltage source for running the thermistors, the pressure transducers, and the thermocouple psychrometers. Okay, that's it.

As I said, the goals of some testing we did in December of 1990, was to compare the calculated permeabilities using saturated air versus nitrogen, determine of the calculated permeabilities are dependent on injection rate, and monitor to make sure the system is isothermal.

This is single hole testing in this particular case. Here's a schematic of our single hole test system. Again, we have a gas source up here. It goes through a mass flow controller so we can keep track of our injection rate. We send it down. We have two guard intervals right here to make sure we're not just leaking past our packers, and ideally, it flows out into the injection zone.

In this case, I put some fractures in that shows you--you're never quite sure what you're testing down there. Initially, the flow out here is into the matrix or small fractures that may be present in the matrix, but you never can tell when you might flow up and get into a pretty major fracture. We had borehole television logs of these holes, but sometimes they don't quite show it all.

Here is a semi--a single hole semilog plot, pressure square differences, the pressure squared to compensate for the compressibility of the gas. You can see it came out very nice. This is our straight line solution. If we assume radial flow, basically, it follows a Theis curve. Draw a line through there, take the slope, we can calculate out a permeability. As I said, this is a single hole test, so we weren't even thinking about a porosity on it, just trying to get a permeability.

Here is the same data as we just looked at on a log-log plot, and you can see the characteristic slope of one here in the early data--this is about the first 20 seconds--which signifies well bore storage. Then it appears to start to follow a radial solution, and then it flattens out, goes to steady state here. I'd say we hit a boundary, probably; a major fracture.

DR. DOMENICO: Why the square of the pressure?

MR. LeCAIN: The pressure squared, when you--it's just a trick mathematically that allows you to linearize the system and work with a compressible fluid. I can sure show you the mathematics of it.

DR. DOMENICO: It's not the same as the simple Theis equation or--

MR. LeCAIN: It is the simple Theis equation. That--it's basically the same thing, only instead of using just the differential pressure, we square the two pressures and use that.

DR. DOMENICO: The permeability to air would be the same as the permeability to water?

MR. LeCAIN: No, no. I'll get into that a little bit here.

DR. DOMENICO: Okay.

MR. LeCAIN: In theory, permeability should be the same, but it never does quite come out the same. The results of the prototype single hole injection tests--now, all these assumptions inherent in the system, radial flow model, semilog analysis, 3.1 meter test interval--we were on an interval here without visible fractures--but, again, I can't guarantee that means it's not fractured. Permeabilities are in meters squared; flow rates in standard liters per minute.

See, we started out with our first test. We inject a 5 slpm. This is saturated air, air that's been run through a water column, basically, to increase the saturation, to bring the saturation up to, hopefully, saturated conditions. We can see a k of 6.4×10^{-16} m^2. We drop the flow rate to one. We get the same calculation. We went up to three, it changes a little bit, but error in our system, I mean, they're the same answers.

We then switched over to dry nitrogen injection, started out with 1 slpm, and we got a little lower permeability; went up to three, we're back right to the same range. However, Test No. 6, we jumped up to 8.1, kicked it up to 8 slpm and it stayed at 8.1. I'm not sure quite why it showed that peculiar rise and then flattening out, especially when the air injection seemed to give the same results.

Conclusions from the single hole tests is that testing showed small permeability differences between saturated air versus dry nitrogen tests. The saturated air injection tests were independent of injection rate. The dry nitrogen tests suggested increase in calculated permeabilities for the 1 to 5 slpm range, but the increase was not continuous up to the 8 slpm test. Temperature changes were limited to less than 0.2°C.

My next field trip out to the Apache Leap test site was for prototype cross-hole fracture flow testing. Here we were injecting with air at ambient temperature and humidity, injection at variable flow rates, two monitor zones at 10 and 10.1 meters distances from the injection zone. Monitor zones and injection zones are 1.2 meters long.

The goals of the 1990 cross-hole testing were to determine if the calculated permeabilities are independent of the injection rate--as we did in the single hole--measure temperature changes to determine if the system is isothermal, and evaluate the thermocouple psychrometer's ability to monitor the humidity changes during the testing.

Here's a schematic of our cross-hole testing system. Again, this was our single hole system right here, with an injection zone and two guard zones, only this time we're monitoring over in another hole, in three monitoring zones separated by four packers. I've just drawn this one to show injection on a fracture, which is what we looked for in this case, and flow up could pick it up in this zone, might get a detour and get some down in this zone. Maybe it'll make it down to that zone.

You're never quite sure what the exact flow system is you're working with. It's always a surprise to get on two fractures, same dip, same azimuth, two holes right next to each other, start injecting on one and not pick up a thing in the other one, and then have a lower zone which didn't show any fractures in your television logs or caliper logs, and all of a sudden it starts to show a response.

Here's data plotted from one of the cross-hole tests. This is the solution for spherical flow, complimentary air function solution right here, and this is the data from one of the monitoring zones. You can see it tends to follow it very nicely, assuming a spherical flow solution does work. As I said, we had 1.2 meter in length monitor and injection zones, and they're 10 meters apart, so we're probably getting into the range where a spherical flow solution would be expected to work.

Results from the prototype cross-hole fracture flow testing were very nice, very good. We started out injecting at 50 slpm, dropped to 23.4, then up to 74, then down to 13, and up to 98.5. We showed a pressure response in two monitoring zones, and a third one we got no pressure response in. Monitoring Zone 1, the calculations show 9.9 x 10-15 m^2 with a porosity of .25 per cent, not 25 per cent, a quarter of one per cent, and right on down the line, they all came out real close, real close, and so did the porosities.

Monitoring Zone 2 shows a calculated permeability for between the injection zone and this monitoring zone of about half of Monitoring Zone 1; 4.5, 4.7, 4.4, 4.6--this should be 4.2--and, again, a porosity of about half, a little less than half, of the other zone, but very consistent. I was very pleased with these results. They came out very, very nicely.

We also, at this same time, had thermocouple psychrometers in our monitoring zones. They're a little more difficult to get to work, I'm finding out. They're temperamental little beasts. This plot, labeled M3 here, micro volts output over days. This is our testing period for about six days. We put the equipment down the hole, inflated the packers. M3 showed no connection with our injection zone, and you can see it starts out high--this is dry--it becomes more and more saturated, starts out at somewhere about -50 bars potential, down to around 10 bars, -10 bars potential, and M3 shows how long it takes for these systems to come into equilibrium after we've inflated the packers.

M1 was one of the monitoring zones where we did have a connection with our injection zone. These little blips where it drops down are where we were doing injection tests, and we can actually see when the, not the pressure front, but the actual air makes it from the injection zone through the fracture system to our monitoring zone, and we get these little blips again. The depth, how much of a drop you got was dependent on the injection rate. The higher the injection rate--this was our greatest. This was the--one of the higher injection rates. This was 98.5 slpm. The little one right here, this one was 13 slpm. You don't see much--or this was 13 slpm.

The I3 zone, this was a guard interval located directly above the injection zone. Now, you can see it's very reactive to any time we started injecting air, which consequently means we were getting some leakage up into the guard zones. Now, I don't think it was leaking past the packers. I think we were just in a basically fractured zone and we had some connections that went up. I'm not sure. It could have been leaking past, but the response, the pressure response in the guard intervals was not instantaneous like you'd expect if you had a bad packer seal, just immediately to flow up in there. There was a time lag in there.

Here's the temperatures that we recorded in some of our zones. Here's M1 and M2, which are the two monitoring zones we did conduct tests in, and you can see the temperature stayed perfectly constant. There was no temperature change. This is the injection interval right here coming across. You can see when we start injecting, we got an immediate little drop in the temperature, but look at our scale here, in degrees Kelvin. I mean, we're talking about less than a half a degree change, really, and then slowly it starts to climb here. It's not like the temperature change you might expect from gas expansion or something like that.

What I think it is, is we started these tests very early in the morning when it was still nice and cool, and I think what you're actually seeing here is just the cooler gas going down, and as the day goes on and it warms up, the gas flowing through this hose, across that black plastic and down the hole, it starts to warm up.

Our conclusions from the cross-hole tests are: (1) The calculated permeabilities are independent of the injection rate for the range that we tested in. (2) Temperature changes in the injection zone were less than 0.5°C and no change was seen in the monitoring zone. This system appears to be behaving isothermally. (3) The thermocouple psychrometers did monitor the gas front reaching the monitoring zones; however, none of the thermocouple psychrometers reached equilibrium during the six days of testing.

When you consider that we're talking about test periods in a day, one single day, two days at the most, we might be out of luck when it comes to trying to measure humidities, relative humidities, transfer that to the potentials of the test intervals. We may just be out of luck. We'll have to go with the core samples and long-term monitoring.

DR. JONES: Gary?

MR. LeCAIN: Yes.

DR. JONES: Tim Jones.

These injections, or these permeability measurements take about a day, you said, to make?

MR. LeCAIN: In a cross-hole test, the test itself usually takes about six to eight hours, four to eight hours. The reason you need a full day, though, is you've got to let the system re-equilibrate before you can do the next test at a different rate, and that takes--well, the longer the better, you know; usually, generally, 14-16 hours. So it's a day for each test on that individual zone.

Now, if you're going to move, you can start another test up right away, assuming you're out of the range of influence of the last test.

DR. JONES: Is there a way to confirm that you're not changing the permeability there by removing the added water?

MR. LeCAIN: Right, by drying or forcing water out of the fractures?

DR. JONES: Yeah.

MR. LeCAIN: That's a worry. That was, again, part of the reasons on multiple tests. We did--we're not only looking for, is the injection rate having influence on the calculated permeabilities, but does the calculated permeability change with the injection rate? Do we blow water out of the fracture systems? That could always be a worry, and the only solution I would have is that given the assumption that the calculated permeabilities are not dependent on the injection rate, we'll do two tests at each zone.

DR. JONES: But they were for the dry nitrogen? They increased? It's the right direction, anyway, from drying them out as you go.

MR. LeCAIN: Right, right. I'm not--if it was something there, you'd think it would have gone on again to the fourth test and gone up, but it didn't, and the fact that the nitrogen tests all showed the same, I'm not so sure that it's actually there or it's something in our methods, and we'll have to check it. We'll have to repeat it.

DR. JONES: The psychrometer dip was wetting. Did that happen with the dry nitrogen, or just with the saturated water or air?

MR. LeCAIN: This particular one was for the dry nitrogen. The thermocouple psychrometers weren't working at the time I was doing the dry injection.

DR. JONES: So that water must have been coming from the rock, not from your gas, that the psychrometers were picking up at the test, or the--

MR. LeCAIN: Well, no. What's happening there, I think, is that what you're seeing is as time went on, the--after you inflated the packers, the zones started to wet up, come into equilibrium with the rock, and when the air would come by you'd see a drop because it basically transported fluid through there, and the drop--my only explanation would be that maybe it was flowing something along one of the fractures or something. I don't think it was actually drying out any of the formation, but it's a possibility.

DR. JONES: But you took water, added water to the--you raised the humidity in the packed off borehole as your air came through?

MR. LeCAIN: In those tests with the thermocouple psychrometers, we were using air at atmospheric humidity. We weren't going through a saturator on those.

DR. JONES: But the, I mean, when you injected air in your injection you had one humidity, and as that air arrived at the other side, it had a higher--it raised the humidity in that other section.

MR. LeCAIN: Right.

DR. JONES: So that was taking water out of the rock. I mean--

MR. LeCAIN: I'm not sure exactly what's happening there.

DR. JONES: Okay. One last thing, is there any compatibility between your instrumentation and what we just heard before so that, you know, before you could lower them both down or you could make measurements on exactly what the conditions are? I mean, you measure an in situ permeability. Now, what would you associate with? With a water content?

MR. LeCAIN: What does that mean?

DR. JONES: Can you measure that independently with this other instrumentation and then do your--

MR. LeCAIN: Well, originally, the thermocouple psychrometers were in there to try and get a potential of the rock, go back to the potential curves for that rock. You could come to some conclusion about permeability. Right now, we're--we generally think that the matrix permeability of these rocks and the Yucca Mountain rocks is very, very low; 10^{-17}, at least, let's say, less than 10^{-16} m^2. So generally, anything above that, we are measuring fracture flow.

The standard thinking has been, up 'til now, that you've got potentials such that most of the fractures that account for a majority of your permeability are dry. That's something we'll have to check. We'll use Alan's cores to see what kind of potentials he gets before we go in and do testing. We'll use Joe's long-term monitoring, assuming that comes back into equilibrium. There will always be the question of: Are you testing the equilibrium system--

DR. JONES: But the fracture permeability is relatively a constant that you just measure. It's not associated with a potential or a water content?

MR. LeCAIN: That's the common thinking of today right now. The fracture, the permeabilities that could really account for any gas flow or water vapor transport is basically dry at the present state at Yucca Mountain.

DR. JONES: Now, the porosities you calculated from your measurements, that--the .2 per cent and the .1 per cent--effectively fracture porosity. I mean, that's the porosity that is moving the gas that's traveling through?

MR. LeCAIN: Well, that's even too high--right. As was pointed out to me by some of my colleagues, that's even too high. Those--if you take and did some theoretical calculations on that amount of porosity in fracture, you should have higher permeabilities than we're showing there. I'm not sure exactly what it is. I can think of a number of scenarios that might cause that; easiest would be that you're on a large fracture that's intersecting quite a few large fractures, but you're monitoring zone is on a fairly small fracture that just taps into that large fracture. So you're getting a smaller response, let's say, for a tremendous more --for a larger amount of porosity.

Also, what about vertical fractures out here which we know exist in this site connecting you to the surface. Also, how much influence do all the different boreholes that they've got out here drilled into the--there are a number of boreholes in this area, and that's why it's not unusual to hit a constant head boundary out here, and they may explain it, but you're right in the assumption that what we're trying to measure there is fracture porosity, and we were hoping for sort of a double-hump response to actually be able to see a break in the curve where we go from fracture porosity or fracture storage gives way to matrix storage, and be able to identify--but we haven't seen it. We haven't seen it yet.

DR. JONES: Is there some kind of a long-term strategy to--I mean, right now I understand you're working on just developing the techniques and being able to measure at a particular place. Obviously, you can't make those measurements everywhere, so what--I mean, you've already indicated that the correlation, at least the cursory correlation between what you measure and what you see with the camera don't jive very well. Are there any other strategies on how to get these things from other properties, consistency within formations, or what--

MR. LeCAIN: What we plan to do at Yucca Mountain is we plan to use the drilling logs. Up 'til now, in their prototype drilling, they haven't had a real good handle on the amount of air they lose while they're drilling. What we really want out of them is those zones where they lost a lot of air. That's to start with.

Then we want a full set of geophysical logs, and our first couple holes out there will be a little more in-depthly (sic) studied and, hopefully, we can go back to all our logs and say, maybe now we start to see some correlations. Hopefully, the DOE is going to get a prototype hole out there sometime here soon, we hope, and we hope to get into that, and basically test the whole hole with the packer system; start at the bottom, test all the way up, and then go back to our logs, go back to the drilling records, and maybe we'll have a little better idea, one, of what the permeabilities of the formations are; and what to look for, most importantly, what to look for.

DR. JONES: Am I correct in interpreting this as this is method development? This is, you know, practice?

MR. LeCAIN: Right.

DR. JONES: This is trying to get how you're going to do it, and in a nutshell, or are you there? Are you ready to go if they had the holes out there? What's left--what do you think is left to do and have you ever--I mean, you haven't gotten to the point where you've started making measurements.

MR. LeCAIN: We haven't done as much as we plan on doing. We have several more prototype tests planned. We also have to prototype test the eight-inch and twelve-inch packer systems and support vehicles, but they all use the same instruments as this system that we're testing right there. Once we get the instruments down, we feel comfortable with the technical procedures and the methods, and we feel we've got at least somewhat of a handle on how to preliminarily analyze the data in the field, then we would be ready to go. It's--I would hesitate to say we have everything down until we've done those first three holes, basically, at the site.

DR. JONES: And you indicated to Pat that there was information that related the air to the water, and permeability to conductivity? Did I miss that, or is that coming up?

MR. LeCAIN: Oh, right, right. Yeah. No, no, I was just going to discuss that. What we can do is--what we've got here is effective permeabilities, just like you were saying there, dependent upon the moisture content. You can take that effective permeability, combined with the viscosity and density of whatever fluid you want to work with, and come up with a conductivity for air or for water, nitrogen, whatever. But I think we should remember that it is effective, the key word right there, the assumptions that go into those calculations, and that is that if we were saying it's fracture, we're assuming that the fracture is dried out.

DR. DOMENICO: Are you saying you can get that value, multiply it by the density and water and acceleration to gravity, and divide by the viscosity of water and you have come up with a hydraulic conductivity for water?

MR. LeCAIN: For--you can come up with a theoretical conductivity for anything you want.

DR. DOMENICO: But it should--will definitely be higher than it would be to water; would it not?

MR. LeCAIN: Yeah. My past experience is generally you could be within a magnitude. Work done here at this particular test site, Apache Leap, by Evans and Rasmussen, did exactly that. They did water injection and air injection, came up with a water permeability and an air permeability, and generally, they were off by about one order of magnitude. So it's a theoretical calculation.

DR. DOMENICO: Well, what does this--then what does this study impact? What problem does it address?

MR. LeCAIN: Well, it addresses the gas flow.

DR. DOMENICO: Gas diffusion?

MR. LeCAIN: Gas flow and transport, water vapor and gas transport throughout the mountain, from the repository, or gas flow down and towards the repository. One of the problems--this received fairly high notice from the--a group of people on the prioritization task force because of the C_{14} problem. Maybe we could say, okay, well, you're going to exceed the C_{14}, though you still want to know how much.

DR. DOMENICO: You can calculate a pneumatic diffusivity as well, and you probably have.

MR. LeCAIN: Um-hum, you could. I haven't, but it could be used for that. I don't think you can do much of anything as far as the long-term modeling of this without a good handle on the permeabilities, the in situ permeabilities of the mountain.

DR. JONES: Have you given up the correlation between the air and the water permeability?

MR. LeCAIN: One of the last things we're supposed to do here, following Joe's long-term monitoring, is water injection; and also, in some other long-term monitoring in the ramps or exploratory study facilities.

DR. JONES: But isn't the advantage of large scale air injection at Yucca Mountain versus large scale water injection at Yucca Mountain an advantage that makes it, you know, a worthy goal to try to be able to relate the gas permeabilities to hydraulic activity for water?

MR. LeCAIN: Um-hum, yes. Yes; definitely.

DR. JONES: I got the impression you had almost, you know, written that off, you know.

MR. LeCAIN: No, no. I haven't written that off because, like I said, at the very end of this we'll have all these zones that we've done air testing. We're going to come back once the long-term monitoring is done and do water injection at these intervals that have been stemmed for the long-term monitoring. After that, we should be able to have a comparison between our air permeabilities that we calculated, and a water permeability taken after the fact. We should have some good data there to take a look at.

DR. JONES: How do you even address the problem of scale of measurement or geometry of measurement?

MR. LeCAIN: It's a big problem. Our what they call the representative elementary volume, at what test interval are you actually--

DR. JONES: Have you got a straight shot in fracture, or--

MR. LeCAIN: Right. Are you in one fracture, or are you on a matrix?

DR. JONES: Or has 90 per cent of it gone somewhere else and you picked up one little thread, or--

MR. LeCAIN: Um-hum; right. Exactly.

DR. DEERE: A question, please; Don Deere.

Did you do profiling, where you have in a given hole these tests all the way down?

MR. LeCAIN: No, I didn't.

DR. DEERE: Because that should determine immediately what happens when you're in the fractures and when you're not.

MR. LeCAIN: That's exactly what we're going to do at Yucca Mountain. Our packer system is designed to vary in test interval, the larger ones for the 12-inch holes, from five feet to 55 feet, and what we will do is we'll test on up the hole at different test intervals until we reach a point where we think--well, things start to stabilize out and we start getting the same results.

DR. DEERE: Okay. I think we better move on to the next topic.

DR. WILLIAMS: Don, I have one more question; Roy Williams.

Are you sure you can't--you can do this without inclined boreholes?

MR. LeCAIN: That's a good question. Well, the faults at Yucca Mountain are predominantly vertical, and the chances of intersecting the faults with the surface-based drilling program would be much higher, giving a good test if we had inclined boreholes, but right now we've got the ramp coming in, two ramps, a north and a south ramp, which has drastically increased our opportunity to test on these faults, and right now we have 12 tests across and in the faults planned if the ramps go in as they're supposed to right now.

MS. NEWBURY: Okay. We're going to kind of switch topics a little bit and U-Sun Park is going to talk to us about gaseous and semi-volatile radionuclides and their release potential from the repository.

DR. DOMENICO: In view of the time that we're running into here, I'm going to waive the break, but there is fresh coffee back there and anybody who feels the need to get some and take a stroll, please do so, but I think we better keep going. I'll take the first stroll.

DR. PARK: Dr. Van Konynenburg of Livermore presented the gaseous radionuclide two years ago. Since then, actually, no new work has been conducted; however, during the last two years, gaseous radionuclide, especially Carbon-14, has received a lot of attention.

In addition, as the board members may have had a chance to be briefed, the surface-based testing task force report came up with the top two activities relating to the gas and the complex geology related to gaseous release, and out of 32, after going through the screening, still the gaseous release ranked the highest among the 14 surviving the prioritization evaluation.

Now, why is gaseous release so important? To begin with, Yucca Mountain site is in the unsaturated zone, which means a gaseous pathway could become the shortcut to the accessive (sic) environment, and as you well know, both the EPA and NRC regulations did not adequately consider release of gaseous radionuclides, and there is some effort by both EPA and the NRC to do something about that. We'll have to see later. And third item, I just mentioned that.

There is no single study addressing gaseous radionuclide. We draw results from much other studies and combine to assess the release of gaseous radionuclide in order to assess the relative importance of release. That is an overall objective, so to achieve this, we need to identify the data needs, as well as we need to develop study plans. And the results of these analyses and studies will provide input to test plans, test prioritization evaluation, as well as performance assessment to address compliance with the regulations.

First, I will briefly review the gaseous radionuclide and the release potential and what particular data we need, and what kind of test plans we need to obtain those data and also model them, and will summarize the results.

Probably you've seen this table before already. There are two differences. First, some radionuclides I haven't listed here which you saw before. That is because after the spent fuel is discharged from the nuclear reactor, the fission products and activation products decay very rapidly and actinide decays very slowly, and this is the total activity. However, the fission product activity drops several orders of magnitude in a very short time, and in terms of relative percentage, actinides, which starts about a little over ten per cent, becomes almost 98 per cent after about 400 years or 500 years, and relatively, the fission products which starts at close to 90 per cent drops to only a few per cent after about 400 years.

For this reason, I have dropped all those radionuclide Van Konynenburg presented earlier because they really don't have much significance. Another difference between what you saw before and this table is that I have divided the radionuclides into two groups; gaseous radionuclide and semi-volatile radionuclides. This is more the traditional nuclear industry jargon, because these gaseous radionuclides you see at ambient temperature. Semi-volatiles, you rarely see anything except during high temperature excursion in reactor accidents, or during the reprocessing we also tested a volaxation (sic) process in which we roasted spent fuel at the 500 C. Only then some of this came out, although they deposited close to the burner walls. They didn't travel very fast, very much.

DR. LANGMUIR: Langmuir.

I presume you've--you're discounting the aerosol problem? In other words, the release of these things by aerosol is not likely to occur. That's at least what has been written up in the past few years.

DR. PARK: Well, the aerosols--yes. They can be carried in an aerosol, but in the geological repository, we do have a geologic medium which will act as a filters, because even in the volaxation, 500°C, those--it is, you know, the highly, vigorously agitated, tumbling environment. On those, the particles didn't travel very far. And, you know, most of them have a relatively short half-life to be meaningful, because even the radionuclides with a 30-year half-life will decay a thousandfold within 300 years.

Among these radionuclides, truly, the gaseous radionuclides, radon is a little different from the rest and even though it has a very short half-life, I listed here because the radon that may come out from the spent fuel is very insignificant after it travels a certain time period. However, because of the thermal pulse, the radon locked in the geologic repository and overburdened itself, but that is natural radon--may be released in much larger quantity.

Now, they do not origin from the spent fuel. However, because of the emplacement of spent fuel, the release of radon may be accelerated, and it may give even higher dose than from other radionuclides from the spent fuel itself. So this is being studied by Dr. Pescatori (phonetic) at Brookhaven National Laboratory.

Now, then, how much quantity of these radionuclides do we have? Typically, we show the inventory in terms of curies per thousand metric ton; however, I converted this into 62,000 metric ton. That is the entire spent fuel inventory we expect to be emplaced at the--within the repository. With that total, what is the allowed weight, the EPA 1,000-year cumulative release? If you annualize it, it comes to about .62. That is from the entire repository.

And also, the NRC's post-containment release, which it defines at 10^{-5}, a 1,000-year inventory comes to about one curie for most of these radionuclide, except technetium. I'd like to also point out that iodine, even if you release the entire amount, is still below the EPA release limit.

I want you to pay attention that the annualized release limit for all these radionuclides is about one curie per year. Now, if we compare this with releases of same or similar radionuclides from other nuclear industries, you can see the relative magnitude. Now, this is not a table from this particular report or other. I took data from this particular report and compiled them, and since those data are not all--come from the same bases, some of them come from less number of nuclear reactors, I represented it in terms of a curie per giga watt year. The same goes with the reprocessing plant.

To give you some idea, in 1987, the total world nuclear power generation was about 189 giga watt year. One giga watt year is equivalent to one very large nuclear plant. So if you multiply 189, you can figure out how much curies of these radio elements we are releasing into the environment without affecting public health or violating any regulations.

Reprocessing plant is also represented as a curie per giga watt year, and for example, U.K., the Sellafield processing plant has about 2,000 metric ton, which is about 80. So it's equivalent to about 80 giga watt year, so if you multiply that, you can see they are releasing close to about the 7,000 curies a year, and that is the total inventory of fast release of Carbon-14 within the repository, and that is also the total release limit for the EPA for 10,000 years.

The point I'm trying to make here is the release limit on these gaseous radionuclide from EPA and NRC is orders of magnitude lower than comparable other nuclear facilities.

Now, then, what are their release potentials? Gaseous radionuclides can be released under both disturbed and undisturbed conditions. However, under disturbed conditions, the total number of waste packages affected is very small, and also, release due to defective waste package could be very small. Therefore, most of the gaseous release will come under undisturbed conditions, which is greatly influenced by its near-field environment.

Then what is the environment of the near-field? First, the repository will be located in an unsaturated zone, and the waste packages will see the peak temperature only 35 years after repository emplacement. The temperature will drop very rapidly in the first 300 years, and very slowly thereafter. And the same, the 300-year time period coincided with the period in which most waste packages are expected to remain intact. Therefore, if you look at the environment, probably the temperature we are really concerned with is about this time period, which is somewhere between 100 and 200°C.

Again, you've seen this view graph before. Essentially, the assessment of these radionuclides remains the same. The Cesium will not be present in any volatile form. Iodine could be volatile a little bit, and ^{14}Carbon would remain in gaseous form. And again, you've seen this before. Essentially, the carbon dioxide will remain in gaseous form under all the conditions. Iodine has relatively high vapor pressure at 200 C and it drops very rapidly at 100 C. The other two has fairly low vapor pressure.

I'll skip the next one. Now, from that previous view graph, it is quite clear that among those gaseous and semi-volatile radionuclides, only Carbon-14 and Iodine has very significant release potential, but the other two semi-volatiles do not have that high release potential.

Now, then the question is: Can we release gaseous radionuclides, primarily I-129 and C-14, without violating the EPA and NRC regulations? And the answer is, probably not for the reference conceptual design we have, conceptual design of the waste packages we have. The inventory and release potential for both radionuclides are too high, and we need a little more information and analysis.

Now, we can ask the same question on the semi-volatiles. Can we safely dismiss the release of semi-volatiles? And some of you have copies with a typographical error. It says "yes" here. The "yes" shouldn't be there. The answer is probably we can dismiss the release as radiologically insignificant. However, they may exceed the current EPA and NRC regulations. Now, this I have to clarify because in a strict sense, this is not really true, but what is meant here is that, as you know, EPA release limit is not a release limit for each individual radionuclide; rather, once you have released one particular radionuclide to its limit, then all the rest of the release has to go to zero. In that sense, if we do have some release here, then others has to be reduced. For that reason, we cannot completely dismiss their release at this point without any further information.

Then what kind of information do we need to address whether we can dismiss the release or not. The best way to identify the data needs is by looking at the release mechanisms. I'll not go through this. Essentially, the waste package has to breach and gases will leak out, either go through the near-field and far-field environment, and while it goes through the far-field environment, some retardation mechanism will act on it, and then eventually, it'll reach to the accessive environment.

For the C-14, the preliminary analysis show that that transport time is relatively short. Then, with that release mechanism, we need information in those steps. Basically, we need information in the four different groups. First, we have to know how much those radionuclides we have in the spent fuel, and we also have to know where they reside in spent fuel, what is their release potential.

We do have test plans at Livermore addressing spent fuel waste form and waste package environment. In true sense, these study plans do not cover these areas specifically; however, by the time that we actually conduct these tests, I believe there will be more specific tests added to address the gaseous and semi-volatile radionuclides.

For the waste container, we have to know the container breach rate and the cladding breach rate, and we do have two test plans at Lawrence Livermore at present.

Now, once those radionuclides come out of the waste package, then they have to be transported, which means we need to model the release and transport. We need some input from other studies. Primarily, we have to know the air circulation within the mountain which will carry these radionuclides, which will be addressed by next speaker, Ed Weeks, and there are some USGS, as well as the performance assessment modeling study plans listed here.

Now, once they travel through far-field, there are several potential retardation mechanisms existing in the mountain. The most likely retardation mechanism for C-14 is isotopic exchange with carbon dioxide, natural carbon dioxide in the mountain, which is in equilibrium with carbonate and bicarbonate ions. Dr. Al Yang will address some of the recent analyses on this subject tomorrow.

In conclusion, C-14 is the most significant gaseous radionuclide from a regulatory compliance, but not necessarily from health and safety point of view. The DOE is currently considering alternative strategies to resolve the C-14 issue, which includes the alternative EBS concept for which we had a workshop last week here in Denver, and we are also trying to resolve these gaseous radionuclide by conducting some of the studies mentioned earlier on a high priority basis. And one other possible solution to this is a rule change in the EPA and NRC.

The amount of release and the resulting health effects to population from both gaseous and semi-volatile radionuclides are expected to be very insignificant. The test plans and the data needs have been identified. They are largely in place; however, from--those studies will give us a better idea on the magnitude of the release, as well as their health effect, and their relative importance.

The current regulations for the release of gaseous radionuclides--primarily C-14, but it also applies to other gaseous radionuclides--is overly restrictive. A regulatory relief through the repromulgation of 40 CFR 191 would be the most cost-effective way to avoid costly solutions that provide no measurable benefits to health and safety to the public.

Thank you.

DR. DOMENICO: Domenico.

Could you comment on how severe this problem would be if the repository was kept at temperatures under the boiling point of water permanently? Would this still be a problem?

DR. PARK: Probably yes for C-14. That is because at this point experimental data is not really conclusive where that carbon dioxide comes from. There is a theory that there is enough oxygen inside the fuel, and they combined while the fuel is still hot to form carbon dioxide. We don't know yet because the tests conducted used the high purity helium. However, when I calculated the total amount of oxygen within that helium, it was enough to oxidize carbon at this--about a thousandfold of the same precise they used. So, still, it is not quite clear. They are trying to conduct another experiment using ultra high purity oxygen, or use the hydrogen or something, oxygen scavenger, but that test has never been conducted, primarily because of the budget concerns.

DR. LANGMUIR: Langmuir.

Just wondered if you were aware of any literature on isotopic exchange rates, because I--it occurred to me as well that C-14 exchange with C-12/13 in dissolved and solid carbonates might be a natural process that would scavenge the stuff out. Is there any data on these exchange rates with temperature?

DR. PARK: None that I know of; however, Dr. Al Yang will address that tomorrow. I think his analysis, recent analysis indicate the exchange may not be as high as we hoped for, and I think he's better qualified to answer that question.

MS. NEWBURY: Thank you, U-Sun.

We're going to push on now and have Ed Weeks talk to us about air circulation in Yucca Mountain.

MR. WEEKS: Well, it's a pleasure to be here. This way I'll get tangled up and trip, provide a little comic relief, and--

(Laughter.)

MR. WEEKS: Okay. I'm going to discuss the physical characteristics of gas circulation for Yucca Mountain, and leave the chemistry to Don Thorstenson. They always--whenever I talk about this and cover the chemistry, I get all the questions on chemistry that I can't answer, and if he gives it, I get all--he gets all the questions on flow.

This is an update of a talk that I gave 18 months ago that involved both liquid and water, or and gas flow through fractures. This time we're only going to talk about gas flow, and I want to emphasize that we're going to talk about two processes that we identified then, plus a new process.

The two that we identified at that time are changes in barometric pressures and topographically affected density driven flow, and I want to refresh your memories on what causes flow due to these effects.

For barometric flow, if the barometric pressure changes at land surface, it's going to take some period of time for that pressure pulse to be transmitted through less permeable rocks to highly permeable rocks at depth. It takes some time for the flow to transmit through less permeable beds to highly permeable rocks at depth, whereas that pressure can be transmitted instantaneously down the well so that we develop a pressure imbalance across the well bore or rock interface. If the barometric pressure increases, we'll have air entering the fractured rock; conversely, when the barometer reverses and we have a declining barometer, we'll have higher pressures in this rock and the well will exhaust.

One thing to keep in mind is that basically this is a compressive flow phenomenon. The air flushes back and forth but does not actually transport through the mountain.

The topographic effect, on the other hand, rises--if we have hilly terrain--from the fact that if we have, say, in wintertime, cold, dry, dense air extending from the fractured rock outcrop along the hill slope, up to the hill crest, we have a cold, dry and, hence, dense column of air moving through the fractured rock and up this well bore. We have much warmer water vapor saturated air that is, hence, much less dense, and if we think of a U-tube, this dense fluid pushes the lighter fluid out the well bore and we get, essentially, continuous discharge all winter long.

Moreover, this is an important transport process in that air is actually moving clear through the mountain, through the entire outcrop and out the well, or if the well weren't here, would still be going out the mountain crest. If we think of the barometric effect with this geometry, we'd have a pressure divide here. We would never get that kind of circulation.

Okay. About in January of 1986, we heard a presentation describing a dry well that John Carey had installed in the Snake River basalts in a bluff overlooking the Snake River at Twin Falls, Idaho. John built a greenhouse over that well, and the well blew warm air into his greenhouse all winter long.

Well, as soon as we heard about that, we felt that we should have a similar phenomenon going on in the two open boreholes that had been drilled at the crest of Yucca Mountain, a deep well, UZ6, penetrating to well below the canyon floor and Well UZ6S that is only 40 feet from the bluffs here, and drilled through the non-welded tuffs located here. We assumed that those should be showing that same phenomenon, at least to some extent.

Well, we got there and they were blowing like--in February--and they were blowing like crazy. This particular photo showed better as a slide than it does as a transparency, but this is flagging tied to a hammer and the exhaust out the well is keeping the flagging nearly vertical. Snow is on the ground, indicating that air temperatures are low. This would be for a flow velocity of about three meters per second.

Okay. We also measured the water vapor, which was water vapor saturated, as you might expect, and we found that the CO_2 was elevated relative to atmosphere about three and a half times atmosphere. On a typical winter day, we would discharge about 10,000 cubic meters of rock gas that would contain net water vapor of 100 liters. Some water vapor would enter the outcrops, and the net discharge of 2.3 kilograms of CO_2, indicating that, in fact, this could be a very significant transport process either for drying out the rocks, vapor discharge from the mountain, and also as a mechanism for transmitting gaseous radionuclides to the atmosphere and accessible environment.

As those of you that were here a year and a half ago remember, we had done a--we'd taken ten-day block averages flow to get away from the barometric effect, or to zero out the barometric effect, get a regression analysis of flow velocity from the well bore versus temperature, and got quite a good regression. However, we know from theory that we should have zero flow when the temperatures of the atmosphere and rock gas are equal, and yet, we have a very large offset. At that time, we told you we had no explanation for that.

Well, now, we have identified one more mechanism that should help explain that. We've found, in looking through our daily flow records, that every time we had high winds, we also had high flows, and we hypothesize that this arises because as wind, say, is striking the mountain from the west, as it hits the mountain there's a bluff or form drag effect resulting in high pressure. As it moves over the mountain, we get a lift effect, like an airfoil, and we get low pressure immediately over the crest. If we had an ideal fluid flowing over the mountain, we should have a pressure that's proportional to the wind velocity squared.

That didn't seem to bear out all the time, which frustrated me for some time, but then going through Schlichting's Boundary Layer Theory, I found that we should get back-flowing eddies, particularly near a sharp break such as occurs right at the crest of Yucca Mountain, boundary layer separation, and our Bernoulli equation breaks down. So we then felt that our theory probably was adequate.

In terms of air flow over mountains, we don't have anything, or very much on pressure build up, but we do have a result from flume studies using water in laboratory flumes. They basically stabilize this dune with epoxy, and put pilot tubes at various points perpendicular to the dune's surface, measured the pressure at these points reference to the dune peak and, in fact, we do find that we get a pressure build-up as here at the--along the side of the dune and the lowest pressure at the top of the dune. Here we're relating it to .05 row U^2, the Venturi effect equation indicating that, in fact, this phenomenon does occur.

Okay. To analyze our data, they went to a much more elaborate regression equation than we had with the ten-day block averages. Basically, we're going to correlate well-loss corrected flow velocity--and I'm going to come back to this well-loss correction--is equal to a constant plus a long-term barometric memory effect that probably represents leakage from the non-welded tuffs underlying the Tiva Canyon; a much larger coefficient representing short-term memory, air out of the fractures in the Tiva Canyon itself, a temperature term, and this is the term I want to emphasize right now, a wind influence function that is highly direction-dependent, due to the very irregular shape of Yucca Mountain, and to compromise U to 1.5 because U^2 didn't always work.

Fortunately, to make everything else fit, I have to go back to U^2, but our final projections are all made using $U^{1.5}$ power. I separated the data into 36 sets, overlapping sets of 20° sectors, did a regression analysis to determine that wind influence function versus direction. This shows that we get about a .05 x U^2 to get velocity if the wind's at 280°. This is while using 6S sub-polar coordinate plot, and if we lay this on top of our oblique and get the well, or get it centered directly over Well UZ6S, then I think we get a remarkable fit to the topography.

Wind coming straight on to the bluffs gives us the largest influence. We actually--this breaks down quite a bit in terms of the U^2 phenomena. Then as we're coming outward so that this partly an artifact of being at such an angle here, we get severe boundary layer separation. We get an improvement as we come over the bluffs this way, and fairly good response; declines sharply--I think I've got that rotated slightly. It should be breaking sharply here at the crest so that as wind blows down the ridge or up the ridge this way, we get very little effect.

We feel that this sharp V here is due to the cut bank of the UZ6 pad that's under natural topography, we'd have a shape like this, again, fitting the topography very nicely, diminishing sharply here, fairly constant as it comes across here, and so forth. So feel that the shape of this wind influence function versus the actual topography is quite a good representation of what the wind effect actually is.

Okay. Now I want to go back a minute and talk about the well loss correction factor. First of all, came to recognize that we'd need this in some early regressions where we find that plotting our flow adjusted for wind and barometric effects versus flow, we get an S-shaped curve, with the high absolute values of flow being attenuated. This suggests, in fact, that we are getting pressure losses in the well due to pressure friction losses, or due to turbulent flow up the well bore, as given by this equation, just the standard well loss equation, where the friction factor is a Reynolds number dependent--friction factor times constance times the velocity, flow velocity squared.

Okay. We substitute or compute a Reynolds number for one meter per second, which would kind of be a geometric mean value. We come up with a Reynolds number of 10^4. Well, it turns out, if we look at a friction versus Reynolds number graph, that, okay, we're really then from a value of about 3×10^{-3} up to about 10^5 if our roughness elements aren't too severe. We have a friction fracture that is proportional to the Reynolds number to the minus one-fourth power.

Okay. If we substitute--since going back to our equation, it's the friction loss was proportional to V^2, but since the Reynolds number is proportional to the velocity for isothermal flow, and we end up that the pressure losses, pressure flow losses are proportional to the velocity to the 1.75 power, and substituting in various parameters, we get a coefficient of .25, and through more algebraic manipulation, we can show that the well loss corrected velocity would be equal to the measured velocity plus .25 times the measure velocity to the 1.75 power. So that was all theoretical.

I might mention that it seemed, in the initial go-around, that actually the well-bore is pretty rough below about 30 feet. We ought to maybe just be able to get by with a constant times V^2, but that gave me too much correction and then I figured I better look at it more closely.

Okay. Let's look a little bit at how well we do on an hourly basis. Basically, I'm going to go from hours to days, to months, to years. If we incorporate wind effects, we get reasonably good result between predicting flow completely from barometric temperature, air temperature, and wind data, versus the measured flow, maybe over-predicting slightly here, but pretty much dead on here. Here we've missed some by--probably by restricting ourselves to one parameter wind velocity model. This direction and speed we did much better; actually, did a--had a pretty good simulation even on an hourly basis, whereas if we do not include wind effects, we over-predict during calm periods and under-predict quite substantially during windy periods, the over-prediction trying to compensate for the winds--and this we feel fairly confident that helps explain why we were coming up with such a large intercept or so much flow when we--temperature effects alone suggested they should be zero.

Okay. Next I want to show what happens if we look at diurnal effects. Because there are a lot of barometric and temperature effects kind of interfering with each other, we get some problems due to our hourly data, but if we take daily averages, plot them out, subtract out the wind and barometric effects from the measured flow or the well loss corrected flow, we get this diagram in which the X's include all of the errors due to barometric and wind effects, as well as temperature effects, still get a pretty good match, or a good fit; also, adjusted the temperature since we're referencing everything to 20°, adjusted to make the--essentially, each adjusted temperature degree has the same effect on density.

Okay. We still have a little offset, a little too much flow at our theoretical zero, but we're a lot closer. We also seem to have just a little bit of S-shape. Whether that's due to an inadequate wind function or too little well-bore correction, I'm not sure.

One thing, though, we are getting a real good fit to the temperature and I want to compare that magnitude on a theoretical basis to that that we would have with the wind effect. Okay. First of all, we do have an equation courtesy of the mine ventilation engineers for the pressure difference due to two separate isothermal columns of air of high delta Z, or this delta Z would be the height from the fractured rock outcrop to the crest of a hill. This would be the rock virtual temperature and the air virtual temperature, with virtual temperature being defined as the temperature that dry air would have to have to have the same density as the air at its prevailing moisture content. It's a ploy used by the meteorologists and mine ventilation engineers to handily incorporate the effects of moisture on air density.

Okay. So the two variables are delta Z and the difference in temperature, so first of all, we need a delta Z, and we have some flow logs for UZ6S that show that the percentage of flow versus depth is relatively constant, and for about half the flow comes in at a depth of a little over 60 feet, or just almost exactly at 20 meters. So if we insert that 20 meters into the equation, we come up with a .7 of a pascal per degree Celsius pressure difference, and since from the regression I just showed you we have .2 of a meter per second per degree C slope velocity curve, then our velocity in meters per second will be equal to 0.3 delta P.

Now, this effect of wind on circulation through a porous medium or through fracture rock, I couldn't find anything in the hydrologic literature, and the mine ventilation engineers hadn't seemed to have looked at it much, but it's a hot topic among the glaciologists right now. They're looking at it both to explain some phenomena at Agassiz Glacier, and to explain dry deposition in snow.

Okay. So they have an equation. If we can pretend that Yucca Mountain is a series of sinusoidal ridges, we have basically Jet Ridge to the west, Yucca Crest, and then Boundary Ridge, and so forth, so if we're fairly imaginative we can come up that perhaps it is a series of sinusoidally-shaped ridges.

Further, if we assume that we can express altitude as equal to H x 2πx divided by the wave length, then the pressure difference is just given by π, and I must emphasize that this is for H small relative to lambda; otherwise, you come up with some nonsensical conclusions.

$(\pi H/\text{lambda})\cos(2\pi x/\text{lambda})PU^2$. Okay, then so to compare that to, or make the comparison to our temperature effects, well, assume that air enters the mountain at the same point due to wind effects as it does for temperature; namely, 20 meters below the crest. Go to the topographic sheets and see that H should be 100 meters, perhaps, and lambda, 1500 meters. Substituting all of those in to our equations, or into Colbeck's equation, we come up with delta P equal to .13 times the wind velocity squared.

Okay. Then we have to go back and remember to convert that to a velocity out at the well. We need to multiply by .3, so .3 times .13 gives us .04, the velocity out the well is equal to .04 times the wind velocity squared, and if I put my hands on that wind influence function, the length of this vector is .05, and of this one is .03. So we basically bracket the .04. Moreover, the mountain is steeper on the west side and shallower on the east, so that, too, is consistent. We don't actually have a sine wave so much as a much steeper break here and a gentler one there, so I feel that that agreement is really quite good and does confirm to me, at least, that we're looking at the same mechanism.

Okay. Now, moving on, let's talk a little bit about how this breaks down on a monthly basis, and this is a slightly different graph. It's a three-bar graph rather than the one in your handouts. The maroon shows the temperature dependent flow. Notice it's quite high in winter, slides down, get fairly significant summer discharges. The blues are the wind effect flows. They're always positive. We always get most pressure at the crest and highest along the sides. In June, for example, it totally--it well over-compensated for the temperature effects. In July we did get net discharge to the--or net intake in the well. In August it balanced out, and then by September, we were getting small intakes, but it shows a relatively constant wind effect and very highly variable temperature effect based on season.

Okay. We also want to look at annual flow, and we want to look at two different annual summaries. First of all, I argue that natural flows--if we're going to use our well flows as the surrogate since we can't measure natural flow--to infer relative importance of the two mechanisms for producing natural flow, we might assume that escape from the fractures was basically a laminar flow phenomenon. Then the flows in the absence of well losses would show us the relative important of the two effects, and we find that the wind-based flow is responsible for about 30 per cent of the total flow; and, hence, temperature about 70 per cent.

If, on the other hand, we're concerned with the actual water and CO_2 balances, we'd need to be concerned with what actually blew out the wells. We find we have 800,000 kg due to temperature or density effects; 500,000 due to wind-based effects. In this case, because of the attenuation of high flows, wind effects become even more significant, counting for about 40 per cent of the total flow, resulting in a total air flux of 1.3 million kg. This is 30 per cent larger than I told you 18 months ago. We overestimated temperature effects to some extent, but--by ignoring the wind effects, we were underestimating total flow. Our net water vapor flux is now 16,000 kg and our net flux of carbon as CO_2 is 490 kg.

Both of these numbers are also 30 per cent larger than I reported 18 months ago, representing if we had a half a millimeter of recharge a year, this would represent the net recharge over a radius of about 75 meters. This might be the net flux of carbon over a 65-meter radius of root respiration; significant, but still within the realm of reason.

Okay. I have a long summary slide, but I've covered everything pretty well except the last point, which is that despite the fact that we've blown at least 5,000,000 cubic meters out of Well UZ6S, we've seen little or no change in rock gas chemistry, and that's why I'm glad I have a chemist to take over, because I find that really surprising.

My conclusion slide, I just say that wind and temperature effects are both important, and then I reiterate my excuses for spending all this time studying a phenomenon that's fascinating in its own right, basically, that we could both enhance the rate of gaseous radionuclide release and dry out the mountain.

All, I feel pretty confident now about our various explanations. I want to remind you, it's last year that I obviously got this all explained quite well. Just follow me into the cave, and everything will be okay. There are those in the back that 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.

So with that, I'll open it up for questions.

DR. DOMENICO: You gave some figures on the water flux, the water vapors. It was on a radius of 75 miles that represents--

MR. WEEKS: No, meters.

DR. DOMENICO: Say it to me again. What was it that you gave?

MR. WEEKS: Okay. I forget the exact numbers, but they're on the order of--if it's a half-millimeter a year recharge, it would take a 75-meter radius around Well UZ6S to supply that amount of water.

DR. DOMENICO: And you would suggest that this happens in vertical fractures; I mean, the well is a surrogate for your vertical fractures, so that a lot of the recharge, whatever--however it might be--may never penetrate far into that system?

MR. WEEKS: Yeah. That'd be our hypothesis, but one thing we have to keep in mind, this is for water that enters on the hill slopes and along the crest. Since a lot of the moisture might enter in the valley floor, it would probably--that water would probably escape this phenomenon.

DR. DOMENICO: And with all that wind blowing through the mountain, how come it's not dry; with all the evaporation going on?

MR. WEEKS: Seems like it should.

DR. DEERE: Don Deere.

I have a related question. If you didn't have the borehole blowing, you still get circulation through the mountain through the vertical fractures, but you haven't found that any place?

MR. WEEKS: Oh, not on the crest of Yucca Mountain itself. On two carbonate ridges--Devil's Hole Ridge and Point of Rock Ridge, which are about 30 kilometers south/southeast of Yucca Crest--there are a number of fractures that blow all winter long. Actually, Will Carr found these in October of 1985, so we'd have probably got onto this phenomenon without hearing about the greenhouse well, but on the Devil's Hole Ridge there are just two, but there are large swarms of fractures on the very crest of Point of Rock's Ridge that follow fracture patterns and they develop moss around them. They get slime growing from the moisture condensing.

Also, a fellow by the name of K.D. Johnson, who's now a helicopter repairman, has--went to Jet Ridge, camped out on the flank of Jet Ridge near Well H6, just west of where we were; went out with a thermal infrared imager and found some faults in Jet Ridge that were emanating warm air, and he--so that I think if we did an aerial survey in the far infrared, we might be able to find some.

We went out--as soon as we heard about it, we went out with our hand-held infrared gun and pointed it all over, and we couldn't find anything. But when you're walking around in the dark and it's cold, you can't cover too much ground. But we have seen the phenomenon--naturally occurring phenomena in the limestone ridges, and actually, John Carey got the idea for the dry well from his neighbors warming their fingers in air coming out of a fracture in an adjoining lot.

DR. JONES: Do you think the--you've taken one specific site and analyzed so that you, you know, you can describe pretty well what's happening there. Is the value of that because the actual data from this site can be used, you know, directly to calculate air flows through the mountain, or is the value that now the modelers who were modeling the air flow through the whole mountain could go to that specific site and use that to calibrate or to validate their models, or how do you really see what goes out the well as being, you know, used to explain what's going on in the heart of the mountain where the repository, you know, will be.

MR. WEEKS: Okay. Well, one thing, the repository--this is within the repository block. It's also probably fortuitously the point on the mountain where we have the most flow.

DR. DEERE: Could you put the cross-section up?

MR. WEEKS: The cross-section, or the--

DR. DEERE: Yeah, the Yucca Mountain or whatever you had there that--

MR. WEEKS: You want the photograph or the cross-section?

DR. DEERE: Cross-section would be better, I think. It would show where the repository is with respect to this welded tuff. The one that has the wind effect.

MR. WEEKS: Yeah. Okay. Yeah, the repository, at least as in the site characterization plan, comes--okay, it would basically be at about this horizon and coming close to the Solitario Canyon Fault and extending for a long distance in this direction.

The mountain is narrowest and steepest just a little ways to the south of this, so had we been designing a test of this phenomenon, we would have probably picked this site. So it was blind luck that they just happened to drill the wells there, and then the Weather Service, as soon as they had a road, put a station here with a ten meter tower to measure wind right on the crest, and actually, it's only 60 feet from our well. So, once again, blind luck really helped out.

But in answer to your question regarding modeling, I think that in order to extrapolate the results to natural fluxes through the mountain after the wells have stemmed, it is going to require modeling, but I feel that the modelers are going to have to be able to simulate what we've observed and then, of course, that'll give some validation to their model. But in terms of the repository as a whole, I think it will have to be determined from modeling.

DR. JONES: When you went through your analysis, I got the impression that you sort of slowly added more effects and, you know, and is that information--I mean, did you have a chance to sit down with the people who were developing the conceptual model for air flow through the mountain and say, "Listen, on this specific site, we had to take into account this kind of pressure differences across the surfaces. We had to have these wind effects. We had to have the temperature effects." I mean, is that all being transmitted, or any hope of that being--

MR. WEEKS: Well, I would hope so. It isn't--right now, we are trying to finish a report that describes all of this. We're still in the report preparation stage.

DR. JONES: Is this a candidate, I mean, is this UZ6 a candidate well to have the kind of characterization that we just heard about from Gary in characterizing air permeabilities and then try to simulate the varying phenomenon you've had with the surface boundary effects, or--

MR. WEEKS: I think that we'd actually have to look at UZ6S. UZ6 was drilled with a 24-inch bit down to 380 feet. It continually caved. They put a 20-inch casing in it, poured a couple hundred feet of cement in the bottom to stabilize the rubble. The cement all kind of went away, and so we've got an open hole here, plus huge washouts or whatever you want to call them with a reverse air vacuum, but a very irregular hole. I think UZ6S would be a quite viable candidate.

DR. JONES: That's the well that you've been measuring the air flow out of, is the 6S?

MR. WEEKS: Right. Yeah.

DR. JONES: That's what I meant, was using that well, and your equation--I don't know enough about this, but it--I mean, is there a conductivity or a permeability hidden in that equation somewhere that you could--

MR. WEEKS: Yes, there is. I didn't--

DR. JONES: Can you back it out directly, or can you just use it sort of empirically to judge in a more traditional way?

MR. WEEKS: Well, okay. I was going to present it but then I thought it'd take too much time. If we assume that the canyon edge is a stream and invoke well hydrology, plug everything in, we come up--well, with a lot of assumptions, we come up that we need a permeability of 5 x 10^{-9} m^2 to support that flow, which interestingly enough, differs as compared to 4 x 10^{-9} that the glaciologists were using for their snow circulation. It's a high permeability. It would, for uniformly meter-spaced fractures, require about a 2 mm aperture.

On the other hand, when you look at the TV log of Well UZ6S, there are some very large fractures. It's extremely badly fractured from the upper cliff unit on down. The upper cliff unit is a verge which extends about from here to the surface. Upper cliff glass cap rock is virtually unfractured, and there's only one fracture and essentially no flow coming in, then it's highly fractured throughout most of the rest of the section.

And so, I think, in fact, I was thinking of photographing the TV log and showing it. I think it's easy to believe that permeability if you look at the TV log, all the fractures.

DR. JONES: Is there any problem with--people have alluded to the permitting problems and getting on site. Is there--are there those kind of problems that would prevent you from, you know, taking your equipment and going right to that well and using, you know, starting that right away, or--

MR. LeCAIN: Well, we hope to be out and actually testing UZ6S. We hope to, this next spring, get out on UZ6S with our packer systems and go down there and see if we can find the permeable zones.

DR. DOMENICO: Ed, this is the beginning of a nice, conceptual understanding of the natural flow in that mountain, I presume. You're going to continue some studies. Do you have any plans to do it on the natural fracture system, or how do you plan to extend these? Because it seems to me this is a very important component of the advective system, you know. The flux through the mountain may not be--may be smaller than some people even think.

MR. WEEKS: Well, yeah, I agree that it's important. Part of my problem was that while we was making all these measurements, I kept being told to stop and to show them that I didn't have to pay any attention to them, I continued until finally I realized that I had a lot more data that I hadn't looked at, and so basically I had to discipline myself to try to figure out all we know and what we don't know, and what would be the next course of action, and who should conduct additional studies.

Right now, Tim's certainly right. We just kind of stumbled along, step-by-step, and saw some of my notes from last September. I was really frustrated at some of the--what still seem to be discrepancies.

DR. JONES: There's a lot of us that would like to stumble as well as that through our research.

MR. WEEKS: But yeah, I think that once we have a little more analysis, it would be important to develop plans for future study, whoever were to conduct them out. I'm right now kind of uncertain what.

DR. DEERE: Don Deere again.

But you do have a confining bed there. It was after you penetrated with the bore in the confining bed that got the flow. You short-circuit it.

MR. WEEKS: Right.

DR. DEERE: So if we take the boreholes out, the fractures are not being very effective, because once you put something in that had a high vertical conductivity, you completely start a circulation system. So when we come in with our inclined accesses and get into that unit, we again give an outlet to the barometric pressure at the opening there, and we may have a very large driving force in there. I mean, am I right?

MR. WEEKS: Right. Yeah, I was one time speculating we ought to make the ventilation shaft right in the crest and let the heat just blow water and air out there at a great rate, if we could ignore the CO_2 problem.

DR. DEERE: Well, I'm not going to laugh at that, because I can see that it could affect the cooling rates that have been proposed.

MR. WEEKS: Right, right.

DR. DEERE: I think it could have a great effect on that. Maybe you want to take advantage of it.

MR. WEEKS: Right. I agree.

DR. DOMENICO: I think maybe we ought to hear about the chemical aspects now, or did you tell us that already?

MR. WEEKS: No, no.

DR. THORSTENSON: You're going to see, essentially, all in graphical form, all of the chemical data that we've got. That's what's new since we last talked to you. I gather that the FAX machine ate my bibliography. This proves that I exist.

We need to look a little bit--refresh our memories a little bit about the boreholes, the boreholes in question. We're going to be talking about the data from UZ6S--which Ed just spoke about at length--from a series of nine neutron logging holes along the crest of the mountain from Borehole UZ6, the deep borehole, and some data that comes out of Al Yang's project, collected by Al and Charlie Peters and a variety of people at UZ1, which is about two miles--not kilometers--to the north/northeast.

Cross-section, the only purpose for this, really, is to point out that the stratigraphic interval roughly sampled by UZ1 is as shown in the cross-section. I guess I'm better off waving at the screen here. Most of the UZ1 sampling intervals, in fact, are within the Topopah. It does occur in a topographic low in Drillhole Wash, and presumably would not be subject to topographic effects even if it was not filled up with grout and instruments.

We also didn't say much last time we talked to you about our conceptual model that sort of is the basis for our interpretations of what does or doesn't happen in terms of the chemistry of the mountain, and that's what you're looking at here. This is basically what we think diffusion-dominated gas transport should produce in terms of CO_2 profiles in a soil zone. The data here are from a paper by Reardon, Allison, and Fritz back in '78. It's the first data set of its sort that we're aware of. The model was an attempt--quite successful, although I'm not showing you the data--to model some unsaturated zone stuff that Ed and I were doing in North Dakota.

A key point here is that CO_2 is generated shallow in the system. We assumed one meter in the model, largely by process of root respiration. It's put in there by the plants, and its productivity, obviously varies seasonally; low in winter, zero in North Dakota, high in the summertime, so that what you get out of this is that the input of CO_2 to the unsaturated zone basically occurs during the summer.

Now, this can be, obviously, influenced if you've got advection to help it along. The assumptions that are built into this, as I say, are diffusive.

DR. LANGMUIR: Don, just a quick question.

DR. THORSTENSON: Yeah.

DR. LANGMUIR: Can you discount the possibility of microbiological activity at depth having a significant input to this?

DR. THORSTENSON: Oh, absolutely not.

DR. LANGMUIR: So it could. You could still have that going on?

DR. THORSTENSON: Yeah. Next overhead.

So basically, you get CO_2 flux out of the soil zone all the time, into the unsaturated zone on a periodic basis. And I've provided Don with a bunch of prompts here, see? What can happen to that CO_2 and the things that we're interested in, obviously, is to look at its isotopic signature, because that's where we'll spend all our time trying to interpret in terms of data at Yucca Mountain. It's a little bit of an artificial separation here, but the factors involved are basically the following:

CO_2 comes in via root respiration, so it's a function of plant metabolism. It's a function of a balance between CO_2 production rates and diffusion. This is basically Thure Cerling's contribution to the conceptualizing of this thing. These effects also influence C-14, but you don't--geochemists do not generally worry about the effects because the fractionations are in per mill. 20 per mill is only 2 per cent. You usually don't worry about ±1, 2, 3 per cent in terms of trying to interpret C-14.

What can happen to the CO_2 subsequently? It can react, and if it reacts with sources or puts into sinks carbon of different isotope, different isotopic composition than what's in the gas phase, then what's in the gas is obviously going to change. Diffusion may be a factor; that is, the downward diffusion as this stuff works its way into the unsaturated zone. We don't know the answer to that because we have not yet modeled it.

The same factors affect C-14 with the addition of time, and there's really two different ways that you have to think about time in this. One is time as in old, pre-bomb age; the other is post-bomb time. Where does the C-14 abundance fall in terms of the atmospheric input signal, if you want to call it that.

Most of the conclusions that I am going to be talking about here are sort of the 2 x 4 and a donkey kind of thing, baseball bat and a geochemist. They're not subtle. If nothing changes, it's likely that nothing is reacting. That's the fundamental concept that I'm going to try and get across here.

There is another place where that process seems to occur, which is in alluvium in Jackass Flats. There are several hundred measurements represented here. I think you can see the shape of the data envelope is essentially identical both to the other data and to the model that we've put together, suggesting that out here in the alluvium, diffusion is at least the primary process involved in what's happening. Note that CO_2 concentrations, the mean annual concentrations are quite low. Biological productivity in Jackass Flats is not overabundant; talking about roughly a tenth of a percent.

Also to be noted, although we did not make a detailed isotopic study, is that in these deeper samples, we find C-14 at pre-bomb levels at the same time--I should have written this the other way around--the C-13 is getting heavier. It suggests that there has been or is going on--take your pick--some reaction between the CO_2 gas and the soil carbonates in this alluvial system.

So now we want to sort of try and talk our way down this profile at Yucca Mountain, and where to start is the shallow soil gas data. There aren't many soils deeper than about a foot on Yucca Mountain, and comparing the 30 cm or shallower data, all of the data collected--variety of times, variety of seasons--from both the shallow soils on Yucca Mountain, from equivalent depths out on Jackass Flats, I would say the primary conclusion that you have to come to is that the general data envelopes for the two are the same. Are they absolutely identical? No. There's a hint that the soil gases, particularly these in here from Yucca Mountain, may be a little bit higher in CO_2 than those on the caisson, but I would say, overall, you've got to say first cut, not different.

We do not have, unfortunately--Alan Flint mentioned the same problem in terms of looking at some of the water vapor stuff--we don't have any data from one foot to six meters. Six meters is the shallowest of the neutron logging holes that currently exist at Yucca Mountain crest.

Okay. This slide precedes this. This goes with the figure I'm going to show you next. In the neutron holes, as I said, we've got data from the soil zones and we have a five meter interval in which we simply have no data. We presume those higher CO_2 contents to be present in essentially filled fractures, fractures with roots in them, et cetera, in the unsaturated zone, but that is an assumption. We do not know that.

When we hit the neutron holes, we look at the neutron holes at Yucca Mountain crest. These are samples, multiple samples per year for three years--in some cases, four. With one exception, they're all basically running in about .12 per cent CO_2. These are sampled in the springtime while UZ6 and at least some neutron holes are abundantly blowing. Notice that this, in fact, is quite close to the mean annual concentrations that we saw at ten meters out in Jackass Flats.

I think what this is basically saying is simply the overall biological regime that we're looking at on Yucca Mountain is not substantially different in terms of what it's putting into the system as far as CO_2 abundance than, in fact, what we're seeing out in Jackass Flats. I don't think that's particularly unreasonable.

I need to talk a little bit about--we never have fights in here. We have tiffs amongst ourselves. What this figure is purporting to show you is a comparison of CO_2 concentrations as a function of depth from two different locations. One is Yucca Mountain crest with UZ6 and UZ6S and the neutron holes and, for that matter, the soil zone. The other access on the left are depths in the instrumented borehole UZ1, and I simply tied these stratigraphically at the top of the Topopah.

The dashed lines here are misleading. I simply didn't know how to make this slide. You'll see this several times in the next coming slides. So what we're seeing here, what this is supposed to be telling you is that at the crest of the mountain, UZ6, UZ6S, et cetera, the non-welded tuffs are roughly in this interval. At UZ1 they are essentially 80, 70--70 meters, roughly, thick. It does not necessarily imply stratigraphic continuity between the two. Like I say, I just wasn't quite sure how else to do this.

And so what do you see as you go down the mountain? Here's the CO_2 data, really should extend out to about here. The data from the neutron holes essentially just about at the same mean annual concentration as we saw it in Jackass Flats. All of the UZ6 data, you're looking at four to five years of measurements; generally, five to ten repetitive samples per year. There's a lot of numbers in this slide.

And what we see is roughly uniform CO_2 content in the fractured tuffs of the Tiva Canyon until you get down here into the non-welded Paintbrush at 6 and 6S. At UZ1 we have very high CO_2 concentrations. We're not sure exactly why. We believe these high CO_2's are basically surface effects. We think they've got to do with buried plant material on the drill pad.

The point here is we're going to look, basically, at three things: CO_2, C-13, C-14. We're looking at CO_2 through the mountain, and basically what it's telling you is that in the Tiva Canyon, until you hit the non-welded, in the Topopah in UZ1, as well as the samples that we've now gotten out of UZ6, until you get down near the bottom of UZ1, this appearing to be drilling fluid contamination from G1. Finally, the deepest samples from UZ6, down well within the non-welded tuffs--again, much, much different range in CO_2

concentrations. The CO_2 chemistry in the non-welded tuffs is different than it is in the fractured tuffs, and in the fractured tuffs it appears to be essentially uniform throughout the mountain.

I have to give you my own slide of this because the hazards of last-minute reproduction, something bad happened. I don't believe that your copies have any of the shallow data up here, and in fact, all of the data are shifted about four per mill to the light end. These things are supposed to be up here at about -18 as opposed to -22. So here--presumably we can get this copied for you, but this is what you need to look at, not what's in your handout.

Carbon-13, again, from neutron holes, UZ6S, all samples multi-years, repetitive samples, year-to-year, different sampling techniques, and you see, again, in the shallow, fractured tuffs essentially constant Carbon-13, little bit of swap. Keep in mind that the range of things that are available to react are from near zero in near-surface soil carbonates, -4 to -8 in fracture-filling carbonates, about -7 to -12 in most of the Yucca Mountain groundwaters; air at -8½ on the mountain. If there were any organic material in the system, presumably it would be somewhere in the -20, -25.

So the contention here is that in looking at the open borehole data--and in this case, through the non-welded tuffs--little change in Carbon-13. Again, we see some jumping around here in UZ1. Whether you want to say, is this a significant shift or not, something's going on here, it would appear. We don't know what, but the argument that I'm putting forth here is, again, first crack, you've got to say, hey, given the potential range of Carbon-13 and the sources and sinks, taken as a whole, things look pretty constant.

The reason for emphasizing that is no evidence--at least in our opinion--that we're seeing any systematic reactions with either time or depth in the system.

DR. LANGMUIR: This presumably must relate to the permeability, the gas permeability as well as the rates of the processes, too.

DR. THORSTENSON: Yes.

DR. LANGMUIR: It should tie into that.

DR. THORSTENSON: As we see--

DR. LANGMUIR: Did I prompt you that time?

DR. THORSTENSON: Yeah, you did. When we look at Carbon-14--and again, I'll put the figure up here--this is kind of--this is sort of what, at least in terms of the geochemistry, the CO_2 chemistry, this is what the game's supposed to be all about, what's Carbon-14 doing, because that is what potentially puts the time signature on the data.

All of the neutron hole data, with one exception--and this is an outlier point, one of three from the same hole. Why it's different, we have no idea--all of the neutron hole data, all of the UZ6S data down to 110 meters are greater than 105 per cent modern carbon. The analytical precision on this is essentially better than counting statistics. The hole, the UZ6 is reproducible--I mean, the counting statistics are essentially plus or minus a half a per cent to plus or minus one per cent, and that's the sort of reproducibility that we're getting from year to year in looking at the UZ6S data.

UZ6, once we started flushing it and then pumped during sampling, we've got pre-bomb carbon throughout. This is simply UZ6 gas in the casing that Ed mentioned, as it's moving up and out the hole at about 1,000 cubic meters a day, which was roughly our pumping rate.

Three years' worth of measurements down here at 550 meters in UZ6, all at about 50 per cent modern, and then, again, UZ1 up here in the non-welded and, in this case, near surface, again, once we're into the Topopah, UZ1 Carbon-14, in fact, is declining quite systematically. UZ6 is pre-bomb. The conclusion from this seems, to us, unavoidable. Something very different is going on up here in the Tiva Canyon than is going on beneath the welded Paintbrush tuffs--non-welded, excuse me.

We have data from this spring. Unfortunately, they are not analyzed, not even one single number, as of Friday.

What we are hoping very much to see is that with additional flushing, that the C-14 contents of UZ6 gas begin to approach more closely those of UZ1, but that's a presumption. We don't know if that's going to happen. We hope that that's what we see.

DR. LANGMUIR: Langmuir again.

What kinds of corrected dates can you attach to those C-14 values?

DR. THORSTENSON: If you make the assumption that basically you're looking not at reaction, but at time, okay--and that's where the importance of the C-13 comes in--if you look at the lower UZ1 dates, you're talking about 10,000 years. These three, 50 per cent modern, you're talking about 5700 years, if those represent time. And, as I'm saying, I'm proposing first approximation. We have no consistent--I'd say very little indication of any systematic geochemical reactions to say look at something other than time, you know, or transport, you know, a variety of questions you can ask.

But what I am saying is that I think the evidence that I've just been showing you here is very strong that these changes, that the difference from here in the Tiva to here below it are not due to chemical reaction. They're representing difference in transport regimes. They're representing difference in time. I don't believe them to be representing reactions that we have not made appropriate corrections for in terms of trying to model these things.

Okay, a sketch which you don't have. It occurred to Ed and I that we haven't really put any sort of a picture up of kind of what do we think is happening overall in the mountain, and the major conclusions that we think come out of the combined physical and chemical studies in the Tiva Canyon above the non-welded tuffs.

All of the physics says once you put the borehole in there to act as a conduit, gas moves fast; a thousand--a million cubic meters a year, et cetera. Above 110 meters, every measurement that we've made in the Tiva Canyon from UZ6, UZ6S, nine neutron holes over four to five years shows post-bomb Carbon-14 significantly.

So it's very difficult for me to see, for us to see any conclusion other than if you end up getting repository generated Carbon-14 dioxide into the shallow Tiva Canyon system, I think the burden of proof lies on those who would maintain that it will stay there. It's not clear how you can get bomb Carbon-14 dioxide in and not let repository Carbon-14 dioxide out, regardless, totally regardless of whatever numerical values you want to put on retardation factors in the models, et cetera, et cetera. You can't argue the fact that all of this stuff showed up within 30 years; quite possibly, lots less than that.

DR. DOMENICO: What happened to the conclusion that the

Paintbrush tuff was a significant barrier to gas flow?

DR. THORSTENSON: I think maybe I'm simply not stating my case very clearly. I think that this is saying that very strongly. It's saying we have a tremendous amount of Carbon-14 dioxide everywhere that we've looked, both in UZ6S, all the neutron holes. We've got some soil gas measurements, et cetera. You see only post-bomb Carbon-14 in the Tiva. You begin to see pre-bomb--whether by age or different chemistry, hard to say, but you begin to see pre-bomb in the intervening non-welded. When you're down in the Topopah, you see definitely pre-bomb all the way down UZ6, 50 per cent modern at its bottom, and in UZ1, Carbon-14's that, if interpreted directly as ages, would give you 10,000 years.

So, I mean, our conclusion from that is that the time scale of the circulation regimes are very different, either time scale or the physics, the flow paths. Something's very different, and we don't appear to see any significant gaseous communication between the two.

DR. DOMENICO: Those are flow patterns for the gas; correct?

DR. THORSTENSON: Right; sketched.

DR. DOMENICO: A sketch for the gas.

DR. THORSTENSON: And let me make sure we all understand the ball park we're playing in here. We don't know for sure which side of the mountain the gas is coming from.

DR. DOMENICO: Okay.

DR. THORSTENSON: We have a variety of indirect evidence to suggest the majority of it is coming from the east side, but it is indirect. We do not have definitive evidence that it's not coming from the obvious, which is the cliff face. The absence of any atmospheric post-bomb C-14 in the UZ6 samples suggests that if there is any significant outcrop associated flow, it's at least not getting to where UZ6 is.

Now, UZ1, you could argue from a set of different perspectives because you wouldn't necessarily expect it to be subject to the topographic effect because of the physiographic setting, et cetera, and that, I think, I already said.

We've got physical and chemical data. They're in some extra slides in the back of the handouts. Time is short. We've got flow data from UZ6. There's a tremendous amount of air circulation in UZ6, and it goes into particular permeable zones in the Topopah. That was the basis for locating the sampling intervals.

You've got compositional diagrams that say, hey, basically, the rock gas in the fractured tuffs--not in the non-welded, but in the fractured tuffs--looks the same in both intervals. Nevertheless, in spite of the fact that you've got--now, how much is a lot? A lot is what goes down a half-meter diameter hole at velocities of one to two to three meters a second. That's a lot. That's a lot of gas, and a great deal more from cross-formational flow. But nevertheless, we don't see the atmospheric input of the bomb signalled in the gas.

So under natural conditions, it's pretty hard--at least for us--to conclude anything other than whatever it is, something's different in the Topopah. There doesn't appear to be anything chemically different. The only chemistry that's different is C-14. CO_2 content's roughly the same. CO_2 methane profiles look roughly the same. Carbon-13 contents are roughly the same. So the assumption is, basically, no reason for retardation factors to be any different in the Topopah than they are in the Tiva.

The only thing that's left to be different is something connected with the physical system, I would say, is the--is sort of the primary conclusion to be drawn there. What happens when you put a repository in there remains to be seen.

In response to the modeling question with respect to Ed, I mean, I think the answer is the same here. I mean, you've got to be able to say something about what's going on, given all of this data, and explain it at least to the extent that the data exists before you're going to start changing temperature regimes, retardation factors, et cetera, and then say, oh, here's what's going to happen when we put a 250°

repository in here. This is my own personal opinion, and I suspect it's time for me to quit, too.

DR. DOMENICO: Any questions?

DR. JONES: Yeah. Tim Jones again.

Are you--you've got bomb C-14 in that top zone and it's not gotten down below. You're suggesting that there's a restricting layer preventing it from going down, or is it just fortuitous that in its natural progression downward, that's as far as it should have gotten?

DR. THORSTENSON: How to try and answer this. Ed, jump in here if I miss something. The diffusion control--if it was--if we were only looking at diffusion control in this system--which, obviously, we're not, but then you could say if you're starting to talk about, you know, depths of 100-150 meters, and so on, you know, sure, quite possibly maybe you shouldn't see post-bomb C-14, but you shouldn't see it here, either, okay? So that's one.

Advection has to be going on. If you make the comparison, which we did at the last--which Ed did at the last meeting of this--if you look at the comparison between the open borehole data with those from UZ1, you see that the open borehole data are showing post-bomb Carbon-14 at 200 meters. UZ1's down to about 60 per cent modern by the time it reaches the same depth.

The CO_2--there's a gremlin hidden in here, too.

The CO_2 contents of the non-welded tuff here and the deeper one are higher than we see them currently in the fractured tuffs. So there is a discontinuity. There is a maximum in CO_2 concentration here, so this doesn't definitively answer your question, but on CO_2, if anything, it should be diffusing out both ways. Why that maximum is there, again, at this point, I have no clue.

If you do the time calculations for diffusion of Carbon-14 dioxide across the non-welded tuffs down to the nearest sampling interval in UZ6, I believe that it would say it couldn't get there. Is that--have I got the times in my head right? So the--

DR. JONES: So rather than a physical barrier of diffusivities or something, you're saying it may be a concentration barrier that's preventing--

DR. THORSTENSON: Well, no. I think--

DR. JONES: Is it a maximum in the CO_2 concentration and not almost like a high point in water, a divide that's--

DR. THORSTENSON: I guess--I have no definitive answer to that. I guess I would turn the question around and say, if we're seeing advection continuously--keep in mind, you know, we're seeing constant composition all the way down to here in UZ6S. I mean, it just is invariant in time and depth for all practical purposes. If this were not a barrier, then why don't you see the same thing on down into the Topopah?

Ed, do you want to throw anything in here? I have a feeling I'm still--

DR. DOMENICO: I think we better move on here.

MR. WEEKS: Carbon-14 seems to be retarded relative to CO_2. Don and I did a paper about eight-ten years ago. I just don't think it would, diffusion alone would get the modern carbon across that non-welded tuff, and it's only diffusion that's transporting it across there. So it's a barrier to advection, but not necessarily to diffusion.

DR. DOMENICO: I think that diagram is obviously clear. It's an advective system. I mean, that's the way you've presented it, but I think we better move along, though, at this stage.

DR. JONES: But you are saying that the gases released from the repository are not likely to--is that--I mean, I'm trying to--what's your bottom line of this? It's that you've got lots of circulation above that non-welded, but not a connection between?

DR. THORSTENSON: I'm trying not to tell stories here, okay? I'm trying to say, hey, where are the obvious things? You see atmospheric input on a time scale less than 30 years. It may be less than three months. I mean, we don't know. Our supposition is that it's natural and the borehole is just providing a conduit. So we think there is a blanket of post-bomb gas everywhere here, and we're just letting it out by putting these holes in. It's not inconceivable the whole system was different and UZ6S changed it instantaneously, but either way, things are happening in here very rapidly, and you're seeing atmospheric bomb CO_2 in this unit. You don't see--at least, unless this year's data shows it--you don't see that same contribution either at UZ6 here in an open borehole at Yucca Mountain crest, or at UZ1, two kilometers down.

DR. DOMENICO: Donald, your point is well made.

DR. THORSTENSON: I just--I don't know how else to--

DR. DOMENICO: Your point is well made. Can we move on, please?

MS. NEWBURY: Pat, we've got about another hour worth of presentations. Do you want to--it's five now. Do you want to go on to six?

DR. DOMENICO: We'll do one more.

MS. NEWBURY: Okay. The next one is Dale Wilder from Lawrence Livermore, and he'll be talking about the effects of repository development.

MR. WILDER: Well, as was mentioned, I will be talking about the effects of repository development. This is a follow-up on a presentation which was given in '89 by Bill Glassley. What I'm going to try to do is to give you an update from the presentation of '89, and there are a couple of things I'm going to do a little bit differently than what

Bill did.

I'm going to talk a little bit about disturbed zone characterization, because it seems to fit with the subject of this meeting, and I also want to talk a little bit about the emplacement effects and some design options which Bill had not got into.

In terms of the update, I'm going to stress both the hydrologic and chemical understandings that have been put forward. Now, in doing that, I'm probably going to focus more on the modeling advances. Since '89, there has not been an awful lot of laboratory work that has been performed, although there has been some. There's been a fair amount of analysis of the work that had already been done, and we've also made a fair amount of progress in our geohydrology modeling.

In '89, Bill pointed out that in making some comparisons between model predictions and the laboratory and field studies, that there were some important data and model needs. Subsequent to that time, we have continued to do not only the rock water interaction studies, but also, analysis of those studies, and what I want to do is to briefly go through these studies.

I think Bill had already reported to you that we were doing rock-water interaction using Topopah Spring tuff, and we have looked at devitrified, vitric, and zeolitized with the water chemistries that we felt would be pertinent. In this case, we used J-13, both concentrated and J-13 simulated, as well as distilled water over a wide range of environmental conditions, from 90 to 350°C--and this is kind of important to remind you that that was where some of the data inconsistencies came about--at sufficient pressure that we didn't vaporize the water in the rock-water interactions and, in time frames, almost up to a year.

The results since '89: One is we found that the aqueous silica activity plays a key role in the paragenesis of secondary minerals. I think Bill may have told you about the amorphous silica that was developing in some of our experiments. This is a very important item in that the secondary minerals that were forming may very well come out zeolites, and so forth, at temperatures that we're talking about, depending on what those activities are.

Secondly, we've been somewhat gratified in that what we see at the mountain is basically what we're finding in the experiments, and in a sense, I guess you could say that it's a type of an analog or a very low level of validation of our models.

And then, finally--and I will show you a couple slides on this--the zeolites that were produced during our experiment contained the same cation compositions that we were able to predict once we have included the ion exchange into EQ3/6.

DR. DEERE: Do we have that slide on the right in our packet?

MR. WILDER: I'm sorry. You have that slide on the right, but they were scrunched together and so I put them--I pulled them apart. They were too cramped.

DR. DEERE: Oh, okay. But it's in here someplace?

MR. WILDER: Yes. It should be in there. There's a single slide, and it should be headed, "Rock-Water Interaction."

Now, I have pulled a couple of extra slides out that--as a result of the meeting last week that I've inserted that are not in your packet. I will try to point those out to you.

As I indicated, our model predictions versus the experimental data have been very gratifying. This is rather preliminary in that we have not done a lot of this work, but the results to date, at least for the work that we were comparing with the data from Los Alamos has been surprisingly good.

I'm going to now shift--and I don't know if you have this as a place-holder. I didn't realize I had two slides available when I put the packet together, so that may not be in your package. I know this one is not.

I want to talk a little bit about the

characterization of the altered zone, because I feel that for the waste package, it's extremely important, and it's also a very large percentage of the--well, I shouldn't say a large percentage, but at least a significant portion of the overall Yucca Mountain rock.

But the specific reason I'm going to talk about it is that it's very important to waste package performance and influences the source term, and to make that, perhaps, a little clearer--and I'm not sure you have this one either--what I'm saying is that in terms of the waste package--and in this particular case, I'm showing the reference design--whether it's in the drift or not is immaterial. As we're looking at source term for radionuclides, we have to consider what the source of water coming into this disturbed zone is, and as it crosses the various barriers, be they the borehole wall, liners, whatever may be in the drift, the container, and then into the waste form. And so, we have spent a fair amount of time looking at some of the hydrologic aspects.

The first work that we did had to do with fracture flow. We feel that in terms of the waste package, if you maintain the capillary barrier, that matrix flow is not a major issue for us. The thing that we have to be concerned about is fracture flow, so we needed to come up with an understanding of the fracture flow, and since '89, there has been a fair amount of work looking at the balance between matrix and fracture flow, looking specifically at what happens when you pond water above a fracture, or at the opening of a fracture. How far can that water infiltrate into the fracture, and what happens once the ponding goes away?

And the analytical work showed that once you remove the pond from the top, there is no more penetration of water into the fracture; that at that point it is then imbibed into the matrix and eventually, if there is no more cycle of water coming down the fracture, that water can be--at least part of it--removed by vapor transport.

This has implications not only in terms of the water getting down the fracture, but also the radionuclides. Now, what you're looking at is the surface of a fracture. If you had a mirror image of this, then you'd be looking at a fracture, and this is a two-hour contaminated pulse in which water has penetrated, and I think this is a 100 micron fracture. At approximately 30 meters--maybe somewhat less than 30 meters--you can see that the contaminant has followed down to the depth of penetration within the imbibition front, followed later--30 days later--by a four-hour clean pulse, and you'll see that the water pulse comes much further down into the fracture--essentially double. It's twice as long, the pulse--but that the contaminant does not get flushed further down the fracture. It gets imbibed and moved over into the matrix, and that's a very important consideration for us when we are looking at radionuclide transport.

We did a little, I guess you'd call it a prototype experiment. What it was was just a demonstration with a plaster block, and I apologize, I did not get a--get time to have this one turned into a nice image like the other one. What they did was they took our photographs and image-processed it for us. But what we did was had a fracture in the plaster block, and if you'll remember the previous slides I showed, this is very similar in terms of its shape, and certainly in terms of the flow regime, in which we put in dyed water--and that's what the black is representing--and you can see that after 62 minutes of wetting, the wetting front gets out to about where this red line is, which is the same as this blue traced line on the next figure.

After we had finished injecting or dripping the blue dyed water, we then followed up with clean water, and that's essentially what you're looking at here. I should point out, this is 13 minutes. We continued on, and you can see that the water has continued on beyond this point, but if you'll look at the blue dye--and I apologize since they aren't the same figures anymore, they're not both photographs, it may not be quite as apparent--but the tip of this blue dye was never pushed any further down even after we injected the clean pulse of water. And so that gives us a little bit of confidence that perhaps we are starting to understand this balance between matrix and fracture flow.

In terms of the repository, what that tells us is that we can start to get a handle on what would happen if we did have radionuclides released from a borehole, and what we're looking at here is the same case that we looked at earlier, but with much more permeable rock properties, and you'll see that the same phenomenon takes place except in this case you don't get penetration down the fracture. It basically all gets imbibed.

On your right, what I'm showing is what happens if we do get a release, and what we see is that once it gets into these much more permeable zones, that we see that same horizontal spread of the front rather than vertical penetration down any fractures. Of course, I'm not saying Calico Hills is fractured, but I'm saying that even if it were, we get a lateral spreading of the radionuclides.

MR. BUSCHECK: Tom Buscheck.

This is the same 100-micron fracture. I want to point out that we increased the matrix permeability a thousandfold, and we see about half the penetration down the fracture. This is what I was referring to earlier. You actually see less penetration with increased matrix permeability.

MR. WILDER: Okay. Well, let me move, then, into the effects of the repository development.

There has been a lot of talk over the years about what happens with the temperature, and of course, there's still a fair amount of concern over what are the effects of temperature should we try to limit the amount of temperature. What I'm trying to point out here is, these are calculations that are made based on the fairly hot scenario, the 8½-year-old spent fuel. As you can see, the boiling point isotherm in terms of the volume of rock which is above the boiling point, is a fairly limited percentage of the rock. That's about 100°C, and assuming that you're not really changing the boiling point too much by any of the capillary forces, you would expect that there's a small percentage of the rock out after approximately 100 years which will be above the boiling point isotherm.

That does have implications for the repository. Now, I think this figure--I wasn't there the first day, I had a conflicting meeting, but I think this figure may have caused some questions to arise during the workshops last week. What I'm trying to depict here is if you look at the matrix saturation--of course, we realize like anything in nature, it's going to have some variability, and there is some sort of distribution.

This is strictly conceptualization on my part. I don't have solid data to give me the shape on this distribution, except I do know that we expect 65 per cent saturation, ±19, based on the data we've looked at to date, but certainly, there is going to be some sort of tails and how far they extend, I don't know. I expect it'll probably be some sort of a normal distribution, so if you'll accept that with a little bit of a grain of salt, then what I said was at the time when we are characterizing the mountain, that's essentially the situation we're looking at.

As we construct a repository, we are going to be removing a portion of the water at least in the very near field, and so what I'm looking at right now is the saturation right around the waste emplacement borehole. Now, if this is a drift, it may be a slightly different story, but due to the ventilation, and so forth, you're probably going to change the saturation distribution. How much, I don't know, but I'm just showing it conceptually.

But certainly, after you emplace that waste, because of the heat that we are generating--as I showed on the earlier slide--you're going to be drying out the rock around the waste packages. Once again, this is probably not pertinent to what will actually go into the repository, because these were early calculations done assuming fairly young spent fuel, but as you can see, that after 25 years, we're expecting that there will be a dried out zone that extends a meter or two into the rock, probably a few meters, and there will be a zone where we were expecting--and this is one of the things that I think Bill talked about last time, about some surprises from G-Tunnel--that we expected some increases in saturation, you know, saturation halo, and certainly, elevated temperatures where we can be concerned about geochemical activity, and that's one of the reasons for the rock-water interaction studies.

Well, as a result of that kind of process, we do expect that after emplacement, we will essentially dry out the rock around the waste package. Our calculations show that the temperatures will remain high--and it depends, of course, on the assumptions you make--for some probably thousand-year period of time, where you would expect the borehole itself to remain close to the boiling point, if not above the boiling point.

The other thing is, in the work that we did at G-Tunnel, we saw that it took much longer for the rock to re-saturate, for the water to come back in than for us to drive the water out. And our--my feeling is that we're removing a fair amount of the water, and I'll talk about that in a second, and so my guess is that even out to 10,000 years, we're probably still going to see a somewhat drier environment, given that there's no climatic changes.

The other thing that we've looked at recently is trying to estimate, well, what if you do have fracture flow, what percentage of the waste packages might you expect to see water? And this is work that Duane Chestnut has been pushing based on chemical flood studies that he had done in the oil field, as well as some very detailed studies done where they took the 1 x 2 meter plastic squares or rectangles and measured the water coming in through the fractures and found that even though the rock was fractured and they were saturated, that there was only a small percentage of the fractures that actually made water. And so we've tried to put some numerical values to this, or Duane has, looking at a SIMA value for the heterogeneity.

And so what we're trying to show here is that there's probably a large percentage of the repository--and for argument's sake, we've said maybe something like 70 per cent--which will see no water. Whether it was saturated or not, it's just a function of many of those fractures are going to be too small to make water. And then you'll have some percentage of the area--and we're doing this by a percentage of total repository area--which will see some low values of flow, but you cannot rule out very high values for a very small percentage of faults or whatever.

This has then given us a means of trying to come up with some design values, and once again, you'll see the same phenomena with time, that in your pre-construction, emplacement and so forth, you're going to be drawing things out and, therefore, some of those fractures which may have made water earlier would probably not after they were dried out.

By looking at the cumulative, which is showing here on the bottom, we feel that we can come up with a design value, and we picked 95 per cent as a target design value that we can look at what would be the flow conditions and, therefore, what would we need to design waste packages for. And based on that, we've said that preliminary design basis for the, essentially, emplacement to 300 years is zero liters per year per borehole, because the temperatures would be above the boiling point. From 300 to 1,000, it could be as much as three liters per year per borehole. From 1,000 to 10,000, it could get up to as much as five liters, and then beyond 10,000 years, it could be something approaching five liters per borehole, which is the same value that's in the SCP, but this was not considered at all when we made these estimates. It was very fortuitous.

I mentioned the concern over hot versus cold. We have been trying to look at that and, frankly, with everything else that's been happening, we just have not had much chance to do that, but John has done some calculations for me, and I think that they indicate some interesting possibilities.

What he's done is--keeping the spacing that's in the repository plan right now, the reference design, and just look at different ages of fuel. And if you did that, then, of course, you'd have a decreased areal power density. Based on that, what would be the radius of rock dried out due to the temperatures imposed by the waste? And for 80-year-old fuel, you can see that essentially we don't dry anything out. I think that there's a little zone, but not much. For 60-year-old fuel, you can see that there is a meter or two that may be dried out, up to as much as 800 years. For the ten-year-old fuel--which is basically the case that I had already talked about--you can see that the radius is somewhere around 20 meters.

I'd like to focus a little bit on this 20-year. It's kind of an intriguing case that we hadn't thought about, but if you'll look at the 15 meters, one of the things which we did see at G-Tunnel--I think Bill had talked about--was that the water did not build up in the condensation zone, and the reason that we feel that it did not was that we went through these--well, I hate to call them gravity-driven heat pipes, but basically it was a cycle of vaporizing of water, and then it would condense and try to come back in. It'd get re-vaporized and eventually move its way over to the side, and then was able to drain downward, and we picked that up in our instrumentation at G-Tunnel. We also have been able to show that in our calculations.

This is a--gives us a possibility of perhaps looking at that 20-year-old case--and let me move this over to the other view graph so you can keep that in view. If we're looking at the 20-year-old case, we've got about a 15 meter radius of dry-out. The current design is for about a 19 meter half space between drifts, and so, to me, this appears to offer us a possibility of removing a major percentage of the water--and by the way, I don't care if it's in the drift or if it's in a borehole. This is just the reference case--by using the same phenomena to let the water drain off to the side. We maintain a pathway, if you will, for the water to drain; whereas, if we happen to allow those isotherms to merge, then the water can no longer drain because it's being boiled off and it's held up above in what I call a thermal ponding condition.

DR. DOMENICO: What's your margin of error there? Have you got--side-by-side canisters. What's that unaffected zone in the middle? What length would that be?

MR. WILDER: Well, right now, it'd be about eight meters. But, I mean, I wouldn't want to put any real accuracy to this. This is just kind of conceptual calculations at this point.

Tom?

MR. BUSCHECK: This is Tom Buscheck again.

In our validation efforts modeling the G-Tunnel experiment, we found that we could very accurately predict the volume of the dry-out zone. That and the temperature predictions were quite accurate, and we found that what we weren't able to predict was what was happening in the condensate zone directly with the use of our models, but that was partly due to the fact that the continuing model would not allow this ready drainage to occur off the sides of the boiling zone, and we have subsequently been able to model that through some auxiliary modeling.

MR. WILDER: Now, obviously, there are some assumptions we're making. The assumptions--one is that the rock is fractured. I think that's a pretty good assumption. The second assumption is that we know the properties of Topopah Springs tuff, and until we actually get underground, I wouldn't want to put any real hard numbers to this, but I think the concept is there.

Another concept in terms of protecting the waste package from seeing water, if you will, is one in which--we talked about the heat which will be driving the moisture away from the waste package. Even if we have episodic fracture flow--and once again, as I said, I don't think that the case of matrix flow is one that's going to be of concern to the waste package. Our concern is if we do have episodes, somehow, of fracture flow, can that fracture flow get onto the waste package?

Because of the heat that is generated around the waste package for at least the period of 300, and maybe a 1,000-year period of time, water which comes down these fractures will tend to vaporize. One of the things we probably should have shown was a pathway down here, because this water coming down can't continue on down and, therefore, will continue on down this fracture which had been flowing water before we put the drift in.

The other important point is that a backfill drift will, of course, serve to diffuse the water coming down the fracture, and so then that water will not just continue down the same fracture, but will be spread out and enter other fractures, and so we feel that there's a fairly good protection of the waste package. However, Tom's going to report--I guess it'll be tomorrow--on some work that he's done looking at the natural system, and from that, has come to the suggestion that why don't we mimic what's happening at Yucca Mountain, in which we can put high moisture-absorbing crushed tuff, non-welded tuff, in the invert section of the drifts, which will then serve as a diversion for flow away from the waste package, and what we're seeing is that some of the units can be nearly saturated, if not totally saturated, and still not get fracture flow below them because they hold that saturation and drain it off to the sides. And so, by mimicking that, we can protect the waste package.

Thank you.

DR. DOMENICO: Any comments, questions?

(No audible response.)

DR. DOMENICO: Well, I hereby adjourn us, and we'll start in the morning with the presentation that was supposed to be last today.

Thank you very much for coming, and see you all in the morning, and would the Board and the consultants stay for a few minutes? Bill Barnard made that request.

(Whereupon, the meeting was adjourned, to reconvene on June 26, 1991.)