# UNITED STATES

# NUCLEAR WASTE TECHNICAL REVIEW BOARD

## MEETING OF THE PANEL ON HYDROGEOLOGY & GEOCHEMISTRY FRACTURE FLOW AND TRANSPORT IN ARID REGIONS

June 26, 1995

Holiday Inn Crowne Plaza
San Francisco Airport
600 Airport Boulevard
Burlingame, California 94010

### BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Donald Langmuir, Session Chair
Dr. Edward J. Cording

### CONSULTANTS

Dr. Patrick A. Domenico
Dr. Dennis L. Price
Dr. Richard Grundy

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Dr. Carl Di Bella
Dr. Daniel Fehringer
Mr. Russell McFarland
Dr. Daniel Metlay
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# I N D E X

# P R O C E E D I N G S

(1:00 p.m.)

DR. LANGMUIR: Good afternoon. Welcome to the Board's meeting on Fracture Flow and Transport in Arid Regions. My name is Don Langmuir. I'm Professor Emeritus of Geochemistry at the Colorado School of Mines in Golden. I serve as the Chair of the Board's Panel on Hydrogeology & Geochemistry. The panel is sponsoring today's meeting.

First, let me introduce my colleagues on the panel. We are very pleased that the Chairman of the Board, Dr. John Cantlon, is with us today. His field is environmental biology and he is former Vice-President for Research & Graduate Studies and Dean of the Graduate School at Michigan State University in East Lansing. Dr. Cantlon has served as Chairman since April 1992. As Chairman, he is an ex officio member of all panels. Dr. Edward Cording is Professor of Civil Engineering at the University of Illinois.

Dr. Patrick Domenico is Professor of Hydrology at Texas A&M University. Dr. Domenico co-chaired this panel with me until his term with the Board expired. Since then, he's been serving as a consultant for the Board pending Presidential action to reappoint or appoint a replacement. We also have with us Dr. Dennis Price, hopefully. Is Dennis here yet? Should he come, he is Professor of Industrial &

Systems Engineering at Virginia Polytech Institute and State University.

Also seated at the head table today is Dr. Victor Palciauskas who is a member of the Board's Senior Technical Staff and who supports this panel among other activities. I wish to express my special thanks to Victor for his efforts in planning and organizing this meeting. Several other Board staff members are with us today. Among the Senior Professional Staff members are Dr. Carl Di Bella, Russ McFarland, Dr. Daniel Metlay, Dr. Leon Reiter, Dr. Daniel Fehringer, and Richard Grundy, our consultant to the Congress, is with us today. We also have Linda Hiatt in charge of meeting arrangements at the back of the room, and Davonya Barnes, a member of the support staff.

Our Board was created by the 1987 Amendment to the Nuclear Waste Policy Act. Board members are nominated by the National Academy of Sciences and appointed by the President. The Amendments Act provides that the Board shall evaluate the technical and scientific validity of the Department of Energy's activities under the Nuclear Waste Policy Act. The Act itself was passed in 1982 and charges the DOE to develop repositories for high-level waste and spent nuclear fuel following an ordinary--not ordinary, totally un-ordinary--an orderly process of repository site characterization, approval, and construction. Currently, only the potential repository site at Yucca Mountain, Nevada is being evaluated as directed in the 1987 Amendments Act. Site-specific work for a second repository is not authorized and cannot be under current law until the year 2007 at the earliest.

An adequate understanding of fracture flow and transport, the topic of this meeting, is essential to a determination that Yucca Mountain is a suitable site for a repository and subsequent licensing for construction and operation. We have set out several goals for this panel meeting. Historically, it has often been assumed that unsaturated zones in arid climates were potentially good sites for isolating waste. This assumption was based on the "common knowledge" that flow in low permeability rocks is generally very slow and that, although the rocks might be fractured, the fractures are dry most of the time. Even during periods of extreme precipitation when water penetrated the alluvium and saturated the fractures, it was thought that fracture matrix interaction was sufficiently strong that the water would quickly imbibe into the matrix preventing deep penetration. Thus, transport of contaminants through these zones was thought to be primarily through the matrix and extremely slow. Significant fast transport through fractures was considered unlikely. Recent evidence challenges this view.

The purpose of this meeting is to learn about such evidence from experts who have studied transport in arid climates in various regions around the world. We are particularly interested in delineating the physical parameters and processes that control the infiltration of water and result in transport in arid regions such as Yucca Mountain. In particular, we would like to address and hopefully answer questions, such as, are present conceptual models of flow and transport adequate for modeling in arid environments? For example, is the composite porosity model reasonable in modeling in arid environments such as Yucca Mountain? Second, do we have a sufficient understanding of the important parameters that control transport processes in these environments? Third, what measurement techniques can be used to quantify flow and transport in these environments? What are the limitations of these techniques? For example, can the fast pathway be detected and predicted, and can the significance of such a pathway be quantified? Fourth, how do the existence and potential importance of fast pathways influence our views about the suitability of Yucca Mountain? How will groundwater travel time and total system performance assessment computations be affected by the site-specific isotopic age data that are and will be accumulating? These are some of the questions I hope we will consider today and tomorrow.

I know that each speaker has much more than could be said in his or her topic area than fits in the time allotted. I'm very concerned that we stay on time so as to allow those speakers late in the day their fair share. So, I will ask all speakers to stay close to schedule. I will help by reminding you as the end of your time of presentation approaches. An essential part of this meeting is the discussion of the work presented. There is time scheduled after each presentation for questions and discussion. After each talk, I will solicit questions from the Board, staff, and if time permits, from the floor. If I don't get to your question or comment, please try to hold it until the roundtable discussions or the public comment period at the end of the day.

If there are no general announcements--if there are, this is a good time for it. If not, let me introduce our first speaker, Dr. Ronit Nativ, who probably set the record for having to travel the furthest to make a Board presentation of any speaker we've invited to our meetings. Dr. Nativ is from the Hebrew University of Jerusalem and will speak to us about her studies of contaminate transport in the Negev Desert.

Dr. Nativ?

DR. NATIV: I'm going to talk today on groundwater recharge and solute migration in a fractured chalk aquitard in the Negev Desert in Israel. This is the work that was done by my colleague, Eilon Adar, and myself, our two graduate students, Ofer Dahan and Ilan Nissim, and a colleague, Mebus Geyh, from the Geological Survey in Germany.

This is a map of central Israel, the Negev Desert, Tel Aviv is here, Jerusalem here, the Mediterranean Sea. The framed area is enlarged over here. Over the past 18 years, the North Negev Desert in Israel has become a prime target for siting a variety of chemical industries that have been rejected by or transferred from more populated areas. In addition, the National Site for Hazardous Waste is located here and has been operating there since 1975. The area is pretty arid. Annual precipitation vary anywhere from 50 millimeters per year to 200 millimeters per year in the North Negev Desert.

Okay. This is how the area looks like. This is the least cover that can be found all over the North Negev Desert. Only the ephemeral streams contain some sort of vegetation. And, when the cover is missing, what we get to see here is fractured chalk, eocene chalk in outcrops, across the entire Central and North Negev Desert. I bold this line so we can get some impression of how intensive the fracturing and the fissuring is. Now, the aridity of the area up to 200 millimeters per year and the low permeability of the chalk aquitard, chalk formations which run up to 2 millidarcies, that's all, have been considered the major asset in preventing potential groundwater contamination resulting from all the industrial activities taking place in the North Negev. However, the effectiveness of this combination is a variable to contaminate migration. Low permeability chalk in arid areas was challenged once monitoring wells surrounding the National Site of Hazardous Waste were placed in '85.

What you see in the upper triangle is the distances between the three monitoring wells. On the left bottom is chloride concentrations way above the background salinity, and these are the water level fluctuations starting from '85 up to '90. All three wells contained high salinity way beyond the background salinity, organic materials, heavy metals. Remember, the National Site started to operate in '75; I'm talking '85 and on. So, within 10 years, a vadose zone of up to 20 meters experienced solute migration from land surface to the water table.

Although the chalk is not a major water source for the Negev area, this is just a geological section. The light blue on top is the eocene chalk that we are talking about. To the left top is the coastal plain aquifer. In the bottom is the Judea cast limestone aquifer. And, although the chalk aquitard is not a major water source, its relationship with the adjacent aquifers, the coastal plain aquifer and the underlying limestone aquifers, are not clear and our source for concern, once water in the chalk becomes polluted.

In order to evaluate the potential damage, we studied the origin and hydrology of the aquitard. We looked into 23 wells marked here in black dots all over the area. We monitored them for one year for water level electrical conductivity. We sampled them twice for both isotopic and chemical composition and this is what we found out. We found it was quite clear by looking at the outcrops that the chalk is fractured and fissured. We also found secondary mineralization within the fractures. The fractures contained either oxides--in this case, it's manganese oxide--or gypsum, as you can see on top of that fracture.

In addition, we were able to observe seasonal fluctuations in the water level. Every single well displayed some sort of seasonal variations in the water table. These are just three examples out of the 23 wells which we monitored. These slides would show carbon-14 in the upper layer in the upper numbers and tritium in the bottom two numbers. Almost every single well contained tritium in its groundwater, and I would like to remind you that, I think, within a decade, contamination of groundwater. And, finally, there is this disturbing similarity between precipitation marked here in black dots and groundwater marked here in open circles.

So, looking at the fissures and the fractures and the secondary mineralization and the evidence of water recharge is displayed by seasonal fluctuations of water level. Contamination in groundwater is indicators for fast moving tracers from land surface and no obvious evaporation on top of land surface in a desert area. It all means to us that basically just from information in the saturated zone that water and solutes shortcut through the low permeability chalk using the fractures and the joints as the pathways in escaping evaporation.

Now, the third mechanism of groundwater recharge and contamination was examined more closely in the vadose zone. We cored six boreholes all the way to the vadose zone which was 20 to 28 meters below land surface. These are the various boreholes. This was a control. This was the industrial site. We cored it with a special, largely--grade, foundation--grade and the purpose was, first of all, to get some dry core rock samples for various profiles and to observe the vertical fracture distribution with depths in these cores. What did we actually--we used the water extracts from these cores for taking profiles to estimate water-percolation rates. We looked at chloride profiles to assess the nonreactive solute transport. We looked at bromide profiles in these cores to evaluate normally active contaminant transport since in this industrial zone, there is a plant producing bromide variabilities. We looked also into deuterium and oxygen-18 profiles to assess evaporation near land surface, at land surface, and what's going on in that.

These are the various profiles. I prefer to present just four of them. In four boreholes, RH2, RH8, RH10, and RT18, this is depth in all of them. The contact between the undisturbed chalk and the unconsolidated sediments is marked by these small arrows. Water table is marked by the upside down triangles. What we have on the axis is water content in percent, tritium in tritium units, chloride content in--per 100 grams of dry rock, bromide, and bromide to chloride ratios. And, I'm going to discuss those profiles in the coming few minutes.

First of all, we observed very high moisture content in the vadose chalk. You can see or perhaps you can't and I should help you see by telling you what you should see. The water content here goes up to 40% in these boreholes. Forty percent is the proper porosity of the chalk on the basis of co-analysis. So, we saw almost near-saturation water content. Apparently, the very small pore size of the chalk matrix inhibits gravity drainage and the matrix remains almost fully saturated even in the unsaturated zone except within the depth of direct influence of plants roots which can absorb high suction.

We also observed the tritium front dated to possibly 1963 in all coreholes at a depth of up to 2.5 meters below land surface. Parallel to that depth, we also observed salt peaks at a similar depth of up to three meters, the peak of chloride and bromide in most of these boreholes. As we looked at stable isotopes, we also saw positive values close to land surface. As we go down with depths, the stabilized composition is getting lighter. The salt concentration is getting lighter, too, like non-diluted perhaps. The tritium drops to zero except for some tritium spikes that can be observed in most of the coreholes.

So, on the basis of mere saturation water content in the vadose chalk, the low permeability of the chalk, two millidarcies, the presence of tritium spikes below the tritium front, vertical deplition of stable isotopes, vertical dilution of salts, and the mineralization that I mentioned earlier, we suggested that water entering the fracture is not immediately imbibed by the matrix, as was the general knowledge that was mentioned here earlier. Instead, we suggested that water wets the fracture walls and rapidly percolates through the major conduits, the fractures.

Now, there is one borehole here that might attract your attention and this is RH8. That one allows us a unique observation on a slight different setup. The unconsolidated materials on top of the chalk here was relatively thick, seven meters, as opposed to less than a meter and a half in the other coreholes. This one was seven meters thick. If you compare the profiles that we saw in this borehole to the others, it's really clear to see that, first of all, the tritium spikes are less obvious. The salinity here is definitely lower than in the other boreholes. We don't see that deplition of solutes--as was so obvious in the other coreholes. What we think is that this thickness of the unconsolidated cover overlying the undisturbed chalk is an important control on the likelihood of initiation of fissure flow. The much greater spread of four sizes in this material provides a baffle for storage of rainwater and allowing it to be released into the undisturbed chalk more slowly and, hence, reducing the frequency of occurrence of fissure flow. In addition, as shown up here, the stored water is available for more efficient flushing of salts from the vadose zone resulting in reduced salinity.

Water infiltration velocity along preferential pathways in the chalk is somewhat reduced because of matrix diffusion as documented in the profile here. As recharge containing tritium, for instance, moves down through fissures, a concentration gradient exists between the fissure water and the relatively immobile water in the matrix pore spaces of the blocks adjacent to the fissures. Under the influence of this concentration gradient, tritium would move down by continuous exchange between the matrix and fissures through diffusion.

As part of our original study, we calculated the infiltration velocity into the groundwater using both tritium and contamination as tracers. We ended up with values exceeding 1500 and 2400 millimeters per year, respectively. The 1500 refers to the tritium which might be percolating in from land surface to the saturated zone since the '60s and contamination which had only 10 years from '75 to '85 before it first showed up in groundwater. Now, these values have been moderated by matrix diffusion already, but they are still two to three orders of magnitude higher than the calculated infiltration velocity if we look at the tritium formed in the matrix or if we look at the bromide formed in the matrix. Those are only 30 and 110 millimeters, respectively. So, on one hand, there's evidence for migration of tritium and contamination at that rate into the groundwater and, on the other hand, this is what we see in the matrix, in the vadose matrix chalk. And, again, we concluded that it's the fracture flow which accounts for the two order of magnitude of difference between these numbers.

As we presented these observations and conclusions to all our peers, colleagues, German reviewers, and decision makers in the Ministry of Environment in Israel--they, by the way, decided recently to consolidate and move all landfills in Israel to the eocene chalk assuming that the permeability is so low that no one should be frightened because of groundwater contamination. As we presented this data, we faced many reservations and suspicions. In fact, many of our observations that I just discussed now that we interpreted as evidence for an active fracture control system were viewed as evidence for stagnant immobile system.

What I'm going to do now until the end of my talk is to present the report sheets that I have from our peers who viewed them and hopefully convince them why our interpretation makes more sense. In fact, I'm going to discuss here the type of observations that one should look for when evaluating the possibility of fracture controlled flow in an aquitard. As you're well aware of, this is not just an intellectual exercise since low permeability environments are prime targets for siting repositories for toxic, hazardous, and radioactive waste. It is these continuities, such as fractures, joints, dissolution channels that threaten the integrity of an otherwise great hydrological barrier.

So, what are the warning signs that one should pay attention to when assessing the suitability of an aquitard for these purposes? I'll start with the saturated zone. The presence of contamination and tritium in deep groundwater was interpreted by us as an everyday fast migration from land surface to deep groundwater and fissure flow. These observations definitely disagree with the measure of low permeability matrix and require bypasses.

The questions that we heard when we presented this type of data--and this is how it looks like. I go back to the data that you have seen, the slides. Carbon-14 on top, tritium, two measurements on the bottom of each well. Forget about the contours. I'll discuss them in a moment. They are meaningless at that point. So, abundance of tritium and contamination, what we heard was that it is quite possible that tritium and contamination did not come from above, but was more literally from adjacent streams where tritiated water is flowing in the ephemeral waters. And, near the industrial waste site, liquid waste water was released every now and then and could have flowed laterally to adjacent boreholes. So, no one needs the fracture flow in order to get those tracers into our boreholes. The truth is that we found contamination and tritiated water also in boreholes that were far away from streams. So, this argument, we think, falls down.

The same goes for the resemblance of precipitation in groundwater as a--position of precipitation in groundwater. A comparison of stable isotopes values in groundwater and precipitation can shed light on the amount of precipitation in surface water infiltration. In the Negev Desert, evaporation is relatively high even during the rainy season, the winter, and amounts to two to six millimeters per day in January and October, respectively, when most precipitation occurs. If rainwater concentration is slow and contoured by the matrix low permeability, the water should become isotopically enriched relative to the precipitation in both oxygen-18 and deuterium as a result of its exposure to evaporation. On the other hand, if--focused recharge by other fracture network occurs, the isotopic composition of the percolating water should remain constant and similar to the composition of rainwater and this is what we get to see here. The question that we had was again is it possible that the similarity doesn't stem from fracture flow from above, but from focused recharge through the porous riverbeds where the ephemeral flow is stated only by extensive and isotopically light precipitation during rain events. So, it comes laterally rather than vertically. And, again, it was possible to document light and isotopically similar groundwater away from any ephemeral streams.

The other issue was more disturbing. This was the heterogeneity of both chemical and isotopic composition of groundwater. One would expect that adjacent wells that belong to the same unit would display similar chemical and isotopic composition. One should also expect some evolution in age and in chemistry downgradient. The contours here that you see are water potential or--head contours. So, the flow according to the water level measured in these 23 wells is going like that towards the coastal plan aquifer.

Now, there was no way we could document the expected evolution of groundwater both in age and in chemistry along apparent flow paths. This, for example, carbon-14 here is about 34 pmc, and downgradient, it's younger rather than being older, 92 pmc, and so on. So, we couldn't demonstrate similar chemical and isotopic compositions in adjacent wells and there was no expected evolution in age and chemistry with the flow.

The interpretation was, of course, that if there is no lateral flow, that water is confined in enclaves, the groundwater is stagnant, and if the groundwater is contaminated, we shouldn't be worried about it because it's not flowing, it's stagnant. So, there is no risk involved. Our response to this type of argument was that because flow carries along fractures, the re-flow path cannot be directly deduced from potential matrix surface maps. The connected flow paths typifying the aquitard may account for the special variations.

So far from evidence from the saturated zone, what can we learn or what are the arguments coming from our data in the vadose zone? First of all were the isotope profiles. The tritium values were ranging anywhere from 12 to 24 in the tritium front dating to the 1963, and this was held at a depth of about two meters, two meters and a half, in most boreholes. Below this depth is the unweathered chalk. Tritium content dropped to zero with a few peaks at around 2 TU. Some of the peaks ranged up to eight and four TU and so on. You can see them here. These are significant tritium values as the detection limits for the enriched samples was .6 tritium units. The prevailing tritium concentration in groundwater in the vicinity of these boreholes was 2.3 tritium units. We interpreted these tritium spikes as evidence for shortcutting water which bypassed the low permeability matrix. The low permeability matrix only controls the tritium front whereas the--water shortcutting through the fractures account for the spikes here.

The question that we faced there was is it possible that those spikes are contamination? Well, we feel very comfortable with these spikes. The dry coring method that we used in all boreholes, the zero values that we got from the laboratory batches had only few increased tritium values and the large deviation from background values, as you can see here, suggest that at least some of these values are real. In fact, we would argue that due to the core sampling technique, the observed sporadic tritium peaks are probably representative of the higher tritium concentration associated with the fractures. Because of the small matrix volume in the immediate vicinity of the fractures, these big values are likely to have been diluted by a much larger volume of matrix spawned water which was devoid of tritium.

Now, if we look at the deplition pattern of the stable isotopes, there we faced another type of argument. The general vertical deplition of the stable isotopes shown here for oxygen-18 and deuterium remind some people of the exponential shape of the diffusion controlled profiles that were suggested by Zimmerman in the late '60s and others who looked at stable isotope profiles in the vadose zone. What they argued was that you can get these exponential type or-- shaped type profiles simply by molecular diffusion and no one needs advection, you know, to come up with such a shape.

What we said and what we think is that although the oxygen-18 profile looks pretty small, the deuterium is not as monotonous. If we look also at the chloride profiles, they are also quite spiky. No one can account for the tritium spikes or the mineralization simply by molecular diffusion. So, although one could see the resemblance in the oxygen-18 profile and argue for molecular diffusion, the other profiles of the deuterium, the chloride, the tritium, and the mineralization cannot be explained by simple molecular diffusion and no advection involved.

Finally, the mineralization was the last issue that we got criticized for. Mineralization of various oxides, gypsum, and calcite with any fractures was interpreted by us as a sign of active groundwater flow. We faced the question that mineralization is evidence for fast flow and currently acts as a flow barrier. Now, in order to assess that argument, we looked at the tritium content in the gypsum molecules in the secondary mineralization. What we found out, that the water molecules in the gypsum all contained tritium. We had like 25 samples taken from the fracture fillings. Tritium in the gypsum varied anywhere between 1 TU to 63 TU with a mean of 30 TU. And, these tritium values in the gypsum suggested that either the mineralization is recent, or alternatively, that water molecules within the gypsum crystals recently altered by modern groundwater flowing through the fractures. Again, tritium content together with the moist filling indicated for us active flow across the fractures.

The last thing that I would like to touch upon is the sulphur isotope composition of the same gypsum. The gypsum--the so-called ion--the so-called isotopic ratio, 34 to 32, in the gypsum was 15 to plus 15 per million. In precipitation, it was plus 7 per million. In groundwater, it was just in between, 9 to 13.5 per mil. And, again, the same process was suggested by us. Precipitation that has light sulfur ratio of 7 per mil would dissolve the gypsum with 15 per mil to generate groundwater with values in between 9 to 13.5 per million.

I'm done. Thanks for being patient.

DR. LANGMUIR: Thank you, Ronit.

Questions from the Board? Pat Domenico?

DR. DOMENICO: Was any attempt to arrive at any correlations between the small variations in precipitation, the variations in water salinity, and the variations in the water level changes? Was there any attempt to correlate those, at all? Or is it just they're quite too rapid and you lose a lot of information?

DR. NATIV: Let me rephrase your question and make sure I understand it. Are you saying that--are you asking whether we compared the precipitation amount of the precipitation concentrations?

DR. DOMENICO: The precipitation amount versus the water level response to that, as well as the change in salinity in the saturated zone that occurred in response to that?

DR. NATIV: This is something that we are going to do just now because the type of monitoring are showing water level fluctuations which were monitored once a month. So, there was no way under these circumstances to watch for event based response. Now, we are going into the boreholes with pressure--and data loggers and this is exactly what I would like to see. What is the type of fast response to a rain event, to a flood event? Is it affected by percolation from the vadose zone or perhaps really laterally as was argued by some of our peers? So, this is a different type of operation that we have studied only this year around the site of hazardous waste where they finally figured out that something more serious needs to be done there.

DR. DOMENICO: Thank you, Ronit.

DR. LANGMUIR: You have a very solid amount of data here obviously on all the input parameters. I didn't hear anything about precipitation chemists, but presumably you have information on precipitation, isotopy, and chemistry?

DR. NATIV: Definitely.

DR. LANGMUIR: You've got vadose zone chemistry. You've got groundwater chemistry. Have you taken that information, and from it, backed out what you think the fraction of fracture versus matrix water chemistries are contributing to the groundwater chemistry? What percentage of those two kinds of water have become groundwater chemistry? Have you done that?

DR. NATIV: Yeah. In fact, this was submitted to chemical geology, the very same question. We looked at the entire component of the hydrological cycle from precipitation to vadose water to groundwater and including surface water which you haven't mentioned.

DR. LANGMUIR: Yes. Well, you argued that that was not close enough, I thought, in this case to be an issue in your system.

DR. NATIV: That's true, but we looked at it, nevertheless, because it was important in desert--it's interesting in desert conditions. Groundwater--let me put it this way. The place where everything changes chemically is in the vadose zone because this is the storage of salts. This is where all salts are being stored, accumulated, and then flushed down. Isotopically, it's precipitation that controls. So, the salt comes mostly from the vadose zone. The light isotopic composition, the tritiated water, the modern waters show a much shorter residence time. So, there are--I would consider this as a dual system. The vadose zone which builds up salinity that is being taken or flushed down by the fast moving isotopic and light and tritiated water that comes from precipitation and perhaps surface water, too.

I don't believe that addresses your question because I'm not talking about fractions now. I'm talking about two sources in the most qualitative way and I realize what I'm doing.

DR. LANGMUIR: Well, I know it's very difficult to take your information and go to specific percentages or fractions, but that's the kind of thing at Yucca Mountain we're worrying about right now is how much of the flow is fracture-dominated flow and how much of it might have gone through matrix. Maybe, it ends up being a little of both going back and forth which complicates the interpretation.

DR. NATIV: Exactly because if you look at the isotopes, definitely it's not just water moving down and exchanging with the fracture walls. When the fractures are being drained, gas diffusion moves out and back into the fractures and leaving a heavier portion behind again in the matrix. So, it's an exchange process. I would say that most of the groundwater is fed by fast precipitation and I'm not daring to come up with a number. But, if it was matrix controlled, we wouldn't have that modern water as groundwater. So, I would say that most of it is contributed by the fast flow. However, the salinity is a different story. It's the exposure of the vadose zone, the upper vadose zone. to extensive of a--throughout the year.

DR. LANGMUIR: There was one other thing which I should have asked first in that I read your article, but I've forgotten now whether all the contamination sources were dry materials or that some were liquid wastes which, of course, is an issue that we don't have at Yucca Mountain. We're not going to have any liquid wastes, as such.

DR. NATIV: No.

DR. LANGMUIR: You're giving it a boost if you distribute liquid waste with contaminants in it.

DR. NATIV: Well, the contamination that I was referring to was dry.

DR. LANGMUIR: Was dry?

DR. NATIV: It's the National Site for Hazardous Waste.

So, there is an industrial area with a lot of waste storage ponds and a lot of--and, there's a hydraulic head that activates flow. This is one story. But, the monitoring wells that showed contamination so fast were only around a relatively dry site where we store organic materials, batteries, stuff like that; very lethal wet material. So, in that respect, it's closer to what you're talking about.

DR. PALCIAUSKAS: A brief question. You've mentioned the fact that the chalk was very micro-porous and thus it would be not too surprising that it was almost 100% saturated in the matrix. But, that's just basically where most of the water would be expected and it's consistent with the fact that most of the infiltration would be through the fractures. Is this picture consistent through the whole vadose zone, that it is almost 100% saturated? Then most of the infiltration would have to be occurring through the fractures?

DR. NATIV: Well, the vadose zone is almost fully saturated except for the last--for the upper two meters or so where we have extensive evaporation and suctions by plant roots. And, I think it's a combination of high moisture content of the chalk combined with the low permeability of the chalk that would push into fracture flow because chances are that within a rain event, the amount of water that can overcome the low permeability of the matrix without significant hydrolic gradient is slim.

So, one should expect fracture flow under such conditions. When I discussed it with Bridget two years ago, that's what--this is something that came to mind that whenever we have this combination of high porosity, high water retention, and they'll fall very close to saturation in the matrix combined with low permeability, this is where we should be especially careful.

DR. DOMENICO: But, I think you suggested also that the fractures overwhelmed the flow during your reasonably high precipitation events and perhaps not so much during the low precipitation events. Is that part of your conclusions?

DR. NATIV: It's not coming out of my conclusions because I couldn't compare event base response, at all. This is something I'm going to do now. All I could look is into monthly measurements which wouldn't tell us about high or low precipitation amounts and their input.

DR. LANGMUIR: Thank you, Ronit.

I think we're on schedule. Our next presentation is titled "Experiences in Other Arid Environments", and the speaker is Bridget Scanlon.

DR. SCANLON: I actually switched titles, but really it's not that important. I'm going to review unsaturated zone studies in general and talk about implications for contaminant transport. Some of what I'm going to talk about today will be the result of some of the discussions we had during the Ward Valley meetings, the Ward Valley site, proposed low-level waste site in California.

Unsaturated zone studies have been conducted in arid sites for a long time. However, the earlier studies focused mostly on groundwater resource evaluation, and for these studies, a lot of them assumed early uniform recharge. And, for evaluating water resources, it doesn't really matter whether you assume early uniform recharge or not.

However, more recently, the focus has shifted to waste disposal and contaminant transport and here it is critical. Spatial variability in subsurface flow is really critical. This seems a very basic concept. However, a lot of people still use early uniform recharge rates to evaluate contaminate transport. For example, some studies about two years ago evaluating plutonium migration found DOE's Plantax Plant in north Texas used early uniform recharge rates in an area where most of the recharge is focused beneath playas and got vastly different values than you would get if you used spatially focused recharge.

Some of the issues that I'm going to talk about today are what are the controls on--examine some of the controls on subsurface flow including soil texture, vegetation, topography, preferred pathways, and climate. And then, another basic question for many sites is which way is the water moving and this seems a very basic question, but it's oftentimes very difficult to answer. Is it moving up or down or laterally? I'll talk a bit about that. I'll show some results from previous studies which show very variable rates of water movement. Then, talk a little bit about spatial variability in subsurface flow beneath washes, playas, and also spatial variability in a more local scale, preferential flow. Temporal variability in subsurface flow related to seasonal variations in precipitation, annual variability in rainfall, and also paleorecharge.

And then, I would also like to talk about the mechanisms of flow, piston flow versus preferential flow, and liquid versus vapor flow, et cetera. And, finally, we'll discuss some of the techniques for estimating subsurface flow. So, this can basically serve as an outline for my talk, and I may as well stick it up there.

This is a review of some of the studies and I don't really expect you to be able to read that, but studies that have been conducted throughout the world on subsurface flow in arid settings and the varying rainfall rates from 80 millimeters per year to 400 millimeters per year and the techniques used to evaluate subsurface flow, environmental tracers, physics, and the fluxes estimated for these different sites from .03 millimeters per year to Ronit's data where she estimates 110 millimeters per year, and then some different types of settings. Based on reviewing data from these various sites, we got some idea on the controls on subsurface flow.

First, I would like to mention some terminology concepts. People generally talk about recharge rates when they're talking about subsurface flow in arid settings. However, oftentimes, especially with the very peak and saturated sections, it's difficult to determine from studies conducted near the surface whether that would actually recharge the water table. So, I think it's better to restrict the use of the term "recharge" to cases where it is highly likely that it is actually recharging the groundwater and to use more specific terms; maybe "infiltration" for water movement into the surface and "percolation" for deeper penetration of water. And, if you don't really know which way the water is flowing, if it's up or down or laterally, then you should probably just restrict yourself to talking about water fluxes.

DR. DOMENICO: Excuse me, that table, is that all of those indicative of the unsaturated zone or are there some saturated zone studies there?

DR. SCANLON: These are unsaturated zones.

DR. DOMENICO: All unsaturated?

DR. SCANLON: Yeah.

DR. DOMENICO: Thank you.

DR. SCANLON: So, to evaluate the controls on subsurface flow examine soil texture, vegetation, topography, preferred pathways, and climate. A lot of people suggested soil texture is very important in controlling subsurface flow, particularly the texture of the shallow surficial sediments because they provide storage capacity and retain the water near the surface where it is more readily evapotranspired. And, this concept is used in the barrier design at Hanford for the surface sediments and use a ticking off section to retain 100 year storm or a 1,000 year storm or whatever.

And, the studies by Cook in Australia also suggested that there was a negative correlation between water flux and clay content in the upper two meters of sediments. Some of the studies in that review showed higher fluxes in coarse grained soils; for example, the sand dunes in Saudi Arabia where you have 80 millimeters per year of rainfall and you have 23 millimeters per year flux. That's up to 30% of the rainfall infiltrating in that pretty coarse grain section.

Variability in soil texture is also important. A layering of sediments in natural capillary barriers and also layers that may hold up water and form perched water conditions. So, sufficient sediments and, I guess, the thickness of sediments above fractured rock at Yucca Mountain is pretty important.

Vegetation is also important in controlling water movement. A number of studies have documented higher water fluxes in bare soil than in vegetated soils. For example, lysimeter studies by Gee and others and also by--in Las Cruces. And, the most obvious demonstration of the effect of vegetation is the tearing of vegetation in Australia where the mallee vegetation was removed and fluxes increased from .1 to .6 to 4 to 28 millimeters per year. Different types of vegetation are not as effective in removing subsurface flow or transpiring water. For example, there is very little difference in the sandy soils with annual grasses at Hanford versus the bare soil. So, you need to plant with the deeper roots, et cetera.

Some studies have shown that plant roots may act as preferred pathways. Tritium has been found down to 10 meters depth in Australia and it is attributed to annual flow along the eucalyptus roots. But, they go down to 20 meters. Most shrubs in arid settings in the southwest are probably shallower. Vegetation is pretty opportunistic and will concentrate where there is quite a bit of water and you often see vegetation concentrating in washes and fissure zones and some of fissures in Texas are--by lineation of mesquite trees.

So, where there is a lot of water movement, vegetation will move in and then act as a pump and pump out that water. And, Phillips has suggested that one of the reasons we may not see large variability in subsurface fluxes in arid settings in the southwest is because the vegetation acts to remove a lot of the water and make a lot of different sites pretty similar.

Topography, I guess, Alan Flint will talk some about that this afternoon. But, where you have ponded water conditions in the surface, you will get subsurface flow, washes, playas, sinkholes in Australia have shown deep tritium, and fissured sediments in Texas where there's basically ponded water conditions.

Climate variation, a lot of people are asking when you're talking about a site what is the long-term mean annual rainfall of the site? And, really, I don't think that is a very good indicator of subsurface flow because the seasonal distribution of precipitation could be much more important than the average precipitation. For example, winter precipitation is much more effective in infiltrating soil and moving down beyond the zone of evapotranspiration because ET rates are much lower in the winter. Also, you have a lot of interannual variability in rainfall in arid settings and you may have no rainfall for 10 years and much higher than normal rainfall, you know, in one year and that can more effectively recharge the system.

And, chloride profiles in Australia and

southwestern U.S., reductions in chloride at depth have been attributed to higher fluxes during Pleistocene times; Beatty site, Nevada Test Site, et cetera.  And, here is an example of the some of the chloride profiles; the Ward Valley data and Beatty data from a report by Prudic and you can see extremely high chloride concentrations at Ward Valley and also high peaks in the Beatty data, but a reduction in chloride below the peak and, particularly, at the Beatty site.  This is attributed to higher recharge during Pleistocene times when the climate was cooler and wetter. Some people suggested maybe the reductions in chloride below the peak could be a result of preferential flow.  However, I think if water is moving preferentially, you wouldn't expect complete flushing of the chloride as you see at the Beatty site.  This is actually--I think, Prudic suggested this as the an old paleo channel.  Some more in Texas showing the relationship between chloride concentrations and decreasing fluxes to the peak and then increasing fluxes below peak and then a higher recharge rates during Pleistocene times, 10,000 to 20,000 years.  And, Nevada Test Site, I think, Tyler and others report high recharge rates during the previous Glacier period of 120,000 years, also.

And, lastly, preferred pathways and actually fractures, dessication cracks, root tubules.  I guess, most of the documentation on preferential flow has been from fractured rocks in arid settings and also, I guess, dessication cracks and root tubules. But, I'll talk more about that when I discuss the mechanisms of flow.

So, I want to move on from controls in subsurface flow to talking about the direction of water movement. You would feel if somebody asks you which way the water is flowing, you couldn't answer the question because maybe you didn't know much about the system. But, it's a pretty difficult question in arid settings and there are a number of reasons for that and some are that the fluxes in natural arid settings can be extremely low relative to the uncertainties of the techniques that we have to estimate these fluxes. And, also, it still can be quite complicated because you can have liquid and vapor flowing. You can have a variety of driving forces; water potential, temperature, and osmatic potential. And, also the flux direction can vary spatially and temporally. I'll elaborate on that a little.

Liquid and vapor fluxes and liquid flux controlled by hydraulic heads, some of matrix and gravitational potential gradients, and vapor flux, isothermal vapor flux controlled by major potential gradients and thermal vapor flux by temperature gradients. So, in isothermal systems, if you have upward decreasing matrix potentials, they have upward water flux and vice-versa. However, in anisothermal systems when temperature is also important, you have to consider liquid and vapor movement and in the zone of seasonal temperature fluctuations from two to ten meters, you would have a net downwards thermal vapor flux. Below that, you can have the effect of geothermal gradient resulting in upwards thermal vapor flux. Then, the next water flux will depend on the balance of these.

If we look at some water potential data for a typical water potential profile from Texas--and this is similar in Nevada Test Site, Beatty, and Ward Valley--water potentials--this was sampled after a rainfall event. So, you have high water potentials shown in blue, close to zero near the surface, and then decreasing below the wetting front. So, you have a pretty shallow wetting front and very steep gradient there. But, below that, you have a gradual increase in water potentials. And so, you have an upward decrease in water potentials from about -4 to -12 and this suggests an upward driving force for liquid in isothermal vapor movement.

We can also compare this to the equilibrium line. This is basically a no-flow line where the major potential force is balanced gravity. So, there's no flow. Water potentials that clock to the left of the equilibrium line suggest upward flow under steady flow conditions and to the right suggest downward flow. So, these potential profiles suggest that there has been a net upward flow of water and this is similar of Beatty and all the other sites that I just mentioned.

So, for some time in the past, we've had a net upward flow of water and how long is represented by this is difficult to determine. If you were simply relying on evaporation alone, it would take a very long time, thousands of years. But, if you include roots as a sink term, then it may not take that long. At the Nevada Test Site, Sully and others recently reported that below a depth of about 40 meters, the water potentials move to the right of the equilibrium line and suggest downward liquid flow below that point. But, also, they have an upward geothermal gradient and Sully suggests that the upper geothermal gradient, upward vapor flux balances the downward liquid flux and there is no negligible flux. So, it's quite complicated and that's why the direction of flow is sometimes a difficult question.

I'm going to skip down to mechanisms of water movement. And, here, the two basic end members, piston flow versus preferential flow, and most of the studies recently have been focusing on preferential flow and it seems like, well, flow is always preferential. You get the impression that, you know, there's just no piston flow anymore. But, piston flow is basically talking about displacement of the initial water by the infiltrating water. Experiments conduced at Las Cruces, infiltration experiments where they applied two centimeters a day of rainfall for 80 days--they did various experiments, but that was one of them--and they visually observed the wetting front and they didn't see a lot of irregularities. And, also, they looked at the--bromide tracer and they looked at the position of the bromide tracer relative to the pressure front. And, under piston flow conditions, you would expect the pressure front to precede the solute front by an amount equal to the displaced water. And, they saw that at Las Cruces. And, also, as time progressed, this separation should increase. A piston flow is occurring and that was found. And, also, when they increased the initial water, the separation showed increase. So, all these findings were consistent with piston flow. Chloride profiles in Australia after the vegetation cleared show also the relationship between the pressure front and the chloride front also suggested piston flow under those conditions. And, chlorine-36 profiles, Gifford and some of the profiles in Texas, single peak, suggests piston flow conditions.

Preferential flow then, as you all know, refers to water moving along preferred pathways and can include macropore flow or other flowing along non-capillary size pores, unstable flow where the velocity of the wetting front increases with depth, and then you have fingering associated with this and it can from organic-rich topsoils or various other reasons and funneled flow where you have flow along sloping sedimentary layers. And, I guess, most of the evidence for preferential flow is provided by the two studies that we're going to--Nativ and Fabryka-Martin and Al Yang's tritium data. There's no real evidence for unstable flow in arid settings. I think the reason is that you need to be in the gravity flow regime for unstable flow to occur and maybe the flux is too low and the soils are too dry for unstable flow. The other thing is the importance of liquid and vapor flow and this is important for nonvolatile tracers, tritium, Carbon-14, and radium. And, it also comes into play when you compare different tracers like tritium and chlorine-36. But, I'm not really going to talk about that.

I just want to show some slides or some overheads of fissure flow in Texas. This is where surface water--its intercepts run off. So, there is ponded conditions. The blue represents beneath the fissure and the green is 10 meters away from the fissure. You can see that the water contents are higher beneath the fissure. The chloride is flushed out, but it's restricted to the upper 10 meters and then chloride increases to a value similar to the profile 10 meters away. It's a very localized effect. It would be called more focused recharge. If some of the opponents to Ronit's studies were suggesting recharge and then lateral flow, there isn't really much evidence for a lateral flow. And then, the water potential data, high indicating wet conditions and then decreasing to values similar to the profile adjacent to it. So, in this example, it doesn't extend to the water table. I mean, it's fairly shallow; in fact, 10 or 15 meters. And, there probably is some preferential flow and probably is more lines of tritium beneath this peak, but I think most of the flow is not moving below this depth. And, I think the reason why we're seeing this sharp increase in flow is probably natural capillary barriers caused by the layering of sediments at this site, some sandy layers.

We also did some tracer experiments. This is a fracture. The term "fissure" is used to describe the gully at the surface and then, beneath that gully, there's the fracture that varies in width from three or four centimeters to one or two centimeters and extends down to at least six meters. Tracer experiment using bromide, et cetera, showed that there was preferential flow along the fracture, as you would expect, and not adjacent to it.

So, there are a number of issues with regard to preferential flow. Can we estimate the relative importance of piston and preferential flow for different types and different size and also what is the importance of these two? Preferential flow is probably not that important for the contaminants like nitrate because you need to move large quantities of those contaminants to exceed the health standards. But, for something like a pesticide where it exceeds health standards in part per billion range, then preferential flow is pretty important. So, you need to evaluate it with respect to the contaminates that you're looking at. Also, continuity of preferred pathways is pretty important in arid settings. Sediment type settings like Ward Valley or Texas, if there aren't fractures--I mean, a lot of the preferential flow would probably be associated with roots and they don't extend very deep in the system. The local input conditions, for a while it seemed people said that you needed ponded conditions at the surface for preferential flow to occur and now they've gotten away from that and said that, well, you don't need ponded flow. But, you may not need it for preferential flow, but I think if you have it then you are much more likely to have preferential flow. Input conditions are still important. And then, the interaction between the preferred pathway and the surrounding matrix, if your surrounding matrix is close to saturation, then it's not going to be able to imbibe the water. Whereas in sediments, if you have extremely dry sediments, you would expect that the sediments would imbibe the water and for that you need to evaluate the retention function and stuff like that. And, also it's important, the flux. I mean, maybe the flux through the preferred pathway is so rapid that the rate at which it's being imbibed just doesn't have any effect. I'll talk some about the techniques for evaluating flow in a minute and then the types of information required for modeling.

So, next, I want to move on to techniques. I'm not really going to discuss any of the soil physics techniques, but they are pretty important in providing us with an understanding of current processes and what's going on at the moment. We'll talk about the environmental tracers. I presume most of you are familiar with the meteoric chloride approach. Basically, the flux is equal to the chloride deposition rate divided by the chloride concentration in the soil water. So, if the deposition rate in the area is constant, then the flux is inversely proportional to the chloride concentration in the soil water. So, you can simply use it qualitatively. If there is no chloride, there is high flux. It has either flushed out any accumulated chloride and it prevents the accumulation of chloride. If there is high chloride, it suggests low flux.

There are a number of assumptions associated with the chloride approach and some of these are being questioned. For example, the downward flow assumption, I just showed you some water potential data that suggests that net upward water flux in the unsaturated zone in the southwestern U.S. is in the top 10 to 15 meters, evidence for transient conditions, Australia and southwestern U.S. associated with high recharge during the Pleistocene and in Australia associated with the removal of vegetation. So, steady state flow assumption does not apply. And, do we really know what the chloride deposition rate is? It may be difficult to estimate, but it seems like when we use the prebomb Chlorine-26 ratios to estimate chloride deposition rates, it seems pretty uniform in different areas. So, here, just showing playa setting in Texas where you can just use the chloride qualitatively. No chloride beneath the playa and higher chloride in the inter-playa setting.

Chlorine-36, you can use it in three different ways. You can look at the bomb pulse signature, you can look at temporal variations in cosmogenic production of chlorine-36, or you can look at radioactive decay. Chlorine-36 is pretty good because in arid settings generally we have high chloride.

Limitations are that oftentimes the bomb pulses within the root zone--however, that's good because it suggests that the flux is low. In zones of high flux, for example, beneath the playas and in fissure settings, the chloride concentrations were too low to evaluate chlorine-36. The cosmogenic production signature is only two times greater than background and oftentimes not preserved probably because of diffusion. And, temporal variations in cosmogenic production would also lead to uncertainties in age estimates based on radioactive decay.

So, lastly, tritium. Chlorine-36 indicates liquid flow and tritium indicates liquid and vapor flow. Limitations with this, the same as Chlorine-36. Oftentimes, it's just found in the root zone. Natural arid systems have low water contents. So, it's oftentimes difficult to get sufficient water for tritium analysis and the samples can be contaminated during collection. One of the issues at Ward Valley was possibly occurrence of preferential flow because of tritium found at depths down to 100 feet. From 3200 feet, tritium levels ranged from one to two tritium units and were greater than plus or minus two times the standard deviation associated with the analysis. So, it suggested that they were finding quantities of tritium at depth. However, it could not be explained by vapor diffusion alone because most of the tritium--most of the water molecules because of the large density difference between liquid water and water vapor, five orders of magnitude, most of the tritium was in the liquid phase, and vapor diffusion in equilibrium with the liquid just would not allow migration of the tritium to that depth. It couldn't be explained by liquid diffusion because at the low water contents at the site the diffusivity was too low. So, it was attributed to the possible contamination during sampling; took air samples, large volumes of air, and could possibly be some leakage in the lines or some contamination from present day tritium levels.

This is another example of tritium beneath the playa in north Texas and you can see quite variable tritium levels. This is a structured clay soil, and I think when you get to this low water content, it corresponds to a sandy layer and the highest tritium levels here--I think this can maybe be attributed to the fractured clay soils contacting the granular material and the end of the preferred pathways and then moving out into the granular layers and then acting as possibly a reservoir for tritium.

Then, we want to talk about how we can evaluate preferential flow. In most cases, the preferred pathways are vertical and so it's really difficult to intersect vertical preferred pathways with vertical boreholes. I think the tunnel boring at Yucca Mountain should give some direct evidence possibly of preferential flow along fractures. In shallow soil systems where it's a lot easier and where most of the studies have been conducted in more humid settings, they're still simply doing dye tracing studies using blue dye or red dye or whatever to delineate the pathways and really have not made much advances in quantifying the relative importance of piston flow versus preferential flow. A recent article in WRR in the structured clay soils suggested that less than 1 to 2% of the flow was flowing along dessication cracks in the clay and the rest of the water was flowing in between the ped surfaces, 6% flowing between ped faces. So, it's quite difficult to evaluate the relative importance. But, sampling groundwater provides good evidence. It integrates a larger area and can be a good way of evaluating preferential flow also. For example, if there are perched aquifers at Yucca Mountain, then sampling for bomb pulse tracers, et cetera, would be important.

Soil physics information is important for understanding the processes. And, in sediment settings, we've been monitoring different soil physics parameters for seven or eight years and we haven't found anything to suggest that there is a bypass flow in these sediment settings. But, they may be able to find some information in the fractured rock.

And, environmental tracers are good, but there sometimes can be a lot of explanations for different types of tracer distributions and it's not a unique solution. For example, if you have levels of chlorine-36, 490 or something times $10^{-15}$, it could be prebomb or again it could be post bomb. It could be extremely rapid flow. So, you have to consider a lot of factors. I think, basically, based on the discussions for Ward Valley, you have to include all the different types of information that you have; soil physics, environmental tracers, and come up with a comprehensive view of what you think the processes are and how important the different processes are.

So, to conclude, I come back to the basic issues; these various factors that are important in controlling subsurface flow with regard to Yucca Mountain. The thickness of the alluvial cover may be quite important and also vegetation. Most of the hydrologic models have basically excluded vegetation. I think we need to start considering if it's important. The direction of water movement as more information becomes available, we get a better understanding of what are the controls of water movement in different settings. Spatial variability, focused recharge versus he preferential flow. I think, oftentimes, some people like to call focused recharge beneath playas or washes macroscopic scale preferred pathways, Gee and others a couple of years ago, but other people distinguish preferential flow as fractures and cracks and stuff like that. But, I think, oftentimes, when you have focused recharge, you can also have--more likely to have preferential flow associated with it. Then, the temporal variability, episodic recharge. We usually use recharge, we say millimeters per year, and oftentimes that's for comparison purposes between different techniques. But, it may be better to say millimeters every ten years because you might get recharge only one year or flux only one year.

DR. LANGMUIR: Can you wrap it up, Bridget? You're over schedule here by five or six minutes.

DR. SCANLON: Oh, okay. Well I'll just push.

DR. LANGMUIR: Okay. All right.

Questions from the Board? We're going to get you to comment on everyone else's talk, I think, later on. We'll get you involved in that way, I think. Any questions?

DR. PALCIAUSKAS: I noticed in one of the studies you mentioned that removing the vegetation increased the percolation or infiltration by 40 fold in one particular area. That is an interesting piece of information because if one has a regulatory type of a phenomena for 10,000 years, it basically says that whatever we characterize today in terms of preferential pathways or percolation or infiltration is sort of meaningless over the next 1,000 years. So, would you care to comment on that?

DR. SCANLON: Yeah, vegetation is really important. I mean, studies at Hanford where they had bare lysimeters for several years showed increasing water storage with time and then the lysimeter was vegetated and within three months all that excess water was removed. I mean, it's very important. And, one of the problems that they're facing at Hanford is to try and predict land use over the proposed life of their low-level repository because they think a lot of it may become farm land and crops and stuff like that. But, in Australia, that example where you have 40 fold increase, you're going from eucalyptus vegetation which has roots down to 10 to 20 meters to crops which have very shallow roots. So, there is maybe an extreme case there. You know, I mean, most of the shrub vegetation in the southwest, creosole probably, generally roots in the upper one to two meters. It's a problem. Also, I mean, if you ask performance assessment what is the recharge rate at a site, there is no "the" recharge rate. I mean, it's spatially variable, it's temporally variable, and you need to include the variations in climate like Alan Flint has included in some of his simulations and stuff like that. So, it is complex.

DR. PALCIAUSKAS: I just have one more brief question.

DR. LANGMUIR: Okay.

DR. PALCIAUSKAS: You talked about piston flow and preferential flow and when they occur and so on. I'd like to make one generalization and perhaps you can back me up if you think it's an appropriate observation or not. Even in a very clean sand, displacement experiments have shown that you always bypass a certain amount of water. So, you have piston flow occurring along, let's say, 60% of the pore volume and 40% is being bypassed. And, as you go to a more and more heterogeneous systems, you get more and more preferential flow. Can you corroborate that?

DR. SCANLON: My feeling is that--I mean, Wierenga's experiment, the Las Cruces trench experiment--and Tom can comment a lot on that, I think--is that there they really saw preferential flow and most of the water--I don't think there was enumerable fracture. I mean, I think all the water that was in the soil was partaking in the flow. Heterogeneity was an issue that came up at Ward Valley. You know, people talked about on the local scale if you go from gravel to clay, but on a larger scale it appeared more uniform. I mean, there was no distinct layering or stuff like that. And so, you know, when you talk about heterogeneity, you have to also talk about the scale that you're talking about. In very dry settings where most of the water is absorbed on the grain surfaces, I don't think heterogeneity has much of an effect, you know. You're talking--

DR. PALCIAUSKAS: I guess what I meant was that the heterogeneity is much more important when you increase the flux, because then most of it has to be accommodated by the preferential paths. With a very dry environment, you have basically static, water trapped in a very, very slow matrix. But, as soon as the flux is increased, then, of course, preferential pathways become much more important, maybe even dominant.

DR. SCANLON: Right, right. Well, that's because--I mean, most of the preferential flow studies are in the humid northeast. Cornell, I mean, nearly all the studies have been done there. So, yeah, in a higher flux setting, yes, I think you see more preferential flow. But, as far as the southwest, the desert southwest, under natural interfluvial settings, I don't see much evidence for preferential flow. Jon Hendricks did studies in Holland on stable wetting fronts, finger flow, and stuff like that where they had organic topsoils and he moved to New Mexico and he is still trying to find preferential flow. So, yeah, in humid settings, higher fluxes, yes, there is much more preferential flow, but in arid southwest in interfluvial settings where it's really dry, I don't think it's that important.

DR. LANGMUIR: Thank you, Bridget.

DR. SCANLON: Okay.

DR. LANGMUIR: We're scheduled to take a break. Let's do so and return at 2:50.

(Whereupon, a brief recess was taken.)

DR. LANGMUIR: Shallow infiltration and initiation of fracture flow at Yucca Mountain and the speaker is Alan Flint, U.S. Geological Survey.

DR. FLINT: While everybody is getting seated, actually Victor was expecting me to be somewhat entertaining, and so I thought I'd start off with a little story to give you an idea of my philosophy with which I'm currently working. We're in somewhat difficult times, I suppose, in the Yucca Mountain Program and we have to make decisions and push the limits of what we know and what we understand to get some kind of information out. This is a story of something that happened to me several years ago, actually 21 years ago, that helped me to develop my philosophy.

I was in Southeast Asia in an air base called Utapao and we were flying missions into Cambodia into the airport in Phnom Penh. It had been surrounded by the Khmer Rouge and one of our planes was in there and they couldn't get one of the engines started. It was a C-130, a four engine turbo prop. They said, well, we can't fly it out; it's not safe. So, they had two engine guys and myself, I worked in instruments. We flew in to this surrounded air base to get this plane out and we got off our plane and asked the pilot what the problem was and he said, well, the engine won't start. We said why don't you just fly it out and he said it's not safe. So, we went over to the engine and got out to start working on it and the Khmer Rouge opened up on us with their 105 Howitzers. As they were walking these shells closer trying to get the range, the pilot came down and yelled let's go, let's go. We said what about the engine? He said three engines is more than enough. So, that's where we are right now. So, we're sort of flying on three engines, I suppose.

I'm putting information out and it's the best information we have at the time. I think it has some relevance to what we're talking about. I'm going to talk about shallow infiltration and the initiation of fracture flow. The goal of the infiltration study, our overall objective, is to provide the upper boundary conditions for numerical models that are realistically variable in time and space. As you've heard from the previous speakers, we know that infiltration has a seasonal distribution and we know it has a spatial distribution. I want to talk about those.

The methods that we've chosen to meet that goal--these are milestones that we're trying to produce over the next six months to a year--is, one, the development of a map of net infiltration based on 10 years of record. This is a statistical analysis of flux. We also have a numerical model that's based on deterministic and stochastic processes--stochastic like rainfall--that can reproduce that map under current conditions. This is a soil physics approach to solving the problem. And, the third milestone that we're trying to incorporate now is to model past and future climate scenarios that we can change the soil development over time, in particular, change the vegetation. Even if we're looking at 10,000 year scenarios, we have ways we think we can do that. And, changing atmospheric conditions; even things like ozone can change evapotranspiration by the way it affects radiation loads.

The objective of this presentation is to present the meteorological conditions that existed during the collection of the infiltration data that I'm going to present. I think it's important to see where we are in that data collection. I'm going to present an overview of the field data that was used to develop our conceptual model, and I'm at the same time going to present our current conceptual model of infiltration. I'm also going to present some of the methods that we've chosen to extrapolate point measurements of infiltration to the new 3-D site scale model.

The data set that we're going to be looking at has a large temporal and spatial precipitation data that's available from all over the region and some of our own stations. I'm going to concentrate on the 90 neutron holes that we have, 6 to 67 meters deep, and a lot of information. These are topographically located in ridgetops, sideslopes, terraces, channels. And, we've collected monthly readings at .1 meter increment from three to nine years. We do this monthly and, if we have runoff events, we can do this more frequently. So, this is the data set we're going to cover, and I'll try to go through the three different techniques that I'm going to use to estimate flux. The statistical technique and the soil physics technique, I'll get to those.

In terms of where we are in the region for precipitation, this is a map. This comes with a report that is in technical review now. It's going to be turned over to

DOE at the end of July. This is an estimate of average annual precipitation. We also have an estimate of flux on an annual basis from this. You can see the Yucca Mountain right in this location, Las Vegas down here. You can see the Spring Mountain's fairly high rainfall rates. In this map alone, we're looking at rain from 40 millimeters to over 400 millimeters. So, an order of magnitude difference in precipitation even though it's an arid climate. Things that you can note are the rain shadow effect of the Sierras, very important in looking at climate change. We have elevations up at the north end that are very similar to what we have on this end, only they're lower rainfall rates because they're in the rain shadow. In French's report, he's suggested that this is an excess zone to the right and that seems to be the case that we see here. But, we're looking at about 170 millimeters a year average precipitation. Those of you that want to use Ranier Mesa as an analog site for Yucca Mountain, just keep in mind that it's got double the precipitation under the current conditions.

On a local scale, we also have to keep in mind that any particular storm has its own spatial variability. This is the storm of March 10 and 11. This is the one that caused all the runoff and Forty Mile Wash swept away some engineer, not a hydrologist I want to point out. And, look at the large distribution here. Again, we have 40 millimeters from one storm upwards of 130 millimeters to the north end of the mountain. So, quite a bit of distribution in rainfall. And, even over the potential repository site, you see a large variation. We have to keep that in mind when we start looking at recharge and infiltration that these events are quite variable.

In terms of the long-term record, this is from Station 4JA. This is about five miles east of the mountain itself. This is near the field operations center, the hydrologic research facility, and this is a record from 1958. The kinds of things that you can think about when you see something like this is if we have our tritium peak back in this period of time, how does this system respond when we have these low rainfall rates? But, where does the record in infiltration that I'm going to talk about lie in here? This is average precipitation. This is a five year sliding mean because I'm going to show you five years of neutron hole data, the results, the statistical analysis on five years of data. The key points here are, one, the annual precipitation on a five year sliding mean, the last five years have been the wettest on record. In particular, the winter has been the wettest on record. And, as was pointed out by Bridget, the winter precipitation is very important in terms of recharge.

So, what we're seeing today and what you're going to see is the wettest environmental conditions we've seen and, most likely, the highest fluxes we have seen. Keeping in mind again if you're looking at--when we start talking about tritium, chloride-36 movement, we're at depositional periods back in this case where we have fairly low precipitation. That may be important and we may get underestimates of recharge because of this distance of this time series. Also note the trend. So, in another 50 years, we should have maybe 300 millimeters a year if you believe in trends. It's something we can predict and at least try to get at.

Okay. Let's talk about the site for a second. It is an arid environment, average of 170 millimeters a year. Volcanic tuffs, welded and nonwelded. Variable thickness of alluvium, again one of the most important things we're going to find. And, that we have faults and fractures under these highly variable surfaces. Those become real important.

For those that haven't seen it, this is looking at Yucca Mountain from the southwest. You can see some of the bedding plains, the Tiva Canyon on top, the nonwelded PTn, and I believe the tunnel boring machine has gotten below this zone now and is down in here somewhere on the other side. This is looking just 180 degrees different. You can see some of the washes we're looking at. The footprint of the repository might be somewhere in this general area right in here. So, we're over mostly the ridges and the sideslopes in this case.

This is the site scale model that we worked with several years ago when we made our first estimate of a flux map and looking at spatial distribution of flux, the potential repository boundary. What we did is put together all the data we could on matrix properties and neutron log data to come up with a flux of what's flowing through the matrix. That's what this graph was. This was from high-level waste last year. This is only flow through the matrix. You can see the range of fluxes we estimate using an assumed unit gradient, the relative permeability of the rock at its current saturation. With the Paintbrush nonwelded tuffs, up around 13 millimeters a year down to the Tiva Canyon Welded over most of the repository area on the order of .02 millimeters a year.

The next part of our program has been to incorporate the fractures into this. What role do fractures play and how do we initiate fracture flow? Is that number going to go up? Most likely, yes. An important point was that there were some fairly large fluxes there even in the matrix, much higher and not uniformly distributed, as was said earlier. Things to consider, variable depth of alluvium. The nature of the fractured bedrock; a lot of fractures, a few fractures, are they filled with carbonates?

What about the porosity? How does the porosity affect things? Topographic position, radiation load, soil depth, and timing of precipitation?

We look at the kinds of locations we have for our boreholes. This is Pagany Wash. This is the north end of the site. We have a series of boreholes in channels, we have them on terraces, we have them on sideslopes and ridgetops; varying thicknesses of soils. To show you an example, here at N7, we have neutron logging going on. You can see that we're moving out of the channel as we go up and down either side. You can see the vegetation. The creosote here has rooting depths on the order of at least five meters and up to 10 meters is the estimate from some recent studies.

Active channels, this is from that March runoff event. Channel flow in a lot of places. These holes we've logged on a more regular basis, on a daily basis in some cases. It takes a lot of manpower, but we think it's real necessary to capture this kind of information. And, it's something we don't see too much of. You see the foam floating around there. If there's any question about drilling fluids that we used earlier in the program, there is some still remaining on the sideslopes that comes down with the wash.

This is a terrace location. Note the rain gauge; on every one of these holes, we have a small rain gauge; over

150 of these rain gauges out at the site so we can capture storm information. And, sideslopes moving up the hill, there are some reasons we can't drill on the steeper sideslopes. It's, more or less, a safety issue, but those could be more important. And then, finally, ridgetops.

What do we look at when we get our neutron logs and what kind of information can we gain from this? This is an example of what you might expect in a channel without runoff or even in some of the terraces. You can see wetting fronts moving down, more or less, kind of a piston type flow and very little change at depth, although we may have some changes whether it's due to the neutron logs or due to how we do the measurements. In ridgetops or sideslopes, it's real important to point out these big changes with depth that we can see down to 12 or 14 meters. Very shallow soils, less than a meter of soil, and we get these large variations. So, this data then we can put through our statistical analysis to try to get an idea of what's controlling flux and maybe some idea of what the flux actually is out here.

We used one technique taking the water content over a year or several years and take the standard deviation. That way, you can put all of this information on one graph. This is the standard deviation. You can see a large variability right above the interface tuff/alluvium and then a smaller variability, but still different from some level where we think that we have either a steady-state condition or, at least in the 10 years of record or five years, not much change. But, we can identify where we may have a depth of a wetting front. We're going to use this information in a correlation matrix that I'll show you later.

Okay. If we want to evaluate the potential for fracture flow, two things that we felt we needed to know. One is what are the properties of the filled fractures or the open fractures at that interface. And, number two, what's the water potential at the tuff/alluvium interface? Those seem to be the controlling factors to whether or not we get fracture flow and how it represents itself in our neutron logs. So, I'm going to spend a little bit of time talking about these in a couple of slides later.

This is an example of a very interesting borehole, N11. This is up on Mile High Mesa. It's a fairly high rainfall area, considerably higher than over the main repository area. We don't feel we have as many filled fractures here as we do at lower elevations. What we're seeing is not much of a change if you look at the first four meters, but as we go from a welded to a moderately welded--in fact, probably up in here, it becomes more moderately welded where the fractures may tend to terminate at least from field observations and those terminating fractures then have to dump their water into the matrix. There's no other place for them to go. So, you can see this nice increase in water content as it comes across this transitional zone and we see this in several of the boreholes at these high elevations. But, we wouldn't have any evidence of flow through the fractures, yet we see the water ending up at the bottom. So, we know it happened and we know it can happen fairly quickly.

In another case at a lower elevation where we know we have lots of carbonates because we drilled the holes and found lots of carbonates in the fractures, although again we see at the near surface not quite as much evidence for a very large area. We see an increase in saturation more uniformly distributed over the site. And, in some cases, we know that this is fracture fill material because the water contents exceeds the porosity of the rock which means there's probably carbonates there. The carbonates increase the porosity and filled with water give us those higher readings. So, these are different kind of evidence where we have shallow soils of pretty good fracture flow.

Now, this is the standard deviation of water content at the tuff/alluvium contact in the alluvium. First, there's a depth of alluvium. That's one of the ways we can look at how things change. If nothing is changing based on what we had when we drilled these holes several years ago when it was a much dryer climate, the water potentials were not enough to get fracture flow to occur. Really, it's these short-term events where we increase our water contents for a short period of time, weeks or months, and then reduce back that we can get fracture flow to occur. A couple of runoff events--but I think it's real striking to see that if you have more than about five meters of alluvium, you don't change the water content at the tuff/alluvium interface in at least the last five years. So, this depth of alluvium becomes very important. It's these large changes that allow us to get flow into the fractures, we believe.

If we subtract the standard deviation above the interface with that below the interface, that is separate the top meter of rock versus the bottom meter of soil, we see the same kind of trend at about five meters. The changes in water contents, if there are any, are basically the same. We think that there's a good reason to believe there's equilibrium existing between the rock and the soil with the exception of anything above about five meters. So, this soil depth becomes very important in this analysis.

Well, here's an example in time series and looking at the same kind of data. Those standard deviations would come by just simply taking the standard deviation of all these points. This is the meter below the alluvium, a meter above the alluvium. As we go through time, at some point we have an increase in water content, but we don't see a change in water content in the rock itself until we cross some critical level and then we see a sharp jump. And, based on the analysis of all the neutron logs, we can make an estimate of this being fracture flow because it's fairly rapid and it exceeds the conductivity of the matrix itself. Well then, we drop back down again. So, the alluvium loses that water and we see a decrease in water content of the tuff until some point where we can cross that line again and this tells us something about what it takes to get fracture flow to occur in this particular site. So, we're going to ask two questions. What's the flux and what's the duration? And, the only other thing we're not going to be able to answer is what's the direction? So, is this drying out because it's moving downward or is it coming back up through evapotranspiration processes? This is an estimate of 18 millimeters a year going into the top meter. We have to figure out which way that's going because that's a fairly high flux. But, that's how much water we're moving in this particular system.

Well, if we look at the duration that this soil stays wet, we see something about soil depth. From 1990 to 1995, how many weeks could we maintain a water potential wet enough to keep fracture flow going? Whether it's a channel, a ridge, a terrace, a sideslope, the real important point here is that these very shallow soils, we can maintain fracture flow water contents for over 60 weeks. Remember now, wettest five years on record, particularly in the wintertime. So, we have a lot of potential for flow in this particular case and these events, all of these in here, are those conditions where you had channel runoff and water getting to the tuff/alluvium interface with deep alluvium, but through channel flow. So, that's very important.

This is an estimate of flux now by adding up all the times the water content went up. So, every plus was an added and anything else was just left alone. So, these are the estimates of flux going into the top meter of alluvium. As you would imagine, again anything deeper than five meters, any more than five meters of alluvium, we didn't see a lot of flux at least in the last five years. But, anything shallower than that, we saw quite a bit. Somewhere around 80 millimeters a year was our highest. The question is is that water continuing on downward or is it coming back up? There are mechanisms to make it go in either direction and that's something that we have to quantify the direction right now. We're just starting to get some information on that. If we look at this in terms of alluvial thickness, we see another pretty good picture.

If you remember from the previous graph, we didn't see any--we couldn't maintain a water potential at the interface for fracture flow to occur. That's because there was no soil there. But, why do we have so much flux? It's because these are exposed bedrock that can take water directly from rainfall and many of these are channels where we have runoff. Where do the channels exist that have no soil and fractures in them? Well, fortunately, they're up in the--right over sort of the repository area and we have easy access to those and can get some pretty good measurements. So, we have quite a bit of flux. Again, you can see upwards of 80 millimeters a year. On average, this might be somewhere around 25 or 30 millimeters a year for the whole area.

This is our correlation matrix. This is the statistical analysis now we're trying to do because we have only 90 neutron holes and we have to represent a much larger area. So, we're trying to come up with an estimate of the distribution. You can go through and look at these, but the one that really stands out is if you want to know flux through the top meter, -.69 correlation with depth of alluvium. That is, as the alluvium gets thicker, the flux goes down. So, if we just knew depth of alluvium, at least we could make an estimate that would give us an $R^2$ of about .5 or something like that if we knew that. We can also run multiple correlations and try to do a little bit better job, but we need to start looking at this in some detail so we can distribute that data at least in this case statistically.

This is our first attempt at a depth to bedrock map and it's not complete.  We have more information down below this old 3-D site scale model and this area that we're looking at now and then further down is about the size of the new 3-D site scale model.  Zero to 5 meters where we have our highest flux is probably in the top meter of 40 millimeters a year going into that top meter of tuff over this brown area.  And then, somewhere around 2 millimeters or less going into the area that's greater than 3 meters.  Although I said that 5 meters was the real critical depth, 3 meters was a little easier to do and we're still working on this.  So, we can use that information then to build a better map of the fracture flow, add that to the matrix flow, and get a better idea of what the distribution of flux is.

Okay.  Now, we're going to look at another approach for making the calculation of flux and that's using a soil physics approach rather than a statistical approach.  This is from heat dissipation probe data to near surface.  We put these out in the field during this very, very wet time.  So, they all start out at around zero water potential.  And, we can see over time the near surface, 7 meters, drying out and picking back up again every time it rains, but a general trend overall to much dryer conditions.  The tuff/alluvium interface is at about 74 centimeters and you can see that that stays at about a half a bar.  At a half a bar, we can still keep fracture flow going on in some of the filled fractures at any rate. It's this kind of information then, if we knew the fracture properties, that we could calculate a flux into the fractures.

These are some of the fractures that we see at Yucca Mountain and the exposures in the Tiva Canyon. Variable soil thickness. From here to here is about 12 feet or so. This is at NRG-5. I think some of you may have been there. These are the fractures that are filled with carbonate materials. We've taken these carbonate materials out and brought them back into the lab to come up with some of the properties. This is a rock sample with carbonated fill. We've cut it into slices so that we can measure water retention using a CX-2 system. We've also taken larger pieces and cored them and gotten saturated conductivities. So, we can get some important soil physical properties to make the calculations of flux. Generally, a water retention curve would look similar--or this is the one that we got from those particular samples for the rock and for the carbonates. And, the carbonates are distinctly different from the rock, but you can see we get some pretty high porosity rock out of that. We're doing more of this now. We have quite a few more samples that we're processing. But, now, we have a water retention curve, we have a conductivity, we assume a Van Genuchten function which is something that really needs to be tested for these particular soils and rocks.

We can also take some estimates of unfilled fractures. These are just certain techniques that were used to develop water retention curves for certain assumed mean aperture and a distribution of apertures within a particular fracture. And, we can put all this together and make some calculations of flux using a Darcy's law type calculation. And, that's what this is.

Volumetric water content, that's the red, over five years of record using a water retention curve for the soils, we have estimated the water potential at about 10 bars at that bedrock interface. We have some psychrometers downhole that we're trying to get working now to get some support for this. Just to give you an idea of what's happening, this is the air entry value of the filled fracture and this is an air entry value of a 2.5 micron fracture and you can see that the water potential stays much dryer and that we have very little flow into these fractures. In this particular hole, 8.3 meters deep, as you would expect, that's not going to have a lot of things happening.

If we look at a different system--this is .8 meters of soil thickness--and you can see when our water content jumps way up from a winter event, the water potential jumps down, and we actually cross over this air entry value, we can calculate some flux at that particular point. How long does it stay at this particular point? Well, these are monthly readings. So, it could have been that day, it could have been a week, it could have been two weeks, no more than four weeks certainly. But then, for the rest of the time, we don't see much change. So, here we had a flow event into the fracture.

If we look at another borehole, we see these--this is 2.1 meters in a channel--we see high increase in water content, but the water potential didn't reach the air entry value for those and I'm just using air entry as sort of a descriptive line on the graph. We can make the calculations to get the actual fluxes and I'll do that in a second. But, two other important periods where we got pretty good flow in fairly wet conditions.

Now, this is an example of a calculation then using that information for an assumed fracture. We have a fracture density of 10 per square meter, a 2.5 micron fracture or a 2500 micron carbonated filled fracture--that basically would be a one inch fracture every meter which you saw from that picture may be a little bit more than we would expect, but still not unreasonable--and 2.5 micron fractures per meter may be not unreasonable, but at least for illustration it's kind of useful. So, early-on, this is going from 1985 now. We start off at a pretty good flux, maybe a millimeter a year going through the carbonate. The soils stay wet enough for a period of time. This is coming off that '84-85 big rain events that we had in southern Nevada. But, the fluxes drop pretty quickly, a few peaks. But, the open fractures, we don't see any flow. And then, those two times we see pretty good flow, but because of the high conductivity of an unfilled fracture when it does flow, a lot more water flows through it versus the filled fractures. The filled fractures can have some small amount of flow for a long period of time under dry conditions. So, you have two systems both contributing to fracture flow. Average those out, the unfilled fracture might be giving somewhere around 30 millimeters a year over this period of time because we're flowing at 100 millimeters a year for 15 days or longer than that versus the filled fracture may be on the order of 15 millimeters a year under this example, these assumed fracture properties.

Okay. We can now take one other approach. That's the soil physics approach with the data that we have. One other approach is, more or less, a modeling approach that we're going to use with real conditions and stochastic conditions. So, this is a soils map. The rest of it is pretty much complete now. This was done by Scott Lundstrom. And, we're applying all the properties we can to the soil; relative permeability, saturated permeabilities, textures, all of that information. Overlying this is a vegetation map. So, we tried to put all this information together about the properties of the soil. This is what's going to feed into our Richards' equation based model.

For this area, we also have a solar radiation model. That's the basis for our ET modeling; solar radiation, net radiation, soil heat flux. This is a radiation model for the--this is, more or less, the 3-D site scale model, the new one. The potential repository is right in about this area. What you're looking at is zones of low radiation. This is December 21. So, all the blue areas are zones that if we had a rainstorm would be prone to staying wet for a longer period of time. Oddly enough, one of the most important wet zones up in here is sitting right over the top of the intersection of the Ghost Dance Fault and the presumed Sundance Fault. So, that may be a very important pathway. At any rate, we're using this information, all this information to try to model what we saw in the neutron logs.

That's what this is, an example of the one calculation. This is volumetric water content in the top meter. The red squares are neutron logs, the blue is the rainfall. There are two functions here. There is a continuum function for a Priestley-Taylor model which was developed in humid lands up in Oregon and it was simply applied just from information out of literature. There's a lot of stuff in the literature that's really pretty nice that you can use to make some first approximations. We developed a step function, Hevesi did and Lorri and I, that does a much better job. The step function has to account for the vegetation changes. It's real important that you know how your vegetation is responding in the winter versus the summer and that's what made all the difference in this particular model. So, what we were able to do is model the top meter. Now, with that top meter modeled in this particular borehole, then we went on to do the rest of the modeling using a Richards' equation approximation and using soil properties from Beatty because that's the only place we had soil properties from, but we figured that was okay for a while; similar textures. We do a fairly good job. In this case, we get to the tuff/alluvium interface at about 10 meters. We can see what we're doing as we're drying out from a part runoff event that occurred in 1984.

With this approach, though, then we can model in time the water potential at a specific location applying the Priestley-Taylor function, the radiation model, the soil properties. We're now modeling--although I don't have time to talk about it today--we're now modeling on a 30 meter grid size that holds--the site that I showed from the previous graph--and going back to 1987 to the current time, we have reproduced the runoff events or at least the occurrence of the runoff events that we've seen since then. So, we're pretty encouraged by the results of the model. We're able to get at water potentials at the tuff/alluvium interface using this technique. And, if we get the properties right, we may be able to make some pretty good estimates of flux.

This is where we are now. This is the 3-D site scale model. This is where we're working and this is what we're trying to get running over the next couple of months, and this is one of the major milestones we have is to produce this infiltration in space and time for this scale which we're pretty encouraged about being able to do.

So, the summary which I think these guys already said it--I think they saw my slide and knew what to say. First of all, the most recent years were the wettest. Near-surface fracture flow readily occurs when you have fairly wet conditions. The depth of alluvium may be one of the most important factors controlling the temporal and spatial distribution of fracture flow because the depth of alluvium is so variable over the site. The deterministic and stochastic models may be a viable way to investigate the influence of future climate change.

That's it.

DR. LANGMUIR: Thank you, Alan.

Questions from the Board?

(No response.)

DR. LANGMUIR: Let me ask you one, Alan. You didn't speak about it, at all, but obviously looking at the deeper flow in the system and the mountain, we have to get at the amounts of water that might be coming from some place like the Solitario Canyon Fault laterally into the system. So that all the water you're looking at is one input perhaps. You have historic inputs that maybe preceded anything you looked at, too, which the age dating stuff will come up with. But, you've also got lateral flow. How is it all being put together?

DR. FLINT: Well, what we're doing is putting together our best guess of what's happening at the near-surface. We're working, of course, very closely with LBL, Bo Bodvarsson, and his 3-D site scale model and taking our information and applying it to his model. Then, he looks at his model results and tells us where we might have to do some more work. And, putting it together by taking this--I think, a fairly well-distributed infiltration map in, more or less, one dimension. That's what goes into the surface. And then, using the 3-D site scale model to look at the potential because the Solitario Canyon is important unless you go--by the time you get down to the fault, there's so much alluvium over that that we don't think there's a lot of water moving in at that location. Where it would most likely be moving in is in the Topopah just below the PTn where the soils are fairly thin. Our data suggests that. So, by putting that into the 3-D site scale model, I think we can answer a lot of those questions, but we have to resolve the difference in scale between what we're doing on a very detailed surface map versus the scale that we can deal with on a large three-dimensional model.

DR. DOMENICO: Alan, part of the objective, of course, of your work is the upper boundary condition for the 3-D model and you're having some success with that. The thing that bothers me, how can the 3-D model be incorporated in the system performance to develop a realistic spatially and temporally varying flux through a repository? There has to be a big connection between what you and Bo are doing and what the people in system performance are doing as getting some sort of idea on the variability of flux through a repository under a variety of climate, vegetation, and soil conditions. That's a big key to me.

DR. FLINT: My group is working fairly closely with the people that are doing performance assessment, but there are--and, I'll maybe let some of them talk if I don't answer it correctly. But, there are two different groups in performance assessment. One group is very happy to take the surface flux that we've produced--and, again, this surface flux that I showed isn't the final answer yet because we don't know whether that water goes down or back up again. When we get the water that goes back down which we hope to have in the next six months, one performance assessment group can take that directly and they run that through their model and that's the group that's trying to look at groundwater travel time. There's another performance assessment group that's really starting at the repository level and moving down.

So, I'm up here at the surface and I'm making myself the tie between past and future climate and trying to say, well, I can turn your climate numbers or rainfall numbers into real fluxes, but I'm stopped at the surface because that's where I'm working and that's where I have my information. And then, this other group is down here. The only connection there may be between the 3-D site scale model--and I guess I agree with you. I think we have a problem right there in trying to take this very detailed information and get to the repository scale because the 3-D site scale model is a large scale model to look at the large system. Its purpose was not just to give a flux right at the repository because there are things that may be as important to the north where we have large fluxes that roll over faults, how we get infiltration to the saturated zone, the influence of the unsaturated flux into the saturated zone. So, all those things are real important. But, I agree. I think that's something that we really have to work on.

DR. DOMENICO: Is Bo's stuff sort of a connection between what's going on here and what might be coming through the repository?

DR. FLINT: I think Bo's stuff is the only connection we have right now. Is Bo still here? Bo, do you want to--if he can answer, Bo is right there. Yeah, Bo knows.

DR. BODVARSSON: This question you have, Pat, is a very good question. It's something that we have been struggling with quite a while because, like Alan said, the 3-D model as it is now is fairly coarse and there is a lot of issues we have been struggling with. They include, for example, the effects of the faults. That was questioned just a while ago, the Solitario Canyon Fault and the Bow Ridge Fault. The effect of going from the surface through repository region and the fact that maybe there's a fracture flow. All of these, we have been thinking about with Alan and some other people in the project. What we are doing now is this. He talked about extending the model in all directions and that's the step we are taking to address the Solitario Canyon Fault and the Bow Ridge Faults because that would allow us not only to prescribe a fixed boundary conditions, but also let the flux go through those faults. So, you can investigate the effect of direct infiltration at those locations.

With respect to the repository horizons and how we go from the surface to the repository horizons, we are now refining the grid tremendously in the repository block. We have taken the latest design data from the design people to try to break up the repository block to allow us much more detailed representation of heterogeneities and fracture flow in the repository region. So, instead of the global approach that we started with with Alan and the GS to start with to get the feeling for the three-dimensional flux, we are now looking at much more refined areas where we know it's much more important to refine those areas. That's certainly from the surface to the repository region.

So, those are the steps we are taking now and we hope this grid will be completed fairly soon so that we can look at these results and then give some of these models to the Sandia people that are doing the detailed groundwater travel time calculations. I hope that answers some of your questions.

DR. LANGMUIR: Thank you, Bo.

I think we need to go on. We'll certainly have a chance to revisit these questions. We're over time right now. Ed, a short question?

DR. CORDING: My one question is in looking at all this, what average or ranges--what do you see as where the ranges of average flux from the surface into the rock beneath? What do you see as an average flux for the mountain or some range where--where are you now in terms of what you think is an average overall area flux?

DR. FLINT: You mean, in terms of the numbers?

DR. CORDING: The number?

DR. FLINT: In terms of the number over the 3-D site scale model, I'm on two engines now, okay? The matrix flux, our best estimate was 1.4 millimeters a year. That was an area average, but I think it's real important that there are higher numbers by an order of magnitude in some parts of the mountain which contribute to probably the perched water bodies and things. In terms of the fracture flow, that's a hard one. It's probably another couple of millimeters a year that we think may get through the near-surface. The highest fracture flow we'll probably see right over the top of the repository and to the north. So, the biggest numbers are going to be over the repository; the lowest numbers are going to be everywhere else.

DR. CORDING: But, your numbers right now is an additional increment. It's not an order of magnitude more than the matrix flow. Is that what you're saying?

DR. FLINT: No, it's--

DR. CORDING: For the fracture on the average--

DR. FLINT: Well, it's three orders of magnitude more than the matrix flow in the welded units. The welded units are on the order of .02 millimeters a year. The fracture flows in the order of 2 millimeters a year, that's a couple orders of magnitude more flow, but it's at the same order of magnitude as the highest matrix flow. Because of the high permeability of the matrix, it may be--for instance, the Paintbrush nonwelded tuffs in Drill Hole Wash may be the fastest flow areas we have in the near-surface because you don't have to go through the Tiva. You go right into them and then into the Topopah. But, yeah, we're looking maybe on that--no more than that, certainly.

DR. CORDING: So, instead of 2 millimeters per year, it could be from the surface 4 millimeters per year?

DR. FLINT: Instead of .02 millimeters, it could be 2 millimeters a year or 4 millimeters a year, right. The flux map that we put out last year on high-level waste suggested over the repository itself, the flux was on the order of about .02 millimeters a year. The new data that we're getting from the fracture contribution says the flux may be on the order of 2 to 20 millimeters a year. So, that's quite a difference over the repository itself. I mean, that's a lot of water. It exceeds the capacity of the underlying units to carry that without fracture flow going on. But, because we don't see perched water existing on top of the vitrophere underneath areas like UZ-16, that's an indication that those fluxes are probably diverted laterally. My guess is that a tremendous amount of flux is diverted laterally by the PTn itself. So, that's why 3-D modeling is so important because you can't take that vertical flux and stop without having a way to get it to go sideways. And, I think that's probably what happens a lot from this water that we're seeing.

DR. LANGMUIR: Thank you, Alan.

Let's go on. Our next speaker will be introduced by Dr. Tom Nicholson. He will be Gregg Anderson (sic). His topic will be geochemical evidence of fracture flow in unsaturated tuff, Apache Leap, Arizona. Tom Nicholson, by the way, heads the research group on fracture flow and transport at the Apache Leap site for the Nuclear Regulatory Commission.

Tom?

DR. NICHOLSON: Thank you very much, Dr. Langmuir.

I want to thank the Board for inviting us to share some information we've learned at the Apache Leap tuff site. The purpose of our talk today is just to give you some insights and some information that might be of value to you people.

This work, as Dr. Langmuir said, is sponsored by the U.S. Nuclear Regulatory Commission. The principal investigators are Dr. Randy Bassett, Pete Wierenga, and S. Neuman. The work that will be reported on is confirmatory research studies that have the objective to independently develop datasets for the evaluation of conceptual models and strategies for understanding groundwater flow and transport through fractured rock. The studies specifically focused on technical uncertainties developed by the licensing staff.

The conduct of experiments is to develop independent datasets which will be specifically tailored towards looking at a variety of strategies being looked at by both the NRC, DOE, and other interested parties. The strategies will cover the range from equivalent porous media to a dual porosity/dual permeability and will be done in coordination with the Center for Nuclear Waste Regulatory Analyses.

The technical issues developed by the licensing staff based upon their evaluation of DOE reports, site characterization, and other special study plans are those listed above. I won't go through them in detail except to say that it covers a range of processes and model confirmation to understand where are the technical issues, such as preferential flow, scaling from various size experiments to large experiments, and handle multi-phase flow and transport.

There's a series of field experiments that are actively being planned right now. They cover a whole range. I won't go into those in detail, but we're looking at both crosshole pneumatic and gaseous tracer experiments, large scale three-dimensional hydrolic and tracer experiments, and a variety of experiments on scales ranging from one meter to over 100 meters. We're lucky at the Apache Leap tuff site to have an underlying haulage tunnel in which we can look at focused recharge through certain preferential fractures that we think exist.

With that background, I'd like to now introduce Gregg Davidson from the University of Arizona, Department of Hydrology & Water Resources, who will provide you with some information on geochemical evidence of fracture flow that has been developed at the Apache Leap tuff site. Gregg?

DR. DAVIDSON: Thanks, Tom.

Just tell me I'm not supposed to quit at 4:00 o'clock.

DR. LANGMUIR: You've got more time than that.

DR. DAVIDSON: Okay. One of the questions that Dr. Langmuir put up at the beginning of introducing this meeting was the third question, what measurement technique can be used to characterize or quantify flow and transport in these environments, being air environments. In other words, can the fast pathways be detected, predicted, and quantified as to their significance and what are the limitations of these techniques?

In terms of this can fast pathways be detected, predicted and quantified, the reason that that question is being asked today is because there's very little information available concerning that question. One of the reasons has to do with the nature of fracture flow itself. Fracture flow by definition is somewhat of an anomaly. In other words, what I mean by that is you can't talk about fracture flow accurately in terms of being a uniform behavior across the formation. Where fracture occurs, it occurs in discrete locations. If I just walk out arbitrarily into a study area and sink a borehole in the ground, I've got a very good chance of missing that phenomenon.

I've got a schematic that I put together here just to highlight this point. If I have just in this hypothetical situation a fracture zone that for whatever reason is--perhaps there's ponded water in a low spot and it's generating fracture flow through this area and it's occurring in a very localized region for whatever reason--I mean, if I go out into the field site, it's not just a matter of finding a fracture, it's finding fractures that are conduits for water if that's really, in fact, what's happening. So, if I go out unaware of this particular situation, I choose my study area, I fill it with boreholes, I spend a lot of time mining the recourses, and I come out of my study and I conclude, well, fracture flow is not a very significant event. Well, when, in fact, what I may have done is I simply didn't intersect that phenomenon with my borehole. Now, if that doesn't make it difficult enough to try to--you need to intersect these things in order to find them, in order to be able to tell that they're actually occurring if they are. I can put a borehole through this phenomenon and I still may not see it because in a fracture plane, we often think of the preferential flow as being through the fracture plane, but even within that plane typically we have preferential flow. You have fingering, you have water that's traveling during--that's picking its own pathway down through a fracture and just because I intersect it with a borehole doesn't mean that water is actually going to enter that borehole.

So, in the case where maybe the water does enter the borehole, well, then I've got a new--it will give me information if I find it, but for one thing, I have to be there. When we're talking about arid environments, we're talking about typically episodic events. So, if there's going to be flow into that fracture, I have to be there or I have to have monitoring equipment in that hole to catch it. If I'm fortunate enough that I do see it, that's going to tell me some information. But, if I want additional information about the geochemistry or whatever, then I have to somehow sample that. I have to get a sampling container down into that hole. I have to be able to get the water and isolate it from the atmosphere or isolate it from drilling materials that are on the sides of the borehole. It's a very difficult process. So, as a result, that's largely responsible for why we have so little data about fracture flow.

So, at the Apache Leap, we initiated this investigation of fracture flow in an effort to see if we could answer some of these questions about how can we detect it and can we quantify it? So, what we did at Apache Leap we tried to first ask the question if we were going to see fracture flow, where is the most likely place to find it? Well, the most likely place is going to be beneath areas where water is for whatever reason concentrated and that we have the fractures exposed to a positive head that would initiate flow through those fractures.

This is a photograph from one of the mountain tops looking down to the south of the Apache Leap. The edge escarpment is over here through this ridge and you'll see a series of parallel valleys running towards the west. And, during rainfall events, we get runoff and a few days out of the year, we get water flowing down through the bottoms of these small valleys as ephemeral streams and the fractures are exposed to water. So, what we decided to do was to put a borehole in the base of one of these little valleys. You can actually see this was--during the drilling was one of the few days of the year where we do actually have water running down through there.

Now, we also determined what one of the predominant fracture patterns was. We found that they were cutting roughly perpendicular to some of these valleys and sloping at about a five degree dip. So, we drove the borehole at about a 40 degree angle to try to intersect as many of these fractures as possible and went on into a perched aquifer that's down here about 144 meters. Now, the idea was that we would then run a video log and we'd run geophysical logs down and see if we couldn't identify potential water-bearing fractures. And, to try to get around this problem of trying to be there when a flow event occurs, to try to get around the problem of flow possibly not even entering the borehole even though you're penetrating a water-bearing fracture, by taking--as I said, trying to identify potential water-bearing fractures and taking core from adjacent to those fractures, getting pore water out of those, and then comparing that pore water with pore water taken from intermediate zones that were moved, isolated from those fractures, and see if maybe we had higher levels of carbon-14, higher levels of tritium in those pore waters.

So, that's what we did and these are geophysical logs. We have neutron data, density data, and resistivity data. Now, the missing data up at the top is because casing remained in place up near the top of the hole during this time. It also explains the shift in the density data. But, the most important thing I want to point out is right here at about 73 meters the video logs showed water actually--we were fortunate enough to actually see water entering the hole at that point. Some of the things you'll see is a spike of lower density which is typical of a discrete fracture which we also observed in the video logs, we have a small increase in the neutron response, and a decrease in resistivity which can also be indicators of increased water content.

So, what we did then was to look for other regions that had similar patterns and to target them for taking core adjacent to those intervals and comparing the results of the pore water with samples taken from intermediate zones. The dotted lines here were not specifically targeted, but you'll see in some of the later slides some of the influence of some of the features that are showing up in this region, as well.

This was a water content slide. We took some of the core and determined what the water content was gravimetric analysis and by a distillation technique where we measured the water that came out from a given mass of rock. You'll notice that we have anomalously high water contents at each of those fracture locations which supported the conclusion that what we were seeing in the geophysical logs was indeed higher water content. Up at the very top where you see the very high water content is largely due to high porosity. The upper 20 to 30 meters of a formation has a much higher porosity, not necessarily higher saturation.

This is a slide of the radiocarbon data. What you're looking at--up at the very top, you'll see surface runoff. At the bottom, these are perched aquifer samples. You have some formation air samples which I'm not really going to talk about very much today. The red circles up near the top are water that we took Al Yang's squeezing cell and we put our core in it and squeezed it and got water out and measured the radiocarbon content of that. The greenish-blue or the green is distilled samples. And, what happened was most of the core, we could not get water out by squeezing. So, we had to develop a new method for getting at the carbon that was in that pore water. We developed a distillation technique. I don't have time to talk about that technique itself today, but for the moment you can compare and see the results of squeezing the water up where we were able to take core from the same intervals, squeeze it, distill it, and you can see we have very close agreement. So, it seems to be an effective method. So, we have the most complete record of the borehole with this distilled data. So, this is pore water data, the carbon-14 that's in the pore water.

The most important thing is what we're seeing is not a monotonically decreasing C-14 activity profile with depth. What we're seeing is in association with many of these fracture zones, we're seeing elevated C-14. We're seeing elevated below this zone, we're seeing it elevated in association here, we're seeing it elevated up in here, and again up there.

Now, the very top one, perhaps there may be a fracture zone that we didn't identify, but then each of the following ones are associated with some of these fracture events. Now, this point down here, mining activity has been close by. They have been pumping water from their mine for years. We suspected all along that they probably dropped the level of the perched aquifer and this seems to indicate that that's true. We suspect that the perched aquifer was possibly up in this region before and what we're seeing here is simply another value from the saturated zone or what was previously with the saturated zone.

Now, one thing to notice is that in this region which was the only region where water was observed entering the borehole, it's associated with the smallest increase in C-14 activity. I think that the explanation has something to do with the phenomena that actually controls fracture flow. In this interval, it's a relatively unfractured section with a discrete fracture that water is entering from. Now, when water is initially coming in to a fracture, some of that is going to imbibe into the matrix and, as the matrix near that fracture becomes saturated, then further imbibition is going to be restricted encouraging water to continue farther down the fracture. Now, in this area where we've got a discrete fracture with water coming down it, perhaps subsequent or successive flow events are forced to take a similar route each time. So, as water is moving down through this fracture, it's encountering matrix that's already fairly saturated and not much imbibition takes place. Whereas down in this region, this is a very, very fractured region and successive flows may be able to take many, many different routes and there may be more time for the matrix to drain. So that the next time flow encounters that matrix, you have greater imbibition and we see the results of it with a greater increase in C-14 activity.

The tritium data seems to support this idea. Unfortunately, we don't have tritium for these two water samples. The samples were lost. But, for most of the other samples, we have tritium data and in every case the tritium is near or below the detection limit of .1 tritium units. We have very good detection limits. And, what that seems to be saying is that with each flow that's coming down--that we're getting some imbibition into the matrix, it has to be a fairly small imbibition because if there was more, we'd see a larger tritium value. The fact that it's less than .3 TU in almost all--well, in every case, it's less than .3 TU--suggests that we don't have very much moving in and yet over time--that's per flow. But, over time, over years and successive flows, the total amount of water going in is a substantial contribution to the unsaturated zone based on these C-14 numbers.

Now, there's a couple of questions that could be asked from this. One is it's been asked if perhaps you could have gaseous diffusion down drive fractures that would give you a similar look. Well, it's possible that you could get--you would get more rapid diffusion down an open dry fracture, but that's not what this is for three reasons. First, we see water or we observed the water entering the borehole at this step. Second, the geophysical logs and the gravimetric analyses indicate that we have higher water contents associated with these fractures. So, they're not dry fractures. And, third, we've got post bomb carbon at 133 meters. According to my calculations, the diffusion rate of C-14s and $CO_2$ is not sufficient to get post bomb carbon down to this step. So, we're looking at water coming down through these fractures.

The second question is are we looking perhaps at an artifact of a wetter climate? And, again, the answer is no for virtually the same reasons. We actually observed water entering at 72 meters and we're looking at post bomb carbon here. So, this is a current phenomena that we're dealing with.

So, the next question is, well, that's the unsaturated zone. How much of this fracture flow is actually making it down to the aquifer? Now, if you notice here, there's a large discrepancy between the pore water taken from core from the saturated zone and samples taken from the aquifer by pumping. To address this discrepancy, you have to consider the sampling method. When we pump water from the aquifer, we put a pump down in the borehole and we're pumping out a large volume of water to take care of atmospheric effects, to take care of drilling effects, and what we end up doing is drawing water from some distance away. Now, keep in mind, we're beneath an ephemeral stream here. So, if fracture recharge is occurring, it's likely to be enhanced beneath that zone; whereas, as we move away from that zone, it's perhaps not occurring as much. So, what we end up having is a mixing zone beneath these ephemeral streams where we have aquifer water moving into this mixing zone and mixing with water that's traveling down through fractures. I believe that's what we're seeing right here is that this core has come from that mixing zone; whereas, these represent samples that were drawn from outside of that mixing zone.

Now, what's interesting in both cases, the tritium is below detection. But, we can use the C-14 and the tritium data together to try to calculate the amount of water in that mixing zone that came from fracture flow. We can do that by thinking of this mixing zone as a box where we have fracture flow contributing some, we have the aquifer contributing part, and then we have discharge at steady state, and there's a certain residence time that's dependent on the flows in and out. The residence time is going to be part of the equation in determining how much fracture water we can input into that box and maintain this measured value of C-14 and keep tritium values below detection.

What this figure is is residence time of water in that mixing zone versus fractional contribution from fracture flow. What the vertical axis means is essentially what percentage of the water in that mixing zone was derived from fracture flow. Now, that's very different than is it recent water? Don't confuse recent water with fracture flow because some of this water may have been recharged through a fracture 1,000 years ago. So, it's sitting in the aquifer and it doesn't look young because it's not young. But, yet, it was recharged rapidly 1,000 years ago.

So, what this is is saying, all right, if I assume, say, for this line that my mix zone can't be more than .3 TU when I'm done, how much fracture water can I add to it, how much matrix water can I add to it for a given residence time? At the far end, if the residence time is 300 years, then every year that's saying that the entire aquifer is from fracture recharge and basically what I'm doing is every year I'm adding 1/300th of the volume of the aquifer to the mix zone through fracture flow versus, on this end, this is saying a recharge of one year and I can only mix about 6% fracture flow and the rest has to come from the aquifer and it's being replaced every year.

Now, we're doing the same thing for C-14 here maintaining the measured activity at .82. Now, where these lines cross is theoretically the solution for how much of that aquifer came from fracture flow. And, that's sitting at about 47%. Now, if the actual tritium is below detection, say below .1, or we're not sure what it is, then rather than lying along this line someplace, it's going to be anywhere in this area which would be along this line anywhere to the right of the tritium line. Now, the contribution from recent bomb carbon and bomb tritium is going to serve to lower both of these lines by a small amount and that's impossible to quantify because we really don't know what the actual values of C-14 and tritium were on a yearly basis for this site nor do we know what kind of variability there was in precipitation. But, keep in mind that the longer the residence time, the less impact that's going to have on--the less impact there's going to be on contributions in the last 50 years because those contributions are fairly small. But, over the long-term, what we're looking at is on the order of half of the aquifer in the mixing zone derived from fracture flow.

Now, that's in the mixing zone. What about the aquifer outside of the mixing zone that's contributing? Where did that water come from? Well, to consider that issue, I want to switch gears from the radioisotope data and look at geochemistry. What I have here is just some average compositions of surface runoff, pore water. and aquifer water. This is aquifer from outside of the mixing zone. Now, just looking at the total dissolved solid numbers, it should be apparent that that aquifer water is not simply pore water that's reached that depth. There's some dilution of pore water that's required in order to get this. Now, if we spent some time to go through many of the individual ions, what we would discover is that it's also not simple dilution of pore water. It's not simple mixing between surface runoff delivered through fractures and pore water. What we have to do is we have to consider possible mixtures of pore water, surface runoff, and then reactions with the minerals in the aquifer.

Some of the common minerals that are in the aquifer that we considered are in the formation; plagioclase, botite, hornblende, $CO_2$ gas. These could all be dissolving and with these minerals we have incongruent dissolution which means we have simultaneous precipitation of clays which could be any of these. Now, what we did then was to take a computer code called NETPATH and what NETPATH does is it's a computer code that simply allows you to take, say, two waters with a known composition, allows them to be mixed in variable proportions, possible evaporation if necessary, and then it will dissolve minerals that you input, precipitate out minerals that you input, and see if it can come up with solutions that will give you the composition of the final water which in this case is the aquifer water.

Now, NETPATH solutions are not unique. It can come up with a variety of possible solutions that you then have to look at and determine if they're realistic. Now, for the particular minerals that I used, I think in that run I got about 12 different possible solutions. I put three example solutions up that pretty much represent all of the others. What we see here is the positive numbers represent minimals per liter of minerals dissolved. The negative numbers are minimals per liter of minerals precipitated. You'll notice that not all of them--the program is not forced to use all of these minerals. It can pick and choose which ones to see what kind of result you get. But, this is less important for the sake of this meeting than the top two rows are. The top rows being surface runoff, the percent contribution from surface runoff, and one of the pore waters that I selected.

You'll notice that according to these calculations, you have to have about 98% of the water in the aquifer being recharged by fracture flow and then sitting down in the aquifer and reacting with the minerals in order to arrive at the chemical composition that we see in the perched zone. Now, if I use other individual pore waters and if I allow for some uncertainties in the chemical composition of the minerals by modifying the sodium and calcium content of the minerals, I get a range of results of possible contribution from pore water that ranges from as little as nothing, which is probably unrealistic, up to a maximum of 10%. So, the geochemical data is suggesting that upwards of 90% of that water outside of the mixing zone was ultimately derived from fracture flow. That water moving into the mixing zone beneath the ephemeral stream then gets an additional 50% dilution from fracture flow.

This chart is looking at the significance of fracture flow in other areas of the Apache Leap. We have very little data from other areas. But, one of the things that we have here are surface runoff, C-14 activities, and from DSB-1, it's .67, and from two other locations; one beneath the tunnel--there's a tunnel that actually comes underneath the perched aquifer and gets seepage from the perched aquifer. C-14 data is virtually the same, as well as at another location at Oak Flats. Well, runoff right now has a tritium value of about 5 TU and, in DSB-1, it's less--it's below detection. And, yet, at these other locations, we have fairly high tritium values. Now, what that would suggest is that annual additions of fracture water is even more significant at those places. But, we have so little data that I wanted to check that out to see if that, in fact, was what was happening. So, I looked at sulfur isotopes and the chloride/sulfate ratio. Now, why that's useful at the Apache Leap is because there was a smelter that was operating for years and the fallout from the smelter, sulfur fallout, has a distinct isotopic ratio. So, we can use that as a modern tracer. And, in modern runoff, we have the isotopic values of -4 to +1; whereas, precipitation, now that the smelter activity has stopped is more like +8. So, there's a nice distinction that we can compare things with.

So, in DSB-1, the aquifer is fairly close to the precipitation value. So, we can now go and compare the tunnel and Oak Flats with these numbers. If these values were actually correct, then we should expect the isotope values to be intermediate between these two. And, in fact, here, we do have intermediate values; whereas, here, it's virtually the same as this. So, so far, this is adding support to the idea of this value being correct; whereas, this one is somewhat in question.

Again, with the chloride/sulfate ratios because of this sulfer fallout the runoff has much higher sulfate than it would otherwise. So, we have values--chloride to sulfate ratios of .1 to .2; whereas, at DSB-1, we have values that are 2.1 to 2.3. Now, again, if these values are correct, we expect intermediate values here and, indeed, we do. We have intermediate values; whereas, the Oak Flats, they're closer to DSB-1 again. So, what it looks like is this value is perhaps in question, but at the tunnel, it looks like not only--what's significant about what we're seeing at the tunnel is not only does it appear that fracture flow is significant from the surface to the perched aquifer at that point, but that we have substantial fracture flow from the perched aquifer down to the tunnel, beneath the perched aquifer. So, that's what data we have from more of a regional perspective.

Conclusions of the occurrence and significance of fracture flow at Apache Leap. At least, four fracture sets are identified that carry water. Pore water associated with the deepest fracture set contains post bomb carbon. So, we're talking about a current phenomenon. Imbibition per flow along the fracture is minor based on the tritium data, but is a significant source of pore water over time based on the C-14 data. Carbon data distribution in the unsaturated zone appears to be controlled by fracture flows. Flow through fractures intersected by DSB-1 may account for half the water in the aquifer beneath DSB-1 in the mixing zone. By the way, I didn't say DSB-1--DSB stands for deep slant borehole. Reaction pathway models, the net path models, suggest that the remaining water in the aquifer is also largely derived from fracture flow. And, finally, the chloride/sulfate ratios, the del is 34, and the tritium data indicate fracture flow may be more substantial even before the mined haulage tunnel. Now, it's important to remember that we're not talking about--when we say more substantial, not necessarily as a percent of the whole aquifer, but in terms of the annual contribution, that we have a larger volume of water per year flowing into that region from fracture flow.

Questions?

DR. LANGMUIR: Thank you, Gregg.

Pat has a question.

DR. DOMENICO: Only the second dot--incidentally, that was very enjoyable. I wonder what NRC will do with this. But, only the second dot, I believe, relates to the fastness of the pathway. I mean, I think everything else relates to the fact that these are pathways, but what you're saying it's the presence of post bomb carbon that suggests that the pathway is fast. Is that a fair statement or do we have other information?

DR. DAVIDSON: We have other information also because what we're seeing, say, with the geochemical data is the higher dissolved solid content of the pore water is a result of a long residence time in the pores. What we're seeing in the aquifer is a water composition that does not allow for it to have sat there--for it to have percolated through the matrix to arrive in the aquifer. It required a faster pathway to get there and then it can sit there in the aquifer for a while, but nothing like the length of time required to percolate down through the matrix. And, that time factor is sitting in the aquifer. It has to get there much faster.

DR. DOMENICO: But, that's still indeterminate compared to the measurement of post bond carbon, I presume?

DR. DAVIDSON: Yeah, I would call it supporting data, not self-conclusive data.

DR. LANGMUIR: I just want to congratulate you. I think this is--I'm delighted as a geochemist to see it being used this powerfully as an adjunct to hydrology using both the isotopy and the inorganic chem together to get far more information out of what's going on than you can ever get strictly from hydrologic measurements.

On a more nit-picky question for my entertainment here, NETPATH obviously should be used and it could be used at Yucca Mountain presumably, although there's some complications, but I have not heard it being used as a model there. On the NETPATH application, an obvious thing one wants to look at is if you're going to come up with a series of optional solutions to a NETPATH approach to a problem, the bottom line is are the minerals that NETPATH predicts precipitating, in fact, there and do you have the data which allow you to distinguish? Obviously, the answers are about the same, regardless, in terms of percentages. But, do you have the mineralogy from, for example, groundwater versus unsat zone that confirms one of these options more than another?

DR. DAVIDSON: Well, there's two points from that, one of which you virtually answered yourself. Yes, it's important to get at what the actual minerals are and my conclusions were based on the best knowledge we had up to this point of what the actual chemical compositions were. And, if I had another hour, I could go in and actually tell you about why the particular chemical compositions of, say, plagioclase were chosen and how remarkably well they fit with possible solutions. If you added just a little bit too much sodium, you got no possible solutions. If you went just a little bit too far the other way and got too much calcium and again you had no realistic solutions. It was confined to a fairly narrow range that gave you practical solutions that, in fact, matched what we know about the actual measured values at the site.

On the other hand, you pointed out that for all the different possibilities, the results were virtually the same. So, if my ultimate concern is how much of this water derived from fracture flow and I considered all the possible plays I could think of and a variety of different chemical compositions of the minerals, even though 90% of them are wrong, if they are all giving me the same result, that's a pretty hefty conclusion by itself.

DR. LANGMUIR: Thanks very much.

DR. CORDING: On the deepest fracture set, how deep were you where you were observing the greatest depth to which you observed the post bomb carbon?

DR. DAVIDSON: The water table was 143 meters and we were about 10 meters above that. That's where we saw post bomb carbon at that fracture.

DR. CORDING: Thank you.

DR. NATIV: Just two questions. Did I get you right that even where you saw post bomb carbon-14, you didn't see any tritium?

DR. DAVIDSON: In those two samples?

DR. NATIV: Uh-huh?

DR. DAVIDSON: The way that my traps worked, I had taken my carbon out for sampling, and when I was taking my water out in both cases, the traps burst. I wish I had that tritium data, but that's what happened to those samples.

DR. NATIV: Okay. The other question has to do with the--composition. You come up with some percentage of surface runoff versus pore water. You used sulfur isotopes to confirm some of your observations, but do you have also oxygen-18 or deuterium that I suspect should be different in the surface water than in the unsaturated zone as a constraint on your solution?

DR. DAVIDSON: For this particular site, we have no oxygen or deuterium data. There's some analogous studies that are going on at Apache Leap beneath--the tunnel that I spoke of that goes underneath the aquifer first goes under an ephemeral stream that collects water from all of those smaller streams that I spoke of. So, it has water in it much more often during the year and then continues on underneath the perched aquifer. And, studies that have been led by Randy Bassett have indicated that--or they can actually trace individual storm systems to depth from the surface down into fractures seeping into the tunnel. But, we don't have any data for this particular site.

DR. NICHOLSON: I think I can speak loud enough. Dr. Domenico made a comment. He's interested in what the NRC is going to do with this information. This--

DR. DOMENICO: It was just a passing comment.

DR. NICHOLSON: We take everything literally. The NRC does not consider this to be conclusive proof of fracture flow. We think it's an indication of fracture flow and we want to make sure that this information is going to be crafted into the designs and experiments that are now being planned for the Apache Leap tuff site. Randy Bassett is developing a three-dimensional fracture visualization of the whole system. He's using a dual continuum flow and transport model. An experiment will be conducted, a tracer experiment, in which tracers will be used in cooperation with the Center for Nuclear Waste and then we will simulate the results using the approaches I pointed out in one of the earlier viewgraphs in which we look at the whole gamut from discrete fracture narrow models through the various dual continuum models and we want to do this to determine not only the accuracy, but also the uncertainties that you pointed out earlier with regard to how do you take this very site-specific information and factor this into performance assessment models. So, that's what we hope to do with this information.

Thank you.

DR. LANGMUIR: Thank you. Thanks, Gregg.

I think we need to take our break. The break is to be followed by the roundtable. So, all speakers who have made presentations today, please prepare to sit at the roundtable. In addition, before you--let me speak a little bit here. We need to have Scott Tyler, Ron Green, Richard Luckey, Parvis Montazer also at the table.

(Whereupon, a brief recess was taken.)

DR. LANGMUIR: Okay. Let's get started. The procedure will be as follows. We're going to start out with five minute or less presentations from several folks who were not part of the proceedings earlier in the day. We'll then move to general discussion among the panel members and that will be the protocol. And, as we go around here, if there's sufficient time, we'll go to the floor and discuss questions from the floor, as well as have interactions with people on the floor.

Scott Tyler here at the table was on the Ward Valley Committee and has some things to say, I think--at least, it says here--about his infiltration percolation studies. Scott, try and keep it to five minutes.

DR. TYLER: I'll do my best.

Let me just start off by saying there are four topics that we have listed here. Let me just hit the second one first, if I can, which are what are the common features controlling transport in arid regions? We've been working quite a bit for the Department of Energy on the defense waste site and nuclear testing side both in Yucca and Frenchman Flat on the Nevada Test Site. The work we've done is primarily looking at recharge and solute transport in alluvial basins, coarse textured soils, fairly structureless, without a great deal of fractures present. The results, so far, I think, pretty well concur with what Alan Flint talked about in these coarse textured soils that are fairly thick. There's very little recharge occurring today at lower elevations, if any, in most areas. So, we've got fairly thick soils and the plants do a pretty good job pulling most of the moisture out.

There's a few exceptions to that rule. Places where you have frequent or periodic--very frequent and very periodic ponding driven by topography, in those regions you can see significant recharge upwards; significantly in excess of the annual precipitation because of the time and concentration of fluid at the land surface. But, those are fairly small and somewhat unique to the Nevada Test Site. These are the subsidence craters from nuclear testing.

On the same topic though with respect to topography, catchment size and surface features do become significantly important in alluvial settings when we start to talk about past climates. And, particularly during the last glaciation, certain areas down at the lower elevations where ephemeral ponding or ephemeral storm water passed, we do see recharge in excess of several centimeters a year occurring during the last glacial period. And then, also during the penultimate glaciation which was about 128,000 years ago, plus or minus a few thousand, widespread recharge at areas at the 4,000 foot elevation with coarse textured soils. Recharge occurred everywhere. So, the past climate is a critical factor for determining recharge in general and then topography comes in fairly significantly if we're kind of on the border of high rainfall and less rainfall. So, that's number two.

Let me talk briefly--can I use this microphone over here? I'll try to be as brief as possible. Okay. One of the problems in characterizing fast flow in arid regions--and, what I want to talk about fairly briefly which we haven't talked about today is the role of fracture coatings on fracture flow. Fracture coatings are ubiquitous at--oh, in almost any fracture environment and the coatings themselves may provide a significant alteration to our conceptual model of fluid flow in essentially a dual porosity media. So, I'll just kind of talk very briefly about some experimental results on the effects of fracture coatings on imbibition across the fracture surfaces because we're interested in water running down fractures. The matrix plays a crucial role in how far that water is going to move.

We did some laboratory experiments very similar to what Alan Flint and his group has done in the past looking at sorptivity. Sorptivity is a measure of how water is imbibed into, of course, material driven by capillary forces. To make life easy, consider the sorptivity essentially the square root of the saturated hydrolic conductivity. That's the layman's view. There's a few other terms in there, but primarily it really tells us something about the permeability of the material. All we did is we just dunked the rock into a bucket of water and measured how fast the water uptook--took the water up. And, what we did was we took some samples from land surface. These are surface samples from Yucca Mountain, both Tiva and Topopah Springs tuff. We naturally coated--these were primarily desert varnishes and just looked at how fast water imbibes up across a coated fracture surface. Then, my graduate student got very good with a hammer and a chisel and he broke the rock in half and then measured the sorptivity into a fresh fracture surface this way to look at the differences. How important are the coatings to infiltration and sorptivity.

Now, let me just show you the effects. Again, consider sorptivity about the square root of the conductivity for now. The red lines are coated fracture sorptivities and the green hashers are uncoated sorptivities measured on 10 different samples. And, this is Topopah Spring tuff. What you can see in all cases, the fracture coating significantly reduced the imbibition, significantly reduced the sorptivity by a factor of about 2 and turned that into conductivity and that's a factor of about 4. Okay? So, the fracture coatings--and these were minimally coated fractures, essentially desert varnishes, a significant reduction in the conductivity at the surface of the fracture. That's going to affect how fluid moves down these fractures.

Can you just turn the slide projector on? I'll show you what one of these fracture coatings look like perhaps. Okay. This is a scanning electron microscope shot of a coated fracture and it's--I hope you can see if from back there. What the surface is is primarily these plating structures which is essentially the coating with--and, this is 1000X; a thousand multiplication factor on the scale--with these tiny cracks running in between. Behind this is essentially the unweathered or unaltered--in this case, Topopah Spring--tuff. And so, the imbibition across this surface is very different than the imbibition across a freshly broken fracture surface and it has to be accounted for when we start thinking about how we're going to model imbibition into these kinds of systems or interactions between the matrix and the fractures.

Let me just show you--turn that off for just a second. How's my time?

DR. LANGMUIR: Pretty quick.

DR. TYLER: Okay, quickly. This is just elemental analysis of what the coatings are. Again, these are desert varnishes. So, these are very thin coatings. This is coated and uncoated. The primary difference between these two is the coated fracture contains calcium and iron; just what one would expect, calcium carbonate coating and some iron hydroxide coatings. Okay. The rest is all the same. The gold is not in the rock. It was what was coated on there. We did not discover a gold mine.

So, what are we going to do with these data? What we did was we just used the TOUGH simulator to look and see what would be the effects of fracture coatings on imbibition;

water running down a vertical fracture and imbibing into the tuff, both for coated and uncoated. And, if we can, the second one here, results showed significantly deeper wetting after a two hour rainfall event, just a simulated two hour rainfall event, in the coated Topopah Spring than when we compared it with the uncoated because the fracture coating was acting as a low permeability skin, if you will, on the fractures.

Okay. The difficult part of the whole thing was these were two hour simulations and we had to have very small grid spacings in order to be able to converge on a solution, particularly in the matrix, and very small grid spacings in the fractures also in order to get solution. And, it was extremely difficult if you ask the student who did this. And, I won't say it's not impossible anymore because there have been some significant improvements. Zimmerman and Bodvarsson just published something where they simplified dealing with the matrix to essentially a sink term which does reduce the complexity of the numerical simulations, but you'll still need to be simulating essentially at the matrix block scale which might still be on the centimeter to small meter scale which still means if you've got 100 meter cube of rock you want to sample or simulate, you're talking about a million nodes or something like that. So, it's going to be a significant problem. But, the fracture coatings from what we've seen--and, again, we've worked with very nominally coated fractures. Alan showed the slide showing the extent of coatings much more significant than what we had and they really do significantly impede water infiltration into the matrix which may be one of the reasons why we see more rapid transport is because the matrix that the fracture water sees is really not the matrix that we sample in the lab, but rather the fracture coatings.

Thanks.

DR. LANGMUIR: Thank you, Scott.

Something I should have done earlier is to introduce all of our speakers of the panel. Ron Green is from the Center of Nuclear Waste Regulatory Analyses. Ron will be coming up in a moment here. Maybe you could raise your hand, Ron? You just heard from Scott Tyler, Desert Research Institute, State of Nevada. Parvis Montazer representing Nye County. He's an old hand at Yucca Mountain Project from past experience. Richard Luckey, U.S. Geological Survey, Water Resources Division. I guess that's our group.

The next brief presentation by Ron Green from CNWRA.

DR. GREEN: I'd like to make, I guess, a quick comment on the relative virtue of laboratory scale versus field scale experiments. This comment is based on a perspective of recently completing a five year laboratory scale study on thermally driven moisture redistribution through partially saturated coarse media. This is work that was sponsored by the Nuclear Regulatory Commission.

Coming out of this work, we identified a conceptual model of the thermal regime of a high-level waste repository and that's essentially starting off with a initial heating period during which moisture is essentially redistributed as water vapor and it's advection driven. And then, later on, you have a cooling period during which moisture is redistributed as liquid and that's mostly capillarity driven. Then, you're separated by a transitional period. This conceptual model is predicated on having a heat source that's sufficiently hot and a medium whose bulk permeability is sufficiently low that gas pressures can be built up.

The ramifications of this conceptual model is illustrated in this slide where you have advection driven moisture redistribution. Your moisture balance is going to be somewhat different than when you have buoyancy driven. In this case where you can have gas pressure built up, you're going to have moisture moving both upward and down. This is for the case with a sufficiently high heat level and sufficiently low permeability compared to buoyancy driven where you have either a low heat load or a high permeability or both. In this case, your moisture would be redistributed somewhat differently--or quite a bit differently than this case. You might ask yourself or question this conceptual model saying, well, the repository is going to be placed into a fractured or a highly fractured welded unit. So, one might not expect to have these gas pressures built up. And, that's what we initially thought when we performed this study.

However, looking at some results from the Livermore G-Tunnel experiment that was conducted about 10 years ago in the Grouse Canyon member which has properties similar to the proposed repository horizon and that's a welded high-fracture unit, they placed three sensors to measure gas pressure within about .8 to 2 meters from the heater. The temperatures reached about 240 degrees in the borehole. And, in all three pressure sensors, they detected high pressures. These are just two of the results; these are taken from Ramirez. And, in one case, they had pressures that were measured up to close to three bar which is much more than you'd need to have advection driven gas. Then, the other two cases, you had several psi of pressure. So, on the very little evidence that we have of a heater put into a fracture coarse media, we do see gas pressures built up which questions the conductivity of fractures and their ability to dissipate pressures.

Just some quick results. There's currently a large block test that's under construction and supposed to be started next year and this just looks at the numerical model using VTOUGH and the block goes from about a meter and a half upwards. The heater elements are at three and a half meters. So, we go below the ground surface in this model. In this case, we have the heat source at full power which is about 300 watts and this one, it's half. And, just by this model which is sensitive to the input parameters--but, in this case, we see that it's essentially buoyancy driven; in this case, it's advection driven. I guess what this tells me is that for laboratory based experiments which we conducted and with our models, they are very sensitive to what you're assuming as far as the properties and what goes into the model.

I guess, just to summarize, I would say that conceptual models can be supported by laboratory scale experiments, but they may not be valid for larger problems. Likewise, the physical mechanisms that are present at full scale may not be reproducible at the laboratory scale. And, the most important thing here are the matrix/fracture interactions because it's very difficult to replicate these type of mechanisms at smaller scales; likewise, other possible larger scale heterogeneities and perched water conditions. And, finally, boundary conditions may not be--or they may be prohibitive when you conduct experiments at laboratory scale. I don't mean to say that laboratory experiments are not useful because information gained from laboratory experiments are very often necessary for this type of analysis. However, final conclusions or final determinations of conceptual models may not be possible without doing larger scale experiments, such as field scale.

That concludes what I have to say.

DR. LANGMUIR: Thank you, Ron.

What I'm going to ask is that we hold questions for these speakers of short periods here, few minute period presentations, and hold until the discussion following their five minute presentations.

Those of you who are speaking to us now who have overheads, please see to it that the gals in the back of the room get those overheads to copy so that all of us have access to your figures.

The next presentation is by Richard Luckey of the U.S. Geological Survey, Water Resources Division. I'm told here the rumor is he's going to talk about perched water at Yucca Mountain and how this bears on our understanding of fast pathways flow and transport if that's still correct.

DR. LUCKEY: I'm going to talk about several items extremely briefly, mostly about perched water, but a couple of other items that may relate to the discussion at hand. I'll give you just a little bit of information update.

In late May, we started pumping at the C-Hole complex. We did some evaluation pumping to make sure that the pipeline would hold and then we started a 10-day test at about 270 gallons a minute in an open hole. Perhaps, the important observation here is while we had more than 20 feet of drawn-down in the pumped well, at about a quarter of a mile away at the Nye County well, ONC-1, they saw water level change there of about something of the order of a half a foot. Now, that doesn't mean that water moved that quarter of a mile in 10 days. That's just a pressure response and it's real important in this to keep pressure and water separate from each other because they move at quite different speeds. But, anyway, that's a little bit of information on-- I won't call it a fast pathway in the saturated zone, but it indicates some connection between the two. In that particular test, we haven't fully processed the P1 and surrounding WT holes. If there was any response in those, it was hidden down in the noise level. We're going to be working real actively, but if there was any response, it was way below a .1 of a foot.

And, the other thing I wanted to remind the Board of is that we have a recharge study in Upper Fortymile Wash. Alan talked about some of the wet events that we've had in the study in Upper Fortymile Wash. We have some holes that actually go down to the water table. We did see some recharges. It's fairly shallow depth to water up there, less than 100 feet, but at least we did get water down to that depth.

Okay. We'll get about two minutes on perched water. We've talked about perched water a lot. We've spent a long time talking about UZ-1, UZ-14 in past times. More recently, we encountered perched water in SD-7. This is the first borehole that was not up in the Drill Hole Wash, northern Yucca Mountain area, where we encountered perched water. So, we encountered perched water in that one. We dutifully stopped and ran some hydrolic tests and got some hydrochemical samples. This is Test No. 4, our best test. We did three of them previously. This is the first couple of hours of data from that test. It's kind of interesting. It sort of looks like one would expect. It was only when we pumped longer that we got something that didn't quite look like that. This is a draw-down recovery for that test. This is the starting point, a little over 16 feet. At the end of the test, it recovered back to about 12 feet. When we started this whole series of tests, the transducer was submerged to 20 feet. So, we actually lost about eight feet of water here.

As a hydrologist, I just got to fit this to a tight curve. The tight curve I chose is a straight line. This is how it fit with a correlation coefficient of about .96. So, basically, this tells us that we're pumping out of a bucket and that the edge of the bucket is darn close to the borehole. I'm not sure what this all means, but this is not an extensive well-connected water body like we saw up to the north. That doesn't mean that there's not a lot of perched water around there. It just probably means that if there is lots of perched water, it occurs in fairly isolated pockets.

Do I get the record? I'm 15 seconds early according to my watch.

DR. LANGMUIR: Thanks, Richard.

Parvis Montazer is going to talk to us next. He is from MEI Corporation, I guess, in California here. He represents Nye County, and he may have changed his mind since he told us this, but he's going to talk about Nye County's view of what fracture flow means for Yucca Mountain.

DR. MONTAZER: I've just got some comments if this is okay. I don't want to make a formal presentation because we're going to be here longer than you would like.

I'm really excited to see that 11 years ago when we published the conceptual model of the Yucca Mountain, a lot of the things that we stated without any data or any observations are coming true. The fracture flow at that time, we believed, was real. I'm kind of disappointed to see that the common knowledge is still that the matrix flow is generally very slow and the fractures are dry most of the time. It has been demonstrated, as we heard it today and in the past over and over, that the fracture flow is dependent on the contrast between the properties of the fracture and the matrix that's involved. The initial conditions and the boundary conditions and all those affect when the fracture flow occurs.

I believe the present conceptual models are acceptable. We have long way to go. This is on the first bullet. And, as far as adequacy, that's a strong word. It's my believe that if we get all the experts in this area and put them to work for 20 years and ask them this question on adequacy, they're still going to come up with a no or a maybe answer. So, I think for the decision making process, the conceptual models are--to state that they are usable, but still they are not adequate. So, there's still a lot of work to be done to improve upon those.

As far as the sufficiency of the understanding of the important parameters that control, I believe that there is enough history in soil physics and fracture flow phenomena that give us a good feeling as far as the understanding of what is important. The problem is how to measure what is important. And, more difficult--probably an order of magnitude more difficult in the fracture/matrix type situation is characterization of the chemical properties and, basically, hydrochemical characterization is much, much more difficult than flow characterization.

The two phase flow processes should not be forgotten. We heard about the environmental transport today and there was not as much emphasis as I would have liked to hear as far as the advective transport of the chemicals in the vapor phase or in the air phase. A lot of the tritium results that we see could be possibly explained by advection in the air phase.

We're in one of the Nye County boreholes, seeing fluctuation responses in the barometric fluctuation all the way down to 1200 feet. We've seen it in the other boreholes at Yucca Mountain at UZ-1 and NRG-4 at the shallower depth. And, these are behaving--these parts of the mountain, the responses are lagged and delayed differently. Therefore, there are different potentials as far as the pneumatic potential is concerned. Basically, what is required to move the mass of air is different at a different part of the mountain at different times. This causes the mass of air to move laterally or vertically in various directions. And, with that, the environmental tracers move also. If you consider this going on for tens of thousands of years, you can see that we can have a considerably different picture than if you just look at the water or percolation vertically. And, the same thing goes with shallower depths and studies in other sites.

The question is how do the existence of potential importance of fast pathways influence our views about the suitability of Yucca Mountain? That is the question that I think the existence of the pathways were suspected from early-on, as I recall. It's a matter of whether these fast pathways--or how many of these are and how much of these, as far as volumetrically, as far as both air and water, how much they contribute to the transport of the radionuclides from the repository down to the accessible environment. And, that requires basically--I think the way I see the program, they're just beginning to investigate that because without going down into the tunnel and the ESF facility, this type of question really cannot be answered with vertical boreholes.

DR. LANGMUIR: Thank you, Parvis.

Let's shift gears just a little bit and now begin more general questioning and commenting around the table. In this process, I'm going to suggest that Pat Domenico and Victor and I co-chair this part of the meeting. One comment that Parvis made that maybe is an appropriate starting point would be how do you measure what's important? You're looking at a three-dimensional system. The more you puncture it, the more you violate its integrity and you spend more money. So, the question is how can you make measurements? It's simple enough perhaps if you can believe that all the flow is vertical, that you could get a handle on that. But, clearly, Yucca Mountain is not that way. We've got, as Alan suggested, significant amounts probably of lateral flow on impermeable beds. How can you get a handle on how much of that is going on? What measurements can we make perhaps from an ESF that would give us some idea of these unmeasurables right now and solve those questions of what we don't know at depth? That's a very vague question which perhaps we can get some of the folks involved in thinking about.

Alan, any thoughts?

DR. FLINT: Well, I guess I like--I was going to come up with an answer to your first question on what are the problems in characterizing fast flow? And, I was just going to say that's money. I think if we had money, we can do that.

I think the analysis from vertical boreholes is actually a good analysis for characterizing the influence of lateral flow. I think it was a while back, Ed Kwicklis put out a paper where he looked at the appearance of lateral flow using a soil physics approach where we take the water contents, the water potentials, the properties, and show that there are different zones in the bedded units that have upward gradients. So, it's a good indication that there's been lateral flow. And, I think if we can look at a variety of locations, we have a much better approach at handling that.

I'm not sure how much the geochemistry will be able to help answer that question, but I guess my basic philosophy on all of this is that it has to be a combination of field measurements and observations and numerical models that can reproduce some of those results that can account for the tritium values or carbon-14 values. It has to account for two things; one, the occurrence of tritium and it also has to account for the lack of tritium in a lot of the locations that we've looked. And, I think that's real important that we be able to devote to those.

DR. LANGMUIR: Dick Luckey, any thoughts? Let me shift gears for you a little bit here. I've been concerned that you may or may not know as much as you'd like to know or need to know, perhaps, by 1998 from the surface based testing effort at Yucca Mountain to competently contribute defensible positions to DOE for their site suitability decision. That's a loaded question for you. How do you feel about the status of information collection from surface based testing at this point and where it's headed?

DR. LUCKEY: Well, if I put kind of my limited hat on as, you know, saturated zone studies, I don't get a whole lot out of the ESF unless somebody badly miscalculated. I think that one of the things that--

DR. LANGMUIR: You're subsurface and Alan is surface and we didn't have anybody in the middle today.

DR. LUCKEY: One of the things I do think we are getting out of the surface based testing that I think is going to really help us a lot in understanding these fast flows is this perched water information. Now, we don't have a lot of it. We've only been able to test it in two boreholes. One of them, I think, is at UZ-14. People are going to be uncomfortable with any isotopic and chemical results that come out of that because of the contamination that was encountered from presumably drilling G-1. But, places like SD-7 where we were able to get the chemical samples, we finally have a place where we can maybe catch some of this fast water moving down, collect it in one place, and get a sample of it. And, that's going to be a major--once someone sorts that out--and I'm interested in sorting it out. I don't have that information. Once the picture of perched water is all sorted out, I think that's going to tell us a lot about fast pathways and the unsaturated zone or possibly lack of those.

DR. LANGMUIR: You know, I had an idea some time ago that the perched water might represent a meaningful mixture of matrix versus fracture flow and a way of backing out proportions of the two historically that might have contributed to that perched water. I don't know whether that's gone anywhere or not. That's a nice geochemical problem. Lots and lots of geochemical tools could be brought to bear on making that calculation of mixture.

DR. LUCKEY: I think that's going to be a tough problem and I don't expect the results of that to be dependent because I think that perched water is probably going to be a mixture of lots of different fast paths from lots of different places at lots of different times plus some matrix flow. And, that may make it a very difficult problem, but it's the best shot we have at it.

DR. LANGMUIR: Alan, do you have a comment?

DR. FLINT: Yeah. Of course, I think we all realize that it's not a question of matrix versus fracture flow. Both go on into different extents and different locations. If we look at the perched water body underneath, a large exposure of nonwelded tuffs.

There's one thing we learned from the ESF for those that have been in there. If you haven't, it's very interesting. It's the intact nature of the Paintbrush tuffs. You can't find fractures that look like they're flowing there and you can't find--the only way you can tell there was a fault is because of the offset of the beds. In many cases, it seems to me with that information the fastest pathway from the surface to the water table may be through the PTn where it's exposed and then into the Topopah Spring where we have enough of a saturation to induce fracture flow in the Topopah and then move it down. Where that happens at the high enough rates, it seems that we get down to the vitrophyre, the basal vitrophyre, of the Topopah which has a low permeability which is where we see the perching occur. Anywhere else, if you have to go through the Tiva first, you're certainly delayed. But, it seems that we have to account for the fact that we may have fast fracture flow in the near-surface until we get to the PTn unless it's exposed directly. Transition into matrix flow.

And then, we have to start the process up again. I talked about initiating fracture flow at the surface because of rainfall. The next issue that we have to address is how the PTn deals with this water and whether or not it can re-initiate fracture flow and if it re-initiates it somewhere else over a different kind of area. And, if it can initiate it over a fracture area, such as going through Drill Hole Wash. And, that is something, I think, we have to account for; that we do have both going on and for a long period of time it is going through a matrix.

DR. LANGMUIR: Maybe a little shift in gears here. I'm wondering if Tom Nicholson and Gregg Davidson over there have thought about how they might apply your chemical methods to Yucca Mountain. If they know enough about the Yucca Mountain problem? Tom shakes his head no.

I can see some ways that I would like to see it applied and maybe attempts have been made to apply it, but I guess it's not a fair question without geochemists sitting at the table who are working at Yucca Mountain. We shall wait for tomorrow when we're going to have more isotopic discussion.

Victor, do you have some questions?

DR. PALCIAUSKAS: I'd like to ask several of the people, Scott Tyler or Bridget or Ronit or even Alan, what is the prospect of getting a respectable relationship between the overall precipitation and the deeper percolation, a functional relation between these quantities that will be useful in the future modeling in the next couple years? And, how much will it cost?

DR. FLINT: That comes free. I think we've seen a good relationship between rainfall and infiltration and fracture flow in the near-surface. But, based on the properties of the rock, based on the PTn and some modeling we had done a couple years ago that was in high-level waste, what we're seeing going on in the PTn now, lateral flow and divergences, is a result of what the climate was like 5,000 or 10,000 years ago. So, anything that we see in this period of time is not going to be--we're not going to see the results of that with the exception of possible geochemical signatures that we might see at depth having tritium or bomb pulse chloride-36. But, I think we've developed a fairly good technique for taking rainfall and past/future rainfall for thousands of years based on what we understand about the system and we can, I think, do a real credible job of controlling near-surface flux. We can have that flux go for thousands of years. We can put the conditions that we think might occur.

The question is is integration between the near-surface flux and how it responds to climate because it responds almost immediately and what happens deeper down in the mountain and the fact that our model becomes much more generalized, much larger grid block, and those pulses that change by orders of magnitude over 30 meters to 60 meters may not be reflected very well and that signal may get maybe very, very diffuse where we can start very specific pathway flow, very specific locations. And, taking that information and have to have it match up with that same pathway down deep is going to be a real trick.

DR. SCANLON: I guess, from the data that I was showing, it seems like--the soil physics data, the water potential data, there's a net upward flux in the top 20 or 30 meters in the Nevada Test Site, the Texas, and probably Ward Valley. You know, so how long that takes to develop is questionable because it depends maybe how actively the roots can remove the water. But, it suggests, you know, if you depend on evaporation alone, it would take thousands of years to develop. So, we may be in a drying cycle near the surface and the deeper water that's moving down may be older, probably older water, and it's balanced by upward geothermal vapor flow and so negligible fluxes below that. So, I mean, that's probably not much help.

DR. FLINT: Well, that's something Bridget and I talked about for just a couple of seconds this morning is that the paper that we had done for Yucca Mountain, I think, was two years ago where we show the only way we could match the profiles in some of the boreholes we saw was if we had over 1500 years of an upward flux, a dryout zone in the near-surface, which penetrates down to a couple hundred feet. Those are on south facing slopes, high radiation loads. But, she's absolutely right. In some parts of Yucca Mountain, we're going to see downward flux. In other parts, we're going to see upward flux. I think that's real important.

DR. LANGMUIR: Alan Flint, as I recall, a few years ago, you suggested you had a template; in effect, you could predict the places at the surface where infiltration was sufficient and the properties of the rock were sufficient so you could predict that you have perched water below those areas and not below some other areas. Is that a correct recollection of where you'd gotten a year or two ago that you could predict where perched water would be?

DR. FLINT: We had surface fluxes that we know if the lateral diversion was not too great would exceed the capacity of the underlying rock even with fractures to allow it to pass in unsaturated state and that perched water bodies would occur. We showed a couple of zones even within the Topopah Spring that have had enough flux that you would get perched water occurring.

I think it's important to separate when we start dealing with perched water, there are several mechanisms that lead to perched water. When we see the large mechanism to the north, we think that has to do with, one, high rainfall rates; two, the higher infiltration capacities of the exposed bedrock. I think those are all important.

DR. LANGMUIR: This is this very steep--

DR. FLINT: This is to the north actually where the steep gradient is. And, we would predict perched water occurring down farther south at SD-7 under a completely different mechanism. And, it was a little surprising--it's good to hear what Dick has to say about it being a fairly limited system because that's what we would expect. On the upgradient side of a fault, we would expect to see perched water if water is finding that fault to be a barrier and that's what Montazer and Wilson put out 10 years ago. On the downgradient side of that fault, we would not expect to see that which is, although a long way away, that's what UZ-16 showed. So, we have two holes either side of a fault, some distance apart; one has perched water, one doesn't. It's another mechanism. We have to always keep in mind that there are different mechanisms of play in the development of perched water and we don't want to think it's all the same thing. We have to keep that separate.

DR. CORDING: Alan, you had mentioned that it could be a combination of the vertical fracture flow and then matrix flow when we get to perched areas. But, in your view of the lateral flow, what do we know at this point about the lateral fracture flow? A lot of the lateral flow could also be fracture flow either on bedding or on vertical fractures that are within a given portion of the stratigraphy or a given formation. So, is your view on the lateral flow--what do we know at this point about lateral flow in terms of the types of permeabilities or fluxes you would be anticipating?

DR. FLINT: Where we expect to see most of the lateral flow is in the Paintbrush nonwelded tuffs. In a soils physics analysis, whenever you have a contrast in properties, whether it be an increase in porosity or decrease in porosity, you have an enhanced potential for lateral flow. And, with the dipping beds that makes quite an contribution. The only place that I've really noted horizontal fracturing is in the top of the Tiva--or it's in middle Tiva. We have quite a few horizontal features that would enhance lateral flow even through matrix blocks. As we go deeper down into the Tiva into the columnar unit and then into the moderately welded columnar unit, they're very much vertical fractures.

The most impermeable rock we have when we get into the Topopah which is the vitric cap rock of the Topopah it's extremely fractured vertically, although we can't find anything but fill in those fractures from old flow pathways perhaps. For the most part, we see vertical fracturing.

So, my guess is and, based on the analysis that Ed Kwicklis did and some that Lorri has done in the paper she has coming out fairly soon, we can strip off a whole lot of that water in the PTn more than anyplace else we have on the mountain. That's the most effective place. Of course, the question becomes where does it go?

DR. CORDING: But, you could also get lateral flow in a zone--let's say, a vertically fractured zone sitting on top of something that has a relatively lower fracture permeability.

DR. FLINT: Right. You can have the fracture flow in the--you could have horizontal flow in fractures. In fact, we believe that the vitrophyre where it's causing perched water to occur above that in the Topopah highly fractured zone, probably a pretty good pathway down toward the Bow Ridge Fault. And, that becomes critically important if you have waste that manages to get into the water that's moving down vertically. It hits this vitric zone if the flux is high enough because you had a lot of condensate or whatever that lateral flow which would occur if we exceeded probably

.2 millimeter a year because that's the capacity of that vitrophere, would head down toward the Bow Ridge Fault, and bypass the whole Calico Hills unit. And, that makes that crucially important. So, in that case, we might have quite a bit of flow going through welded tuff through the fractures.

DR. CORDING: And, ultimately, dumping it into the major continuous features which would be the fault system.

DR. FLINT: Well, that's a question, I think, that Dick really has to address is what is the role of those faults in the saturated zone? Are they conduits, barriers, and that's something I think they're working on quite a bit.

DR. CORDING: Well, I'm thinking just of getting down to that point, you know, even in the unsaturated zone.

DR. FLINT: Yeah. I think the faults are very important and one of the approaches we've taken in applying our near-surface infiltration studies to faults is that we talked about the fastest pathways. We simply defined the faults that have been mapped, and if you look at the fault segments in a GIS system, there are over 3,000 of them that have high fracture densities. If you're going to look for a fast pathway, that's a good place to start. I mean, why go for unknowns when you have 3,000 that you know about? And, the question we get to is are they covered, do they have a lot of soil, very little soil? The second question is how do they behave when we get lateral flow underground. At the surface where it rains, they get wet and water goes straight down. If it's coming sideways and comes upon the same fracture, it's a different mechanism, it's a different process that goes on. So, they have different roles at different locations in the mountain whether they're barriers or pathways because they could be both and probably are both.

DR. DOMENICO: I have a question for anybody, almost anybody. Will the ESF in the sense that it's consuming most of the resources--will the ESF contribute to our understanding of some of the problems that we're talking about today and, if not, how can certain things be designed so we would further understand the question of fast pathways perhaps or transports through the vadose zone? Anybody? It is actually the large consumer of resources here.

DR. MONTAZER: I think from the beginning it was known that really all the surface based testing that were going on were designed or planned for the site were going to really address the features that are, more or less, horizontal. It was required to--most of the fractures are vertical. In order to see and characterize fractures or to really get in touch with the fractures in the repository horizon, an ESF is really essential. Without that, you will have to drill many, many horizontal boreholes maybe from Solitario Canyon or something like that to be able to statistically intercept features that are not amenable to be sampled by vertical and near vertical boreholes. So, in that sense, I think that just adds a different--ESF adds a different dimension and understanding.

The other thing is that I worked underground for three years when I did my dissertation. The perspective that you get from the behavior of rock underground is totally different from what you see in the surface. What I learned underground, there was no way I could have learned it at the surface by looking at rock and poking holes and testing it. Large scale observations and tests underground, I think, are essential.

DR. DOMENICO: We haven't said anything about the possibility of faults being the fast pathways. Will the ESF testing resolve that issue one way or another? Will that come out of it?

DR. MONTAZER: It was originally planned, if I recall. I don't think much has changed.

DR. DOMENICO: Which, I think, is a big question, you know.

DR. MONTAZER: The intention--it's not my place to say what the project is intending to do. From what I recall, the design of the ESF--which I wrote as far as hydrologic testing was concerned which I wrote the majority or part of it--was designed to address the characterization of the faults in both the Topopah Spring and Calico Hills.

DR. DOMENICO: Well, we have a tunnel at Apache Leap, I believe. What has that contributed to understanding the hydrology there, your tunnel system, in terms of the pathways, in terms of the things that we've talked about?

DR. NICHOLSON: Okay. The haulage tunnel which is underneath the Apache Leap tuff site began as a very small grain which they just wanted to say what is the nature of the geochemistry and can you measure water coming through certain fractures they thought extended from the surface down under Quinn Creek. They were able to measure flows from seeps up to 10 liters per day. They were able to collect geochemical samples that said that, yes, in fact, there seemed to be a connection. They have now over the course of the last three years begun to do some very detailed analysis of the geochemistry and they've been looking at the lag between rainfall and runoff from Quinn Creek with the subsequent seepage measurements at depth. And so, that is the basis on which now we're proposing to do experiments.

Now, what we want to do, as I said earlier, is that Randy using strati-model is visualizing the fracture system there under Quinn Creek and we are going to then design a series of experiments, infiltration and tracer experiments, that's going to quickly identify are there fast pathways? And then, looking at the range from the, what I'll call, discrete fracture network which is really more of a condition, discrete fracture network model, all the way to the equivalent porous media models, we're going to try to collect enough information to discriminate between those models.

Now, the answer is we do not categorically say, yes, there is very fast pathways and we know exactly where those fast pathways are. We think there's a very strong indication there's a fast pathway. The relationship between the rainfall runoff and seeps at depth indicate that in geochemistry. But, to be able to point to a discrete fracture and say that's where it's coming from, we can't say at the moment. And, that's why we want to do the tracer experiments.

DR. DOMENICO: You know, really how Alan addressed the first question, the problem characterizing fast flow, he said, was money. I think it's time. A lot of things have been raised here within the scope of this program. It seems very, very difficult to accommodate all those questions within the time frame that we seem to be operating under. But, I guess, time is money.

My last question is--and anybody can answer this because I'm getting confused now about what the prevailing conceptual model for the vadose zone is at Yucca Mountain and I think it differs whether or not you're in performance assessment or whether or not you're in science. Does the prevailing conceptual model at Yucca Mountain from a performance assessment perspective in any way seemingly reflect some of the things that we've been talking about today? I know your view of the conceptual model and my feeling differs depending upon different parts of the program.

DR. FLINT: I agree. I think that it differs in different parts of the program. I think that the performance assessment people have made tremendous strides in characterizing--in putting site characterization information into their models, putting the data into their models, putting a lot of the ideas into their models, and I think that we're coming to much more of a consensus now with the performance assessment people and the site characterization people. I think we have worked out a pretty good relationship where we exchange information, exchange ideas, and I think we're doing actually fairly well now with a lot of things going on in performance assessment.

DR. DOMENICO: So, you think there will be one prevailing conceptual model for all groups someday?

DR. FLINT: I think, we're getting there because I think that the science can't play too many more tricks on us. And, hearing from Israel where we see that, gee, thick soils don't seem to respond the same as thin soils and what Bridget has done, I think we're all sort of finding out that hydrology is hydrology. And, if we keep attacking the problem as a science problem and--I think the reason that performance assessment is doing better is because I think they came over to our way of thinking a little bit.

DR. DOMENICO: I'll buy that. I'll go along with that.

DR. MONTAZER: The way I perceive performance assessment interaction with science is that performance assessment studies the effect of the impact of the extremes with simplifications. To science, the site characterization, all the other part of the program, are trying to minimize that range of uncertainty so that those extremes will be narrowed down. I think, performance assessment has to make a lot of simplifications, but those simplifications has to be conservative. They have to be conservative. And, it's up to the scientists to try to come up with the data and information and support to justify basically bringing those--reducing the conservatism.

DR. NATIV: I may be breaking into an open door since I'm an outsider and I'm not fully aware of all the range or the spectrum of studies that have been carried out throughout the years.

DR. LANGMUIR: But, you lack our prejudice. So, it will be refreshing. Go ahead?

DR. NATIV: There are three issues that haven't been touched upon in this meeting and may be fairly well-studied outside this room, but I think they bear some importance. The first issue has to do with colloids, particle movement. I've heard about water. I've heard about solutes moving in the fractures. I haven't heard anything about colloid migration in the fracture zone and maybe this is something that has been studied. We just didn't mention it today. I think it's important once potential contaminants are going to be introduced into the system. We really need to know when we talk about sorption, are we talking about the sorption into immobile skeleton or onto mobile particles that may carry the contaminants perhaps easier in a fracture than in a porous system? So, this is one thing that has not been addressed. I think it's worthwhile thinking about it.

The other one has to do with the connectivity and the extent of the geometry of the fracture system. This is something that has been discussed over and over. And, again, it's possible that this has been done and I'm sorry if I'm going to waste words on something that has been done, but was not mentioned here. When we tried to characterize the fracture system in the chalk in the Negev Desert in Israel, there was a lot of support for the hydrologist that came from structural geologist because you see so many fractures. Some of them are small, some of them are not connected, others are filled with possibly impermeable material, others may be open. A lot of insight into the fracture system came from structural geologists who looked at the lineaments from land site and from air photos and they went and did some extensive field work trying to confirm and verify what was studied or what was the result from the picture interpretation forces using softwares. And, it seems like not all fractures bear the same importance. Not all joints have the same importance. There are these super highways that we would like to find out and there are others that are less important. As a hydrologist, I couldn't sort them. We got a lot of help from the structural geologists in that respect. And, maybe this has been done and I'm just talking and wasting your time.

The other issue that I think is important and Scott Tyler mentioned it, he was referring to the coating of the fracture as a potential barrier for imbibition. Aside from the retardation that comes with the coating, there is the chemical issue that has been touched upon. What will sorb or interact with these coatings and what will not and what type of--what effect is it going to have? The chemical interaction; I'm not talking now permeability or retardation. What is the impact of this coating in terms of attenuation of radionuclides and other contaminants?

DR. BOSSOUD: If no one would answer that, I'd like to answer that one.

DR. LANGMUIR: Go for it. Come on up and introduce yourself.

DR. BOSSOUD: Okay. My name is Gilles Bossoud. I'm project leader for site characterization and regulatory studies at Los Alamos.

We have as part of our game plan to do transport which incidentally has not yet been mentioned here because the transport we're doing is purely of a chemical nature and how it combines with all the hydrologic parameters that are given to us. And, we are studying colloids. We have been directed by the DOE to do so. We are looking at the scaling and doing scaling studies to see how we can scale our models and, therefore, build confidence in them. This is also what other laboratories are doing for hydrology. So, scaling issues are being addressed.

We are looking at the connectivity of fractures, but as you pointed out, because every fracture is unique, number one, and not every fracture is involved in flow--as a matter of fact, we have not established that the fractures are involved in flow at Yucca Mountain, by the way--that we have to look at how we can integrate this into our modeling efforts. And, we are working with the USGS, with Lawrence Berkley Labs and Lawrence Livermore, all these laboratories are working very hard to input these scaling properties into what we're doing, both in terms of discrete fracture studies, how we can couple them to dual permeability, dual porosity, equivalent continuum models, and 2-D and 3-D. So, in other words, even though it's not represented, there is a major, major effort with a 10 year/15 year history behind it. And, what I would like you to pay attention to tomorrow when the talks are about retardation, the true retardation, sorption, diffusion, and the like.

And, I would like to finish with, yes, we're also looking at fracture coatings. We have a group studying fracture coatings and how they sorb. But, I think it's important to know that these efforts are going on. They're very active and very mature.

Thank you.

DR. LANGMUIR: Thank you. I might add to that that the DOE has a very active colloid program which I don't always agree with, but--as to the importance thereof. But, there's no doubt that if you get a colloid to a fracture in rapid and fast pathway, it's going to move. I think we can talk about this some other time, but getting it to the fracture from the waste packages is the issue in my view. So, there are very active programs in all of these areas. There's fracture mapping on the surface and fractures at depth which will be looked at from the ESF and that's all being tied together, as well.

Tom, did you have a question or comment?

DR. NICHOLSON: I just wanted to mention that we didn't even discuss today some of the work going on at the Apache Leap tuff. Our--and building measurements were specifically to look at the role of connectivity of certain fractures. So, single crosshole tracer experiments are looking and using air and helium as a way of understanding that connectivity. And, to some extent, the work that Gregg discussed, there was also geochemical evidence of looking at connectivity in the so-called fast pathways. So, we are very much addressing that. And when we talk, we don't always talk about connectivity or super conducters, but that's what we mean.

DR. TYLER: Kind of addressing the gentleman who was just up and also a question that Victor asked earlier with respect to predicting fluxes. I think there's an important issue that we need to discuss here and that's that we probably can predict fluid fluxes reasonably well based on the climatological and hydrologic characteristics at the surface and also couple that with what we understand about the groundwater system, as far as the magnitude of recharge that the system can accept. But, I think the more principal and important issue is that recent bomb pulse tracers have been found at depth which the fluxes may still be very small, but the velocities in certain regions are extremely high.

So, it's important to disconnect a little bit here the issue of just general fluxes and then the issue of transport because they are in these fractured systems quite disconnected in many ways. I think I'd like to see a little more discussion on the issues of fast pathways that have already been documented. Is the ESF going to be able to--one fear I have is that the ESF won't find anything and then how do you explain the tracers that have been found at depth from the surface based testing to date?

DR. LANGMUIR: I hope you'll be here tomorrow when we talk about--June Fabryka-Martin talks about chlorine-36 data. Al Yang which will be around, as well, and perhaps can talk a little bit about carbon-14 data.

Ron, a question or comment?

DR. GREEN: I just have a belated comment to Scott and that's the issue of flux versus fast path. You know, we've seen a lot of evidence--maybe not here yet. I think, tomorrow, we'll see some of--very small amounts of certain tracers showing up at depth. But, I think, one question that has to be asked is the quantities involved. If we see very small pathways taking very small amounts of bomb pulse tracers at depth, is that sufficient to disqualify an area or a site if these quantities are extremely small, or I think one of the questions we have to ask is what's the total flux involved because we're never going to find a site that acts as one continuous body that acts all in the same way.

DR. FLINT: I think that I've talked to June about this a little bit, but when she talks tomorrow, she's done some real interesting work that tries to show the relation between the disconcordant ages between chloride-36, chloride, tritium, and in terms of a percentage of mixing and how much water you would have to take to get one signature and that these things really can provide a coherent story. And, I think, hopefully tomorrow she'll be able to talk about that, but I think she's done some real interesting work in that area and can address that.

DR. LANGMUIR: Several times we've talked about--Scott started the ball rolling on fracture coatings. I thought of this, but never really have gone as far as he obviously did go with it with imbibition versus minimized imbibition. And, certainly, this is affecting the interaction between fracture flow and matrix flow. Barbara Carlos, as I recall, for Los Alamos did a lot of work on fracture coating at Los Alamos some years ago. I have not seen it being applied to characterization of flow in Yucca Mountain materials in the geology there.

Maybe, Alan has or someone else has, maybe Ardyth Simmons who was here earlier knows whether this has been done?

DR. FLINT: While she's getting up, I want to say that we've done a lot of work similar to what Scott has done and there is a large range in the properties. In many cases--in fact, almost all cases at the surface of Yucca Mountain, these coatings are an enhancement to flow because they can--or they don't make it go faster through the fractures. They hold it up so that it can go into the matrix which is what we see in some of our neutron logs. And so, the fracture coatings can both increase the sorptivity of the rock for water or it can cause it to go faster. It can go both ways.

DR. TYLER: Yeah, we've found the same thing also in the Tiva Canyon; that the Tiva Canyon coated material was indeed higher permeability or higher sorptivity than the uncoated. But, when you moved into the Topopah Spring, it went the other way.

DR. FLINT: You're talking about desert varnish?

DR. TYLER: Right, surface coatings.

DR. LANGMUIR: Have you left anything for Ardyth?

MS. SIMMONS: Just a little. Barbara's work has been ongoing for a number of years and it's being applied in two primary areas. One, it's being used to improve the level of detail that we're able to incorporate in our 3-D geologic model and that serves as the framework for the site scale hydrologic model that the USGS and Lawrence Berkley have been working on. So, that's one area.

A second is that Barbara has been looking in detail at the composition of these fracture coatings more from the standpoint of how they could help in terms of radionuclide sorption. So, the aspect in which her work would apply to the hydrologic model with regard to flow would probably be in the way that it's input into the framework model for geology, but it will be brought in and is being brought in in detail as we look at the role that those fractures play in terms of sorption.

Right now, it's been shown and I think it was discussed at your Board meeting last July that calcite in the fracture coatings could be significant for the retardation of neptunium and that hypothesis is being looked at with regard to some of the other coatings, as well.

DR. LANGMUIR: I guess my question, more specifically, was more what Alan was commenting on; whether there's been any measurements of the interplay between flow and fractures and getting into the matrix as you retard flow down a fracture zone and the matrix interacts as a hydrologic sink and feed. How much do we know about the effective coatings on that process as it impacts the overall model for the mountain?

DR. FLINT: Well, we are doing some measurements in the laboratory, at least. It's a fairly young study that, I think, all of us are talking about stuff that Scott's done or that Van Genuchten had done on this interaction. We think it's important. It's certainly in the near-surface. 90% of the fractures are poly-filled. The kinds of observations that we make now--and we do deal a lot with the structural engineers' geology to find out which features have open fractures and which ones have closed fractures. And, the open ones are probably, you know, the northeast trending features going against the stress field. But, for the most part, we have to deal with coatings, absolutely. It's a real dual permeability model, one being a matrix of coatings and the other being a rock. But, there's a lot more to be learned from coatings, a tremendous amount.

The work that Joe Whelan in the USGS trying to get age dating information, looking for dissolution surfaces. That tells us a lot about how old these coatings are, about some of the pathways, and which fractures are actually active and have been active in the past and how we change from one path to another path. So, there's a lot of information to be gained from coatings. I think they're real important.

DR. LANGMUIR: Thank you.

Any more questions or comments from folks at the table?

DR. CANTLON: I'd like to get Alan to comment. Many of those fractures have rooted shrubs. You can see where you've exposed the area. Do you have any feeling about the role of the transpiration of those shrubs in changing the water balance and, therefore, the movement of water down the fracture?

DR. FLINT: We've seen the same thing. In particular, if you look at the Ghost Dance Fault, there are shrub roots that are down probably 15 meters or more that have been taking water out of those zones. I think those are more unique in those cases where the fractures are fairly wide. In 99% of the fractures that we've looked at at the near-surface, they don't have roots in them. But, I think that the evapotranspiration processes are critical to understanding how this system works. As I showed earlier, we have up to 80 millimeters a year going into the tuff, very few roots in there. Does that water go back out through evapotranspiration processes? And, I think it can by drying out the soil which is covered with plants. And, even if we go to even deeper soils, we know that there are roots down to at least 10 meters. So, I think that that process is very important. We have ongoing studies now to look at that. We're getting instrumentation into the bedrock and, as Bridget said, I think that's a question about what direction the flow is going and those near-surface processes are critical to understand and the plants play a role not necessarily by being directly involved in the fractures, but by drying out the soils near the surface. That water can come back up.

DR. LANGMUIR: If there's no more from the table, let's open this up for public comment. I have a little note from Linda Lehman that she'd like to make a public comment or question if Linda is still here. If not, I can read it.

(Pause.)

DR. LANGMUIR: I can read it. This is Linda Lehman speaking.

--would be useful in defending fast paths and general flow paths is temperature data. To date, little of this has been collected or, if collected, not released. Example, UZ-14, two years later, we don't have the temperature data on the perched water encountered. We do have other chemistry data, such as isotopic data.

I guess, this is appropriate for a response from DOE folks. Ardyth or anyone else who might be prepared to comment on that? Go ahead?

MS. NEWBURY: First, one question. Dick, did you collect temperature data?

DR. LUCKEY: Yes, we did collect some temperature data of the samples. They were included with the chemistry analysis. We didn't do any detailed temperature profiling or anything like that in that borehole. So, I think what data--there was some data collected. I recall that Joe Rousseau made some presentations on what the data might have meant. He filled it with a number of caveats, but I think that the data is included with the water chemistry information and I don't know whether Linda has that information in hand. But, that's what the temperature data is.

MS. NEWBURY: If it's been reported, then we'd be more than glad to whoever asks for it.

DR. LANGMUIR: Thank you, Claudia.

DR. LUCKEY: Well, we've talked about temperature data. I've been dying to answer Pat's question a little more about what we might do in the ESF. There's some temperature information that we're planning in the radial borehole study where we're going to be doing very detailed, precise temperature measurements across the fault trying to look and see if the fault is carrying water and carrying away heat in the process. And, in that same sort of study, we're doing a lot of crosshole testing in the alcoves of the ESF. Naturally, we'd like to have a lot more alcoves and do a lot more stuff and, until the second alcove is completed, we won't be able to do that. But, that is some information that is being collected in the ESF that relates directly to fast pathways down a particular fault. Whether we've looked at the right fault and in the right place, you know, I don't know. But, at least, it will be a data point.

MS. LEHMAN: I just wanted to kind of finish up on the temperature idea. I think that temperature data can be very significant in both the saturated zone and the unsaturated zone. And, it's been my experience that it's highly under-utilized in this program. For example, talking about UZ-14, if that perched water is hot, then we may have a better idea of where it comes from. It could be coming from the west, say, in the hotter zones at Solitario Canyon. If it's colder, then we might think maybe it's coming across the steep hydraulic gradient to the north. So, temperature data can be extremely useful and I would like to see more of it utilized in the program.

DR. LANGMUIR: Thank you.

MR. MIFFLIN: I'd like to follow up and make an observation and maybe stimulate a comment or two. Back to this idea of the relative merits of distribution of characterization funds and the ESF versus a surface based program. I think the viewpoint might also fall into camps of expertise and experience. Geologists, I think, that have that type of background don't get too excited about the need to see really how fractures go and what a fault looks like underground in a borehole, you know, a tunnel; and, whereas, I think, engineers feel that that's very important information. I don't know for sure why this is, but I think that from a geologic perspective, the wide range of fracture orientation and the heterogeneity of fractures, even though you may have a dominance of columnar type fracturing and so forth, is already known from lots of observations in field settings.

And, the tunnel is only going to touch upon these types of features, such as faults and various types of fracture zones, in a very, very limited percentage as exist in that repository area. Whereas, a surface based drilling program that expended the same types of funds, maybe just one year of those funds, would provide a much broader base of databases from the perspective of the total vertical picture, as well as the hydrogeochemical and the hydrological results throughout the full section beyond the level of where the ESF might go.

So, say, a million dollars of surface based testing spread out over several years would certainly develop a whole lot more data from the hydrological perspective, I think, in addressing some of these questions that have been talked about. Whereas, the ESF facility touches some of these features at one horizon, and you have only a very limited periphery around that ESF to deal with the questions that are raised.

DR. LANGMUIR: Thank you, Marty.

Leon Reiter had a question or a comment.

DR. REITER: I have a question and I guess, since the initial topic was looking at other places that have looked at unsaturated flow and the concerns about fast paths, I ant to take advantage of the fact that we have three people here who sat on the Ward Valley Committee, Bridget, Scott, and Marty, and ask them are there any lessons you think that's been learned that could be applied specific or general to what's happened to Yucca Mountain?

DR. TYLER: I can at least make one or two. One of the things that we found that was deficient in the Ward Valley work was the level of peer review during the process and the level of input of peer review during the process. As a result, a conceptual model was put forth and sent forward with limited discussion or limited, if you will, devil's advocate about how some of the data fit into that conceptual model. And, as a result, when it came time--at least, this is my opinion--when it came time for the actual license application, the license application was deficient in that it did not completely address the data sets that they had available to them. I think had they had significant input during the process, as this panel perhaps is doing for this project, they would have gotten a lot further and would have been much more successful.

DR. SCANLON: I guess one of my feelings after the meetings were that sometimes you don't need to answer all the questions before you go ahead. I mean, you answer the most significant questions and you can continue doing studies through operations of the facility. As our techniques evolve and our ability to measure different parameters evolve, we will want to use those in the unsaturated zone or saturated zone or whatever. But, we shouldn't think that all site characterization stops the minute they start building the facility. So, monitoring and site characterization should continue on.

MR. MIFFLIN: I think one of the lessons that would be worthy to consider from, say, the characterization that was done at Ward Valley was when there was a level of uncertainty with respect to a database that was developed and that uncertainty wasn't resolved either due to the fact that there was very limited database or it was just no additional work was done to try to constrain what the possible interpretations were, it creates in some cases an unnecessary level of uncertainty. In other words, certain things can be resolved if you put a little more effort into it. And, that almost is what my previous comment was about.

There's certain concepts or approaches that sometimes don't necessarily yield the abundance of the types of databases that are going to answer the questions. And, if the Yucca Mountain program continues to pour most of the monetary resources and the time resources, so to speak, into areas that give perhaps important databases, but not the ones that resolve the issues, then you come to the--you allow a program to get to too high a level of uncertainty in certain key issues. I think that's the history of this program, to be honest, at this late point when the efforts have been going on for 12 years or so--14 years almost, I guess, now on some issues.

DR. TYLER: If I could just add one more point. I think my bottom line is that if you have fast pathways, you better darn well understand them fairly well before you go to licensing. This is again my opinion, not the NRC's, I suppose. And, if you don't have fast pathways, then your database should be sufficient to prove within reasonable assurances that you don't have fast pathways.

DR. DOMENICO: Correct me if I'm wrong, but I do believe the low-level program does not require calculations of any sort or certainly no complex modeling and the decisions are based largely on geologic and hydrologic data as interpreted by the hydrologists and geologists. Is that a fair statement? Unlike this program.

DR. TYLER: If I recall correctly, the low-level Waste Policy Act does have a--I believe, it's either a 500 year or a 1,000 year containment requirement.

DR. SCANLON: They have a performance assessment--

DR. DOMENICO: All taken care of by putting the stuff in cement, right?

DR. SCANLON: No.

DR. DOMENICO: No?

DR. SCANLON: They have performance assessment.

DR. DOMENICO: They do?

DR. SCANLON: Yeah, they do.

DR. NICHOLSON: I can answer the question. 10 CFR 61 deals with low-level waste. It does require that you have to do a doses estimate. It's a 25-75-25 dose to the whole body and individual organs. There is performance assessment. The NRC staff has put out--well, we had a public workshop. We discussed performance assessment. There's a draft position on how to go about performance assessment. But, there really is no true time consideration like there is in high-level waste. There is time periods or horizons with regard to certain aspects such as the waste package that will be considered in the site itself. But, it is different than high-level waste. I think high-level waste has a different perspective than low-level waste does. So, it is quantitative, but it has its own unique culture. We'll put it that way.

DR. MONTAZER: All right. I'd like to make a comment in response to Marty's comment. I agree with you as far as being careful to allocate the funds such that you get the maximum amount of information. There's a certain types of information that I believe you cannot get without going underground and doing underground testing, but that doesn't necessarily mean that you have to have a Cadillac program that costs so much. Now, there are a lot of other reasons that DOE is doing the ramp the way they're doing it right now. There are many, many other ways to go horizontally in the Topopah Spring and collect the type of information we're talking about at a lot less cost. And, I don't want to get into that right now, but if I were going to do it and my purpose was only to collect hydrologic data, I would do it totally differently.

MS. SIMMONS: You said somewhat of what I was going to say, but I wanted to add to that in that the NRC requires us to conduct an in situ test related to how the mountain would behave under repository conditions. That would be our in situ heater tests and I know of no other way which we could do that kind of a test to get information in three dimensions and be able to do it on the kind of scale necessary. There are issues of scaling and heterogeneities that I believe would be very difficult to understand from just a vertical borehole type of program.

So, indeed, the ESF serves many purposes and our approach towards licensing has to be able to satisfy the needs of many different kinds of tests and it's more than just about the hydrologic program and hydrologic conditions under ambient circumstances. So, that needs to be borne in mind in terms of the total picture.

DR. LANGMUIR: I've also understood over the years that most of the fast pathways, apart from potentially perched ones and lateral ones, were very steeply dipping fault zones which wouldn't be found except accidentally, as suggested by Gregg in his setting, by more vertical holes. So, you're not going to find them.  You may miss the important ones with an ESF, but you at least have a chance of finding some and characterizing them by going across the formations.

MR. MIFFLIN:  Can I respond to you?

DR. LANGMUIR:  Yes, Marty wants to respond to me.  I shouldn't be defending DOE here, but go ahead, Marty?

MR. MIFFLIN:  I think that that view is widely held, but I would like to point out that through the surface based program that the evidence of fast pathways has been established by virtue of the hydrogeochemistry and the isotope hydrology in the perched zones already, and that the cuttings showing the chlorine-36, you don't have to have liquid water to recognize some of these things in vertical borehole sampling.  And, that the real key questions are going to be how extensive are these with respect to the overall repository scale hydrology rather than some very localized assessments.  And, this problem with scale is going to be a very difficult problem to resolve and/or apply local field scales--mezzo field scale, I guess you'd call it--findings, such as the heater test, to a slightly larger type of field scale when you have a much heavier load and a much larger load, say, a thermal load--from a heavy thermal load scenario.

And, I'm with Parvis with respect to, you know, how do you get that horizontal or lateral type testing.  You don't really need the Cadillac and I don't think you need the Cadillac to make the heater tests either.

But, the point is I think surface based--and, really thinking about it, you don't have to have all boreholes vertical in surface based testing. There's 6,000 or 7,000 feet angles of boreholes demonstrated in some places already and you've got a lot of options that are a lot less costly.

DR. BOSSOUD: I'm sorry, I'm a bit noisy. But, first of all, I thought that the object of this was to evaluate the data on Yucca Mountain tomorrow, and I think that if you give it a fair shot, your conclusion that the chlorine-36 data is due to vertical fast path may be revisited. And, regardless of whether you believe the different interpretations, it's teaching us a very important lesson. We cannot jump to conclusions with what we know about this data. It's very complex and the mountain is very complex. Before we decide that we can answer all these questions with vertical versus horizontal, we ought to listen to tomorrow's talks, minimum, I think.

The other thing that I'd like to address is that Yucca Mountain is a geologic repository. It's not a Mescalero Indian site. It's not an engineered barrier alone. It's a multi-barrier system. When we talk of retardation, we're not talking about water. Water is just the bus carrying the passengers. We're talking about whether the passengers get off and do they get off on time. In particular, the most important barrier of this geologic repository is the Calico Hills barrier which is specifically retarding radionuclides which are the ones that are mentioned in the CFR points, right; radionuclides accessible to the environment.

This barrier has the capability of retarding the radionuclides by several orders of magnitudes in terms of years. And, before we confuse, once again, the flow trajectory of the water with the retardation or the transport of these radionuclides, we have to look at this barrier, in particular, and we have to--if we are going to address fast paths which, by the way, I despise the word because it's putting a conclusion in front of what is actually there. Yes, we have a mountain of fractures and faults and units that are layered. In other words, we have a filter system on the mountain scale. If this filter system behaves such that it bypasses the multiple barrier system, then you can talk maybe as a result that you have a fast path. We have not demonstrated that these fast paths exist. I think it's important that we demonstrate this, that we at least have a conceptual model of how to approach them with respect to the radionuclide barrier which, after all, is what is buying us our time. And, yes, you can have the ESF, for example, or boreholes and see fractures. You may even see water dripping in those fractures. But, the question of flux is important in this case and, furthermore, the question of does this continue through our barrier? The barrier is the Calico Hills. That's below the ESF.

If we really wanted to know if we have a fast path with respect to radionuclides which is what we should be interested in, we should be going down and have a tunnel or a systems study in the Calico Hills. The problem is we have to ask ourselves do we have the money? Okay. But, if we're really interested in the barrier system of the mountain, multi-barrier system, we have to go to Calico Hills. Now, this has been decided one way or the other, I think, but I think we ought to be honest with ourselves and talk about the real barrier, the one for radionuclides.

Thank you.

DR. LANGMUIR: Thank you.

Tom?

DR. NICHOLSON: From a research perspective, especially looking at Apache Leap, I can't help but think that when you talk about barrier that's the same type of viewpoint in the opposite sense when people use fast pathway. We don't have the luxury of actually saying there are barriers or there aren't barriers, there are fast pathways, there aren't. What we try to do is understand the mechanisms on a variety of scales. For instance, the discussion you had earlier about fracture coatings. That may be a very local phenomena. It may not really make a big difference when you scale it up to a larger effective continuum parameter value.

So, your comment about there aren't fast pathways unless you're biased towards them, but yet you're biased towards barriers. One of the things that we were surprised at Apache Leap tuff is we have a fairly extensive perched system and the assumption is it isn't there unless there is a barrier, a so-called perching unit, a low permeability zone. But, yet, it wasn't identifiable from a conventional standpoint of seeing contrast and lithologies. So, now, we're going back and asking the question what caused the perching to occur? It isn't obvious. And then, going into a variety of details.

So, to me, it's a perception problem to some extent. We don't talk about barriers. We don't talk about fast pathways. With regard to bias, we try to understand the whole system and it may involve using, as they first did at the Apache Leap tuff, incline boreholes so we can look at the fractures. It may involve going underground in a haulage tunnel so we can integrate over 130 meter scale. So, we tried to keep an open mind to say what are the mechanisms and parameter values that help us understand the testing and conceptual models.

DR. BOSSOUD: Excuse me, maybe I didn't make myself clear. Barrier is a barrier for radionuclides. It's not necessarily a barrier for flow. As a matter of fact, tomorrow, you will see that the Calico Hills unit is not really a barrier for flow. It's a barrier for radionuclides in that it contains sorbing minerals like clinoptilolite that actually sorb the radionuclides. It does not stop the water from going through; it simply filters it. It filters it crucially.

The other thing I'd like to keep in mind today is when we talked about the evidence for fast paths, we were talking about subsurface environments that extend to 40 meters, maybe 70. We're talking about a mountain that's 400 to 700 meters to the water table. What we saw today in terms of fracture networks are subsurface fracture networks that are very important to the evapotranspiration infiltration parameters that we need to know about in terms of flux over time. But, the mountain itself is not that environment either.

DR. DOMENICO: I think the term "fast pathway" is appropriate here because this project, of course, is burdened with the groundwater travel time consideration. So, in that particular case, no other project has something like that. So, in that case, when you're given a time, you've got to talk about pathways that are fast. So, I think in that sense the term "pathway" is probably appropriate for the Yucca Mountain Project. Otherwise, I agree we would not be talking about that. We should be talking about fluxes and distribution of fluxes.

DR. FLINT: Yeah, I just wanted to talk a little bit about what Pat said and put some of this in perspective about the ESF, about surface based testing, about identifying fast pathways, and how we're going to do some of this work. I think we all know the reality of the site and the system that we're working in. If we didn't have the ESF running, we wouldn't do any more surface based testing because we'd pretty much stop working altogether. Congress controls which way we go. So, the ESF is essential to do surface based testing. There are lots of advantages in being underground; there are disadvantages and these two systems have to work together, surface based testing, and not just drilling holes.

If you go down into the ESF and you go through what I think is one of the most important barriers to stopping a fast pathway which may be a fault, you go into the PTn maybe 500 feet. You walk through the PTn and you're done and that's your only chance and where did you happen to walk through the PTn? You walked through it where above you is probably 30 meters of alluvium. So, you picked a spot to go through one of the most important barriers at a location where it's the least effective as a barrier because you have something else that's affecting you. It's the consequence of having one shot at going through the PTn and you're controlled by an engineering design that says you have to start here to get through this location. And, I'm sure hundreds of people/thousands of people will go in and look at the PTn at this 500 feet.

If you go over to the other side of Solitario Canyon, there's two kilometers, but I'm not sure how many people here have ever gone over to the other side of Solitario Canyon to look at the PTn. Many of us in the program--not many of us, a few of us. A few of us have taken advantage of that. There's a paper we're putting out that deals with this 2 kilometers of PTn. I think we have a lot of benefits from looking at the surface of Yucca Mountain, seeing the Calico Hills exposure, the Topopah, the Tiva, the PTn. We need to use that and that gives us a lot of insight.

The one advantage we have underground is that you can go down and imagine a borehole going through that and what you would interpret from that one location. And, really, I think the greatest advantage is what you learn by just visualizing standing in there and seeing a nice clean surface to say, boy, if I'd have been over here, I'd have had a real different concept. And, that gives us a lot of advantages and we can pick the points and what we think about the site. And, it tells us a lot about the history of he site. And, we have to be careful about having one ESF or having one borehole. I mean, I looked at the data that Gregg presented and I don't know if all of you noticed, but he had a new theory for every data point and how it all worked together. And, I want him to go drill one more hole and see if he gets exactly the same theory at every one of those locations.

We're not going to get everything out of the ESF. We're not going to get everything out of surface based testing. We have to do both. And, sure, we have different views on what's going to work or not work, but some areas--I mean, the PTn where we got to it is under a zone that probably won't see anything. And, the Tiva Canyon, we missed because we had 100 foot of rock and by the time we got out to where it was shallow, we had a whole bunch of alluvium. So, it's different purposes, different objectives, but it's kind of nice to go under there and stand and look at things. I think it's worthwhile in that aspect. And, the reality is it doesn't matter what our debate is anymore about it. It's there and, if it doesn't keep going, we don't keep going. We have an obligation to do the best we can and take advantage of the fact that it's there and go do something and get some information. As far as money or time, Pat, I've been here eight and a half years now. I've had enough time.

DR. LANGMUIR: Thanks, Alan.

With that, I'm going to close this for the day and thank all speakers and all involved for putting together a first rate program.

See you tomorrow at 8:00 o'clock.

(Whereupon, at 6:15 p.m., the meeting was recessed, to reconvene at 8:00 a.m. on Tuesday, June 27, 1995.)