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NUCLEAR WASTE TECHNICAL REVIEW BOARD

JOINT MEETING

PANEL ON HYDROGEOLOGY & GEOCHEMISTRY
AND
PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

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

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman
Nuclear Waste Technical Review Board

Dr. Donald Langmuir, Chair
Hydrogeology & Geochemistry Panel

Dr. Patrick Domenico, Co-Chair
Hydrogeology & Geochemistry Panel

Dr. Clarence R. Allen, Chair
Structural Geology & Geoengineering Panel

ALSO PRESENT

Dr. William D. Barnard, Executive Director
Nuclear Waste Technical Review Board

Dr. Edward J. Cording, Consultant

Dr. Bridget Scanlon, Consultant

Dr. Tim Jones, Consultant

Dr. Roy Williams, Consultant

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P R O C E E D I N G S

DR. DEERE: Good morning, ladies and gentlemen. I would now like to turn the meeting over to Dr. Langmuir and he will turn it back over to DOE.

DR. LANGMUIR: Good morning, I'm Don Langmuir, Co-Chair of the Panel on Hydrogeology & Geochemistry and I'll be chairing today's session.

Those of you here at yesterday's meeting know that we ran out of time at the end of the day and were unable to schedule Tom Buscheck. So, Tom is going to be our first speaker this morning. Now, you realize we have a full schedule. So, with Tom's talk at the front end, we're looking at a 40 minute addition to the day as planned. After Tom's presentation, Dave Dobson wants to make some comments for a few minutes, as well.

I must ask that today's speakers stick within their schedule and I'm going to be kind of hard-nosed about it so that we can finish on time. We may have to forego coffee breaks and just go back and get coffee as we'd like to have it.

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

DR. BUSCHECK: Since I didn't have a slide made for me by the project office, I'll take liberty of giving credit to my co-author, John Nitao, because I think that's appropriate.

He and I are from Lawrence Livermore National Laboratory.

What I want to talk about today, this is the organization of the talk. I want to talk about the role that nonequilibrium fracture-matrix flow plays in site characterization. I'm going to start with the motivation and the scope of this talk. I'll move on to fracture-matrix interaction and the mathematical approximations which have been used to represent it. Then, I'll talk about the distinction of fracture versus matrix-dominated flow. And then, we'll move on and for the rest of the talk, we'll talk about fracture-dominated flow, about the major flow regimes that arise from it. We'll talk about its episodic nature. We'll also talk about examples of episodic nonequilibrium fracture flow in Yucca Mountain and summarize some of our conclusions of that. Then, we'll talk about the effect that fracture-matrix flow has on physically retarding radionuclides. And then, if I have time, we'll talk about the impact of repository-generated hydrothermal flow on the system.

First of all, if we have pure fracture flow along preferential pathways, there are three general classes of mechanisms which will mitigate against these causing a breakthrough to the water table. The first and most obvious mechanism would be a discontinuity in fracture networks. We also can have dispersion of liquid flow within the fracture networks which could tend to work against preferential pathways being a problem. And then, the third classification is fracture-matrix interaction. We at Livermore have dealt with all three of these, but for the sake of this talk, I want to emphasize the impact that matrix-flow has on fracture flow. And, so the rest of this talk will be dealing with this particular process.

In general, fracture-matrix interaction impacts flow. Capillary imbibition occurs from the fracture to the matrix which retards the movement of flow within the fracture. This has the effect of limiting the vertical extent of the penetration of fracture flow in a fracture network. It also delays the impact of fracture flow in terms of performance assessment and radionuclide transport. And, by delaying the impact of flow, then other mechanisms may occur; such as vapor phase removal of moisture from matrix to fracture. And, as I stated, due to the fact that there is very small matrix permeabilities, the long matrix liquid travel times facilitates this as being a possible important mechanism.

For transport, fracture-matrix interaction also limits the transport of radionuclides. Also, as you'll see, it mitigates the vertical displacement of a radionuclide front which was imbibed during an earlier episodic event by events which followed that event. And, we'll also see that fracture-matrix interaction facilitates the effect of chemical retardation by bringing flow that wasn't a fracture out into the matrix where it can interact with the minerals in the matrix.

At Yucca Mountain, we generally have a system that is not very far from being gravity-capillary equilibrium. Values of flux varying from .01mm per year to .5 don't deviate very much from zero in my opinion and, in effect, what we have is a capillary fringe existing from the water table all the way essentially to the ground surface. And, for the fractures which are going to be a problem potentially in moving radionuclides, those fractures or those channels of those fractures will be essentially drained of water under these conditions.

If we look at the saturation distribution--well, first of all, you have the saturation distribution as is available from the rib. We get these mean values in these units and these error bars. This is what's currently available. I used data from Klavetter and Peters which is very frequently used in performance assessment calculations. Klavetter and Peters have used a somewhat generalized hydro-stratigraphy of the mountain which averages some of the properties over several of the boreholes. In some cases, it simplifies what would be otherwise a more complex system. However, the key features of the hydrostratigraphic system, I think, are captured in their data.

What we see here is this is a case where we've assumed zero flux and, in this case, we're looking at steady state, one dimensional recharge through the mountain. This corresponds to gravity-capillary equilibrium. These other two cases correspond to .045mm and .13mm per year. The key thing to observe here is that even as we increase the flux, we can come nowhere close to the observed saturation values in the vitric nonwelded Paintbrush tuff or the bedded tuffs nor can we come close to the wholly observation point currently available for the nonwelded vitric Calico Hills.

What distinguishes these units from the other units is primarily their permeability. Basically, the welded units, the TCw, the TSw, are very low permeability. The CHnz, the zeolitized Calico Hills, is a very similar permeability. These units, due to their low permeability, do not accommodate very much steady state one-dimensional flux. And, when we increase the flux above a zero of gravity-capillary condition, we rapidly fill the porosity in order to reach a hydraulic conductivity which can accommodate this system. However, due to the very high permeability of these two nonwelded and vitric units, they do not saturate anywhere close to the levels which we see in the RIB. So, there must be, we feel, something other than matrix-dominated flow accounting for these saturation values.

This is just to show a conceptualization of that mountain using the same hydrostratigraphic column and this is actually using the same data using blue scale, this showing increasing saturation. And, what we show is that under the assumed nominal fluxes, we have very dry conditions within these two units. If we were to impose the actual rib saturations, we find that these units are substantially wetter than you would predict from that matrix-dominated flow assumption.

Okay. The other evidence for nonequilibrium fracture-matrix flow are the well-known ^{36}Cl data which has been measured to up to 500 feet in depth at Yucca Mountain. Also, in G-Tunnel, we observed out in the so-called condensate zone beyond the boiling zone, we did not observe any significant increase in saturation. Therefore, the fractures were rapidly accommodating the generation of condensate. So, we did not see very much imbibition within that region and, in fact, the equivalent continuum model used could not predict that. And then, in UZ-7, there is data which strongly indicates it's not equilibrium fracture-matrix flow and I don't want to dwell on this data, but I just want to show that within the welded Tiva Canyon, we have very low saturation values. We go to the nonwelded unit, we see increased saturations, but what's more important is the fact that the capillary tension in the underlying nonwelded tuffs is significantly lower than the overlying welded tuffs which indicate a strong disequilibrium between these units. This unit, apparently, is wetting up to a great extent without very much wetting of this overlying unit. And, our hypothesis is that very rapid pulses of fracture flow through this unit with minimal time to interact with it are sitting and being imbibed into this unit.

Now, I'll talk about the mathematical approximations. In the project to date, the reference calculations which have estimated the nominal fluxes through the mountain have used the zeroth order approximation developed by Klavetter and Peters and others at LBL and that assumes gravity-capillary equilibrium between the fracture and matrix. And, in doing so, we take a system which, if it were in disequilibrium, flow in the fracture would move in advance of flow in the matrix because of it being out of equilibrium. Instead, the assumption is made that this disequilibrium cannot occur and that we smear the effects of this fracture flow across the entire fracture-matrix medium. And, so that's what I call the zeroth order approximation. In the oil industry, they use a first order approximation which assumes a quasi-steady state relationship between potential in the fracture and potential in the matrix. We found that this approximation was not adequate. We used what we refer to as a second order approximation where we discretely account for flow in the fracture and the matrix. Details can be found in a water resources report, Nitao and Buscheck. It will probably appear in August. Again, as I stated, this is what I'm referring to as nonequilibrium flow, when flow in the matrix cannot keep up with flow in the fracture.

Now, what determines whether the fracture of the matrix dominates flow is the relative conductivity between the matrix and the fracture. There's basically a competition between flow in the matrix and the fracture. If the matrix hydraulic conductivity is large relative to that in the fracture, flow which enters the fracture will be quickly imbibed and the fracture front with either move along with the matrix front or may actually lag behind it. If the flux is sufficiently large, the matrix flow cannot keep up with this flow and the fracture will move ahead of the flow in the matrix. We've done extensive analytical solutions determining which case is prevalent and an important relationship for ponded conditions is the hydraulic aperture of the fracture. The critical aperture is given by this relationship here. For Topopah Springs Tuff, we find the critical aperture to be 10 microns. Anything above 10 microns would tend to generate fracture-dominated flow.

DR. DOMENICO: What are the symbols? What do the symbols mean in that?

DR. BUSCHECK: The symbols, fracture aperture. This is a critical fracture aperture. This is the saturation and this is the initial saturation. This is the matrix sorptivity. This is the matrix porosity. Is that clear? Okay.

Moving on to the flow regimes that we've observed in fracture-dominated flow. Primarily, there are three primary flow regimes which exist. The first flow regime is that which occurs very early in time or in cases where the rock matrix is so impermeable that there's minimal interaction between the fracture and the matrix. At this point in time because there's minimal interaction, flow in the fracture--and what we're plotting here is a log of fracture penetration; actually, dimensionless penetration. It does not time the details of that, but we're plotting the log of penetration down the fracture versus the log of time. And, for unit gravity gradient, we find that that penetration moves linearly in time when there's minimal matrix interaction.

After there's been approximately one fracture porosity imbibed into the matrix, the effects of matrix imbibitions start to take over. What we find is this Flow Region I continues to propagate in advance of Flow Region II. However, Flow Region II is what's controlling the velocity of this front and this front now moves as $t^{1/2}$ power. Due to the fact that matrix imbibition is going $t^{1/2}$ power, there's a net flow available for fracture flow which is also a $t^{1/2}$ power relationship.

Now, this shows a no-flow symmetry line between its neighboring fracture. When that region has completely filled, we approach Flow Period III where we again reach a linear and time dependence on the flow. This is what is assumed to be an equivalent continuum model and also the equivalent continuum model applies to matrix-dominated flow. To put in perspective what these flow regimes pertain to, Flow Period I essentially pertains to pure fracture flow where there is no retardation by virtue of matrix imbibition. Flow Period II pertains to situations where the matrix is actively in body water. And, Flow Period III is a situation where the matrix is fully imbibed between fractures. What we get then is just a linear displacement in time which is given by the log of the total initially unsaturated porosity in the fracture and in the matrix and b is the distance between wetting fractures divided by the initial porosity of the fracture or the available flow area of the fracture. So, basically, we're just partitioning flow from this volume to this volume and getting this shift in time. Again, what the equivalent continuum model automatically assumes is maximal fracture flow retardation by virtue of matrix imbibition. It's the most liberal assumption you could make if you're trying to predict fracture movement. And, for performance assessment, it's not going to be adequate.

DR. WILLIAMS: Tom, could you just quickly explain how you're handling transmissivity?

DR. BUSCHECK: Transmissivity?

DR. WILLIAMS: What are your assumptions regarding transmissivity?

DR. BUSCHECK: We're handling--

DR. WILLIAMS: Infinite?

DR. BUSCHECK: No, we're--

DR. WILLIAMS: What are your assumptions?

DR. BUSCHECK: There are no assumptions. We have used a finite difference model. We've discretized the properties in the fracture. We handle it as a porous media with equivalent properties using like the Havercamp sand. The matrix, we use the properties from whatever unit we're evaluating and we're just doing a 2-D--in this case, these are all 2-D flow problems where there's 1-D imbibition or actually there's 2-D imbibition of the matrix where we find it's predominately one-dimensional. But, the model is just a two-dimensional unsaturated--

DR. WILLIAMS: That's my point. I think you're assuming that the fracture is continuous throughout the mathematical domain that you are treating which is a fairly important assumption.

DR. BUSCHECK: Okay, right. Yes. So, we've done calculations where we've looked at finite fracture wetted area. You know, we consider that part of the dispersion problem. In this case, we're just looking purely at the impact of the matrix. And, we're assuming two dimensional flow.

So, now, we're going to talk about episodic behavior of fracture flow. We started with some examples where we maintain ponded conditions at the repository horizon and used the Klavetter-Peters characterization through the whole mountain. And, with a ponded condition at the repository horizon, we find that after only two hours we get about 30 meters of penetration along this 100 micron fracture. The boundary conditions here, as Dale stated yesterday, this is a symmetrical problem. We have a plane of symmetry--actually, the plane of symmetry in this example would be at .15 meters since we have a 3 meter spacing between fractures. And, so this is a periodic system with no-flow boundaries between neighboring fractures and a no-flow boundary down the center of the fracture. What we find is that flow into the matrix is primarily in--with the flow in the fracture. And this is plotting the dimensionless change in saturation. This would be a 10% increase between the initial saturation and full saturation, whatever that happens to be.

Now, if we were to look at what is happening--okay, for a moment, I'll just put on what the equivalent continuum model would predict. The equivalent continuum model, as defined by Klavetter and Peters, would take that flow and imbibe it across the entire matrix porosity and, in this case, it would be imbibed out to .15 meters, not .05 meters. And, you can see that instead of 30 meters of displacement, we get about .6 meters after two hours.

Now, we're going to be looking at saturation conditions in the fracture. What we're plotting here is liquid fracture saturation from zero to 100% and we're again starting from the repository where the ponded condition is maintained. Zero hours now pertains to the time at the end of the two hour pulse and these are all times subsequent to the removal of a pulse. We find that, due to the imbibition into the matrix, water is very quickly imbibed so that within two hours about three-quarters of the water in the fracture has been imbibed in the matrix. Within two days, the fracture has been completely drained of water. An important observation is that the movement of the toe of the fracture front has been minimal subsequent to the removal of the ponded source. This would also occur if we had a fixed infiltration source, flux source.

To look at what's happening in the matrix, what we're now plotting is at a location 10 meters below the source. We're plotting the matrix saturation. At zero hours, that pertains to the end of this two hour pulse, we find a wetted zone in the matrix which penetrates about a centimeter or two into the matrix. Then, approximately, one day after that pulse is removed, that pulse is relaxed to about half of its 100% value. Within one month, it's almost totally relaxed back to background conditions.

So, what we've found is that--this is, again, for the welded tuffs. For the welded tuffs, our observations are that if we were to follow this two hour pulse within a day or two with a subsequent pulse, that subsequent pulse would occur as though there had been no hiatus in time between them. However, if that subsequent pulse followed, say, on the order of month later, that following pulse would occur as though the earlier pulse had not ever occurred. So, there's a limited memory in the system in the welded units. In the nonwelded units, we've found that that memory could persist up to perhaps 10 years between events.

Now, I'll show some examples of fracture matrix flow. Again, starting at the repository horizon for 100 micron fracture, we find that it only takes about four and a half hours for this front to reach the nonwelded vitric unit. This unit has a permeability four units of magnitude higher than this unit or this unit. And, we'll see its impact in a second.

At eight hours, we can see that subsequent vertical movement is stopped and the flow is now being imbibed into the matrix. And, after about 20 days--in this case we have fractures that are 3 meters apart. It has taken about 20 days, but now these two fractures that are neighboring each other or the series of fractures are now interfering. From eight hours to two days, we had matrix-dominated flow. There was no subsequent movement in the fracture. Now that we have interference occurring, now Flow Period III is facilitated. And, in that case, the velocity of the front can again--the front can again propagate, and after 83 days, we have fully filled up the porosity between these wetting fractures and find that in the 84th day, flow can now persist into the underlying zeolitized unit, and that in 87 days, this front has broken to the water table. We see that the travel time from the repository to the water table has been very heavily dominated by this high permeability unit. The low permeability units offered relatively small retardation effects to the propagation of that front.

Now, we ask the question what if these fractures were very sparsely distributed, the wetting fractures, what would have happened? What we find is that due to the fact that imbibition declines the flux, the instantaneous flux, declines as $t^{-1/2}$ power, that eventually even though the matrix had been dominating flow such that there was no net flow available for vertical movement in the fracture, as that flow in the matrix declines it's $t^{-1/2}$ power, the flow in the fracture begins to overtake the flow in the matrix even for this infinitely spaced case. And, we find that after 241 days flow begins now to enter the zeolitized unit. Notice that it's taken quite a bit more time to penetrate this unit when the fractures are infinitely spaced apart. At 290 days, this particular event has reached the water table.

Now, to show the relative impact of this highly attenuating nonwelded vitric Calico Hills unit, we've removed it from the calculation and have found that instead of the

290 days, it takes 52 hours to reach the water table. So, clearly, you can see a high permeability unit can be very beneficial in retarding fracture flow.

DR. DOMENICO: One question. That's true, but how about the degree of saturation of that high permeability unit because these are highly--they're .9, .8%. That certainly has to have some effect.

DR. BUSCHECK: Yes, that does. Certainly, it does, and especially if you have finite spacing. What we've done --and, I didn't prepare this in your package, but we look at the travel time that it takes to penetrate a given unit. Again, we have the matrix porosity. In this case, this is the fully saturated saturation which could be less than 100% if there's air entrapment. This is initial saturation. This is aperture. This is the capillary sorptivity of the matrix. We found that in Flow Period I that this goes as linearly with the initially unsaturated porosity. In Flow Period II, it goes as a square of the initially unsaturated porosity. What's interesting to note is that this dependence goes as the 6th power of the aperture and only linearly with the sorptivity of the matrix and the point that I'm making is we perhaps should not become overly concerned with small discrepancies in trying to characterize matrix imbibition because this is what's going to dominate the time.

Now, to show an example of fracture-dominated flow, I look to the example of 1,000 micron fracture. Instead of taking 290 days to penetrate through the mountain, we find for a 1,000 micron fracture, it takes 350 seconds. You can see that b^6 power dependence on getting through is quite important.

Now, we look at, well, what if the vitric nonwelded Calico Hills is present? We find for Flow Period I, it doesn't make a heck of a lot of difference because the matrix in this case is such a minimal interaction with the fracture that it still--you know, it's negligibly different between these two cases.

We've done an analysis of how close do fractures have to be in order to be interfering. We've done calculations through the whole mountain, but for this plot I only have those units below the Paintbrush. What we're plotting here is the log of the penetration of the matrix wetting zone away from the fracture versus a log of aperture. And then, I should show the relationship which controls this. This wetting front movement for Flow Period I goes as this kind of relationship and for Flow Period II goes as this type of relationship. This was determined analytically by Nitao and Buscheck and then we did numerical experiments and found that indeed these relationships hold. That for Flow

Period I, which would pertain to large aperture fractures--this being 1,000 microns--these curves go to a b^{-1} slope. And, for out here where we have Flow Period II, the slope goes to the -3 which correlates just according to our theory. What you get from this is that in the welded units which are these lower units, that for--for instance, a 100 micron fracture--we get penetrations on the order of centimeters or even millimeters into the matrix. So, wetting fractures would have to be that close to be interfering. However, in the nonwelded vitric unit, we get penetrations on the order of tens to hundreds of meters into the matrix which can show how much the flow is being attenuated in those units.

I want to go above the repository and show examples of flow starting--actually, it shouldn't be depth flow at ground surface, it should be depth below alluvium. We find for a 100 micron fracture that it only takes two and a half hours to penetrate through the Tiva Canyon and starting to enter the Paintbrush nonwelded tuff. After 10 years, now flow as it enters the Paintbrush, there is no--if you can see here, the fracture front is right at the interface between these two units. All the vertical flow in this unit has been fully within the matrix. For 10 years, the matrix has been dominating flow. Now, we're assuming gravity-capillary equilibrium. This is not current saturation values. What we're trying to do is understand how the currently existing saturation values evolve. So, we're trying to go back in time and see what, in fact, could have given that saturation distribution.

So, after 10 years, we're still completely dominated by the matrix in this unit. After 40 years, the wetting zone has fully penetrated this 38 meter unit all the way to the base, but there's still no fracture flow occurring. And then, after 50 years, we find that instead of matrix imbibition only causing lateral flow that we're starting to effectively pond water at the base of the nonwelded unit. Still, there's no fracture flow below it. And, now, due to this ponding effect, we're getting gravity-flow occurring. And, so actually we're getting an additional attenuating effect in this unit by virtue of gravity. So, flux into this unit is no longer declining as $t^{-1/2}$ power due to this additional component of gravity-flow into it. And, what we've projected is that it would take 100 years to penetrate this unit with a 100 micron fracture.

Now, if that ^{36}Cl data at Yucca Mountain is actually relevant, it would occur at about this depth here (indicating). So, what type of a fracture aperture could have arisen to that type of a modern bomb pulse measurement? We looked at 1,000 micron fracture, found that it takes 30 seconds to penetrate the Tiva Canyon, and after about 2200 seconds, the matrix is still dominating flow, but merely penetrated the Paintbrush and the matrix. After 2400 seconds, the flow is now beginning to enter the underlying Topopah Springs. Within one hour, this flow will have broken through to the repository and onto the water table.

So, to summarize these examples that I've just shown you, I'll just use these blocked diagrams here, again using that simplified hydrostratigraphy of Klavetter and Peters and just comparing these examples.

We found for the 100 micron fracture that it takes on the order of 100 years to penetrate through this unit. And that below the repository, it is very important whether this vitric nonwelded unit is present. At the present moment, it is thought that this unit is not aerially extensive under the repository block. So, one could say that there's very heavy dependence on whether this attenuating unit is present. A very important thing to observe is that the travel time that it takes to reach the repository is about 100 to 10^4 times longer to reach the repository than it is to get from the repository down to the water table. So, the travel time is heavily dominated by this Paintbrush unit. And, similarly, even for a 1000 micron fracture, the travel time is heavily dominated by the overlying unit. Though because we're flow-free in one, the effect is not quite as dramatic.

How am I doing on time?

DR. LANGMUIR: Beautifully.

DR. DOMENICO: Shouldn't it also be dominated below the water table by the Calico Hills the same way?

DR. BUSCHECK: Yes, it is, but the Calico Hills according to Klavetter and Peters is only 4.6 meters thick and it's not aerially extensive. So, where it's not present, it has no impact at all. So, it's very important to get that distribution of that unit. But, given the currently available data, the mountain's capacity to attenuate flow vis-a-vis matrix imbibition exists primarily in the Paintbrush.

DR. DOMENICO: Also, I don't want to interrupt you. I know you're running here. But, would you expect the highest degree of saturation today to occur in those rocks that have the highest permeability?

DR. BUSCHECK: Yes. Relatively--not the highest saturation, but relative to gravity-capillary equilibrium, you would expect those rocks. If nonequilibrium fracture-matrix flow has reached those units. we will see a saturation value that is above what you would predict from gravity-capillary equilibrium. That is a strong indicator of fracture flow to those depths.

DR. DOMENICO: Okay.

DR. BUSCHECK: Now, in this simplification of the mountain, I have just chosen to use the same type of boundary conditions. I didn't have tilted beds. In reality, these beds are perhaps tilting at 7 degrees. I'm just trying to show what I consider some of the most important features of the hydrologic flow system at Yucca Mountain. And, this corroborates a lot of the earlier observations by people like Alan Flint and Montazer & Wilson. So, this isn't entirely new. It's just that we have now a quantitative basis for these observations and quantitative in terms of large scale calculations.

What we feel is that it's probably likely that flow is facilitated by some sort of ponding which makes the washes a little bit below the ground surface or above. But, we feel the ponding conditions are going to persist for some very limited period of time during storm events. This flow will probably get quickly through the welded units due to its very low permeability and that a large amount of lateral attenuation will occur within this unit. We found that because of the high matrix permeability you do not have to have continuous fractures here. That you can facilitate high fluxes even though fractures may be discontiguous. And, that we also feel that there's a strong possibility that ponding conditions could be generated by this flow which then could subsequently generate flow, perhaps episodic, perhaps continuous. If we're in a pluvial condition, we could perhaps continue these ponding conditions that maybe persist and we should look for them at the base of the Paintbrush which could then generate flow to the repository horizon and also within the vitric nonwelded Paintbrush, perhaps some similar conditions may exist where ponded conditions may generate subsequent flow below that unit.

In the case of moisture movement through the mountain, I'm trying to show here that the fracture density is going to be very much different for vapor movement than it is for liquid movement. For liquid movement, in order for part of the fracture system to be important, it has to be vertically connected to the overlying ponded source. For vapor flow, we have all sorts of pathways which could facilitate flow from the matrix to the fracture and so the effect of fracture density and the effect of fracture conductivity is much higher for vapor removal of a system if it is occurring than it is for the liquid system.

Okay. I want to talk about retardation of radionuclides and just show conceptually what I mean by physical retardation. And, I also want to distinguish the differences between the equivalent continuum model and the discrete fracture-matrix model. If we were to impose three successive events on each other on these two models, we would get this general type of situation. In the equivalent continuum model, we would predict that Event 1, the radionuclides that were moving in Event 1 would be displaced by Event 2 which, in turn, would be displaced by Event 3. However, in the fracture-matrix model, if these events were significantly separated in time, which for the welded units could be on the order of a week, and if they were the same duration, we would find that radionuclides in Event 1 would move to some finite distance if it was a limited episode and would be imbibed in the rock. Event 2, that it would move a similar distance down the fracture and would laterally displace Event 1 into the matrix and onward. And, what we found is that because the sorptivity of the capillary wetting diffusivity of the matrix is at least as great as the molecular diffusivity for molecular diffusion of radionuclides back into the fracture that advection by capillary imbibition will tend to dominate molecular diffusion of radionuclides back into that fracture pulse. So, that's very important considering radionuclide movement.

We'll talk about hydrothermal flow. As Dale showed yesterday, we observed at G-Tunnel the fact that in the fracture system vapor flows away from the heat source, and even if this is a random system of fractures in general, that flow will be spherically or radially away from this heat source. However, when this water condenses, it will tend to drain vertically downward in the system. It's fairly obvious to see that this will eventually prorogate water off the sides and we saw a negligible increase in saturation here and here (indicating) and we also saw evidence at G-Tunnel where the temperatures out here were pegged at two phase conditions for a very long period of time indicative of persistent condensate drainage. And, what this does at the repository is to create effectively what I call a hydrothermal umbrella for at least perhaps 300 to 1,000 years around the waste package.

Now, in characterizing the mountain, we have to consider all sources of liquid water, not just rain water. Going to our long-term calculations of the repository, John Nitao did calculations for 10, 20, up to 80 year old fuel. Then, I went back and analyzed this data and looked at the dry out volumes through the first derivative and that's the rate at which condensate is being generated within the mountain and plotted that versus time and found that 35 years, which happens to coincide with the peak temperature of the repository. That averaged over the whole repository, we reached a net average infiltration rate of 30mm per year.

So, this is a significant source of water which should be considered when we characterize the rocks, not just the meteoric sources of water.

And, I guess Dale showed how we came up with the idea of using the analogy of the Paintbrush nonwelded tuff and attenuating fracture flow and have applied that analogy to an EBS concept that we're considering right now of using that similar material to attenuate fracture flow which may be propagating through this backfill if it were just welded tuff and possibly reaching the waste package.

I can still have time for my conclusions?

DR. LANGMUIR: You're going to make it just fine. You can breathe.

DR. BUSCHECK: Okay. I'm just going to review things that I've already stated in my conclusions, but basically the importance of fracture-dominated flow is that due to the very small matrix permeability of the mountain, many people have observed this, our feeling is that matrix-dominated flow just doesn't constitute a problem. So, we should be emphasizing what constitutes a problem, the potential for fracture-dominated flow. And, field evidence also indicates that we should be addressing that problem.

And, as others have stated, we've quantitatively shown how this is the case. That the matrix dominates flow when the flux is sufficiently small so that matrix flow can keep up with fracture flow. And, conversely, if that flux is sufficiently large, flow of the fracture will move in advance of flow in the matrix.

As for episodic behavior, due to matrix imbibition, very little additional liquid front movement occurs in the fracture following the removal of an infiltration source whether it's ponded or fixed flux. For episodic events separated by a few days, the cumulative movement within a welded unit or perhaps the zeolitized Calico Hills is nearly the same had all those events occurred consecutively. However, within the high permeability nonwelded vitric units, those events could be separated by several years or perhaps 10 years without affecting the cumulative liquid movement.

And, a key consideration affecting radionuclide movement, we've found, is the intensity and duration of a maximum possible infiltration episode. For the Tiva Canyon, that would be the episodes of being rainfall and for those units underlying the Paintbrush, if we, in fact, get ponded conditions and the Paintbrush, that can be the duration of those ponded conditions within the Paintbrush itself. And, I guess you probably recall. So, in other words, an episodic event below the Paintbrush may, in fact, be how long this condition remains ponded and that will be controlling the duration of that particular episode in that part of the mountain.

Summary of fracture-matrix flow in Yucca Mountain. Due to the small matrix permeability and the welded units and the zeolitized Calico Hills, fracture-dominated flow, I say, is likely--I should say is likely if conditions permit it, but is more likely to occur certainly within these units and that due to the large matrix permeability of the nonwelded vitric units, that matrix-dominated flow is likely in these units. However, conditions may exist to allow the fracture to dominate flow, as I showed in the 1,000 micron fracture case. The high permeability of the vitric nonwelded units may result in substantial lateral flow. This lateral flow, if it intersects a through-going fault could be a limiting or critical problem for performance assessment. And, that the contiguous fracture networks at Yucca Mountain may facilitate vapor phase removal of moisture in Yucca Mountain. And, looking at the effect of the Paintbrush, the fact is that we feel that there's a very large effect in net flux which has to be invoked to explain the saturation condition. So, if this is remaining at some sort of a steady state saturation condition, we have to have a balance of flow.

And, there are three mechanisms which would balance flow to Paintbrush: either direct discharge through faults of the water table; perhaps flow within the fractures which is then imbibed in the welded unit which, in turn, may be carried away by vapor movement to the mountain; or direct lateral discharge either to an outcropping or to the water table. So, there's three ways which, if the Paintbrush is at a constant saturation, it may be at that saturation.

Two of the most important conclusions that I would like to make is that because 99 to 99.99% of Yucca Mountain's capacity to retard fracture flow because that exists--this is vis-a-vis matrix imbibition--exists above the repository horizon. Now, this percentage may change with site characterization, but that's based on the currently available data. Because of this fact, I feel that planning and prioritization of site characterization activities should emphasize units which dominate fracture flow retardation. It's an important thing to consider.

For physical retardation of radionuclides, as I stated, shortly following an episodic event, liquid in the fracture will be totally imbibed by the matrix. Matrix imbibition mitigates vertical displacement of radionuclides imbibed during earlier events by the subsequent events. And, if a radionuclide front is not driven to the water table during the course of an infiltration episode, then its subsequent vertical movement will be largely governed by matrix-dominated flow.

And, as for hydrothermal flow, we feel that this constitutes a very significant or at least the first several hundred years, this constitutes a very significant source of liquid water in the mountain for fracture movement, fracture-flow movement.

Do I have time for questions?

DR. LANGMUIR: Tom, thanks very much. You're right on 40 minutes, but you've done such a great job, let's have some questions, if we may, for three minutes or so. Any questions from the board?

DR. CORDING: Yes. The one question just quickly on this 99% of the capacity retarded fracture flow exists above repository horizon. That's based on a model which assumes the same joint basically going all the way through.

DR. BUSCHECK: Right. Here, I'm saying this is vis-a-vis matrix imbibition. So, as I stated at the beginning of the talk, there are three processes which retard fracture flow. For this talk, I'm emphasizing the matrix interaction.

DR. CORDING: Yeah. So, given different joint patterns at different depths, that percentage could change?

DR. BUSCHECK: Certainly. But, I think it's important for whatever reasons to emphasize this type of flow because I feel that looking at these worst case contiguous fractures, we know what type of saturation conditions would give us a signature 4 fracture flow to depth.

Any other questions?

DR. WILLIAMS: I think it's just the importance of a related comment to repeat that, the assumption about infinitely transmissive--you have no transmissivity term in any of the equations.

DR. BUSCHECK: Yes, you do. We account--as I showed in the balance between fracture and matrix-flow, we consider the finite hydraulic kind of activity of the fracture. It's not infinite, it's finite.

DR. WILLIAMS: No, the point is transmissivity, not permeability.

DR. BUSCHECK: Infinite, when you say the two dimensional problem. Okay. I'll bring up a point. The fact is that a fracture flow will not occur as infinite sheets in the mountain. It will occur, what we call, driblets or worm hole type of phenomena, we think. That fracture-flow will occur over very finite two dimensional regions in the fracture which will tend to propagate radial imbibition to the mountain. We have analyzed that case, but there's not enough time today to look at that. When you get radial imbibition, as in testing a well, we find that in well testing you go to a steady state solution for radial flow. For imbibition, you go to a steady state imbibition. So, in fact, we develop a finite penetration length in the fracture where, as you approach an asymptotic imbibition flux out in this radial flow field, all the flow coming to the fracture is accommodated by the matrix. So, there's a finite penetration that occurs. So, certainly, there are far more conservative than looking at radial imbibition into the matrix.

So, the difference is not infinite versus non-infinite. The difference is linear imbibition which can decline as to $t^{-1/2}$ power versus radial imbibition which reaches at a constant in time and which will tend to retard fracture flow a lot more so than a declining flux.

DR. WILLIAMS: Well, I think the best thing to say as a result of this is this is a worst case scenario.

DR. BUSCHECK: That's one way of looking at it, yes.

DR. DOMENICO: You must have thought of this question. Given the current state of saturation of all the units, what we know about the hydraulic conductivity distribution, you certainly must have thought about how long it would--if an infiltration event took place today, how long would it take to reach the water table?

MR. BUSCHECK: Well, as you can see, it really depends on what the limiting fracture aperture is for that particular pathway.

DR. DOMENICO: You have no information on fracture apertures in Yucca--

MR. BUSCHECK: Well, I didn't state this very well. When we look at, say, the air permeability gas injection data, we cannot use that fracture permeability data in our liquid flow models because, in fact, these driblets of flow in the fracture may be occupying 1% of the fracture porosity. And so, 99% of the fracture porosity is not available for liquid flow. So, if we were just to blindly apply that bulk permeability data to our models, you would think that we would over-predict vertical penetration. However, if we also apply the porosity values predicted by those injection tests, we would tend to laterally disperse flow over the other 99% of the fracture porosity and perhaps attenuate flow by virtue of dispersion in the fracture system artificially. And, I think we have to consider the finite pathways within the fracture system. Right now, I think the best way to get a handle on that is to go underground and observe dripping fractures.

DR. LANGMUIR: I think we need to go on.

DR. BUSCHECK: Okay.

DR. LANGMUIR: Thanks very much, Tom.

Dave Dobson had a few comments he'd like to make or I think the board would like him to make. Perhaps, that was it.

DR. DOBSON: I would like to respond to something that --I guess. I'm not quite certain what I'm going to say.

DR. DEERE: Perhaps, Ed could ask the question.

DR. CORDING: Well, we were interested in what some of the current thinking was. I know that you're still in the planning stage on much of your current thinking in regard to the studies in the nonsaturated-zone in the currently planned ramps and tunnel boring machine mined tunnels in the facility.

DR. DOBSON: Yeah, okay. Claudia briefly showed yesterday a preliminary drawing of the conceptual north ramp--of north ramp access for the Exploratory Study Facility at Yucca Mountain and I've just put it on the viewgraph machine. What it shows--and, I'll just point out very quickly--is what I guess I would call the preliminary planning for the test program that will be conducted in that ramp. As you'll notice, there's a box over here in the lower left that says "test/alcove location". The general attention is that basically the ramp is going to be excavated with a TBM. It will probably be excavated drill and blast at the porthole for 50 feet or so and then they'll bring in a TBM and they'll start running the TBM down as shown here (indicating). And, what we've done is identified quite a number of study locations where we will be cutting alcoves to support testing programs. In fact, this follows on Tom's last comment very well since it will provide us an opportunity to go underground and see if we can find any water dripping out of fractures or, in other ways, simply observe what the hydrologic conditions are.

I don't have the geology overlay for this map. So, I'm a little uncertain as to precisely where, but the contact between the Paintbrush nonwelded unit and the Tiva Canyon is around this location in the ramp (indicating). Number 2 here (indicating) is an alcove that will be built on the Bowridge Fault, 200 to 300 feet immediately underneath where Trench 14 is on the surface. So, you have the Bowridge Fault here on the ramp. You have the upper contact of the Paintbrush nonwelded tuff a little further down. You'll have the lower contact of the Paintbrush nonwelded tuff. We're planning to put in alcoves in each of those locations. And then, we have a variety of other test locations that will be done to support the hydrochemistry program, in situ seals testing, and so on. Maybe you can't read them, I don't know. There's a variety of kinds of tests of characterization of faults and fractures.

And so, this is where we are right now in terms of a preliminary planning phase. Of course, we map as we go and there's an activity in there that's basically for how to test something that you find that you didn't expect to find. And so, obviously, we haven't located those on the map yet. But, this shows, in general, the overall plan. What we'll have is a ramp with a number of testing alcoves on the side and that's generally where we are.

DR. CORDING: Is there a possibility of drifting out away from some of these series? A lot of the radial borehole tests would take place in this type of--

DR. DOBSON: Yeah, well--yeah, the idea is that we would cut an alcove, whatever. It might be 50 feet, it might be 150 feet. If we wanted to go out and test, say, the Bowridge Fault, we might come out and put another drift back into it and have two or three locations where we cut it. The radial boreholes test, in general, would be drilled from outside of the ramp itself. We'll get them out of the traffic that's in the ramp. So, we'll have the alcove so we can have a place for people to work.

DR. CORDING: Your feeling would be that most of the scientific objectives or the testing objectives could be achieved by going out in those alcoves and drifts out away and there would be relatively little that would be required right at the front of the TBM that's advancing down the ramp. Is that--

DR. DOBSON: I think that's a fair characterization. I mean, if there was something that we identified that was a critical scientific need, then obviously we'd establish some kind of measures to get it. But, I think that the way we're going here it appears that we're collecting virtually all of the information that we think we need and we can support the test program in the way I've described here. I don't think we foresee any major problems with this strategy. It seems that, at least in terms of the testing people--and you might want to ask on our break Hemi Kalia from Los Alamos, who is in the audience who has been coordinating and putting a lot of this together, his thoughts on it.

But, anyhow, we do have for a number of these alcoves, anyway, we have more detailed drawings of what the alcoves will look like. I should emphasize more detailed, not detailed, but in a few cases we have, you know, ideas of the shapes of the alcoves and things like that.

DR. DEERE: Perhaps, he would be able to make a few comments.

DR. DOBSON: Perhaps, Hemi?

DR. DEERE: Yes.

DR. DOBSON: Hemi, did you want to add anything to what

I said? This is the overall configuration that most of you are familiar with.

MR. KALIA: This is Hemi Kalia with Los Alamos. I think that, as Dave indicated, we plan to provide alcoves for most of the tests and the alcoves are as deep as 600 feet deep. So, they're good sized alcoves. The strategy is really to look for any anomalous features during the mapping process and identify those and prioritize those in the testing programs, so that you can make provision for those to be done. And, we're integrating that with the designer to assure ourselves the design can provide the ability to look at the rock face if we have to. So, we're working with the TBM configuration to make sure that we can go up front if we need to. We look for perched water as a priority, mineral properties, any anomalous features that we need to look at.

DR. DEERE: Okay, thank you.

MR. KALIA: Thank you.

DR. LANGMUIR: Let's proceed. Claudia, would you like to introduce the next speaker this morning?

MS. NEWBURY: Sure. Our next speaker will be Al Yang from the USGS, and he'll be discussing his geochemical and isotope methods for determining flow paths.

DR. YANG: My name is Al Yang.

So we talked about using the isotopic techniques;

today, how we characterize Yucca Mountain's. So it's mainly from hydrologic transport, but using hydrochemistry in the UZ-boreholes of the unsaturated zones.

Now, what is the objective? So we try to use in the direction, flux, how they flow, gas phase as well as water phase, then how to get the water out from the rocks, then what is the water-rock interactions. So my talk will be divided into three portions. The first one will be general, what kind of parameter we are going to measure, what the purpose of that parameter measurement, the gas-phase phase, gas sampling, degassing, then what the result is; then the aqueous phase, how we get the water out, then tritium data and the stable isotope data.

Now, the parameter we are going to measure, major anion, cations, is a type of ongoing chemical reaction with the rocks, rare earth elements. We find some of the heavy rare earth elements in some of those trace elements in the pore waters, so that they can be used to identify the source of secondary minerals from the source.

Organics, I'm not sure we have organics in Yucca Mountain, but we just put it in there because we thought it may be. There's an organometallic complex transport. They're using the stable isotope. This is the major talk today. Then, age dating, also, of this. Then the gas diffusion. We have talked of some of the tracers using gas tracers to trace the gas movements. Then, finally, these are the isotopes, trace isotopes we're using during the construction phase If they put the water in, they put the air in, we want to trace all these gasses, make sure all this gas is pumped out, so we get the final, pristine gas samples or water samples.

Now, on the side is: what kind of parameter we are going to measure if you know this abundance of the hydrogen, carbon, and oxygens? This is a percentage; tritium, very rare in this. Hydrogen (indicating), this is a percentage, mostly hydrogen, but the tritium half-life is 12.35 years, so you can use tritium to date up to about 100 years. That's about limitations, so if you get older, you go to Carbon 14. Then they have a half-life of 5,730 years. You can get up to about 40,000 years by dating the waters. So, then Carbon 13 (indicating), Carbon 12 as the ratio to identify the source of it. Oxygen 17 is so rare, so we don't use in this. So we're mostly using an oxygen 18 and a 16 ratio. So these are the isotopes we are going to talk on today.

Now, we shall start going to the gas phase, how we collect a sample. This is the system we use at Yucca Mountain. For UZ-1, this is the peristaltic pump. We have a gas probe going down the hole about 15 stations at UZ-1. So we pump during the daytime, pumping through this route; during the evening, goes through the silica gel. And here (indicating), this tries to trap the water so we can analyze the water isotope ratio, Oxygen 18 deuterium. This is in the evening. Put the moisture in here. CO_2 is absorbing here in the molecular sieves, okay? So then we have a formula control, how fast the gas is flowing and collect the CO_2 gas in here.

Now, how does the molecular sieve collect the CO_2 gas? This is a structure of the molecular sieve. It's a 5 Armstrong molecular sieve, and the CO_2 gas molecules are large enough to be trapped. Other molecules too small, go in and come out. Big molecules cannot get in, and that's how we trap the CO_2. Then we check this method with the potassium hydroxides, trap the, you know, absorb the CO_2, and we confirm the method by both methods. That's the technique we use.

Now, this is one I already talked about, okay, and after we finished it, we collect the sample. We put the cylinder in here, molecular sieve. You heat it up, then the gas in the CO_2, CO_2 trapped in here (indicating), H_2O trapped in here. I think this was reversed. This is supposed to be on this side (indicating), this on this--no, no, no. This is from here. Okay. This is--our sample cylinder here. These are corrections.

So we're degassing from here. Heat it up. Then the water is trapped in here, then the CO_2 trapped in here, and then we separate the two in the lab. So this is a simple method in the lab to separate the gas. Then, after that, we send out for analysis. Now, this is the result.

What we got are the results here. Now, we have done about eight years, since 1984, UZ-1. This is for the Carbon 14. I don't have the time to explain too many things, so just talk about the Carbon 14's.

Okay, these are the results. This is the depth. UZ-1 went to 1200 feet. These are the Carbon 13/12 ratios, and that put the Carbon 14 on this side. Now, this one you don't have in there, but I just, because when I explain this, I need this for comparison between the two. Okay. Now, this is the Carbon 14's. Near the surface, about 40 feet of the probe one, down to about 1200 feet at the depth. So this is at the 1984-85, very early stages. What's happened to this?

You know we put in the gas, in with the air. It penetrates into the formation. Lots of the air--more air is in there, so we have to pump this drilling airs out. But during that time, these, they are in charge of those. They didn't know it. They're pumping for a short time, then stemming it. Then after the stemming, the gas sampling tube is very small, 2 mm. in diameter. We are pumping twice a year. So it took a long, long time to pump it. So you can see at the '84 square, it's this side (indicating). What that means, it's more than -- --. That's reasonable because of the air, more than air has penetrated into the formations. How long does it take to stabilize? It takes about two years. Then, after two years, what the data is?

If you can look at this Carbon 14 data, see how stable those are. After how many years? Almost no changes; '88, '89, '90, '91. So you can trust those data now. So, actually, you can impose--you should superimpose this on top of this, so mostly from 1985 or 6, start all stabilized. The view on that is factored in, so much, due to the contaminated air. So we know now we have some confidence in the future. If they drill the hole with air, we've got to pump it and this, from our -- experience, takes about a month, or even about two weeks to pump all this air out before we actually collect a sample.

Now, besides that, what this curves tell you, well, people look at this curve. Okay, you can see I have a Pah Canyon in here. It's a Topopah Spring below. Between here to here, do you see the curve here? It's slopier, or it's too kind of sloped. So what that means here, this is--from here to here is ages, long ages here to travel a short distance from here to here. In the Topopah Spring, it takes a very short age to travel 600 meters. That gives you the permeability, how fast the gas permeates through this. Does that make sense?

It makes sense, because in here we found from the water chemistry there is a bedded unit between here, and it's very wet, saturated, you know, all the water in there. So they--this may be -- the air, going through from here to here and takes a long time too, and besides, you can divide this distance by the age between here. This is about 70 per cent, about 3,000 years old, divide the distance by the age, you've got the travel times.

Now, besides that, this is very fast movements, and because it's dry on the Topopah Spring, so it makes a lot of sense. There isn't much water. Gas travel fast. Now, besides this point, now, why did this come out at young ages and going down? What's the explanation of it? The only thing I can think of is you've got to have younger ages coming from the top. It could be a fracture between here somewhere from this portion on, connects to this, then fracture flow coming in here, with a young age rapid flow to here, causing this bump in here.

If the gas is coming back from the bottle, it should be--go that way and then coming out, because this is all the -- --, and it pushes it and go that fast. So this could counter our --. I just give you some example.

DR. LANGMUIR: Al? Langmuir.

DR. YANG: Yeah.

DR. LANGMUIR: On that plot, if you put relative ages on it, you suggested that at the base of the Pah Canyon you were looking at 3,000 relevant years?

DR. YANG: Yes, about here?

DR. LANGMUIR: From the Yucca Mountain summit to the--down.

DR. YANG: Yes. From here about 100 per cent. Now, this may be during the nuclear tests, okay? Near the 1954, 1963, they are the nuclear tests in that year. They input out the radiocarbon into the air, so it's a rapid increase in the air, and the photosynthesis by those, so they are--so these are very short times, but I'm talking about from 100 per cent before--pre-nuclear test to here, and this, about 3,000 years.

DR. LANGMUIR: Okay. The next section of it, from that break in the curvature to the base of the plot, what's the time involved there?

DR. YANG: Now, this is about 25 per cent. It's giving about 9,000, 10,000 years. Don Thorstenson say yes, somewhere in there, so then--now, can you trust these Carbon 14 dates? That's another thing. That's why I show you up in here. Certain ratio. Now, yesterday Don Langmuir asked me about when CO_2 scavenges out. Are these carbon dioxides exchanging with the calcites? Now, these calcites raise up the ages.

Now, the reason I can trust it--I think I miss one of the--anyway, this is a--I have a, suppose have a '90--'88 to '99. What I'm trying to show you here is Carbon 13 is pretty constant, as you--Don Thorstenson showed you yesterday. That means the original CO_2 gas is about 20 or 18. Now, if it's--if caliche in there, caliche is only about -5, -4. This should shift to this side if a change occurs, because this is, again, the very early stages. As I said in here, lots of air and all, and this air is light, too. That's why it pushed those this way. So for that reason, why we thought it's more stable. I don't know why I didn't have that. I have that somewhere. So it's more, a lot more stable now for the last four years, just like this. It's all on here.

So based on that, I can trust these Carbon 14 ages. If you don't have that, you cannot say too much about these ages. It may screw you up. That's the importance on this.

DR. LANGMUIR: Langmuir again, Al.

So what you're saying, in effect, is that no reactions have occurred between the CO_2 gas and the--

DR. YANG: Right, yes. With exchange with the caliche or anything, and we saw lots of caliche coding in the G-cores. So this makes life a lot simpler.

Now, the next one, I'd like to show you in the aqueous phase, how we are approaching it to get the water out. That is critical. If we don't have water, no hydrochemistry. So it is very critical to us, and you have to get the water out somehow using the techniques, so can I have those slides?

Okay. So this is mainly from UZ-4 and UZ-5. I missed the first slides, okay? Now, UZ-4 and UZ-5 is along here, so that's where we've got most of the core and yesterday I think Alan Flint talked about it's in the washes, Pagany Washes. UZ-4 is at the bottom of the wash. UZ-5 is at the bank of the wash, about ten feet apart, and the -- -- about the moisture content is something like this. So the bedded units, high moisture contents; bedded units, high content. That's important. That's why we start to understand why bedded units are so important in these areas.

And we've proved that data out with the tritium data, that the rapid flow of the modern water coming to this, and there was a high moisture content. Other than that, it's very low, 10 or 2 per cent in these rocks.

Now, how to get the water. We cut the core inside the glove box. This is about 3.5 inches long, about 2¼ inches diameter core. We cut it. We put the platen on both sides. We wrap around with the teflon sheets. We put this into the membranes. Then we put into the compression cell. This is used by the rock mechanics to test the rock strength, and we take advantage of this facility. We redesign the cell and put this in and try to squeeze the water out. Now, this is a machine, costs about half-million dollars, and you put that cell inside here; pneumatic, all operated by hydraulics, then computer operated and control all this. We step-by-step, we increase our pressures to certain level, stay there for half-hour, then increase the pressures so we're using this method to try to get the water out, and this is how it goes.

Rock is in here. You have a confined pressure from the sides. You have actual pressure from them both. Water drain down from this -- into the syringe, from top and the bottom, and this is actually how it looks. Now, this is before and after. This is before the squeezing, after the squeezing. This is a non-welded tuff, so it's about a 25 per cent shrink in the size.

Now, people are asking me, how can I trust your chemistry? You put such high pressures on it, you may change the chemistry. Okay. We have to go another route. We're using high speed centrifugation. At the beginning, we just put this in the cap, spin, it won't come out. So we have to use the perforated plate at the bottom here, put the rock on, using the highest speed, and the water drained out and we finally got the water out. Then we, using this (indicating), now to compare between the two; compression and centrifugation.

This data comes from compression, okay? Now, we have sodium and sulfate. Sodium, calcium, chloride, and the sulfate; major cation and anion. This is the concentration. Remember the scale. I'm going to show you next one with the centrifuge so you know the curve, how they look alike. Okay. This is the same scale. There are the 100 in liters, in the milligram per liters, okay? So this is for compressions; this by centrifuge. Are they about the same?

DR. LANGMUIR: Al, Langmuir.

What's the middle cation that's behind the overhead between calcium and chloride? What's the ion down there?

DR. YANG: Oh. Somebody, yeah, I think this is here on the--I need that, too, so if you move it, I think it's okay.

So, now, this is the data from high speed centrifuge. Now, you can see I put the J-13 water in here just for your comparisons. Okay. This is the groundwater. Everybody say -- water is the same as groundwater. Are they the same? The fact is, about three-four times higher in concentrations. It is not the same. Okay. We just can scan you through this, you know. This is the J-13 water. It's a lot lower than those. So that gives us more comfortable the water we got, it actually represent the original, pristine, pore water.

Now, we started going through the tritium now. Now once we got the water, as we squeeze it, the water, see, a lot cannot come out. How we get the rest of the water out? By distillations. We take every drop of the water out. So by distillation out, we measure on the O-18 and the tritium, and the deuterium O-18, because this isotope is tucked into the water. You don't have to worry or anything. As long as the water get out, you measure on it, and that's it.

Now, let's see the tritium. This is No. UZ-4, okay? Now, this is a unit going down to about 350 feet, and this is the water content, and this is the tritium data. Now, you can see we found a fracture along here, and below this, this is in the Tiva Canyon, and the water just below the fractures, you have a high tritium content. Now, let me explain about tritium.

Before the nuclear tests, the tritium in the air produced by cosmic radiation, natural tritium, is below ten tritium units. So if it's above that, it's more than water, or more than tritium. You know, it then depended on how old it is. It's started to decay. So you can take a look up here. Near this here, -- -- more than water. Now you can see from near the top it's high, about 20-25, so it's more than water. There's no doubt about it, but up to here, about how many depth? About ten meters or five meters. It goes to zero, or over to here. Then it starts to come up.

Then this is the argument: If the water is perforating down from the top, it should be gradually coming down. It cannot go to zero and come up. What does that tell you? Water is not--discharge is not directly from upper.

Somewhere--it comes from the fracture or the bedded units, outcrops from this site, and it's out of the -- -- back up in here and then draining down to the bottom. -- -- coming through this. That's what this tells us. I still don't know where it's coming. We tried to look for the fracture. It could be from the fracture in the bedded unit, and flowing to this (indicating), perhaps.

Then the moisture content, as I showed to you, bedded tuff is high. Bedded tuff is high here at the Topopah Spring, is high here, too, and I think Alan Flint showed you yesterday, near the top of the Topopah Spring they have a 50 per cent porosity, so I think this is caused near the top of Topopah Spring, because there it comes up at 4 or 5 per cent.

This is for UZ-4 and for UZ-5. Still the same things. Now, we don't have any samples. These are not cuttings. There are only a few core. We had to run in these core now. We don't run in the cuttings, because when you are cutting things, boring up, water's drying out, we don't know what's going on, so we prefer to using the whole core to get all the data.

You can see the bedded units, this is even higher; very high in the same place with UZ-4. Then coming up to here, you can see here, near to the bottom here, it's high on the moisture and tritium is low. Now, the reason is tritium is--I told you it only go out to 100 years. Now, the Carbon

14 we dated on this water here give about 49--about 5,000 years old at this depth on this water. But on the UZ-4, along the same profile here, the Carbon 14 aging only give it 1,000 years. Why this so--between this? Why is it 5,000 and why is it 1,000 years? Again, the rocky flow coming through here from somewhere comes in and they get very young waters. So what does that tell us?

A lot of the water running in Yucca Mountain is likely the fracture flow, not much of the matrix flow. Matrix flow gives you the same depth, about 5,000 years, and here it's only 1,000 years; difference of 3,000 years old. That's tritium.

Now, let's talk about some stable isotopes, what a stable isotope can tell us. Now, how the definition of data that you are going to--it's a ratio of 18 to 16 or the DH ratio. Take away the standard. Divide by standard. These standards are ocean water, okay? The ratio you may get from ocean, that that ratio is the standard. You take away -- -- and that's the definition of the --. So using the ocean water as zero, that's the definition. So when the water evaporated, the lighter one evaporated. So using the lighter one and get negatives. So all the numbers are negatives, none positive.

Okay. Now, this is a plot of δD and $\delta^{18}O$. This diagram tells you a lot of story, okay? If the water is raining in there, all of the water in the--it doesn't matter where--when the rain water come in there, you should collect all for on these -- water lines. If this water starts to evaporate, sit on the --, transevaporations. What's the water ratio, the pore water, the pore water go this way, away from these lines. That's what the evaporation causes. If rock exchanges because of the ^{18}O, there is no hydrogen in silicates, it's more in toward this way. You can route the water. How much water into --? It's geothermal, the ratio toward this way from this water line.

So if it is during the summer, it will plot once on the top. During the winter, it's more depleted. It plots on this side. So from this data you plot it. It can tell you a long story. Then, if it's 10,000 years ago and glacier ice, that's probably along here. Now, let's look at the Yucca Mountain data.

Now, these are the first things. These were collected in 1984, four stations from April to October, just during the summer. There's no doubt it's all up on the top there. That's the summer range, okay? There's a small dot in here. It's less than -- less, so the drop is very small. So it's evaporated. You can see it's deviated from that meteoric water line. That tells you it's evaporated. So these are the UZ water. I squeeze the water out, I measure it. It deviates from this line, too. That's what that tells us. The water recharging into the Yucca Mountain, we collect it. It has been evaporated before it's percolating down into the ground. That's what that tell us.

Then if you plot this back to the original point intersection with this, that tells you the original water. Either it's in here or in here, tell you it's the snow or the summer rain. It's penetrated into the groundwaters. So this winter, you can see again it's mostly in the -- with few -- in there and there was one big summer storm in the summer in 1984. There's a whole flood at Yucca Mountain, and that's fit in here. So if I curve this back to here, it cannot interact with those. So that tell me there's a big summer rain. That didn't recharge that much into the ground. It just run off. So what is actually going down, it's during the winter snow storm, something like this. It's probably here, or else in this area here; either one. It's recharging into the groundwaters. So that's some story it can tell us by doing these kind of things.

So where we go from here? Where we go from here. Now we are started doing the welded tuff. We are squeezing that welded tuff -- high energies. Now, up to 2-3 per cent, so far we can get the water out. Now, 4 per cent, we may get a couple of cc's out, and we try to get that for the welded tuff because there's a -- --. So you get one Carbon 14 dates, it's about--need about 100 milliliters of the water.

So you can imagine how much time we have to spend to get that one critical data.

Now, besides that, we have to collect some core from the repository horizons. That's where the criteria is. We want to know when that water is getting there, what kind of water is getting there; during the summer rain, or water in the snow melt? And what is the isotopic analyses?

Then we'd have to see what is the matrix flow versus the fracture water. Now, we try to collect that fracture water, if any, because right now, on the surface, we cannot do it. So we had to depend for this on the expert or the study facility. Once we have the underground there, we go there to collect. Maybe we can spin just beside the fracture. We get the water, spin high speed centrifuge, get the water out, compare with the drill core matrix on the same horizon, see what the Carbon 14 age is. Maybe one fracture is very young, maybe it's very old. That's what I expect. Can we see that? These kind of experiments we try to do, and besides that, we have to have more core. This is only telling you only one core from one area.

Now, you have at UZ-6, -- --. We don't have any -- -- up at UZ-1. People have been asking me, well, how can you date on the gas phase, because all--it's open system, it's breathing, you know, I get 10,000 years out of 1200 feet deep. You cannot negate that. So there is some place like that, some place like that. We have a lot to go and this is the basis, using all the same technique. So that's what we are shooting for.

Thank you.

DR. LANGMUIR: Thank you.

Questions from the Board?

(No audible response.)

DR. LANGMUIR: Well, I have one for you. Your extrapolations of the evaporation lines back from the unsaturated zone, of squeezed moisture, what kind of average temperatures are you getting for recharged that you anticipate has gotten into the mountain?

DR. YANG: Yeah, I still in the--yes, that can be done, because right now data is limited.

DR. LANGMUIR: Well, in what you have done, what does it tell you? Are you looking at close to zero Celsius, 5°C, that sort of thing? In other words, is this clearly snow melt that we're looking at that's done all the recharge?

DR. YANG: Yes. I think those isotope ratio, you know, using all this, you can relate to the temperature. It's not that simple, too, you know. It's depend on altitudes, where you are, -- --. So temperature is one other factor, and other thing you have to take into account--so we still have to deal with those kind of factors before we can exactly tell. So it's not as simple, just one correlation. It's temperature, yes, that's one of the--when the -- precipitate or --, the correlation between those. Then in Yucca Mountain, you depend on height, where it is, depth and all this, and which wind comes in. You know, even talking about those, you have a northwest track coming down, Pacific coast from--and all this different type of water. We have to know that snow is coming from which directions. Then once we set that, then we can tell better.

DR. LANGMUIR: Well, in connection with Alan Flint's discussion yesterday, can you reconstruct which kinds of storm patterns that he mentioned that likely produced the water we're now looking at in the unsaturated zone?

DR. YANG: Yes, exactly. That's, I think, already--I think Benson did on that, you know. He has been talking with NCAR and we collect the storms, and we know the current storms from the--I'm talking north side or Pacific coast, and they have different isotopic signals, and by analyzing this, we know what is actually coming in, is coming from the site and if it's come in. Yes, exactly, that's what we are--

DR. LANGMUIR: Do you know enough yet to make a guess at that?

DR. YANG: No. Right now, as I said, you know, water data, squeezed water data is still not enough to represent the whole thing, so we are trying to go that route, you know, trying to identify--once we know that -- and now we know it's snow, that it's collected in winter, then if it's winter, it's likely from the north. It's unlikely from the Pacific coast or from the gulf coast coming up. So it's likely from Arctic and all these sites from Alaska, that side, coming down. So we are trying to track this, then tracking this storm, then we can tell, you know, what -- -- -- and all this kind of thing. So it's a lot of work to go.

And it's interesting that this is the only thing we can do. That's why isotopic technique is so powerful. You can identify the source, where it's coming in, and the problem, where it's come in. You just cannot take the age and take that data for it. It's wrong. You've got to -- -- and you have to correct for those and that's how you do the science. So some people take face value of it and don't know the source of it, so we have to know where it's come from, the source of it and find those, and that's--hopefully, we can get something out of those.

DR. LANGMUIR: One last question. You talked about the problem of getting water out of the welded tuffs. You could do a C_{14} age. Do you think there's any chance that's going to work for you? How much rock would you need to get your 100 milliliters?

DR. YANG: I know. That's why--

DR. LANGMUIR: You're not going to get it until you get down in the subsurface.

DR. YANG: I know. I'm right now thinking, you know, if DOE would give us--I don't know DOE control all the sample, you know; who want how much. If I can get it, you know, I just get in here those few cores, it doesn't help me, you know. I need the consecutives, you know, the length of the--ten feet or 12 feet, so I can analyze on this and say, this region, what the age is and that's the only thing I can do. So we still have to incorporate, you know, talk to the DOE if we can get that data, and I think that's important. So these kinds of things we have to work out.

DR. LANGMUIR: We have time for a question or two from the audience, if there are any, to still stay on schedule.

MS. FABRYKA-MARTIN: Oh, I have one.

You know that nine million liters were lost on G-1 drilling of J-13 water. How can you--

DR. YANG: J-13?

MS. FABRYKA-MARTIN: J-13 water used for drilling G-1, which is a thousand feet away from UZ-1. How can you rule out the effect of that water possibly on influencing your Carbon 14 results? Do you know that the Del C-13 of the water is?

DR. YANG: Yes, we did on that one. It's very depleted. Now, I think this on the water, Carbon 13/12 ratio is that--now, it's the mud floats. During the draining, they have some of the mud float in there, and it's those kinds of things you have to get out and try to correct this kind of data, you know, from during the past with those drills, using those, and correct those, and I think a -23 or -25 13/12 ratio, you know. So then we have other analysis just on this float, and I think the fingerprint is the same as those floats. That we have confirmed by the chemical, by the isotopics, and with 13/12 ratio, and these actually signal the same as those, and so that's why we conclude those water is come out from this mud floats. It's not from the in situ perched water there, and that we have proved that we have all the data. Isotopic chemistry, everything, they have fingerprints much with those, and organics, too. We analyze the mud float in organics.

MS. FABRYKA-MARTIN: But the CO_2 gas ages that you have, is the CO_2 gas coming from that evaporating--

DR. YANG: Yes. We have a CO_2 concentrate that's very, very high, okay, right now. It gets higher and higher and is getting higher and higher. Then I'm wondering why--what this--as I talked before, CO_2 concentration, I didn't show up on here, is -- like this and at the very bottom go up like this. Now, why does it go up? My thinking right now, it's decay from those polymers; polymers decaying, producing the CO_2 and get the big peaks, and that's what's causing this.

So, that's why I say, you know, these kinds of things you have to know before we can do any ages on it.

These maybe give you--screw up on the ^{14}Carbon ages.

MS. NEWBURY: June Fabryka-Martin has taken over from the work that Ted Norris presented about a year and a half ago, and she'll be presenting next on isotopic constraints on transport models.

DR. FABRYKA-MARTIN: Okay. I'm June Fabryka-Martin. I'm a hydrologist with Los Alamos.

There's two issues that I see about water movement rates in the unsaturated-zone at Yucca Mountain. The first question is does water get down to the repository zone and, if so, how fast? And, another question is, does it move from the repository zone down to the water table and again, if so, how fast?

Now, the best indicator for water movement rates is residence time in the subsurface and Al Yang described how one might use tritium and ^{14}Carbon, for example, to estimate residence time of water in the upper zone of the unsaturated-zone. However, in the Topopah Springs welded unit, the estimated downward flux of water might be from 10^{-7} to .5mm per year, Tom Buscheck, notwithstanding. And, if so, then the water residence times at the level of the repository horizon or Calico Hills would be on the order of 10^{4} or more likely even 10^{5} years. Obviously, ^{14}Carbon and tritium may not tell us residence time if they are indeed that old.

However, nature was kind to us and she gave us ^{36}Cl and ^{36}Cl has a half-life of 300,000 years which means it's useful dating range, if things are ideal, are between, say, 50,000 years to 1,000,000 years or more and that means it's ideal for this sort of problem.

Now, Ted Norris talked to you about this in December of '89. Since then, the study plan has been revised considerably. The scope of work is quite a bit larger now. We also have instituted or are in the process of instituting a detailed quality assurance program for this work, such that standardizing the procedure is to prepare and analyze the samples and I've also modified the model used to interpret the ^{36}Cl data.

What I want to describe to you today is first, very briefly, review some of the characteristics of ^{36}Cl in the hydrologic cycle and the applications of ^{36}Cl at Yucca Mountain. And then, again, look at the results that Ted Norris had presented to you from the UZ-1 borehole. So, up to this point, it will be things that you've heard before. Most of the time I want to spend on, on the mixing model that I am proposing to be used to interpret the data to show how one can better one's estimate of residence time of water in this system and the error analysis that's been done to help us guide future work to tell us where we should put our efforts in order to improve our estimates of residence time. And, finally, I'll summarize with the scope of work that I envision over the next couple of years.

The reason ^{36}Cl should be useful as a tracer of water is that it's chemically inert. It's present as the chloride anion. It doesn't interact with the rock very much, highly soluble, nonsorbing, nonvolatile. And, as I said earlier, its half-life of 300,000 years makes it ideal for measuring residence times on the order of 10^5 years. It can be quantitatively measured by accelerated mass spectrometry at all levels. There's no such thing as a ^{36}Cl ratio in this system that's below detection.

There's three sources that one has to be aware of in the hydrologic cycle and all of them in different cases can be used for dating or mixing studies. There's global fallout of cosmogenic ^{36}Cl. That's just like ^{14}Carbon and tritium. It's made continuously in the atmosphere. And then, that atmospheric ^{36}Cl falls out, gets diluted by dead chloride from the ocean, and so you get a characteristic ^{36}Cl to chloride ratio on the surface.

Secondly, again just like tritium and ^{14}Carbon, there's a massive pulse of bomb-pulse ^{36}Cl injected during the period of atmospheric testing of nuclear weapons. And, finally, there's ^{36}Cl produced continuously in the rocks because there's a low neutron flux everywhere. This is significant enough that one has to take it into account when one is correcting the measured ^{36}Cl ratios in terms of an age. If it's very old water, then the in situ production is significant.

Now, I've got a slide here to contrast some of the input function for a bomb-pulse ^{36}Cl to that of tritium and you can see that the ^{36}Cl bomb-pulse was more like a single pulse, a very sharp increase and it stayed about 1,000 times above natural background and now it's returned pretty much back down to natural levels again. And, this has been used in several studies to estimate the rate of infiltration in shallow soil where one does a slow profile--in fact, Ted Norris did this--and where the peak of the bomb-pulses being in the soil tells one how far down the water has infiltrated through the matrix in the soil as of 30 years ago or 35 years ago.

There's several ways in which ^{36}Cl can be useful for Yucca Mountain studies and site characterization. And, I've listed them here in the order that I considered to be the likelihood of producing useful data for Yucca Mountain. The top priority I give to looking at using ^{36}Cl to estimate the deep percolation rates at the ESF level and below.

Secondly, we can use the ^{36}Cl data to test some of the hypothesis in the conceptual flow model. For example, if Tom Buscheck is right, it should be a cinch to try to distinguish, say, fracture flow relative to matrix flow in this system. There should be considerable difference in the transport time and the residence time in water associated with the fractures compared to that associated with the matrix nearby those fractures.

Thirdly, we may be able to expand the data base that Alan Flint is collecting for the shallow infiltration rates by looking at the ^{36}Cl bomb-pulse and ^{36}Cl in slow profiles. That would be an alluvium. And then, also ^{36}Cl can be considered under some circumstances as an analogue for ^{99}technetium because in an aqueous system, at least at low temperatures, ^{99}technetium should be present as pertechnetate. Again, it's an anion that's considered to be nonsorbing, inert, not reacting with the rock very often. And so, ^{36}Cl and ^{99}technetium should behave fairly similarly.

And, finally, I also added this. It actually comes under Bill Steinkampf's study plan. We're measuring ^{36}Cl in the saturated-zone, as well, for Bill where it can be used to perhaps suggest for zones of mixing between aquifers and again put limits on residence time of water at different parts of the aquifer.

The current, I guess I would call it, baseline design for the ESF and I've put this up here to show you the sort of sampling scheme that I'm envisioning for the ^{36}Cl where we have the first--the north ramp will be going in first, I think at the present time, and the south ramp falling at some other point. But, what I've outlined in green here is the Topopah Springs, the mine openings in the Topopah Springs, and the red are the mine openings in the Calico Hills. We'll be requesting samples for ^{36}Cl as core every 100 meters along the ramps and drifts and then again at the contacts, major fracture zones, major faults. You can see there's access now to the faults which is a great improvement over the previous design of the shaft. And, this will be quite a few samples. There's about 12,000 meters of mine openings in the Topopah Springs and 8,000 meters in the Calico Hills. So, you can see that we're probably going to be collecting, say, 200 or 300 samples. Honestly, I haven't thought about the logistics of this yet for sample storage.

Now, let's go to the UZ-1 results that are talked about so often. Here, I've plotted as a function of depth below the surface the ratio of ^{36}Cl to chloride that was measured in cuttings from this hole. Now, I have the initial meteoric ratio which is pre-bomb ratio at about 530 times 10^{-15} or 5 times 10^{-13}. Samples that plot above that meteoric ratio are a fairly clear indicator of having bomb-pulse ^{36}Cl

present. Now, the source of this bomb-pulse is not at all clear. Whether or not it is from the surface or whether it is possibly from G-1 cannot be distinguished at this time and I'm not sure that we'll ever be able to settle that issue unambiguously. I have a couple of more pieces of data I can collect, but it may always be a mystery. However, in any case, it does prove that fracture flow does occur and water can move fairly fast under some circumstances.

A second--

DR. LANGMUIR: June?

DR. FABRYKA-MARTIN: Yes?

DR. LANGMUIR: Is G-1 a possible contamination source?

DR. FABRYKA-MARTIN: Yes. I did prepare another overhead just in case a question was asked about that and maybe it's worthwhile putting it up. First of all, just for background, this shows the UZ-1 up in Drill Hole Wash and then 1,000 feet away is G-1. And then, this is drawn both to vertical and horizontal scale. This is very important because it makes a big difference whether that bomb-pulse came from the surface naturally or whether it was induced by G-1 drilling. G-1 was drilled in 1980. It was drilled wet with J-13 water, but they had drilling mud and drilling mud additives added. In fact, it had a lot of calcium hypochlorite added and I'm curious about what the ^{36}Cl content of that is. But, while they were drilling it, they lost 9,000,000 liters of water. It had to go somewhere. Most of it was probably lost, it says in the drilling report, in the fractured zones in the Topopah Springs unit. And, one high permeability fracture zone that was mentioned was at this depth. Now, there was no tracer added to this water.

Now, in comparison, UZ-1 was drilled three years later. I've marked in red where the bomb-pulse ^{36}Cl was detected. They lost about 4,000 liters probably most in the alluvium, but there was no--they had a bromide tracer added and there was no indication of this drilling fluid below a depth of about, I think, 76 feet. So, that could be possibly a source for the bomb-pulse also. But, we have chloride bromide ratios measured in the leachate all the way down from the top down to the bottom and there's no ratio as high as--no, let's see--as low as one would expect if it was this water with the lithium bromide tracer present.

So, that's where we stand. And, as I said, it may never be resolved, but to me, my own personal opinion is that this G-1 water moved over there. Because when they're drilling it, the hole is pretty full with the water the whole time they're drilling that and it took several months to drill.

So, we have three categories of samples to consider. The bomb-pulse ones, I just mentioned. Secondly, there are these four samples that plotted fairly near the initial meteoric ratio and, in this case, one would say that the residence time at these depths was apparently less than 50,000 meters and, therefore, not long enough for the ^{36}Cl to have decayed significantly. And then, finally, the samples of greatest interest are these two that fall greatly below the initial ratio and they may provide evidence for long residence times for the water in this system.

In fact, if we look at that lowest ratio, it will give us a lower limit for the average water velocity in UZ-1. So, the lowest measured value was 103 times 10^{-15} at 372 meters depth. And so, that gives us an estimated net downward velocity that must be greater than or equal to about .5mm per year. And, this assumes vertical movement downward through the matrix. It assumes that we know the initial recharge value and what the equilibrium value is and that J-13 water didn't affect the results significantly.

Now, there's a problem with this. If we go back and take the same sample and measure it again, we do not always get reproducible results. And then, that's because you get dilution with rock chloride which makes the ratio smaller.

This shows the problem schematically. There's two sources of chloride in the simplest system. There is chloride in the pores, which is what we want, of course, and there's chloride in fluid inclusions along grain boundaries in the rock minerals, which we don't want. But, when we get a sample--in the case of UZ-1, we got our samples essentially as grit, very fine cuttings. But, under better circumstances, let's assume we get hand size samples that are poor. Well, the first step is to crush that up and leach it. When you crush it up, of course, you release some of that chloride in the fluid inclusions. And, you can imagine that each time you get a sample, even though you follow the same procedure, you still might expect that you're going to get a variable dilution with rock chloride. And, we need to find a way to separate out those two sources so that we can correct the measured ^{36}Cl value for the ^{36}Cl that was introduced from the rock chloride.

The solution that we're investigating now is using chloride bromide ratios and, as a backup, perhaps the stable chloride isotope ratios to estimate the proportion of meteoric chloride that is in our leachate that we leach from the rock. So, here, I've illustrated how one might do this. For example, at our current estimate for the chloride bromide ratio in the rock, end-member is about 500. That for the meteoric end-member is about 130. Let's just imagine that we measure ratio about 250 which is about what we've been measuring our leachates on the average. Well, then, that would suggest that we have about 67% meteoric chloride in this particular leachate. So then, we take the rock end-member value which is for pure rock chloride, 0% meteoric chloride, and the measured ^{36}Cl ratio which now we've considered to be a representative 67% meteoric chloride, and use that to determine the slope of a line which we could project to the 100% meteoric chloride and get a corrected ^{36}Cl to chloride ratio. Then, of course, the corrected ratio is always going to be larger because the dilution is with a rock chloride with a lower ^{36}Cl to chloride ratio.

DR. LANGMUIR: Have you thought of using oxygen deuterium information for fluid inclusions versus the meteoric, mixing that way?

DR. FABRYKA-MARTIN: No, I haven't. I think one would run into problems with the geochemistry being so different with the two.

DR. LANGMUIR: You can compute mixing ratios presumably from that sort of thing, as well.

DR. FABRYKA-MARTIN: Um-hum, okay. I'll think more about that. Because the chloride bromide, it may not work out. I think it will, but I'm not sure yet. I mean, for example, one thing, I've been assuming that the rock end-member is a constant value. It may vary across the mountain in a way that I can't understand or predict, in which case it would be difficult to use it.

Then, the third step then is to use these ^{36}Cl ratios to come up with estimates of residence time. Using the uncorrected ratio, as I did the first time, will give us an upper limit for the age or a lower limit for velocity. Using the corrected or our best guess of the meteoric ratio will give us our best age estimate and, therefore, our best estimate of velocity.

Let me show you the sort of difference this may make. It's very important that the chloride bromide ratio be measured in the exact same solution from which one prepares the ^{36}Cl and the chloride sample. You can't go back and do it later. Unfortunately, for the UZ-1 profile, we do not have matched or paired samples. We did chloride bromide ratios after the fact. And, so I'm only doing this for the purpose of illustration. Don't take the results as fact. But, anyway, again taking that same sample from the 372 meter step, this is the lowest ^{36}Cl to chloride sample that was measured, the measured chloride bromide ratio suggests that we have about 52% meteoric chloride in this sample. If I correct the measured ^{36}Cl value to take this into account, we get a higher ratio, almost doubled, 193 times 10^{-15} and this would correspond to a net velocity of about .8mm per year, net downward velocity. Then, you can again compare this to the lower limit that was established by the other ratio of some velocity greater than or equal to .5mm per year.

As one can imagine, since there are so many more parameters in this model, the uncertainty goes up considerably, as well. There's uncertainty associated with the chloride bromide ratio measurement, with the estimate of the various end-members in the model, and with the measured ^{36}Cl to chloride ratio itself. And, I have tried to summarize the effects of these various parameters in this graph where I've plotted the percent uncertainty and the residence time as a function of the average residence time. And, one point to be made here is that, as long as you have a high proportion of meteoric chloride in your leachate, then you get fairly reasonable uncertainties, but the greater dilution one has with the rock chloride, the higher the uncertainties go up. And, this graph I used to argue that we cannot use rock flour, for example, from drilling operations or very fine grit from drilling to make our measurements because we're going to get unacceptable uncertainties.

I used that same graph to calculate the uncertainty in water velocity estimates as a function of linear velocity for samples from the Calico Hills unit which have an average depth of, say, 425 meters in the ESF. And, here, you can see that the ^{36}Cl method will give us fairly good residence time estimates provided that the average linear velocity is somewhere between, say, .5mm per year up to maybe 4 or 5mm per year. Again, assuming that we get samples where we can maximize the proportion of meteoric chloride in the rock leachate.

Finally, let me conclude with looking at the scope of work that's described in the revised study plan. And, here, I've ordered these in the order in which I think that the tasks will be undertaken, although there should be overlap between the tasks, of course. The very first thing on the list to be done is to establish the meteoric chloride bromide ratio and the meteoric ^{36}Cl to chloride and maybe stable chlorine isotopes for the two end-members. For the meteoric end-member which would just involve collecting surface soil samples and perhaps shallow soil profiles and then in the rock end-member, as well, in order to ascertain whether or not those end-members are constant values or whether there's too much variability to make use of this approach. The rock end-members are determined by a method called step leaching where you leach the sample first. That will have the maximum proportion of meteoric chloride. Crush it, leach it again. This time, it will have less meteoric chloride, more rock chloride. Then, crush it some more, leach it again, keep on doing that until you approach some constant value for the chloride bromide and ^{36}Cl to chloride ratio. We'll do more borehole profiles in order to determine whether or not the UZ-1 phenomena is a common phenomena or whether it was a freak--I guess, I'll call it that. And, finally, the most important thing is proceeding with the ESF samples. What I envision to come up with at the end of the project is a 3-D map of residence time as a function of location in Yucca Mountain to the extent that samples are available and money is available to measure them, too.

Okay, thank you.

DR. LANGMUIR: Thank you, June.

Questions from the board?

(No response.)

DR. LANGMUIR: We have time for some questions from the audience, if there are any.

DR. BUSCHECK: Can I make a comment? You pointed out that that's net velocity and I think it's important to--

DR. FABRYKA-MARTIN: Net downward, that's right.

DR. BUSCHECK: Net downward velocity. And, if it is fracture flow, based on some of the observations I was making earlier, the fact is that the fracture part of that flow could have occurred over hours or perhaps days. Once it's imbibed in the matrix, the flow is minuscule from that point on. So, you know, it should be an emphasis that's the net effect of velocity, but in fact, the actual velocity during that episode could have been far greater than that.

DR. FABRYKA-MARTIN: That's right. And, it's also been a mixture of pulses, too.

DR. BUSCHECK: That's true.

DR. FABRYKA-MARTIN: So, one pulse may have occurred yesterday, the other one a million years ago. Who knows what the average will be? It will probably be dominated by--well, it depends on how much chloride each pulse carried down. Sure.

DR. JONES: June, you used words residence time and travel time. Could you distinguish between those two?

DR. FABRYKA-MARTIN: Residence time, I think, or average residence time is the one I want because travel time implies I know the travel path and I don't. All I can say is what the--

DR. JONES: It sort of gets at what Tom was just saying. You know how long it's been in the rock.

DR. FABRYKA-MARTIN: That's right.

DR. JONES: But, you're not sure if it moved there quickly and it's sitting there or if it's average movement. Is that--

DR. FABRYKA-MARTIN: Or whether it came in from the side or whether it came in from the surface. No, I cannot tell that.

DR. JONES: Yeah.

DR. FABRYKA-MARTIN: If you know a method that can, let me know.

DR. JONES: Well, to compare with the hydrology then, there might be multiple transport hypothesis that would give consistent residence times which is what you're--

DR. FABRYKA-MARTIN: I can't think--the ^{36}Cl method doesn't overlap with any other dating method that I know of nor does ^{14}Carbon nor does tritium except for bomb-pulse. Other than that--it's hard to provide a check on it other than by model calculations.

DR. JONES: Yeah, that's what I was referring to. But, there could be several transport hypotheses that would give you the same result, but--

DR. FABRYKA-MARTIN: But, not a unique solution.

DR. JONES: Yeah.

DR. FABRYKA-MARTIN: That's right.

DR. JONES: Whether it was fracture flow or uniform matrix flow or combinations thereof.

DR. FABRYKA-MARTIN: That's true. That's true.

DR. LANGMUIR: Thank you, June.

Proceed to the next speaker?

MS. NEWBURY: That concludes our presentations on the unsaturated-zone, and at this time, Barney Lewis from the USGS will do some summaries and later discussion.

MR. LEWIS: As Claudia mentioned, I am Barney Lewis. So, we got that out of the way, quickly. I have the enviable task of summarizing and telling you what you just heard and what I felt was important out of what you just heard. So, we may have a difference of opinion there. And, I also notice that on the agenda, I'm summarizing the saturated-zone studies which this could well be a pre-summary, I guess, but that's not really what I'm doing. That agenda is correct, okay.

Okay. What I'm going to do is I've gone through the various presentations that you've heard over the last day and a quarter and for the presentations that I'm very familiar with, I've picked out like the objective from the SCP or the study plan or so forth and then wrote my crib notes as I listened and to what--like I said, what I thought was important that was listed in each one of the presentations. And, in those that Tom Buscheck and Dale Wilder presented, I'm jut going to re-list some of the important points that they made. I am going to do this very quickly, hopefully.

The first presentation you heard yesterday was on the characterization of meteorology by Alan Flint. And, as Alan had stated, the objective of this study is to characterize the meteorology conditions around Yucca Mountain and in the vicinity. Now, Alan did mention that he's looking at differing areas of detail starting with a very large area and working in towards Yucca Mountain and I guess looking at a very large circular area around Yucca Mountain and the vicinity and then looking at Fortymile Wash and then concentrating on Yucca Mountain. He also mentioned that he can distinguish summer and winter precipitation patterns very easily.

That data is an ongoing--date collection is an ongoing endeavor right now and that he is looking at the data now seized from a statistical and a deterministic approach. His ultimate goal in the precipitation studies and meteorologic studies is to produce simulations that will be used to not only predict current conditions, but they'll also be variable enough that he can use them in looking at future conditions with wetter and/or drier conditions. These simulations are going to be used as input for many other studies like the infiltration studies, some of the surface runoff studies and then ultimately for performance modeling exercises.

Alan was on for a long time, as you know. Alan also discussed his infiltration project which is to characterize infiltration related hydrologic properties of fracture materials and also to characterize present day infiltration processes and then to do a spacial determination and a statistical determination of the overall properties around the Yucca Mountain vicinity. He emphasized that it's very important that you understand the current processes that are ongoing at the mountain. He also mentioned that one of the purposes is to characterize the upper flux boundary, if that's what you want to call it, and that this is to develop alternative conceptual models and also develop and enhance sampling and measuring networks, collect/analyze data, and then iterate, of course.

I notice an important thing here is when I went back and was looking through the SCP that the original statement about the infiltration project was to characterize the flux boundary for the upper 10 meters. Well, if you noticed, Alan has, all of sudden, got down to bedded tuffs, the Paintbrush tuffs. And, after Tom's presentation, I imagine he'll be going to 2500 feet next week. So, this could be an ongoing process.

One of the main goals of Alan's projects here on the infiltration project is to design and build computer models for current and future climatic conditions. This, of course, is related to the performance assessment modeling also.

The last presentation that Alan made was about matrix hydrologic properties. This was, of course, to determine flow-related hydrologic properties of matrix material at Yucca Mountain. He made a point of stressing that apparently he does not have to rely strictly on geostatistical or statistical methods to get a spacial distribution for these properties. Some of the recent work that he and some of the other people of his staff have done make it appear that it can be a deterministic process. That you can actually measure some of these things and then correlate them across the Yucca Mountain on other units and so forth. His last slide, I really ought to--because it's how he plans on doing this in the future to sample, test, and analyze model site-wise and PA-wise and then iterate the whole process which I think as job security, quite frankly, that's a good way to do it.

The next presentation that was made was made by Joe Rousseau and then he was followed by Gary LeCain and both of these presentations had to do with the surface face testing program. Joe is project chief for the Deep Percolation Program and it's to define the potential field in situ and also examines, as Joe mentioned, the Solotario Canyon Fault in detail. Now, this has become very important. Not that we're going to look at the Solotario Canyon Fault by itself, but with the enhanced capabilities that we have from the preferred options of the ESF, the kinds of things we're looking for in this project as far as the second bullet there can be looked at in detail in the north and south ramp, in particular. And, I'll discuss that a little bit. My final two slides will be about how these things all tie together, the integrative process.

Joe dwelled on the benefits of in situ monitoring. He felt that these benefits included the fact that you can observe the dynamics of the UZ system in situ, that you can actually measure and gain an understanding of pneumatic pressure and temperature variations and their relationships, evaluate the equilibrium process, and isolate discrete intervals such as faults, contacts, and any other hydrogeologic changes. He also noted that it was an excellent method for collecting rock gases for chemical analysis for Al Yang's project and anybody else that wants that information.

He mentioned his future studies, right now anyway, include the HRF boreholes, if and when they are drilled or augered, the shallow boreholes that are going to be used for instrumentation and calibration and determining whether or not the actual instrument package will actually go in a bore hole. That's a unique test in itself and we haven't done that yet. His data collection will be used, of course, in many, many studies that are related to site characterization and performance assessment.

Gary LeCain talked about the air permeability testing program and these are the actual objectives out of the study plan which includes measuring the in situ matrix and fracture air permeability and estimating the effective porosities and so forth. In Gary's presentation, he talked about how we're going to measure these particular parameters and what type of equipment and interpretive methods will be used. So, therefore, he talked about prototype testing in Apache Leap (phonetic) where he determined that the calculated permeabilities were not dependent on air injection rates over the given range that they were tested under. And, that the Apache Leap Tuff, in particular, appears to be an isothermal system. That there was very, very little temperature change noted in his testing program. He also noted that from an instrumentation standpoint, the thermalcouple psychrometers did monitor the arrival of the air injection front. However, he said that the test was too short of duration to actually determine whether or not the system came back into equilibrium after injection. That was only six days, by the way.

The next presentation was U-Sun Park and the main thing I can say about this is U-Sun does not like the regulatory requirements that we're faced with in this program, that he thinks they ought to be done differently, and--which I think he made a very good point. And, his discussion was about gaseous and semi-volatile radionuclides in the repository and then addressed the data needs and the test plans that go along with addressing regulatory compliance. He mentioned that he thought ^{14}Carbon was the most significant gaseous radionuclide to deal with in this situation and that the release and resulting health effects from the transport of gaseous and semi-volatile radionuclides are expected to be insignificant. But, there's not a real problem. And, again, his main point was that we need to re-examine the regulatory situation whether or not it's based on containment. Do we make those measurements at the point of containment or at the accessible environment where there may be some health effects?

Two presentations were combined into one here. That's what we call the topographic air effects testing which was presented by Ed Weeks and Don Thorstenson. The objectives are to describe the gas flow field in the mountain doing this by measuring open boreholes to develop an understanding of these flow factors, determine the transmissive and storative properties of the gas flow, and then develop a model of the transport of these gases.

Ed Weeks noted that the net air circulation in the mountain is controlled by rock gas and air temperature differences and wind effects, not so much by the barometric effects. The rock gas and air temperature differences dominate the circulation process and I think the numbers were like a 30 to 70% split, something like that. Even with the large volume of air that's been expelled out of UZ-6s, Ed noticed, quite surprisingly, that the gas chemistry had not changed that much over the years and that the air circulation may have in the future significant effects on gaseous transport if indeed the gas released from the repository can make it to the shallow part of the mountain. But, also, can have an opposite effect that if the air is drying out the mountain, as it appears it is, that the downward percolating water that could act as a transport mechanism, once it reaches the repository horizon, will be significantly deterred because of the drying effect.

Don Thorstenson separated the chemistry part of his presentation into talking about the shallow UZ and the deep UZ and he's put these at higher than 10 meters, roughly. And, in the shallow system at less than 110 meters, he said there was an extremely rapid gas flow that everywhere it was measured, there was pulse bomb-pulse in the CO_2 and concluded that if the repository ^{14}Carbon in the form of CO_2 reached this shallow zone, it would dissipate to the atmosphere and the accessible environment very rapidly.

Contrarily, looking at Topopah Springs unit in the deeper UZ, Don mentioned that even with the indication of very highly permeable zones at that depth that there was an absence of pulse-bomb CO_2 in the samples collected. And, he also noted the circulation is much slower than in the shallow UZ under natural conditions. He did not attempt to make any statements about the repository effects on gaseous movement in the deep UZ after the waste was emplaced in the repository. And, his final conclusion was that essentially all the data collected was essentially consistent with the two component rock gas/air circulation model.

Now, the next two I'm going to talk about are the ones where I'm going to have to say this is kind of from what I listened to and what was important and these are the types of things that both Dale and Tom made in their presentation.

Dale talked about the physical effects of the waste package. Modeling activities, he mentioned that they need to describe the hydrologic and geochemical aspects of the laboratory and the field system and that simulations were compared to laboratory and field studies and model validation will be concentrated for future work. Is that correct, Dale? I hope I got that right.

Out of his presentation, I noted that he emphasized that the disturbed zone around the waste package can be very significant volumetrically. It can be a very large amount of rock. That this disturbed zone can affect the waste package performance and also affects the source term for any transport modeling. The water quantity and quality are significant for design and performance assessment considerations and that the properly constructed engineered barrier system--is what I call it, I don't remember what term Dale used--will mitigate episodic fracture flow from reaching the waste packages. And, that's the main conclusions that I got out of this presentation.

I mentioned when I started that the fun thing about this is trying to summarize some of these presentations, and when Tom shows 25 or so conclusions, I had a little trouble deciding which ones were really significant and important. These are a few that I threw up here after his dry-run presentation in Denver where he discusses the effects of equilibrated and nonequilibrated conditions, flow in fractures and the matrix. In a couple of the conclusions, episodic infiltration occurs as fracture-dominated flow in low permeability units and matrix-dominated flow will dominate in the high permeability units.

The greater fracture densities in the welded low permeability units may facilitate vapor removal. Now, one of the important things here that I thought that Tom had mentioned was the inclusion of the waste material in the simulations. It shows that the fracture system cannot actually shed condensate and to that end that the vapor flow away from the heat source actually will later be drained via gravity in the liquid form. So, you've got a potential mechanism for moving radionuclides away from the waste package.

Also, Tom mentioned that the data indicate that nonequilibrium fracture-matrix flow can occur at considerable depths, very deep in this system or at the repository level. And, that for the low matrix permeability, that fracture-dominated flows will occur in welded units and then the opposite in the high permeability units matrix-dominated flow will occur in the nonwelded vitric units.

DR. BUSCHECK: Barney?

MR. LEWIS: Yes, sir?

DR. BUSCHECK: Also, in the zeolitized nonwelded Calico Hills, if it's significantly fractured, its properties are very similar to the welded units in terms of its ability to attenuate flow.

MR. LEWIS: Right.

DR. BUSCHECK: So, I would include that in the low permeability units.

MR. LEWIS: I'm glad you said that. I didn't get that far in my crib sheet before I stopped. Thanks, Tom.

Also, virtually all of the mountain's ability--and I think this was even a question from one of the board members. Probably, one of the most important things that Tom said was about the capability of a mountain to retard flow by matrix imbibition. Would you say 90 to 99%?

DR. BUSCHECK: Well, that's based on a characterization by Klavetter and Peters which they used. So, you know, that's based on that data.

MR. LEWIS: Okay. It's a very important statement, though. It's a critical one.

DR. BUSCHECK: I agree.

MR. LEWIS: And, over the years, many of us have talked and discussed at meetings between the participants that we thought that the bedded unit above the repository level was going to be the key to this whole system and how well it worked and whether or not it would absorb water, whether it would move water laterally along the top of the Topopah Spring or whatever, whatever the conditions were.

DR. BUSCHECK: Barney, there was one other point. I sort of introduced the concept of physical versus chemical retardation. I didn't elaborate on it very much. What we mean by physical retardation is that the effect of retardation you get vis-a-vis matrix imbibition which tends to operate against subsequent fracture flow propagating further downward migration of radionuclides. So, it's something that we feel is very important and needs to be included in large scale transport calculations.

MR. LEWIS: Well, if Julie Canepa is here, she can talk about the other retardation.

DR. DEERE: I have a question while we're on Tom's presentation. When you spoke of a low permeability and a high permeability unit, are you talking about matrix permeability?

MR. LEWIS: I'm talking about matrix permeability, that's very true.

DR. DEERE: Right. Because, you know, when we have a hard welded fractured unit and you say this is a low permeability unit, to me, this is the high permeability unit.

DR. BUSCHECK: Well, if you have equal fracture densities, equal fracture conductivity in given units, you'll find that you'll have the same bulk permeability in those units because when you do the bulk averaging the matrix permeability often falls out if there's any significant fracturing, at all. So, I was always referring to the matrix permeability. And, for this talk, I wasn't--I was, for the sake of comparison, assuming that all units are equally fractured.

DR. DEERE: And, that, I think, is a very, very large assumption.

DR. BUSCHECK: Oh, it is, but it was necessary.

DR. DEERE: And, probably incorrect.

DR. BUSCHECK: It was necessary to show the importance of matrix flow and we agree we are looking at variable fracturing and it needs to be included in more detailed modeling.

Dr. Cording?

DR. CORDING: That was my point. I think that your next steps would be to start varying the fracture characteristics in these different layers. It would seem that you could do that almost with your present model with a series of each--breaking it up into a series of horizontal zones having different fracture characteristics, you could almost use your same model.

DR. BUSCHECK: We even have developed an analytical model called the fracture flow attenuation model which can look at variable density fracturing and also look at variable matrix properties. And, so we're looking at a higher level model which is more economical to run and we can look at more three dimensional effects with the use of that model.

MR. LEWIS: Some of the work that is being done at LBL for us in conjunction with our modeling projects are addressing these same problems.

The other very important thing that I think that Tom mentioned at the end of his presentation is we should concentrate a good part of our effort on that upper bedding unit, as far as characterization.

DR. BUSCHECK: Or if we find that there are more of nonwelded vitric attenuating units wherever in the mountain, whether above or below the repository, we should be focusing on their saturation condition relative to the neighboring welded units or low permeability units to either indicate the presence or lack of presence of episodic nonequilibrium fracture flow. I think that will be a very good signature for whether fracture flow has existed to those depths.

MR. LEWIS: Thanks, Tom.

Okay. Recently, just a few minutes ago, the unsaturated-zone presentations were concluded with Al and June. Al Yang's project, this is the same slide that Al had that presented the objectives, but I won't go over those again. But, Al did mention that the directions of this project will be to continue extraction or pull water from the core to analyze from the chemical and isotopic standpoint cores from the Calico Hills and the repository horizon, in particular. That pull water from the matrix and fracture water should be analyzed for age relationships on a continuing basis and that the core from the UZ boreholes, the deeper boreholes, are analyzed to facilitate hydro-chemical characterization, both general chemistry and isotopic analysis. And, this all goes into a grand hydro-chemistry model.

DR. DOMENICO: One question on that. It was mentioned that the ^{36}Cl interpretation may be compromised by the drilling of the G-hole. Is there any potential activity that could compromise the tritium data in the same way?

DR. YANG: Now, those tritium data, we are very careful, yes. You can be contaminating the labs. Now, for instance, in the G-Tunnel sample, it's very highly--some of them a million picocuries. We find that, too. But, these kind of things is before that time. We corrected the in situ, we corrected the in field, we did this all before that time. Now, recently, we found there's contamination on the lab. We've been cleaning up for eight months now. We've tried to clean up all the labs and to make sure it can be done below level. So, every time we analyze, we analyze the background some--get from EPA. We run it, it is low. Then, we trust that data. Then, after that, we run the sample. So, we are careful very much about those things.

Now, other than that, from the nuclear test sites, if they have underground detonations, if they have any fallout, we should see that. We've been--precipitation in the past three years. We didn't see that. So, I think it's pretty safe to say at the top of the mountain is about 25 tritium units. Below it, at 60 or 100 feet, about 60 tritium units and that's nearly about 1963, if that makes sense, for the peak of those nuclear tests and that's the highest peak in there. So, I think these are pretty good data. Yes, they certainly are worried about this--we don't find anything for those data. If we find in the future anything, we should come back and correct those. There's no doubt about those.

MR. LEWIS: Well, also, don't forget that even the things like the water they're going to put on the roads for dust suppression around any kind of drilling pads or anything like this, they're going to be tagged. So, if you do see some pulses in the subsurface like in Alan's infiltration projects or Al's hydrochemistry--

DR. DOMENICO: Currently, the project has learned to tag water, but in the past that has not been the case. I was talking about past activities.

MR. LEWIS: Oh, okay. Okay. I wasn't here.

DR. WILLIAMS: Barney, as long as we're on this subject of G-1, I wanted to ask you a question which is probably due to my ignorance about how it was drilled. I've always assumed that it was drilled with mud because of lost circulation. Is that wrong?

MR. LEWIS: Well, they did use a polymer mud.

DR. WILLIAMS: Mud?

MR. LEWIS: Um-hum.

DR. WILLIAMS: And, it's unsaturated rock. So, why would you expect the water from G-1 to move upsection to get to UZ-1? It should be under zero pressure. It should be nearer drainage by gravity.

MS. FABRYKA-MARTIN: That's right, but there's a huge head buildup.

DR. WILLIAMS: On what?

MS. FABRYKA-MARTIN: There's a huge head buildup.

DR. WILLIAMS: From what?

MS. FABRYKA-MARTIN: During the drilling.

DR. WILLIAMS: Why would it be under pressure? That's what I don't understand.

MS. FABRYKA-MARTIN: Well, it's under the pressure of the column of mud above where the drilling bit is. I'm not really the perfect person to be addressing this.

DR. DOBSON: When they drilled, they--I'm not sure if they used a mud pit or what. But, normally, they maintain the mud in the hole to the top. They attempt to recirculate. When they say mud was lost, that means they lost circulation. So, the stuff was running out from the bottom, but in a hole like you want, you've got--you know, it is an unsaturated environment and you've obviously got a lot of head because you've got a column of water a couple of thousand feet high. You've got the column of water and mud. But, normally, with a big rig like that with a wet drilling operation, they recirculate the fluid in the hole. And, so that means that they need to maintain a standing column of fluid.

DR. WILLIAMS: I know they do that in saturated-zone, but I didn't realize they did that in the--

DR. DOBSON: They did them in G-holes here. They didn't do that in the UZ holes which were drilled with a mist, as I understand it.

MR. LEWIS: Yeah, I don't know if they drilled with water, if they were just drilling with water and then later when they lost circulation added the mud. Because I don't know where that nine million meters, what the composition of that is.

DR. WILLIAMS: That might be worth pursuing in trying to answer this question.

MR. LEWIS: Um-hum.

DR. DOMENICO: Wasn't polymer discovered in the UZ zone?

DR. FABRYKA-MARTIN: That's right. The polymer was discovered in the water that they encountered at the bottom of UZ-1.

DR. DOMENICO: So, that well is contaminated?

DR. FABRYKA-MARTIN: There's no question about that. The question that arises is whether or not G-1 water contaminated up higher--there's no question it got down lower to the bottom of the hole, but whether or not it could have contributed to the bomb-pulse ^{36}Cl, for example, at 150 meters is an open issue.

MR. LEWIS: Very quickly, June's presentation which was just completed, I can't really say too much about it because she did a very good job in stating these objectives. And, her future work or direction on her last slide, I think, is worth iterating that she's looking at soil sampling and conducting soil profiles to determine chemical and isotopic ratios. This will help determine the shallow infiltration rates. This supplements our infiltration studies. And, to do leaching tests of tuff and to get at rock chemical and isotopic ratios also. Then, of course, the borehole profiles and correct all the ESF samples she can get. If June and Al

Yang both get their way on samples, we'll build a complex called the Fabryka-Yang Storage Complex, I'm sure. That's not a cut. I mean, that's true.

Now, the last couple of things I wanted to mention real quick is, you know, remember, you're only seeing a limited portion of the site characterization program for the unsaturated-zone. We also have an ESF based program which primarily supplements and compliments the surface-based percolation programs, both shallow and deep and at the surface. And, it also will provide information for analyzing fluid flow.

Now, the preferred options or--what is it called--reference design concept that's being used now for looking at the ESF. Actually, this caused us a lot of work as we had to re-do a lot of things like study plans and every piece of documentation that go along with those types of things, but compared to the old ESF testing plan which was two shafts, looking at the repository level and the Topopah Springs, in the old prior--I guess, it was the SCPCD, the consultative draft, or prior to that, we did have one of the shafts going to Calico Hills with limited expiration into Calico Hills. That was deleted due to some comments by the NRC, I guess, or somebody, whomever. The nice thing about this option is not only does it give you the expanded exploration of Calico

Hills so you can look at the Ghost Dance Fault in three different places, the imbricate fault zone, even look at Solotario Canyon Fault at depths where you can do hydrologic properties of the faults testing, you can do mineralogic testing of the faults looking at what is their fault gouges or rock flour, what's occurring in the fault, and their hydraulic properties. The nicest thing about it is these things--this is about a little over this arch here (indicating)--the south ramp is a little over two miles long and you cross the Bowridge Fault, the imbricate fault zone, the Ghost Dance Fault, and/or its extension of being Dune Wash Fault. As Dave mentioned, you cross many contacts. You go through many different smaller fault zones that are unnamed. So, you have a much, much better and an increased capability of looking at whatever structure contacts, whatever rock type you want to.

Fortunately, this is down-dip. Both the south ramp and the north ramp are down-dip from the repository level. They're outside the controlled block area and it would be really nice if we could do hydraulic testing in some of those units that I just mentioned, some of those conditions, not just air permeability testing. This would give us the added capability of looking at our very small scale testing like intact fracture, taking many more samples, taking more samples for Al Yang's hydrochemistry-matrix properties, or whatever. And, also, to look in an intermediate scale to actually go into one of the alcoves and do more percolation testing to extract a three meter cubic block and do those type of tests there and, of course, Fault K where you test the larger volume of rock. And, this additional information, of course, will supplement the surface based testing program very nicely. You'd just have it three dimensionally and, volumetrically, you're looking at a much larger area, a much larger sample.

We didn't know what to do if with the excavation effects tests if we went to a TBM type of drilling method or excavation method. And, as soon as we looked at this, we realized that if you do have a TBM, there's got to be some excavation effects and one of the nicer things about this whole array is you have these little junctions where there's some corners you can do an excavation effects test, drill holes parallel to the drifts or whatever we call those at that point, the shafts or ramps or whatever, and actually do an enhanced excavation effects test. That's really all I wanted to say about the ESF. Finally, don't forget that the purpose of all of this is to develop a feasible, plausible model of the unsaturated-zone.

This is pretty much self-explanatory. Right now, Lawrence Berkeley Lab has the lead on constructing our three dimensional unsaturated-zone model. Recently, over the past year, we've made an effort to include saturated-zone people. We all realize that the bottom of the unsaturated-zone and the top of the saturated-zone is not a no-flow boundary, but we do have to talk about the boundary conditions for our model and their model and also is involved performance assessment people. And, they idea is to make sure that everybody is aware of what everybody else is doing, hopefully. So, we don't duplicate efforts for a change. And, also, we've involved all the testers, all the PIs from the unsaturated-zone so they know what kind of information the modelers require and the modelers also, in turn, realize what they're getting and whether or not it's useful. In other words, I'm just saying I think now we have a very well-integrated program.

The end.

DR. DOMENICO: Can I address a question to the people dealing with isotopes again? I've always felt that chlorine and the tritium studies were very high priority items because of the indirect evidence that they're going to give us. How can I put this? You can do nothing further in this area unless you have accessibility to the site, is that correct? There's nothing more you can do at this stage?

DR. YANG: Right. We need a core so we can get the water. And, right now, we--

DR. DOMENICO: The old core is not sufficient for this?

DR. YANG: Well, right now, we are getting the old core from drilling during 1982. Those core, the UZ-4 and 5--so, they have been stored in core libraries. Now, we have tried to get this because now is the QA Level 1 or Level 3--so, it take a long time to get these core. Now, if we have some prototype hole, we can go in and drill it at the test site. Now, we can get this. Then, we can very roughly get some idea and that's what the purpose of--to get something going if we can get a permit.

DR. DOMENICO: So, I understand that the program basically is stopped until new core comes in?

DR. YANG: Right.

DR. DOMENICO: Is that the same with the chlorine, too?

DR. FABRYKA-MARTIN: Well, not entirely because I need to establish the meteoric chloride bromide and ^{36}Cl to chloride ratios. The chloride bromide ratios, I propose to do by surface soil sampling--

DR. DOMENICO: Which you're permitted to do?

DR. FABRYKA-MARTIN: I believe so. Now, as far as doing soil profiles, I don't know. Because the soil profiles, I'll need to get holes maybe down five meters or so to make sure I get below the bomb-pulse. That's the primary purpose of the profiles is to establish what that pre-bomb initial ^{36}Cl to chloride ratio was and to establish how variable it was. A secondary objective that falls out is the infiltration rate, but that's not the primary objective. And so, Dave, you don't think I need permits for that?

DR. DOMENICO: I didn't mean to bring this question up. It doesn't have to be answered. Because I don't think the chlorine bromide ratios are--well, they're important to your study, but they're not exactly what we would like. We would like the 36 ratio because that's the indirect evidence that gives you some indication of the movement of water through that block.

DR. FABRYKA-MARTIN: But, without those data, all I can do is give you a lower limit for velocity or an upper limit for age and that may be misleading or may give one a false sense of confidence.

DR. DOMENICO: Well, I think an average number is misleading, but it's not a question of that. I'm looking to see how--I'm just curious as to how deep that material has penetrated the block. That's more indicative, I think, than an average number. Thank you.

DR. YANG: Let me give you one more. Just for clarification, I'm not sure you're talking about UZ-1. The tritium data I presented there is not from UZ-1. That's from UZ-4 and 5. So, that's from--air. That's air drill. So, nothing affect that. So, I just want to make that point clear.

DR. LANGMUIR: I think we need to continue and I'm going to ask that we forego the coffee break. Those who would like a cup of coffee or need to stretch, please do so individually, and we'll proceed on.

We're now going to shift to testing in the saturated-zone and our first speaker is Claudia Newbury.

MS. NEWBURY: If I don't talk, we'll only be 15 minutes behind, but I'm going to talk. I'm Claudia Newbury from the Department of Energy. I'm only going to talk a minute, though.

We saw this slide yesterday and yesterday we talked about the unsaturated-zone and today, and the waste package, and the regional hydrology. The rest of today, we're going to talk about the saturated-zone and again the regional hydrology. Both these parts of the program contribute to the saturated-zone program.

Regional hydrology, yesterday we heard from Alan Flint and today we're going to hear from John Czarnecki on the regional groundwater flow systems and he'll be our first speaker. And then, before lunch, we'll get into some of the characterization of the saturated-zone groundwater flow system and we'll hear from Dick Luckey. Then, after lunch, we'll hear from Gary Patterson, M.J. Umari--he's not listed on here, but he's going to be speaking also--and Bruce Robinson from Los Alamos on some of the work that they're doing, and finally we'll move into the characterization of the hydrochemistry and that's Bill Steinkampf by the end of the day.

This is just a piece of the saturated-zone hydrology program and there will be other work that's done both in the surface-based work and in the--I guess, there isn't much in the ESF. Anyway, this is just a piece and it's an important piece of understanding the general hydrology of the system. I'll hand it over to John Czarnecki and maybe we'll get back on schedule.

DR. LANGMUIR: Thanks, Claudia.

DR. CZARNECKI: Good morning. I'm John Czarnecki. I'm the principal investigator for the regional groundwater characterization studies. What I'm going to do today is give an overview of the studies related to characterization of the regional groundwater flow system. May I have the slides and if you could dim the lights?

What I'd like to do is take you from the upgradient side of the flow system down a flow path and talk about how we would characterize flow along a flow path within the flow system and look at various components of the study that I'm in charge of. To do that, we're going to look at flow system geometry, the potentiometric surface of the flow system, how groundwater flow might be characterized using groundwater flow models that have been developed, look at recharge processes and the difficulties in estimating recharge, and end up down at the discharge end of the flow system.

To start off, this is a block diagram of the flow system in question with Yucca Mountain at the top of the screen. The area that I'm concerned with exceeds 5,000 square kilometers. We do have surface water drainage, episodic surface water drainage in the system characterized by big regional drainage systems, such as the Amargosa River and Fortymile Wash. Flow is typically from north to south from Yucca Mountain down to one of the primary discharge areas, Franklin Lake Playa and there are many uncertainties in a system this large and I'll point those out as we go along.

What we're looking at here is a map view of the regional system and what I'd like to do is show you some cross-sections, hypothetical cross-sections, that might extend from, say, Death Valley over to Ash Meadows to show you the third dimension of the hydrogeologic units. Just to point out for reference, Yucca Mountain is in the northern portion of this slide. But, if we go west to east through Death Valley, we have a number of units. This upper brown unit with the hypothetical vector of flow coming out of the slide would represent flow from Yucca Mountain from north to south.

Now, we've had many opportunities that we've taken advantage of of going and looking deep into the system--by deep, I mean 2,000 feet, 600 meters--and the opportunities came about through mining company drill holes and we've converted many of those into multiple piezometers and observed upward or the potential for upward flow from depth. Now, if that is the case, one needs to account for where that water may be coming from and, here, I've conceptualized that water possibly occurring from a deeper carbonate aquifer. Now, we know the aquifer exists at Ash Meadows where discharge is and at Death Valley on the far left side of the screen at Furnace Creek Ranch, major springs discharge at both locations. And, in some cases, the chemistry is very similar. So, this is just a hypothetical plumbing diagram, if you will, explaining how that might occur.

Now, if we look at that cross-section at 90 degrees, we might have something that looks like this (indicating) where Yucca Mountain is to the left side of the screen. The design repository area, 200 to 400 meters above today's water table and flow going from left to right with some discharge occurring at Franklin Lake Playa, some through flow, although minor through flow, occurring through Eagle Mountain. And, again, our upward component of flow from depth from carbonate rocks. And, this wedge (indicating) would represent the east/west wedge from Ash Meadows to Furnace Creek Ranch.

Unfortunately, we don't have deep wells yet that tell us what's at depth. Again, this is hypothetical. The data that we have to date are geophysical surveys, resistivity surveys, gravity, magnetic, and seismic. But, we do have an opportunity coming up which we hope to capitalize on where an oil company is planning to drill three holes into their target which is a paleozoic silurian unit that they hope they find with the intent of finding oil. So, that will be very interesting and useful for conceptualization and to see whether or not this sort of model actually holds up.

Now, we do have several uncertainties regarding the flow system. And, one is whether or not flow occurs from the Amargosa Desert to Death Valley and, if it does, how does it do it? Well, one possible mechanism is by way of a carbonate window through the paleozoic rocks and the Funeral Mountains. In order to understand that mechanism, we will need additional drill holes and, as yet, those are only proposed. There are no firm plans to do drilling in that area.

The other major uncertainty and this is one we may be able to get a better handle on is from where and by what flow paths does water beneath Yucca Mountain originate? And, what I'd like to do is share some thoughts on that and the work that I'll show here is work we've presented at the AGU Fall 1990 meeting. My co-authors were Bill Steinkampf from the USGS and Levy Kroituro from Weston. And what we're going to do is to look at this system from Pahute Mesa down to Yucca Mountain and to see whether or not water might make it down to Yucca Mountain and how it might occur.

Well, let's look at potential sources of recharge to Yucca Mountain. Pahute Mesa and Rainier Mesas are currently thought to provide about 50% of the water to this system that includes Yucca Mountain. And, this is based on models of the flow system that have been developed. In those models, Fortymile Wash was an important component of recharge and represented about 40% of the total recharge to the system. A third, but more minor component, occurs from paleo-recharge at Crater Flat and Yucca Mountain and a fourth one might be from upward flow from paleozoic rocks. But, in the models that have been developed, these two were considered to be minor (indicating). In fact, this wasn't even addressed in the models to date (indicating).

This is a view of Pahute Mesa. The reason for showing this is to contrast this sort of vegetation with that occurring at Yucca Mountain, substantially wetter, pinion juniper forest, much, much wetter, and logically should be thought of as a recharge area. We have a number of holes throughout this region. These are holes related to the weapons testing at the Nevada Test Site. Yucca Mountain holes shown down to the southern portion. We have holes out in the Amargosa Desert associated with mining interests. Notice the black hole around Timber Mountain. It does make life difficult to say what's going on between Pahute Mesa and Yucca Mountain, but we're going to give it a shot here.

Now, we can draw a potentiometric surface using that data and this is a back of the envelope computer run to draw a potentiometric surface. And, indeed, we have the potential for water to go from Pahute Mesa down to Yucca Mountain, at least that's what the contours show. Now, there are other potential flow paths one could draw.

Now, if we look at the general flow direction indicated from the potentiometric surface, again the arrows or vectors one might draw are shown here. But, I would put question marks on these largely on the absence of data around Timber Mountain. And, if one were to conceive of other types of data, other types of data points particularly, say, here (indicating) where we have a topographic high and make an assumption that the heads happen to be higher there, say 1300 meters for the sake of argument, we could put those in and contour these data points just to see what would happen. And, we can produce an island of potentiometric high.

Now, why am I interested in this? Well, it turns out that in other parts of the region, we do see potentiometric highs underneath areas like the Green Water Range further south and we don't have data here to say that this is not a possibility. So, even if it were a possibility, we might need to consider what it looks like in cross-section. Now, those five points might be drilled or located along Pinnacle Ridge. This is Crater Flat off to the south, Beatty Wash up to the north, and Timber Mountain where we have no data. But, even if we saw a mound, if you will, it may only represent a local divide that's superimposed on a more regional system and this water could, in fact, come from, say, Pahute Mesa.

Let me back up again. If we do drill holes, say, to answer whether or not there is a groundwater divide under Pinnacle Ridge, we need to keep into consideration the potentiometric distribution and how we might get reversals at depth.

DR. DOMENICO: John?

DR. CZARNECKI: Yes?

DR. DOMENICO: Do you have evidence of discharge at Beatty Wash or South Crater Flat?

DR. CZARNECKI: No, we don't. We have paleo evidence for discharge at South Crater Flat.

DR. DOMENICO: Wouldn't that conceptual model require discharge at both those places?

DR. CZARNECKI: I'm not sure it would.

DR. DOMENICO: Well, I see flow lines--

DR. CZARNECKI: I know, I know. This is not the best representation of this system. It's taken, I'll admit it, directly out of Fetter with names added to the top of the pictures just to get across the concept that we might have a local divide, but without surface discharge, maybe lateral flow. I don't know.

Now, another mechanism that we can use to characterize flow from Pahute Mesa, the potential for flow from Pahute Mesa, is to look at the groundwater chemistry. Now, there are many factors that affect groundwater chemistry in the area and I've listed those here. First and foremost would be the groundwater/rock interaction. The second one would be the reactions within the unsaturated-zone as water migrates from meteoric conditions down through the unsaturated-zone to the water table. We also have waters from various sources, various temperatures of input. This will affect the chemistry. Where you are within the flow system will certainly affect what the chemistry should be. Evaporation processes effect groundwater chemistry and then we have a problem of groundwater contamination during sampling.

Let's take a look at some data. This is not from Yucca Mountain. This is from Hanford and Bill Steinkampf provided this data to show what an ideal case would be if you had good control along a flow path from recharge to discharge --not even discharge, but tightly spaced holes from the recharge. And, if you look at calcium versus sodium, you get this nice sort of a curve. Now, we're going to look at data from Yucca Mountain in the next slide, but I want you to notice where we are on this axis. This is very fresh water. When you look at the Yucca Mountain data, we're going to be out here on the next set of axis (indicating). Here we are. We're already well into the 100 milligram range for sodium and what this suggests is that this method is not very useful for looking at these various data points throughout the upper part of the flow system to account for flow paths. It's too mature.

Let's look at another type of data that we might use and is commonly used, deuterium versus ^{18}O. No real surprises here. This water falls along a meteoric water line, if you will. This is a fairly expanded scale. This might look like evaporation to some, but it's largely due to the expanded scale for ^{18}O. If we look at chloride versus C_{14}, we can construct an evolution curve. Now, I want to point out where we are with end-members. These red dots on the far left correspond to paleozoic waters obtained from p#1. These purple dots correspond to drill holes in Fortymile Wash. Now, if we want, we might visualize that water at Yucca Mountain is a combination of waters from Fortymile Wash and those obtained in the paleozoics with upward flow and, in fact, that's what it looks like. That one might use this to construct that sort of argument. Where's Pahute Mesa? Well, these holes up here (indicating). It's pretty hard to show how water from Pahute Mesa evolves to form waters down in Yucca Mountain. It's hard to show that.

Let's look at another representation for chemistry data, C_{14} versus C_{13}. Again, we're trying to show a mechanism to get water from Pahute Mesa down to Yucca Mountain. Let's take a look at Pahute Mesa data. Now, when you're looking at data presented in this sort of way, the reason for doing this is to make corrections for apparent age or age in C_{14}. If you have contamination of old carbon, such as those red dots up in the far left corresponding to paleozoic rocks, they correspond very close to the rocks themselves, the carbonate rocks. These indicate that very little correction is needed. Now, if that's the case, then we go from Pahute Mesa to, say, Fortymile Wash which is downgradient or even Yucca Mountain. We've got water that's in this case older for Pahute Mesa than Fortymile Wash, and if we make the correction, we're going the wrong way. We're going from older to younger down the flow path and that doesn't work.

Another way of looking at mixing, we need to look at end-members again. Here we are with the carbonate waters of p#1 and Fortymile Wash out here, U-20a#2 from Pahute Mesa. It would be tempting to construct a mixing line like the one we showed here. But, look where Yucca Mountain water falls, off the mixing line. Now, there are other waters in Fortymile Wash. This happens to be upgradient, UE-29a#2. That's our most upgradient hole in Fortymile Wash. J-12 is down here. One might construct a mixing line something like this where p#1 down to J-12 showing the relation of mixing paleozoic waters with Fortymile Wash waters. But, it's very difficult to show--well, it's difficult to show how Pahute Mesa waters can get down to Yucca Mountain waters without some other influences.

So, if we can make any conclusions, at all, on this it's that water from Pahute Mesa possibly does not flow directly to Yucca Mountain. If waters from Pahute Mesa actually flowed to Yucca Mountain, they might be mixed with waters from Fortymile Wash. Now, we have other possible sources of recharge and those would be local. And, the contribution from those sources is probably minor.

Finally, as I pointed out earlier with the big, black hole around Timber Mountain, our current conceptual models of flow, that is flow from Pahute Mesa to Yucca Mountain, cannot be supported without additional data. And, we do have plans to obtain that data.

DR. LANGMUIR: John, before you go on, you might want to consider--I know it gets fuzzy. You can make calculations, obviously, of three or even more component mixtures which may actually be what's going on. You don't just simply have two mixtures here. You have a series of mixtures which may vary spatially in terms of where you are in the mountain. And, some of that can be handled fairly straight forward algebraically.

DR. CZARNECKI: Um-hum. Yeah. We're not done with the current data set. In fact, we'd like to put this together in a little more refined form and look at some of these different types of analyses like you're suggesting. On the other hand, we would like more data. Everybody wants more data. And, this is where we'd like to see it. These southern-most holes, two of which are planned, would help resolve not only the upgradient flow question, but the question related to the large hydraulic gradient which I'll be talking about in a bit. But, here, we're talking about additionals, one out in Crater Flat, three up in the Pinnacle Ridge area, partly to talk about the groundwater divide question and to look at gradient issues. These CW holes which now have changed their name to something else are proposed by the weapons program as part of their environmental restoration program or environmental monitoring. I've forgotten the term. But, these will certainly be of help in terms of characterizing regional groundwater flow and hydrochemistry.

Well, let's move on and look at the large hydraulic gradient at Yucca Mountain. This is a site feature where we have a 300 meter change in hydraulic head over a distance of about two kilometers. The cause of the large gradient is not understood completely. We don't have a firm cause from data or we don't have the data to show where it is. However, it's probably structurally controlled to some extent. And, if it is indeed structurally controlled, it could be structurally alterable and the main thing is that it's upgradient from the design repository area.

Let's take a look at it. This is a regional potentiometric map of the Yucca Mountain and vicinity flow system. Now, we're looking at contours in meters and notice the bunching together just north of the design repository area. I should add that this is not unique to Yucca Mountain. We have large gradients elsewhere, particularly on the Nevada Test Site, but there we have known causes, 10,000 feet of Eleana formation. That hits you right in the face. You have an immediate cause. Here, we have no immediate cause.

The data at Yucca Mountain literally points out the potentiometric rise. Here, we're going from a very flat surface, 700 meters, 730 meters, to an abrupt change, 300 meters higher. Two control points, WT-6 and G-2 are on the upgradient side. UE-29a#2 is shown up Fortymile Wash at 1187 meters continuing the potentiometric surface trend. Now, we can simulate this and one of the mechanisms that we envision to help explain this sort of a feature is shown here where we have a normal fall. There are many explanations potentially, but here it's a normal fall (indicating). Now, the question is what could happen if, indeed, this were a normal fall and the hydraulic properties across this surface were to change such that the hydraulic conductivity increased. And, that's of concern. And, here's the public's version of that concern showing where we have the water table substantially elevated above the design repository area.

Now, we're going to look at a problem where we take that barrier out of the ambient condition flow system. And, to do that, we're going to report on a two dimensional model of groundwater flow that's been published. I think the paper has circulated here. I want to focus on this part of the model area in this rectangle (indicating) and look at what happens to the potentiometric surface and to vectors of groundwater flow in that block.

Let's take a look at the material properties before we go any further. These are transmissivities of the baseline simulation condition in m^2 per second. What we have to represent the large hydraulic gradient is this orange wedge which is about 20 times smaller than the transmissivity of the area to the north in red. And, if we simulate this arrangement for the potentiometric or for the transmissivity, we get this sort of a flow field. And, this is straight out of Czarnecki & Waddell, 1984, and, obviously, the barrier has a large impact on the direction and magnitude of flow right in the vicinity of the repository.

Now, I gave a paper at AGU in the spring of '89 where I looked at these ambient conditions and took this barrier out and watched the result on flow and the water table rise. To make a long story short, I took that out and looked at a point in the block, and if we look at the water table rise resulting from the removal of that barrier--and, this is change in water table elevation or hydraulic head with time--we see a 40 meter rise at that point in the block. Irregardless of what the storage coefficient is specified as, the rise is independent of the storage coefficient.

Well, this was somewhat good news for me or maybe for the project, but I didn't think it was as bad as we could have made it and I thought, well, I've done some simulations related to increased recharge related to water climatic conditions. What would happen if we made the initial conditions for the flow system such that they correspond to much wetter climatic conditions, use those as initial conditions, and then remove the barrier? Well, I'd like to share the results of some simulations that were presented at the spring 1991 AGU meeting.

To do that, we started with initial conditions shown here taken directly from a model that was published in '85 by me on much wetter climatic conditions. Here, we're looking at a precipitation environment that's twice as wet as today, but results in 15-fold increase in recharge over what was specified in the ambient condition model. We have much higher heads. Recall that heads here were on the order of

730 meters. We're about 120 meters to start with right around the block. So, the simulation involves a 15-fold increase in recharge which incidentally continues with time through the simulation. We're going to assume that that is the steady state initial condition and that the barrier is removed at time zero and watch the response of the system.

Well, when we remove the barrier, this is what we get, much larger vectors of flow. We have to account for that because we have more flow into the system. We have to remove more water out of the system through our constant head notes and this is what happens. So, what we're going to do is follow these vectors with time and step out, essentially, exponentially. So, here, we're very early in the simulation looking at large vectors of flow. Let's watch what happens as we go on.

They actually increase as we go through early portions of time. Here, we're at 14.2 days. To make it easy, you don't need to memorize the size of these vectors. I'm going to have a point here again within the block where we'll look at change and flux with time. But, this is to show you, more or less, what the simulations show in terms of change in groundwater flow direction. Again, going out in time exponentially 219 days, vectors are somewhat larger. Larger still at about 1300 days. Then, moving out to 3,000 days, I believe they're starting to dissipate somewhat down in the block area. Again, the response of the system here is a function partly of the storage coefficient that's specified, but the overall change in flux and head, the magnitude, would be the same regardless of the storage coefficient. It's, more or less, a damping factor. Now, we're going out 50,000 days or more into the simulation and you see the vectors have subsided substantially.

We do have some big ones cropping up. I'll point them out. These are from Fortymile Wash (indicating) where we're still inputting the 15-fold increase in flux over today's .4 meters per year recharge. That's a lot of water. And, it does have a major impact. And, to go out close to 10,000 years. Actually, what, 4E6 would be closer to 10,000 years. The system has dropped back substantially.

Now, looking at flux versus time at that point in the block, we see a little change early in exponential time and then at a rapid increase out at 3,000 days followed by a falling off and what appears to be a new base level about several million days, thousands of years into the simulation. This, by the way, was with the storage coefficient of .1.

Now, another thing that I looked at in this simulation is the role of the barrier itself and what would happen if we removed just a piece of the barrier leaving this much in--well, let's see, yeah, this much in and getting rid of this little piece and looking at what happens. And, this is to compare the flow paths around a partial removal versus a full removal and the effects are fairly substantial. Now, we can see this a little better again with a change in head versus flux or head versus time at a point in the block. And, I'll show that in a bit, but that's to illustrate the effects of full removal versus a partial removal of the barrier.

Before I do that, I'd like to show you contours of change in head with time as we go again through the simulation results. These are contours in meters of the difference between the simulated hydraulic head and today's water table or today's ambient simulated conditions. As I mentioned earlier, the initial conditions under the much wetter climatic conditions put the water table about 120 meters higher in the vicinity of the repository. Now, this is right after the barrier is removed at .089 days. As we step out in time out to 14 days in the simulation, notice this 120 meter contour coming down a little bit from where it was down into the block. As we go further in time, out to 219 days, 120 meters is still creeping down. Notice what's happening upgradient to these contours. There's actually a subsidence in head, as you would expect. You would like and intuitively expect heads to drop.

As we go further in time, what happened to our 120 contour? It went back up. It's actually now getting absorbed up here (indicating) and now the rise is only 110 over the initial conditions with major drops occurring to the north. 3,000 days into the simulation, more drops to the north, not much change down in the block. 2,000,000 days into the simulation--I skipped a few there--conditions are much different than what we started with, with heads below what we saw for the initial conditions under ambient groundwater flow. The analogy I like to think of is what happens if you pull a big rock out of a stream only the stream is full of jello and it's moving? It takes a while for it to re-equilibrate, but it comes to a new state of equilibrium. And, this might be what one could expect.

I promised I'd show you a slide of changing head versus time under the full and partial removal of a barrier and this is what we see. Under the full removal of the barrier, we get our maximum rise and it's not much more over what we had for initial conditions. Whereas the partial removal causes a drop that never really goes any higher than the initial conditions. Now, I've been wrestling with the reason for that and I think the cause is related to the fact that the transmissivity is substantially augmented under the initial conditions with higher heads and it's able to accommodate the flow that's caused by the removal of a barrier.

To summarize these results, the head change that we see, at least when we're looking at coupled climatic systems and increases in hydraulic conductivity possibly related to tectonic events, the head change is dominated more by the increased recharge conditions than by the change in hydraulic properties across the barrier.

The second point is that depending on how you remove the barrier will have effects on what the resultant change in head will be and a full removal of the barrier results in a larger head rise slightly than a partial removal.

Third, the maximum flux that we see underneath the repository occurs several years after the removal of a barrier of this sort. And, it's also influenced by the storage coefficient that one specifies in the simulation.

And, lastly, at least under these preliminary simulations, the repository apparently would not flood. Now, I need to stress that these simulations are preliminary and there are many other factors that we need to consider in analyses of this sort.

Let me move on and go further down the flow system or actually look at a little lateral component and that's Fortymile Wash recharge.

DR. LANGMUIR: John, can you speed it up just a bit? You're getting close to your 45 minutes.

DR. CZARNECKI: Yeah, I'm almost done.

Fortymile Wash is considered to be a potential source of recharge and we have evidence to that. And, one line of evidence is tritium data shown here. The UZ holes up in Fortymile Wash have elevated tritium levels. UE-29a#2 has 200 picocuries per liter at 65 meters depth and right adjacent--I'm sorry, UE-29a#1 has it. UE-29a#2 has a lower tritium concentration, but it was drilled much deeper, 421 meters versus 65. So, as you go deeper in the system, you see less tritium. It's what you'd expect for a recharge condition.

The same thing for C_{14}, younger waters, apparently younger waters, occur in shallow UE-29a#1, 75% modern carbon, versus 62% modern carbon in the deeper UE-29a#2. I should point out that these are composite samples. The entire water column was sampled. It certainly helped to see the profile discretely in these wells and we can do that. We have tools to do that and we have plans to do that.

Another line of evidence that suggests recharge is the dropping in head with depth, again composite heads, but in UE-29a#1, the head is four meters higher than it is in UE-29a#2. They're nine meters apart. The two boreholes are mine meters apart. And, depths to water are only 24 meters.

Now, we do have a series of tests and activities planned for Fortymile Wash and I've shown those here. There are a series of deep holes planned, the FM series holes, and these would go down to the water table at the three locations along the various reaches of Fortymile.

We also have a series of neutron holes that are planned to look at recharge processes by monitoring water concentration changes. And, these will be located such that in the upgradient side of Fortymile, we're likely to intercept water at the projected 50 meter depth of these holes.

We also have ponding and infiltration testing scheduled in conjunction with neutron hole locations where we'll have a neutron hole surrounded by a tank of some sort and monitor infiltration processes.

Fourthly, we're planning to look at in detail hydrochemical distribution with depth along Fortymile and, fifth, we're planning to do some extensive testing in these holes in Fortymile to establish hydraulic properties.

Now, I did want to mention something about the discharge area of the flow system. This is Franklin Lake

Playa where we've done extensive work and have published several papers on this. One is coming out as a water supply paper. What we did was to try to characterize the discharge at Franklin Lake Playa and what drove us to that work were results from this transmissivity versus flux multiplier sensitivity analyses that were done for a model of Yucca Mountain and vicinity. By changing the values of flux in a model of this sort, you can determine how sensitive the parameters are to what you're trying to look at. In this case, transmissivity near Yucca Mountain was being calculated by the parameter estimation model and it's an important parameter for estimating groundwater travel time. To make a long story short, this curve representing change in flux at Franklin Lake Playa suggested that it was one of the most important parameters in this model and we needed to refine it. We did that by going out and measuring evapotranspiration using energy budget, Eddy-Correlation, and this is Dave Standard who is a co-author with me on a paper characterizing the hydrology and the evapotranspiration occurring here. We used a variety of methods. Here, we're drilling holes, Bill Whitfield at the drill rig, where we're looking at not only depths to water, but changes in head with depth and, in almost all cases, we see an increase in head with depth as you would expect at a discharge area. The

Playa is dangerous. You get stuck out there. It's also dangerous to measure water levels. This is a well where we produced water above land surface. Water level is about here (indicating) and this is water that flowed out of the well during construction. The results of this analysis or these studies show that evapotranspiration or evaporation at Franklin Lake which occurs mainly as bare soil evaporation ranges from one to three millimeters per day throughout the year.

We need to look at areas outside of Franklin Lake Playa, too, to get a better handle on how widely distributed this ET is and we would like to go to areas where groundwater is not discharging as ET and a likely place is Jackass Flat. And, Alan Flint has plans--in fact, he's probably got instruments running--to determine baseline--what I would consider to be baseline--ET related to xeriphyte discharge. There will be some ET, but we'd like to know what that is. It's not going to be zero. So, as we go out along the periphery of Franklin Lake Playa, we'd like to know how those peripheral measurements compare with true non-groundwater discharge ET conditions. So, we'll be doing that through the use of Bowen ratio stations. We'll also go out and construct piezometers and tensiometer nests to get at locations where we have upward components of flow based on potential. And, we hope to see areas where this is no upward potential in the shallow system. I mean, that would be the ideal. And then, thirdly, to get to the aerial distribution of ET, we hope that phreatophyte mapping and maybe analysis of satellite data might help us in characterizing this area.

And, I'll stop there. Thanks.

DR. LANGMUIR: We have time perhaps for one question from the board from someone at the table here.

(No response.)

DR. LANGMUIR: If not, we're right on schedule.

Let's continue and have our last presentation of the morning. Claudia?

MS. NEWBURY: Yes, our next speaker is Dick Luckey from the USGS and he'll be talking about site potentiometric value level evaluations.

MR. LUCKEY: I'm not sure if it's tougher to be the last person before lunch or the first person after lunch.

I guess this proves who I am. Let's try to put this activity that I'm going to talk about into perspective. It's part of an investigation of the hydrologic system at the scale of the site. That's one of three investigations involving the saturated-zone. The study that we're involved in is called Characterization of the Site Saturated-Zone Groundwater Flow System and that's one of three studies in the investigation. That study is then divided into eight activities. We're going to be talking about one of the eight activities. We'll be talking about Gary Patterson's. M.J. Umari will be talking about other activities, as well as other speakers. The point I'm trying to make is that this is only a small part of the saturated-zone studies.

The site potentiometric level evaluation is to define the potentiometric surface in the vicinity of Yucca Mountain and, particularly, the uppermost potentiometric surface. We want to determine if any long-term trends in water levels exist that would affect the amount of unsaturated-zone between the repository level and the saturated-zone. We want to analyze water level fluctuations to try to understand what causes fluctuations and, if possible, use water level fluctuation to estimate hydraulic parameters. All of this provides input that's ultimately going to be needed to calculate groundwater travel time.

I'm going to be talking about a couple of different kinds of networks as part of the site potentiometric level evaluation. First of all, the periodic water level network which currently consists of monthly measurements. Previously, measurements were made twice a month in this network. This network dates back about 10 years. The other network that I'm going to be talking about is the continuous water level network. It's not really continuous. What we have there is hourly water level measurements. This network dates back to 1985.

I'm going to talk about a couple of different kinds of wells in this network. The water table wells that are drilled a short distance into the water table relatively shallow, there are only surface casing in these kind of wells that has an impact on how the data are analyzed. The other type of well are the hydrologic or geologic wells; the H Series, the G Series, the p#1, b#1, those kinds of wells. These are relatively deep wells, penetrate deeply below the water table and are cased below the water table.

We've been collecting data for about 10 years starting in 1981. We have, so far, released the periodic water level measurements through 1988. These have been released through two published reports. The periodic water level data for 1989 has been approved for publication and camera ready copy is currently being prepared. The continuous data through 1988 is about to be sent to DOE and the USGS director for approval. A couple of weeks ago when we put these slides together, we thought it's going out next week. Well, it didn't go out last week and it probably won't go out this week. So, we're almost there. Maybe, we'll make June, I doubt it. It will probably be July. The continuous data for 1989, the report is currently in preparation and the 1990 data is still being processed for both networks. It takes a fair amount of time to process this data, check it, and make all the appropriate adjustments and corrections.

We'll talk a few minutes about the periodic water level network. Currently, the periodic water level network consists of 16 wells that are measured monthly and three wells that are measured quarterly. The preferred instrumentation in the period water level network at the present time is steel tape measurements. These measurements have both high accuracy and high precision associated with them. We use a 2600 foot steel tape, adjust for mechanical stretch of the tape, thermal, expansion of the tape, borehole deviation. We have high accuracy determination of the altitude of the reference point. I'll show you a little bit of data from that. The periodic water level network is very useful for determining if long-term water level trends exist, gradients between wells, this is the kind of information that will be used primarily for travel-time calculations.

This is just a quick map of the periodic water level network. Several of the wells are off this map. They're scattered kind of throughout the area of Yucca Mountain.

This is an example of the periodic water level measurements at Well WT-17 from 1983 through 1988. I want to point out a couple of things on this graph. Note that we have sort of a change in baseline between here and here (indicating). In mid-1985, we switched measuring equipment. Prior to mid-1985, the equipment that we used to measure water levels was a multi-conductor cable, kind of a wire line sort of tool. In mid-1985, we switched to the steel tape. We had more variation in the water level with the older equipment. That leads me to believe that the older equipment is probably less precise. There's probably also a slight shift in here that occurred in several wells. There's probably a slight difference in calibration between these two. Since mid-1985, we have these water levels. Note that this is .5m here, 2.5m full scale on this graph. So, we're looking at changes between measurements on the order of .1m, a couple of tenths of meters maximum. This is sort of the range of water level fluctuations that we see in the continuous water level network due to barometric causes.

Let me go on to the continuous water level network. The continuous water level network currently consists of 12 wells. We're monitoring 19 zones. The hydrologic holes are split into multiple zones from two to four zones. That's why we have more zones than we have wells. The measuring equipment in the periodic network consists of a down hole pressure transducer that measures the depth of submergence, a data logger or data collection platform at surface, and as much as 2500 feet of wire line cable connecting the two. We currently calibrate these systems every four months. The calibration includes making a manual water level measurement just like we would in the periodic water level network and also determining the relationship between change in submergence of the transducer as the water level changes versus change in transducer output.

As I mentioned previously, the continuous water level network really consists of hourly measurements that were plotted over time. They look like they're continuous, but they're not really, truly continuous. In some special cases, we are getting truly continuous data in graphical form. It's much more difficult to work with, but we do get some truly continuous data. And, on special occasions, we also collect some high frequency, but again discreet, digital water level data. For instance, if we know that an underground nuclear test is going to take place, in some cases we have collected data on the order of one second and try to monitor the effects of this. This is just a map that shows the locations of the continuous water level network. Again, they're scattered throughout Yucca Mountain concentrated closer to the repository block.

The continuous water level network is designed to observe short-term water level fluctuations. There's two primary causes of short term water level fluctuations at Yucca Mountain. First of all, there's barometrically induced water level changes. We have barometric pressure changes that occur daily to a few days as a storm front passes through. That's the largest driving force for water level fluctuations at Yucca Mountain. A smaller driving force is earth tides. Like ocean tides, these occur twice daily with a cycle that kind of repeats itself about every 14 days.

Let's look at short-term water level fluctuations. This is March 1988 barometric pressure. It's inverted, so 900 millibars, 840 millibars. So, this is increasing barometric pressure. This is the water level change at Well WT-11 for the same time period. I hope you'll notice that those graphs look quite similar to each other. The scales were chosen so that this would represent roughly 100% barometric efficiency.

Let's look at calculated earth tides at Yucca Mountain. I want to stress that these are calculated values. We can't observe these directly other than the water level record. This is March 1988, March 1 to March 31. I forgot to put those on the slide. You can see the earth tide goes through a maximum, minimum. Fourteen days later, we're into a maximum and another minimum area.

This is the water level at Well p#1 for March, 1988. Notice that we have high daily fluctuations during times of high earth tide. We have low daily fluctuations during times of low earth tide.

Let's also look at the barometric effect. You can see we get large barometric effects that transforms into large water level changes. So, what we're seeing at Well p#1 is a nice combination of barometric effects plus earth tide effects. I think that Gary Patterson will be showing you what the record looks like if you take the barometric effects out of the water level record and just look at the earth tide effects.

I mentioned that we use either data loggers or data collection platforms to control the transducers and collect the data out of the transducers. I'll talk a little bit about data collection platforms. The data collection platforms are nice in that they give us near real time access to the data. With the data loggers, it's two or three weeks before we can even examine the data. With data collection platforms, the platforms transmit the data to satellite, it's re-transmitted back to a ground station, and ends up in our computer about four minutes after it's transmitted. During normal operations, the data collection platforms transmit their data every four hours. They transmit the last eight hours of data in case we have a solar storm or something like that that interferes with the transmission. Under an alert operation, we use the flood warning channels that's normally used by surface water people to transmit the data immediately. I'll talk a little bit more about what alert operations is. This is when we have our water level excursions. Under normal circumstances, we examine that data daily. Under these special circumstances, we examine it every few hours.

This is a map of the location of the data collection platforms. I'd like to point out Well G-3 on the south end of the crest of Yucca Mountain. I'm going to be talking more about it. Just for the record, the first data collection platform went in in January of 1990; the most recent one is only a couple of months--has been in service only a couple of months. The reason that we decided that we needed data collection platforms is to try to determine something about water level excursions. In the last nine months, we've had about half a dozen water level excursions or apparent water level excursions occur at Yucca Mountain that we've been able to track through the platforms.

I'm going to talk a little bit about these water level excursions. I want to point out, note the quotations around water level excursions. That's probably a real bad choice of terms. I regret it. I probably should call this transducer output excursions because for the most part we don't know whether these are true water level excursions or not.

For purposes of discussion, I'd like to break them up into four types. Type 1 is a dramatic, but expected response to barometric pressure changes. Rapid changes in barometric pressure cause fairly dramatic changes in water levels. We can explain these things through the physics of the system. Types 2 through 4, we can't explain. Type 2 is a low amplitude excursion that occurs concurrently, basically concurrently, with barometric pressure changes, but the amplitude of them exceeds the amplitude that would be expected only given barometric pressure changes. A Type 3 water level excursion is similar to a Type 2 in its amplitude. It's a fairly long amplitude. The difference is that it's not concurrent with barometric pressure changes. A Type 4 excursion is a high amplitude excursion.

DR. DOMENICO: Don't you have a high level expected excursion like when they set off nuclear weapons or earthquakes from San Francisco, Los Angeles, Mexico, et cetera?

MR. LUCKEY: In these kinds of wells where they're not packed in, we wouldn't really expect a large water level change from these kinds of things because these occur so rapidly and they are over so quickly that the momentum of the water in the borehole kind of totally damps them out.

DR. DOMENICO: You're taking hourly measurements?

MR. LUCKEY: Hourly measurements. So, the chances of picking one of these up are just about nil anyway. I think that Gary Patterson will show that these sorts of phenomena last seconds, a few tens of seconds. So, to pick something like this up on an hourly measurement would be difficult.

DR. DOMENICO: Thank you.

MR. LUCKEY: I'm, first of all, going to show a Type 1 excursion which is a dramatic, but expected response to water level change. I've kind of already shown that sort of thing previously. Here, we have the barometric pressure plotted. This time, plotted correctly. So, this is a large barometric low that came in about January 17 of 1988. The water level in WT-2 rose dramatically in response to that. This is all expected. The only thing unexpected is a very tiny little spike here on the order of less than .1m. This is probably something different. It doesn't correspond to what is seen on the barometer.

Type 2 water level excursion is also concurrent with barometric pressure changes. In this case, if we convert the transducer output to water levels, it would be more than we would expect just from barometric effects alone. We're looking at late February/early March, this year, at Well G-3 on the south end of the crest of Yucca Mountain. We have transducer output plotted over here in millivolts. So, it was going on with a kind of normal expected response at about 2 millivolts. It jumped around pretty badly as this low came through. It never really settled down. I'm not saying this is a water level change. I'm going to come back and talk about this a little bit more in detail. But, if it were a water level change, just for scale this would be .3m water level change. I'll come back to this particular excursion.

A Type 3 water level excursion is also relatively low amplitude excursion, but it's not concurrent with barometric pressure changes. This is kind of an interesting excursion that occurred in 1988 in early March. I lost my little bar over here. If this were true water levels, this would be about a .5m change in water levels if it were real. We don't know if this is real. We only worry about these excursions when they occur in more than one of a well or if they occur at several wells roughly concurrently. If this was just an isolated incident in just this one particular zone of this one particular well, we'd write these things off as instrument malfunction.

DR. DOMENICO: Is each of these wells measured manually each month, each of the ones we have the continuous records?

MR. LUCKEY: No. Only every four months during the period--

DR. DOMENICO: So, if something happens to the transducer, you'd have no way of knowing for four months?

MR. LUCKEY: We collect the data. We collect the transducer output every other week in these wells.

DR. DOMENICO: What I'm saying is as a backup? Like, for example, the thing I'm looking at right there shows that you have a, I don't know, a quarter of a meter rise, if you want to look at it--so if you were making measurements every month manually to check your transducer, you would pick that up and you would know whether that was real or make-believe.

MR. LUCKEY: Yeah, if we could get out there quickly enough, we could make a manual measurement and do this.

DR. DOMENICO: But, what I'm asking is--

MR. LUCKEY: Under normal operations, no, we do not.

DR. DOMENICO: So, there's no scheduled manual measurement of water levels in the continuous water level holes? There is none?

MR. LUCKEY: There is scheduled, but it's four months apart.

DR. DOMENICO: Four months, thank you.

MR. LUCKEY: The logistics of all of these holes mean that to get a water level measurement you have to disturb the transducer. It's a fairly intensive operation. Now, had we seen this one coming, if we'd gone out here and noticed it right at this time, we would have made an unscheduled manual measurement to find out if this offset was really true. This is the problem with doing the data logger. You can see that we only have five days from here to the end of the graph. This thing returned down to a base level within about 10 days. You dump your data loggers every two weeks and it takes you a week to get around to looking at the data. This thing is long gone. That's one of the advantages of having the data collection platforms. You see something like this immediately, you go out and investigate it immediately.

This Type 3 excursion is a fairly rare excursion at Yucca Mountain. A handful, at most, there we're interested in. These excursions look somewhat like fault creep or slow earthquake events. I'm not saying that's what they are. They have some of the same characteristic shapes of fault creep and that's why we're interested in those.

Type 4 water level excursion is a high amplitude excursion. This is an example of p#1 in April of 1987. Again, transducer output goes down to -20 on the bottom of the scale, +30 on the top of the scale. In reality, in this region and again down in this region, it was fluctuating back and forth to +50 millivolts/-50 millivolts which is the maximum/minimum output of this particular instrument. For reference, this would be .3m water level if this were real. I say it's highly unlikely that these Type 4 excursions are real. If we made the water level go up enough, we actually could get a 30 or 50 millivolt output of these transducers. However, these transducers when they're hung in air, they're vented transducers. The output is nominally about zero. So, to get down below zero, you've got to suck on this transducer real hard and I can't imagine anything that would give us that sort of suction on a transducer.

I promised we were going to go back to the excursion at G-3 that occurred late February/early March of this year. Just for reference, this is the barometric pressure plotted correctly. This is how much water level change that barometric pressure change would result in assuming 100% barometric efficiency of this well. The wells out at Yucca Mountain do have a fairly high barometric efficiency. So, that will give you some idea of what we should expect in terms of water level change given just barometric pressure change.

On this side, we have the transducer output from this well (indicating), again from 1 to 5 millivolts. Just to get the scales correctly, this would be .3m change in water level if this were converted to water levels. Because this occurred at station on the data collection platform, as soon as this occurred, we became aware of it. We immediately within a few hours went out to the site to try to determine what was happening and three visits over a two day period were made to check the instrumentation at this site.

DR. ALLEN: This was not occurring at any other site?

MR. LUCKEY: We didn't--no, it was not occurring at any other site where we had a data collection platform. At that time, we had, I think, four data collection platforms, maybe five platforms. It was not occurring at any other site where we had a platform. This phenomena was occurring at Well b#1 where we did have a continuous recorder; b#1 also is prone to excursion so we went over and looked at the graphical chart of b#1 and it was occurring there also. I can't tell you off the top of my head whether it was occurring at other sites.

Right here, we're just looking at the same thing, transducer output only for March 1, '91, which is in this most dramatic part. I said we visited the site three times during this excursion, once on February 28, twice on March 1. During those visits in the morning and afternoon of March 1, we did make manual water level measurements. We took those manual water level measurements, converted them back to what the transducer should have been reading given that water level measurement. That's what these crosses represent here (indicating). So, this would be a manual water level measurement converted to millivolts of transducer output. So, would this (indicating). This gap in the data here represents when the transducer was off scale. The way the data collection platform was programmed to operate, it could read output voltage up to 5 millivolts. So, it was off-scale or possibly down to -5 millivolts.

DR. DOMENICO: What sort of water level change would pop your transducer?

MR. LUCKEY: This particular transducer is a 15 psi transducer submerged about five feet. So, we'd have to be looking at several tens of feet to pop the transducer. Most of these things can over-range from four to 10 ten times without damage and 10 to 100 times and have damage, but continue to operate.

DR. DOMENICO: Thank you.

MR. LUCKEY: Now, we had some backup because we were there. The people doing the field work grabbed a multimeter out of the toolbox and measured the transducer output voltage directly. In this particular case, just prior to making this measurement, the transducer output was registering 10 millivolts.

If you take this 10 millivolt transducer output and convert it to a water level, it would indicate that the water level was up about 6m at that time. The observed water level, via the manual water level measurement, indicated that the water level was up about .3m from its sort of baseline position. This .3m is very consistent with the response expected given that sort of a front coming through. The notes from that conclude that this excursion beyond the expected water level change given the barometric pressure change was not real. This does not mean that all water level excursions are not real. It does mean that at least this one was not real. It gives us some confidence that at least some of them are not real. We have to continue to remind ourselves to not write off all water level excursions based on one data point.

Okay. Where are we going to go in this particular activity in the future? We're going to continue to monitor water levels at all sites to determine if we have any long-term trends. That's periodic water level network. At least, some of the sites, we're going to remove the continuous monitoring network, put it on other sites. Some of these sites have had continuous monitoring for about six years. We've had lots of problems in getting continuous record.

But, for the kind of analyses that we do at three, six, twelve month period, it's plenty of record. So, at least, at some of these sites, we're going to start moving the equipment around.

Current plans are to augment the water level network with anywhere from eight to 14 additional wells to try to help us with our understanding of the system, fill in some holes. As mentioned yesterday, we're going to be monitoring water levels in both the unsaturated-zone and the systematic--or at least some of the systematic drilling holes. We'd like to initiate strain monitoring to directly measure earth crustal strain. We're going to continue to place a high priority on determining if these water level excursions are real. We would like to be able to say at some future time if fault creep is truly occurring at Yucca Mountain.

We need to take our new data that we have collected over the last several years and produce an updated map of the uppermost potentiometric surface at Yucca Mountain. The currently available map is a number of years old. The data is probably better now. We're going to continue to analyze the water level fluctuations to estimate hydraulic parameters. Gary Patterson will be talking about things like that. That will continue in the future. We need to investigate the possibility of estimating hydraulic parameters from the water table holes. Their construction is such that we can't estimate hydraulic parameters from those wells because they're not cased below the water table. So, we're going to see if we can come up with some way of doing that. In a related activity, we're going to investigate the role of faults in the saturated-zone flow system, specifically the Solotario Canyon Fault, but also the Ghost Dance Fault.

DR. LANGMUIR: Thank you, Richard.

We have time for a question or two. Questions from the table?

DR. DEERE: You pointed out in a few holes you're making more than one water level measurement. Could you explain that a little bit?

MR. LUCKEY: What I meant to say is that we're measuring water levels in more than one zone. These hydrologic holes, several of them we have a packer to separate the hole into the upper and lower intervals so we can see what the water level is at depth versus the shallower water level. Well, H-1 up near UZ-1 that we've talked so much about is completed as a piezometer nest. There's four piezometers completed and that's how we know the water level at four different intervals from very deep up to near the water table.

DR. DEERE: Do you have results from those yet that show anything interesting?

MR. LUCKEY: Yeah, we have a lot of water level information from all of those. The results range from virtually no difference in water levels between the upper and lower zones of the well up to, in H-1, several tens of meters. I should know this off the top of my head. I think there's 55m of head difference between the top and the bottom of that hole and it's an upward gradient. We see upward gradients throughout the area.

DR. DEERE: And, that answer then leads me to my next question. Have you considered putting one in that can measure at more than four intervals? For instance, 10 or 15 positions where--it seems to me when we have a stratigraphy that has different fracture characteristics that it might be of interest.

MR. LUCKEY: It really would be of interest. We start running into just some logistical problems. Four tubes in a hole that we're talking about is kind of pushing our luck.

DR. DEERE: No, I'm talking about another type of system like the multiport, like the Canadian installation?

MR. LUCKEY: Yeah, yeah. We probably will get some information of that sort when hydraulic testing is done. Just kind of a one shot kind of information when packers are put in or I think they'll be talking about the packer system that will be used in the C wells. We'll get a little bit of information from that on a one-time basis. Bill Steinkampf and his hydrochemical sampling is going to have a similar sort of setup to where we can measure water levels in fairly short zones. But, again, that's kind of a one-time thing.

DR. DEERE: Well, I think it would be interesting to consider the use of this. Certainly, wherever it's been used, people have been surprised at the complexity of the groundwater movement, particularly in a fault. The groundwater situation near the fault and above the fault and below the fault has proven to respond quite differently from different events.

MR. LUCKEY: Yeah, I believe in this kind of a fracture media that would be very useful information.

DR. DEERE: Thank you.

DR. ALLEN: You made the statement that some of these excursions looked like events that were similar to those that could be related to fault creep. What's the basis for that statement?

MR. LUCKEY: It's only a visual comparison with some published information that comes out of California where they show the transducer output during fault creep events.

DR. ALLEN: At hourly intervals?

MR. LUCKEY: I think they have much more detail data than that. But, it does cover periods of days. These sorts of events are relatively long-lived compared to normal seismic events. I think that if Gary Patterson hasn't sufficiently answered that question, at the end of his talk, ask him because he's much more versed in that sort of thing.

DR. LANGMUIR: Further questions?

DR. WILLIAMS: Is anybody going to talk about the water producing characteristics of the different zones like the tracejector logs using Iodine 131 as the tracer?

MR. LUCKEY: Not to any great extent, anyway. That's kind of beyond anything that we've prepared for this particular meeting.

DR. LANGMUIR: Well, we have an opportunity to get back on schedule totally here. This is remarkable. I have to commend everybody involved this morning.

I would suggest we eat in the hotel and try and get back here at 1:30 to begin the afternoon session. If you eat in the hotel, you can get done rather quickly.

(Whereupon, a luncheon recess was taken.)

AFTERNOON SESSION

DR. LANGMUIR: Our first presentation of the afternoon will be by Gary Patterson. The topic is Analysis of Strain Related Water-Level Fluctuations. Gary.

MR. PATTERSON: A couple of years ago, Devin Galloway began an effort to develop the data collection techniques in order to analyze strain related water-level fluctuations. The objectives of this analysis are to assess the applicability of these analyses for obtaining estimates of elastic and hydraulic properties of aquifers at Yucca Mountain, and to obtain estimates of elastic and hydraulic properties in the absence of permits required for injection tests or pumping tests.

The strain related water-level fluctuations that I am going to talk about are those that are associated with atmospheric loading, earth tides, earthquakes and underground nuclear explosions.

This is sort of an abbreviated summary of inputs and outputs for this type of analysis. The atmospheric loading analysis requires time series of water levels and barometric pressure and will give you vertical pneumatic diffusivity, vertical hydraulic diffusivity and barometric efficiency.

The earth tide analysis requires water level response to earth tides, areal strain tide, and barometric efficiency and provides areal strain sensitivity, porosity, matrix compressibility and specific storage.

The seismic analysis requires fluid pressures or water-level responses to seismic events, specific storage, and then one parameter that I left out is the areal strain sensitivity. And it will provide peak dynamic strain and transmissivity.

DR. DOMENICO: Gary, what is an areal strain tide?

MR. PATTERSON: That is just the term we call, we calculate from the theoretical strain, we calculate the areal strain type for the location of Yucca Mountain for the latitude.

The analysis of strain related water-level fluctuations has certain advantages, one of which is that it may allow us to obtain parameter estimates at several locations where pump tests will be impractical. This will allow us to help assess spatial variability. The second advantage that I've got down there is that it will allow comparison of parameter estimates obtained from strains imposed at various scales much larger than the scale of the well tests. And the final advantage is that it is relatively inexpensive. Most of the required data is already being collected for the water level monitoring network and those parts of it that we have to modify slightly to get the rest of the data are relatively inexpensive compared to pump tests.

A couple of disadvantages, the first one is something that Dick alluded to earlier is that the analysis requires that boreholes be cased to the water table which essentially eliminates the possibility of using the water table holes, unless we make modifications to the water table holes, or make modifications to the equations. And the second disadvantage is that these methods assume a porous medium that based on preliminary pump tests we know is inappropriate at least at the well scale. The scales of the strains that we are analyzing, range from four kilometers for seismic wave lengths from UNEs to as large as half the circumference of the earth for the diurnal tidal effects. So at that scale, it may be appropriate to treat the aquifers as a porous medium equivalent.

The atmospheric loading analysis that we use was developed by Stewart Rojstaczer. In 1988 he developed the periodic study state solution for the water-level response to atmospheric loading in an open well, cased below the water table, tapping a partially confined aquifer. He subsequently expanded this analysis to include unconfined aquifers. Governing equations for Stewart's method come from Van Der Kamp and Gale and from Weeks.

Stewart's method is essentially a type curve matching technique where the theoretical responses are expressed in terms of barometric efficiency and dimensionless frequency. The goal of the type curve match is to find the point where the response is no longer frequency dependent.

Measured time series of barometric pressure and water levels are analyzed using cross-spectral estimation techniques (Bendat and Piersol). This results in values of barometric efficiency and Q in cycles per day, which is then plotted and matched with the theoretical curve.

Where we are able to determine the static confined response or the response where that is not frequency dependent, then the match yields barometric efficiency, a dimensionless frequency R and a dimensionless frequency Q, and Q in cycles per day. The dimensionless frequency R is a function of the depth from land surface to the water table. The angular frequency which is calculated from Q in cycles per day and the vertical pneumatic diffusivity. And the dimensionless frequency Q is a function of the depth from the water table to the monitoring zone, the same angular frequency and vertical hydraulic diffusivity.

This is a summary of the results from five zones that we monitored in the four different wells. You can see that the hydraulic and the pneumatic diffusivities are on the order of 10^4, millimeters squared per second. And the barometric efficiency is ranged generally from 0.8 to 0.87. The 0.95 value for H-6 is we think unrealistically high and we haven't used it for any particular calculations. We don't know why it came out so high. We also noticed the lack of pneumatic diffusivity values for H-4 and H-6. Because this is a type curve matching procedure, occasionally we can't get a unique match on a particular curve, which makes it so that we can't calculate, we can't figure out what the R is, so all we can get is bounds for Q and for hydraulic diffusivity.

DR. DOMENICO: Gary, your hydraulic diffusivity and pneumatic diffusivity are virtually identical. To me it's kind of strange because the K/Ss is diffusivity and the Ss for the pneumatic diffusivity would incorporate the compressability of air and the Ss for the hydraulic diffusivity would incorporate the compressability of water. The compressability of air I believe is several orders of magnitude larger than the compressibility of water, which means that your permeability to air must be several orders of magnitude larger than the permeability to water.

We heard yesterday, or maybe it was today, I've been here so damn long I lose track of time, that there was only one order of magnitude difference between the conductivity to air and to water. I think that is what one of the things that was brought up in that discussion. But this would suggest that your hydraulic conductivity to air is several orders of magnitude larger than hydraulic conductivity to water. Is that correct? Can you break down that diffusivity into a K, N, and S or do you get the lump number?

MR. PATTERSON: I haven't done that.

DR. DOMENICO: Can you do it? You probably can.

MR. PATTERSON: Yeah, I can, but again, I have not done that. I will take that recommendation and do it.

DR. DOMENICO: What does it mean when they are identical? What significance does that make? Any particular significance?

MR. PATTERSON: I don't know. I would think that fractures with high permeability--what it may mean is that the pneumatic diffusivity is controlled by the aquifer above the water table and that may be more zeolitized or has different configuration in the fracture zones internally below the Calico Hills. That may have something to do with it.

DR. DOMENICO: It doesn't mean that the unit is highly fractured, does it?

MR. PATTERSON: I don't think you can infer that.

DR. LANGMUIR: Gary, could you put your microphone a little closer up? It's a little difficult to hear you.

MR. PATTERSON: Okay, the next part of this analysis is the earth tide analysis. The solid earth tide is the displacement of the particles of the earth due to forces of the sun and the moon related to the phases of the moon and changing seasons. Measured water-level responses to earth tides are used to estimate specific storage, matrix compressibility, areal strain sensitivity and porosity. We used the methods developed by Rojstaczer and Agnew in 1989.

By measuring the amplitude of the water-level fluctuations in response to earth tides by estimating the areal strain tide from the theoretical tidal potential, matrix compressibility and areal strain sensitivity can be estimated.

Using the relation between matrix compressibility, barometric efficiency and areal strain sensitivity, the porosity and specific storage can be estimated.

Time series of water-level measurements are processed using a low pass, digital Butterworth filter. The low pass signal contains longer period atmospheric influences, and is subtracted from the raw data to provide a reduced series of shorter frequency fluctuations that contains the earth tides and daily atmospheric loading.

And Dick sort of showed an example of this in his talk. But, this is a plot of a 30-day window of water-levels for the below zone in H-4. The upper plot is the raw water levels, and although it didn't come out very well, this line drawn across the back represents the low pass filter. You subtract that from the raw water levels and it yields the second plot which is the high pass, which contains the earth tides. And the lower plot is the calculated areal strain tide.

This again is sort of an abbreviated summary of the equations that we use in the earth tide analysis. Because of the gamma term in equation one, we can't just measure the water level fluctuation, apply the areal strain tide and Poisson's ratio and calculate matrix compressibility. Instead the procedure we have to use is first to go to equation 2, where we calculate the areal strain sensitivity based on the water level fluctuation and areal strain tide. And then we jump down to equation 4, where we make an initial estimate matrix compressibility to calculate alpha, then input that alpha into equation 5 and calculate B which is a function of a barometric efficiency that we obtain from the atmospheric loading analysis, Poisson's ratio and the alpha term. And once we calculate these then we move up to equation three and calculate matrix compressibility as a function of B, Poisson's ratio and the areal strain sensitivity.

Once we've calculated that estimate of matrix compressibility, we then input that back into equation 4 and iterate through those equations a few times until the initial estimate of matrix compressibility no longer affects the final matrix compressibility.

So, once we've calculated matrix compressibility alpha and B, we can use equation 6 to calculate porosity and equation 7 to calculate specific storage.

This just shows the initial estimates that we used to come up with the results I'm going to show you in the next overhead. The compressibility of the fluid of 4.4 x 10^{-10} Pascals. Compressibility as solid grades is 1.72 x 10^{-11} Pascals, which we obtained the report from Zissman in 1933. Actually it is for a sudbury norite with comparable overburden stress. And, Poisson's ratio of 0.17 which is the average value for the tram member and lithic ridge tuffs at the Nevada Test Site.

DR. DOMENICO: What is norite?

MR. PATTERSON: It's a gabbro.

DR. DOMENICO: It's a gabbro? I would think it would be taken more approximately as the compressibility of the major minerals Pcs in tuff which may be what? What's in tuff? Feldspar? Is it same as feldspar?

MR. PATTERSON: We can probably improve on that. A lot of this is preliminary work.

DR. DOMENICO: But is supposed to be that the compressibility of the individual--if it was sandstone you would use quartz, probably.

MR. PATTERSON: Again, we can improve on a lot of this. A lot of this stuff was done as preliminary analysis and we do intend to improve on these calculations.

This is a summary of the values we obtain again from 4 wells in 5 zones. I apologize for the change in nomenclature. The A of water is the W from the earlier overhead, and the areal strain sensitivity is the A of S from the earlier overhead. You will notice two values in each of the columns in the upper part of the graph. They represent the values for the M_2 and the 01 tide. The M_2 is a semi-diurnal lunar tide and the 01 is a diurnal lunar tide. Those are the strongest tides that are not affected by solar heating.

The values on the bottom part of the graph show the matric compressibilities, porosities and specific storage for these zones. You will notice that above both of the above zones and two of the C-holes which are about 60 meters apart, come up with very similar values for all of the parameters. And the below zones of C-3 and H-4 which are approximately a mile apart are both--the C-3 is open to the Bullfrog and the Tram, and H-4 below zone is open to the upper part of the lithic ridge tuffs. You'll notice that they are very similar in their parameters estimates also.

The final sequential analysis that we are going to go through in the analysis of seismic waves, seismic Rayleigh waves from earthquakes and underground nuclear explosions produce aquifer dilation and concomitant fluid pressure disturbances.

This is the equation that we use in the seismic analysis. It's from Cooper, 1965. The only thing I really want to point out on this equation are the values for transmissivity. This is the equation we use to calculate transmissivity. The A appears on amplification factor. The X_0 is measured water low level response to a particular seismic event and h_0 is the fluid pressure response to a particular seismic event.

When "shut in" fluid pressure responses to seismic waves are measured, and when areal strain sensitivities are known from earth tide and atmospheric loading analysis, the peak dynamic strain associated with the seismic event can be calculated using the relation with areal strain sensitivity and peak dynamic strain.

Just to show what some of these response look like out in the field, there are two ways we can collect this type of data and Dick alluded to them earlier. One way is if it is announced underground nuclear explosion, we can go out and visit the well, reprogram the data logger to measure at one second intervals. This is a representation of that type of data acquisition. This is a water level response in the below zone of H-4 to an underground nuclear explosion from February of 1988. The peak dynamic response I am going to talk about later is the full-range amplitude from top to bottom.

This is another example. This is a response from the below zone of H-5 to the same nuclear explosion. One of the by-products of setting the Campbell data logger to one second intervals is you get a lot of noise and that is what most of that represents. The full range response of the large fluctuations in here.

This is a fluid pressure response from the below a zone in C-1. This is taken off of a continuous strip chart recorder. This is in equivalent feet of water, so it had a peak dynamic response of about 3 1/2 feet. This was to a very similar nuclear explosion of similar magnitude and similar location. I'm going to use that later. I am going to apply it to information from the early UNE at some of the other wells.

Again, just an example, this is from a Los Angeles earthquake in February of 1990, magnitude of 5.5. And the final example is an earthquake at the center near La Paz, Mexico.

This is just a summary of the water level and fluid pressure responses to those particular events. You can see the fluid pressure responses are generally much larger than the water level responses which sort of alludes to what Dick was saying that, unless we have a packer in the access to one of the zones, we have little chance of picking up any earthquakes or anything without the packer.

The next few slides that I am going to go through are really just some mathematical calisthenics to show how we would apply this information. You'll notice that there is a somewhat circular pattern here. While I'm taking values form one well and applying them to another well, which I know is not completely legitimate, but the fact is that right now we don't have a full set of parameters for any given wells so we can't really do these calculations without making some transpositions. And, we are in the process now of collecting data at individual wells, but I am only doing this for demonstration purposes.

The first calculation I'll make is peak dynamic strain. The peak dynamic fluid pressure response to the UNE of 12/08/89 in C-1 was 1.26 meters. Using the static-confined areal strain sensitivity for the M_2 tide for C-3 below was .36mm/Nanostrain. So going through the calculation yields a peak dynamic strain of 3.51 X 10^{-6}.

Similarly the Los Angeles earthquake caused a peak dynamic strain of 4.0×10^{-7}, and the Mexico earthquake was 1.20×10^{-7}. This has some relation to the earlier questions about fault creep in that there are places, particularly California where peak dynamic strains on the order of 10^{-6} have been associated with the advent of fault creep. And just to sort of reiterate what Dick said is that the only suspicion that we have that there may be any fault creep at test site is strictly graphical. We have several fluctuations that we don't even know if they are real water level fluctuations or not, but they have the typical sharp rise or sharp decline and then a fairly steady return to baseline levels. And these responses seem to occur in the absence of any of the other things that we have ever noticed that cause water level excursions. And as Dick mentioned there were only a half dozen or so. We have no evidence and we won't know unless we get some strain monitoring at the site.

The next calculation we go through is to calculate fluid pressure response. Using the peak dynamic strain calculated from C-1 in the earlier graph and using the static-confined areal strain sensitivity from well H4 .83mm/Nanostrain, we can calculate what the fluid pressure response might have been to the earlier UNE. Ideally we would have in situ strain measurements so that we wouldn't have to do these transpositions, but I don't of any way to really calculate the fluid pressure response and the water level response simultaneously with the wells configured the way they are.

So, the only way we are going to be able to do this calculations is to take a value from one well and use it in another well or to institute strain monitoring which would allow us to measure one of the fluid responses or water level responses and calculate the other.

The amplification factor which is the water level response over the fluid pressure response for H4, the water level response was 23.2 and the fluid pressure response we just calculated was 2.91 meters, so it yields an amplification factor of 7.97×10^{-3}.

And now the real reason why I did all this was to estimate transmissivity from this data. If we use the amplification factor of 7.97×10^{-3} and use specific storage obtained from the earth tide analysis, we can then solve Cooper's equation for T which yields 1.05 m^2/d. This compares to 7.88 m^2/d estimated from a borehole flow survey reported by Whitfield in 1985.

In think in light of all the assumptions and the jumping around that I have just made to make these calculations, I think that is actually a pretty good match.

So the conclusions from this preliminary analysis is that we feel that these analyses of strain related water level fluctuations appear to be a viable method for obtaining parameter estimates at least at some boreholes in Yucca Mountain.

The analyses would be greatly enhanced by on site strain monitoring. One reason would be that we would not need to use the theoretical tidal potential which can be influenced by the presence of faults. And also for the reason I just mentioned that we could measure peak dynamic strain in particular seismic events.

Some boreholes near Yucca Mountain, I haven't discussed this very much, but I really feel that some boreholes near Yucca Mountain are sensitive enough to these types of fluctuations if used along with in situ strain monitors, they could be very important components of on site strain monitoring.

Our future plans are to expand monitoring of fluid pressure responses and to obtain full sets of parameters for given well so that we can actually make some real calculations for these values. And we would also like to incorporate additional strip chart recorders so that we can advantage of earthquakes and unannounced nuclear explosions.

In addition, another thing that Dick alluded to is we would like to figure out a way to case the WT holes which will allow us a lot more data points. And we would like to push for some sort of in situ strain monitoring.

DR. LANGMUIR: Thank you, Gary. Questions?

DR. DOMENICO: Gary, I heard this talk at Boulder or Golden at a USGS gathering. Was that delivered by you or Galloway?

MR. PATTERSON: That was Galloway.

DR. DOMENICO: Well Galloway came to a conclusion, but the pneumatic diffusivity of the nonwelded material like the Calico Hills was identical to the pneumatic diffusivity of the welded units in one of his conclusions that I heard loud and clear. Was that meant that the Calico Hills was no less fractured than the welded units? Will you comment on that?

MR. PATTERSON: That was a conclusion that Devin came up with.

DR. DOMENICO: That Devin came up with?

MR. PATTERSON: Yeah.

DR. DOMENICO: That is all? Do you have a different conclusion? The same analysis?

MR. PATTERSON: Well we have other information from borehole television wires from the C-holes that there are fractures in the Calico Hills. The conductivity of those fractures is something we don't know anything about. But we know there are fractures there. So, I can't--I can really neither support or refute that conclusion. Devin really does know a lot more about this analysis than I do. As you probably know, he is the one that initiated all this and I am just trying to continue it.

DR. DOMENICO: But the pneumatic diffusivity incorporates conductivity and the compressibility of raw materials and if they are the same for welded versus nonwelded units--

MR. PATTERSON; We know that compressibility is higher and there are definitely differences in the Calico Hills, but whether the fractured conductivity has anything to do with that--

DR. DOMENICO: So you wouldn't come to the same conclusion as Devin?

MR. PATTERSON: No.

DR. DOMENICO: Okay.

DR. ALLEN: If it turns out that these kinds of measurements really are critical to understanding hydraulic data, then I would certainly suggest that a consideration be given to putting in a modern seismometer, I mean high dynamic range broad band seismometer here to try to get independent observation. We discovered some very strange things in terms of coupling of sonic booms with seismic energy and so forth that I think are worth considering when you are trying to get all these different alternatives.

MR. PATTERSON: I'd agree with you. We would love to see a high tech strain monitoring.

DR. ALLEN: No, I am not sure I could defend it solely on the basis of understanding earthquakes but maybe we could on this basis.

DR. LANGMUIR: If I can get one last quick one. Obviously a lot of assumptions involved in getting from the strain approach to a transmissivity. What kind of uncertainties would you attach to that transmissivity as opposed to one determined by traditional testing of ground water pumping and that sort of thing?

MR. PATTERSON: I don't really know. What we'd like to do is to be able to get a full set of parameters for the C-holes and then when we do the multiple well testing on that we can compare results and feel a little more comfortable with it.

People have done this in the past and other places, and the strain analysis has come out very favorably. But, I think that may be a local phenomenon. There are some places were this isn't going to work so well and there are other places where it will work. And we feel that there are some wells at Yucca Mountain that will work and some wells where it won't. And, we are going to have to be very careful applying this. But when compared to the insurmountable problem of having to go out try and do a pump test at every well that is out there versus having some parameter estimate to at least use to compare it to the modeling estimates and things like that, I think it has value.

DR. DOMENICO: This is a trivial question, but we heard a little bit earlier, people using a coefficient of storage of 0.1 and 0.01 typical for unconfined system. If this is an unconfined system why are we recording barometric and tidal fluctuations?

MR. PATTERSON: This is--

DR. DOMENICO: It is not a trigger question.

MR. PATTERSON: Most of these aquifers that we are dealing with in this analysis anyway are at least partially confined.

DR. DOMENICO: The conductivity is going to be a couple of orders of magnitude smaller than what we've heard a little earlier?

MR. PATTERSON: Yeah.

DR. LANGMUIR: Thank you, Gary.

We can proceed now Claudia with the next speaker.

MS. NEWBURY: Our next speaker is M. J. Umari, from the USGS. He is going to be speaking on multiple well interference and conservative tracer testing.

DR. UMARI: This is normally when I spill my coffee. Instead of that I spilled my water, I think. But this will hopefully get me through the fact that I have a little bit of a cough.

Well, my name is M. J. Umari and I work with the USGS on the Yucca Mountain project. The thing that I wanted to point out at this point is that I have started working with the project in the beginning of April of this year and so some of the information I may have to, if pressed, rely on other people in the audience that would be supporting the information. But, I think in terms of overall presentation, I should be able to handle it.

I'd like to talk to you about two activities that we are going to perform in the saturated zone fractured rock hydrology project. The first one is a multiple well interference test and the other one is testing the C-hole complex with conservative tracers. I'd like to point out at this point that the C-hole testing is for methods development. In other words we are not going to take any of these parameters, determine from this process and use it for site characterization. The first activity involves cross-hole testing between the C-holes themselves and then a large scale test that involved the C-holes and other tests.

I think what I would like to do now is to have this here so we know where we are in the process.

The location, of course, you are familiar with. The C-hole complex is here, southeast of the location of the repository. And the other wells that I would like to point out at this point are the P#1 here, the H-4-1 here and the B#1 here because those will be used among other wells in the large scale pumping test that I will be discussing.

The primary objectives for this activity is to determine hydraulic properties. We would like to determine the spatial and the directional variation of the hydraulic conductivity or transmissivity if you wish, and the storage coefficient. We would like to determine whether models of porous medium assuming anistogropic characteristics would apply. We would like to determine whether that conceptualization works or something else more complicated would work, and I am going to elaborate on that in a little bit.

We'd like to see if fracture-flow modeling would work and we'd like to identify and examine the scale dependency issue which is of course, a very important one. And then we'd like to identify the hydraulic connection between fractures and also between stratigraphic units.

Within the cross-hole testing program, the main idea is that we are going to pump from a test zone of one well and monitor the hydraulic response in five tests zones in all the C-wells including the one from which we are pumping. And we would like to in the process vary these factors here. We would like to vary what well we are pumping from, what pumping interval we are using, what interval we are monitoring, what rate we are pumping at, and this last one means we would like to vary the zones that we are monitoring in terms of the hydraulic conductivity. In other words, we are not only going to test zones that we think are highly conductive, but also zones that may not be highly conductive. And that is what I mean by varying the hydraulic conductance of pumping and monitoring intervals. It may be a little bit confusing over wording.

The C-hole complex is of course oriented like this, with the distance between the wells varying between 100 and 250 feet. This is just a typical lithology column with the saturated intervals shown.

Now in order to discuss these tests, I thought we could look at a hypothetical cross-section, obviously, overly simplified, but for the point of discussing the test, let's assume that we have any two of these wells, C-1, C-2, or C-1, C-3, or something like that and we had two sets of fractures intersecting the area. We would like to place inflatable packers in those wells, isolate test zones, go into those test zones and withdraw water, pump water from those test zones and monitor the pressure in the monitored intervals using pressure transducers. Of course you can see that the possibilities if hydraulic fractures exist and they do, and if they are hydraulically connected, then the permeations of possible responses would be fairly high. And that is what we are intending to study is all of these variations.

The instrumentation by which we are intending to conduct this test, involves what we referred to as our multiple test zone packer system and it involves those inflatable packers. It also involves this tubing in the center which has in it those valves that are referred to as sliding sleeve valves which can be opened or closed using a wire line tool and that way that would allow us to open a zone to test or to monitor. This is a thermistor to measure the water temperature of those test zones, and that is about it.

The second part here the conservative tracer testing mechanism, I am not going to talk about at this point much, but I guess I should since this is the only version of the slide I have, we are going to be injecting along with this process a conservative tracer into all these zones and releasing that conservative tracer at those test intervals. And I will talk about that a little bit later. But I'd like to at this point concentrate on the hydraulic testing.

By the way in terms of timing, these are the numbers of the slides. It goes up to 28, so if anybody wants to help me out with time, I will be happy to oblige.

The cross-hole testing involves selecting test intervals. And so we are going to select these intervals based on cross-hole seismic surveys which I'll talk about in a little bit, and this brace is supposed to be a little bit lower encompassing these two elements. From previously conducted tests at the C-holes, temperature logs have been obtained and tracejector surveys have been obtained and both of those have been studied for intraborehole flow.

From these which we consider intraborehole flow indicators, along with the cross-hole seismic surveys, we are going to try to determine locations for the test holes. And in addition to that, another factor is of course analysis of these previously conducted hydraulic stress tests in general, not just the temperature logs and tracejector survey part of them. And we also have data for fracture distribution obtained from acoustic televiewer and TV camera logs. Now the TV log I understand for C-1 is very good. The ones for C-2 and C-3 are not very good and we are attempting now to have those re-done. But clearly these are the avenues--sources of information that we will use in terms of identifying our locations of testing and monitoring.

The cross-hole seismic surveying is illustrated here. There is one mistake on the slide and that is that these packers will be--can't be there--are not there while you are conducting the test, however, given the fact that there is a mistake in the slide, I'll use this opportunity to point out that those packers were there at the C-hole and that they were removed in anticipation of this cross-hole seismic process taking place, but then it hasn't taken place because of budgetary concerns. So, anyway, when the test does take place they will be not there.

The idea is to place a source for seismic waves from a vibration truck that would emit three types of seismic waves, and you could have either the source at the surface or you can have that source lowered into one of the zones here, one of the test zones, and then you would receive the signal at these three component receivers, and by this process and by the fact that the speed of the seismic wave is a function of the fracture characteristics, we hope that we can get an idea about the fracture distribution. This is going to be done through Lawrence Berkeley Labs. It is part of our study plan, but they are going to do it.

So the cross-hole seismic surveys would allow us to construct a fence diagram of seismic properties that would allow us to estimate fracture location, density, orientation that are estimated in vertical planes between the wells. And this is what I just referred to that the difference, the different fracture characteristics affect the seismic wave which is the basic principle.

Now the other well test that is going to be conducted under this multiple-well interference testing is this large-scale pumping test. The idea there is to pump one of the C-wells for approximately 30 days. And the idea of putting that here is to accentuate the difference between that and the cross-hole testing within the C-wells which won't be this scale of time; quite a bit shorter in terms of days.

Then we would monitor all the three C-holes and also the other wells that I showed on the location maps. So we would be monitoring H-4, B#1, and P#1 and also other network wells. So this is a large scale aquifer test. Then of course we would, and I failed to say that for the cross-hole testing, you would also do the same thing. You would stop pumping and then you would monitor the recovery. In this case for 30 days. In the case for the cross-hole testing you would monitor it for a few days. But, of course, the same idea.

This is a sketch of what we perceive may happen when we conduct this large scale aquifer test. This is the C-pad here and if we were to pump it then you would assume theoretically that you would have radial flow towards it. Now it happens that the Bow Ridge Fault separates the C-well complex and the P#1 well location from H-4 and B#1. And of course this is the surface trace of it. Exactly where it hits in terms of the test zones is another issue. But, here I have question marks to indicate that one of the things that we could get from this aquifer test is to determine whether flow would take place across this particular fault or not so to determine the hydraulic characteristics of it.

Now in terms of analysis of these well tests, the central philosophy is to try progressively more complex conceptual modelS. And in other words we are going to try to start with something simple and then see if it works and if it doesn't we'll go to the next more complicated thing. However, I think what will happen is that at any stage you just won't be able to perfectly interpret the data, so you'll always feel a need to try the next more complicated step. So the issue is where to stop there.

But, the first thing that we are going to look at is porous medium models, of course, assuming anisotrophy introduced by fractures. And assuming that the medium is either homogeneous or non-homogeneous. Now, for homogeneous medium there are a host of analytical solutions for radial flow homogeneous medium with anisotrophy introduced by fracturing. And so we are going to try those analytical techniques and see how well we can match the data.

Then we are going to see or try to see from our test results whether we can support the assumption of homogeneity. And one way to do that is because we are doing this cross-hole testing is that we can conceivably get hydraulic conductivities for example as a function of depth for the different test intervals. So if we were to attempt to correlate that with lithology for example and if there is a statistically valid correlation between the variation and vertical hydraulic conductivity, and the lithology, then you may argue that that variation is a function of the lithology and therefore you have a non-homogeneous medium. And so then you would have to go to a non-homogeneous assumption.

If you did that we can use numerical models. There are a lot of numerical models that would introduce anisotrophy in them and so we would go that route. If we were not satisfied with the quality of our match with the test data, we'd propose to go to the next level of assuming a dual porosity medium. And I'll discuss that in the next slide. And again there this issue of homogeneity of not, and if that is also unsatisfactory, we'd go to composite porous medium assumptions which I'll also discuss in the next slide, and then further along if those don't work we may attempt fracture network modeling. In fact most probably we'll try that anyway.

In order to quickly discuss what dual porosity means, assuming a porous medium but with two characteristics, the fractures are represented by a porous medium and the rock matrix is also presented by a porous medium. However, the hydraulic conductivity is assumed to be much higher for the fracture part of it than for the rock matrix part of it, whereas the storage coefficient is assumed to be the other way around. It is assumed to be that the matrix has higher storage. But again, it is a storage medium concept, but intertwining of two medium.

The composite porous medium assumption, I would like at this point to say that, Mr. U-Sun Park previously pointed out that in the unsaturated zone, composite porous medium means something other than what I am going to say here. So, if that is the case, then please make that distinction. I am just talking about what we perceive the composite porous medium is in saturated zone with this possibility of conflict of the term.

The idea is that you'd have two concentric zones around the well. And the zone near the well would be dominated by a few fractures. And the outer region of these two concentric zones would be extensively fractured, and the two zones would be hydraulically connected. The idea here is that if you start from time zero, clearly at the beginning, the big fractures near the well are going to be dominant and then eventually the average characteristics of the medium will come in. And so this is an effort to match the data of pump tests where we see distinct three segments in the test data. That is one of the ways that that issue has been addressed.

Now I'd like to talk a little bit about fracture network modeling in a generic sense and then try to say something a little bit more specific about our activity. In general if you are talking about a fracture network and Kenzi Karasaki from LBL today pointed out that basically this is the classical approach to fracture network modeling and his model is more specific than that, but in classical fracture network modeling, in order to describe a fracture or a network of fractures, you need to have the length l here, lowercase l, it looks like a one. The lowercase l is the length of the fracture; d is density of fracture, o is the orientation, and I didn't put it here, but you would have something for aperture too. And basically those characteristics, you would think in the world of underground change and vary as a function of space, of three dimensional space, so that is what this is saying. They are a function of space.

Now in order to have a network characteristic inputted into a numerical model, then what you would do is rather than establishing spatial functions for these characteristics, you would for each one of them determine some kind of statistical density function describing the frequency of occurrence of these different values for length, density and orientation within the medium.

This particular density function is not to indicate that I think that they are normally distributed. It is just to indicate that this is a density function of a random variable. So, if you have defined a length distribution, a density and an orientation, you have n, which is what I'm calling a network. This is just for the purpose of being able to talk about it in the next few slides.

Now, this here is what a real fracture network may look like, and this here is from Kenzi Karasaki from LBL's model and it is referred to as a discontinuum model. And the idea here is to have linear segments that represent the fractures, and the most important feature of it is that it is discontinuous because as Kenzi was explaining, essentially if you have all those fractures very dense and all connected, in the limit you wind up having a porous medium. So kind one of the characteristics, salient characteristics of a fractured system is that those fractures stop and are discontinuous. This as you can see an organized, structured version of this that would be obtained. So just to point out that his model that we would be using through LBL essentially simplifies the fracture system into an equivalent one. It does not map every particular fracture, but provides an equivalent fracture network that would hydraulically do what the real one does, at least that is the objective.

Now, what we would be doing then is attempting--now the question is so what fracture network would you use to test our results of the C-holes? So what networks, set of networks would we use to test this model or to try to attempt, and understand our data from the stress test using the fracture network model? Well, one way would be to try different fracture networks and one for fracture network one and two for fracture network two, and each one of them has its component distribution of length, density and orientation. n. And then try various networks and do that so that you would bracket the range of uncertainty because we are uncertain about what exactly the fracturing is. And another guiding light in terms of deciding on a network, would be to try different hypothesis that haven't postulated in terms of how fractures are distributed on the Yucca Mountain. Are they controlled by stratigraphy in which case you'd have one of the postulates is that if you have a more welded unit it would be more fractured, or is it independent from that. And so then these are hypotheses that would produce different networks. So, we could use this kind of logic to produce different networks and try them.

And, also like we said we are going to conduct cross-hole seismic profiling. There are also outcrop studies taking place and the borehole geophysic logs that I referred to that have given us ideas about the fracture distribution, albeit in a restricted manner within the borehole. So all this information would be used in terms of trying to come up with different fracture networks to try.

The last thing I would like to say about this analysis of multiple-well interference testing is that in the process we'd like to see--we would like to test whether obtaining data from a single well is as good as or how does it compare with having observation wells? So, comparing multiple-well tests with single-well tests, so when we are doing our cross-hole testing, we are pumping from one zone and monitoring it in the others, we could assume that we don't have those others and just analyze the one where we are pumping from and get the results and compare it with the other one. So, we are going to try to address this issue. And of course, in general, one would feel that multiple-well testing is more reliable, but we would like to determine whether single-well tests are applicable in terms of giving us adequately close results to the multiple-well test which would affect future plans.

Okay, now for the second part of the talk, I would like to talk about testing of the C-hole complex with conservative tracers. This will be done to a large extent simultaneously with the cross-hole testing with the same instrumentation that I discussed earlier. Basically the overall objective is to determine effective porosity, referring to it here as theta, longitudinal dispersivity, average linear velocity will be obtained in an indirect way, I'll mention that later, and possibly matrix molecular diffusion. So these are parameters associated with solute transport and I would like to talk a little bit about how we are intending to get them.

And of course, our main objective is to determine what conceptual model would best describe the solute transport problem that we would be seeing.

Let's talk about the parameter requirements that we would have. In traditional forced medium setting and it is followed fairly closely by the network modeling approach, you need to characterize obviously the flow of mass balance and the solute mass balance. So if you have the solute mass balance, you basically are describing the flux of the constituent, which in the final picture would be the radionuclide, but in our case would be a conservative constituent. So we have the flux of the constituent is a function of the velocity field, v, it is a function of hydrodynamic dispersion, D, and it is a function of any reactions that could occur.

Now I'll quickly point out that the reactions that could happen in the real world would be by one radionuclide with another, with the rock matrix and all the other solutes. But in our case, assuming that since we have a conservative constituent they are zero. So we take that out. But, having defined what the mass balance is for the constituent, basically that means we have an equation that describe the concentration as a function of space in time. And I am only putting that such that we can talk about what parameters we need.

The velocity that would be needed for solute transport computations, would be the velocity in intersticial velocity if it supports medium assumption or the velocities in the fractures. And, since the flow part of the study would determine Darcy flux to get from that to velocities, intersticial velocities, you need to have the effective porosity. The hydraulic conductivity or if you prefer the transmissivity, and storage coefficient would be determined in the flow or the hydraulic stress part of the set up, and is the network that I talked about earlier. And if you are not using a fractured network, then n has nothing to do with it, you can take n out and then the velocity field would on depend on K, S and theta.

And of course, hydrodynamic dispersion depends on the velocity field, longitudinal dispersivity and molecular diffusion. And we can leave this transverse dispersivity out for the moment to simplify the discussion.

One interesting approach in terms of getting ideas about the dispersivity on a large scale, and is that--if the hydraulic conductivity varies spatially, then it would on a large scale create a dispersion process. A result of varying the hydraulic conductivities. So, one thing that people have looked at is, looking at that spatial variation of the hydraulic conductivity and applying geostatistical procedures to get an idea about dispersivity on a larger scale.

While I have this, I would like to say that as we are progressing in terms of analyzing our test results, we are going to start not only for flow, but with the solute transport with the assumption of a porous medium and do a porosity and then composite porous medium and then fracture network modeling.

So for each one of these assumptions, the parameters would vary that we would need for solute transport. Like for example, like I just pointed out, if we are not considering fractures n doesn't have to be contained, if we are looking at dual porosity model in which case the matrix has a diffusion element to it, then we would look at matrix diffusion. But if we are looking at a fracture network model in which the assumption is flow only in the fractures, then we can forget about the molecular diffusion. So these are all the parameters needed for solute transport, except for K and S of course from flow. But you may not need all of them depending on what assumption you make.

Now, I'll talk about how to do the test in a minute, but before that what are the traces that we are going to be using? Because we are going to conduct these tests simultaneously with the hydraulic stress test and because we may not be able to withdraw all the solute back, the conservative constituent out, we are going to use overlapping tests in which we have to use different tracers such that we can distinguish for the second test, they affected only the second test. So the issue is what tracers to use and the initial test we'll use organic anion trifluoromethylbenzoate. But UNLV has an independent contract in which they are studying organic tracers and we are working with them in terms of providing them what they need such that they would tell us what specific tracers to use.

DR. LANGMUIR: Just wondering, they have a vested interest in those tracers; they make them; they want to work with them; they may or may not be well conserved. There is some uncertainty about that. On short-term tests perhaps they will be, but there are lots of other traces available, which will be cheaper and perhaps more guaranteed to be conservative.

DR. UMARI: Than the ones--well that is, I presume the objective of that contract is to attempt to determine--

DR. LANGMUIR: But if you are assuming that they are conserved in your modeling, that may be a little bit debateable.

DR. UMARI: I think that part of what they are doing is to see whether they are by column experiments to determine that are or not conservative under the particular rock characteristics. I mean they have gotten rock samples from the area and I presume that is one of the things each should look at. But it is a good point in terms of let's not assuming that that is 100 percent of the case.

DR. DOBSON: I might just add one additional fact. UNLV is doing some research for us in support of the survey in terms of developing additional organic tracers. There is program-wide a standing group. I can't even remember what we call it, the tracer task force or something. There is a whole group of people from Los Alamos, the USGS and including UNLV that periodically meet and talk about what the most appropriate tracers are. And they make evaluations very similar--they made observations similar to the one you just made in that group that we basically rely on to come up with a list of preferred tracers.

DR. UMARI: The tests that are planned to be performed are three categories: The injection pump-back tests and I'll illustrate those in the next couple of slides; two-well recirculation tests; multiple-well convergent tests. And for all those tests, those will be done in intervals of high conductance for the test to have high chance of success. In terms of illustrating what would be going on in a simplified manner, in the injection-pumpback test, we would be injecting Q in and we would have the solid lines of solute emanating from the area being tested, and then we would pump it long enough such that we would permit the tracer to move along fractures, and then we would pump the water back with the dotted lines here indicating the direction of solute being pulled back. And this graph should have been dashed to indicate that if you take samples from this location here when you are pulling--when you have the Q out stage, and you are getting the concentration of the sample as you get it out and you plot the concentration as a function of time, we'll wind up getting a front. And that is the front that we would be analyzing with the techniques I mentioned earlier to get those parameters that we need.

The other two types of tests would be the two-well recirculating test. In this one, you would pump Q_4 out here and then you would inject Q_4 back into that other well, and then you would wait until you established steady flow conditions. Then you would add a short tracer pulse. And of course what would happen is that the tracer would emerge from point A and go to point B and again by measuring the concentration that is being pulled out as Q_4 you would get a front that you can analyze again with those equations.

The multiple-well convergent test you would be pumping Q_3 this is just to distinguish different test types you will be pumping Q_3, you don't reinject it back into the other well, but that would still establish a radial flow towards the well and so if you place a tracer at point A and you release the tracer at point A, the tracer will still travel from point A to B at which point you can still obtain a front and analyze it. So these are three various permeations of the same concept.

Now in addition to the instrumentation that I mentioned earlier of the multiple packer string, in addition to that diagram, we would like to illustrate it a little bit more here. We have a tank that would be placed at the surface to establish circulation, and the stubing here, and then we have the packers inflated and they would come to a particular test zone and using a cellanoid valve release the solute at a particular test zone, possibly using a pressure reduction valve if we are concerned about hydrofracking the system, and then that would be introducing the solutes into the system.

This system that we are talking about is in the process of being constructed now through the Bureau of Reclamation.

Now in terms of analyzing these conservative tests, it is very similar and it goes parallel to the analysis of the hydraulics stress test. Again, we would start assuming a porous medium assumption, homogeneous, if you can support homogeneity, or if you can't you go to a non-homogeneous assumption. If you assume homogeneity, then there are analytical solutions for the concentrations of function of time. If we cannot support homogeneity, then we will go to 2-D or 3-D solute transport models, essentially for porous medium. If we feel that that doesn't really represent the results we got very well, then we would step it up to dual porosity solute transport models. If we feel that, although there are fractures, that there is also transport in the matrix because of course dual porous medium assumed that we had flow in both matrix and the fractures.

DR. WILLIAMS: Those are still porous medium models though, right?

DR. UMARI: Yes.

DR. WILLIAMS: Dual porosity is still porous medium, it doesn't change.

DR. UMARI: That is true. So it is basically--in fact, if you want to go with that even the fracture networking concept you know, in one of the ways that it is done is within the fractures you have Darcy's law applying and you solve the convective dispersive equation.

DR. WILLIAMS: The reason I brought that up is to make sure it is clear that none of this actually discrete fracture modeling.

DR. UMARI: Not yet.

DR. WILLIAMS: None of it.

DR. UMARI: The composite porous medium assumption which involves the two concentric areas that we discussed again would be the next stage, and if we were go to fractured network modeling, then we would be using LBL's discontinuum model. The way I understand it from Kenzi it is constructed of discrete linear fracture elements. And within each element you basically still assume a porous medium to solve the flow for Darcy's law and dispersion convection equation.

DR. DOMENICO: I fail to see how you are going to--how you can use a two or three dimensional model in the sense that you are not collecting your concentration data in Y and in Z. Just an X or radially, basically. In order to use a three dimensional model, you need three dimensional concentration distributions.

DR. UMARI: But we have five test zones in each of three wells.

DR. DOMENICO: But each in one fracture, you don't expect it to diffuse to another fracture down below do you?

DR. UMARI: Well, if you inject solute into one test zone of one well, and you observe the concentration in a different zone in terms of lateral location, at a different well, you are invoking a three dimensional flow right there.

DR. DOMENICO: It seems with the packers putting it in one fracture here and observing a break through over here, you've got a radial flow system, basically. And it doesn't seem like you are going to be measuring the concentration in neither Y or Z. You would be measuring it in X or in R if it is radial.

DR. UMARI: Well it seems that that would be a little bit of a function of what we are going to find out in terms of hydraulic connection among the fractures.

It may be like you are saying, you could identify a fracture in which you inject at one end and seat the other in which case you have a one dimensional flow. But conceivably you could have the solute moving in all three dimensions and captured at monitoring intervals that are spaced laterally and vertically such that if you do have some data--

DR. DOMENICO: But if it moves into another fracture, the movement will be due to advection not transverse dispersion.

DR. UMARI: That's true, however--

DR. DOMENICO: It would be dispersion in one dimension and it would be advective system to move it from one fracture to another.

DR. UMARI: That's true.

DR. DOMENICO: Not a spreading dispersion.

DR. UMARI: That's true, however according to Kenzi Karasaki who is you know the author of this model, the other issue there is the flow among those fractures, the fact that the flow goes and moves in the different directions at the intersections of the fractures, itself is a dispersive mechanism.

DR. DOMENICO: That's correct. That is quite correct.

DR. UMARI: So on a larger scale, one could say that--

DR. DOMENICO: It will disperse it; mix it.

DR. UMARI: Yes.

And this is basically a summary slide in terms of the issue of identifying these parameters. This is a repetition of the same three functional relationships that I presented earlier. All I am saying is that for flow it is a function of hydraulic conductivity, storage coefficient, network characteristics, fracture network characteristics and effective porosity. And for solute transport, in addition to that, you need hydrodynamic dispersion.

The idea in an inverse approach would be that you would be observing the concentration as a function of space and time. In our particular case and maybe this is kind of relevant to what you are saying in that it may simplify to being just a function or R and T, or it could be a function of three dimensions in T, but I would suspect that you would at least attempt a simpler solution like a function of only radially distance in T.

So using those measured fronts and mathematical inversion techniques, coupled with these functional relationships, in other words, these would be mathematical techniques, but you would have to have them coupled with the physics of the problem, and of course that is your traditional inverse approach that would give us the parameters that we are looking at for solute transport. Again, if you are not looking at fractures, n would be out; if you are looking at fractures n would be in, but Dm would be out because there is no matrix diffusion. If we assume that there is, I think we would be discussing the issue of whether there is matrix diffusion or not.

So, basically this relationship here in words would say, what is the choice of effective porosity, longitudinal dispersivity, molecular diffusion and network properties, what choice of these along with these relationships here would make the difference between the computed and the measured observed concentration as small as possible. How can we minimize the difference between computed and observed?

And, from my last slide here, we will talk about matrix diffusion and say that the issue of whether there is molecular diffusion taking place within the matrix, is being addressed by experiments using polystyrene microspheres. Those experiments are being done under the reactive tracer site characterization plan activity performed by Los Alamos labs. And Bruce, following me will be discussing that.

That slide, although it looks like a very good transition to his was not intended to be so, but it worked out like that.

So, these are the points that we went through, and that concludes my talk.

DR. LANGMUIR: Thank you M. J. Questions from the table or the Board? Further questions? Any questions from the audience?

MR. MIFFLIN: Marty Mifflin with the State of Nevada. I have heard a lot about the C-wells for many, many years. Are there any tests that have already been established to your knowledge?

DR. UMARI: At the C-holes?

MR. MIFFLIN: Yes.

DR. UMARI: Yes, there have been. That's what I referred to as previously completed test. There have been tests that were done at the C-holes and the data from them have been analyzed and are continued to be analyzed. There were problems with those with the way that those tests were conducted. And we are benefitting in hindsight from that in terms of designing the new wave of tests. There are results from that but it is really not within the scope of my presentation to discuss that.

MR. MIFFLIN: Thank you.

DR. LANGMUIR: We are scheduled for a coffee break. Let's take it at this time. And let's reconvene at 5 minutes past 3:00 p.m.

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

DR. LANGMUIR: Our next speaker is Bruce Robinson from Los Alamos. His topic is Testing with Reactive Tracers.

DR. ROBINSON: You just heard a presentation given about the C-wells and the hydrologic and conservative tracer testing that is being planned by the USGS. That leads into my talk very nicely, because those experiments will be used by us and also we are working in combination with the USGS in order to carry out reactive tracer experiments at the C-wells complex. So, I won't talk in particular about the C-wells per se, but keep in mind that this is where the tests and the testing is planned to be carried out.

At Los Alamos we are charged with the responsibility of characterizing geochemistry, and any chemical related process which may or may not affect transport, the migration of radionuclides including sorption in particular and in perhaps the transport of radiocolloids and/or colloids which have radionuclide sorbed onto them. And the focus of this work is to try to get a handle on those processes by doing measurements in the field. We are going to couple that with laboratory experiments and determine where we can take laboratory data and use it in our field scale models and where we need to go back and rethink what we are doing.

The first thing we like to do in the reactive tracer portion of the study as opposed to the colloid portion of the study is to demonstrate the laboratory sorption data that we are collecting is applicable in a field setting, in transport in the field. We are spending a lot of time and effort trying to characterize sorption of radionuclides on the Yucca Mountain tuff, and this is in total laboratory testing, both batch and column studies. However, we do need to show that those data have relevance when we then try to use the model results from the laboratory data in our larger scale field simulations of radionuclide migration. We need to have some assurance that those parameters are valid when we take them from the laboratory to the field. So that is the main goal is to attempt to prove that our laboratory sorption data and our methodology for interpreting those is appropriate for field scale transport.

We are going to do that by coming up with sorbing tracers, not radionuclides, but just sorbing tracers which mimic in one way or another the way a radionuclide may have sorbed and see if we can come up with predictions based on our laboratory data of sorption characteristics.

A more general goal of the study is to improve our understanding of the transport processes that are occurring in saturated zone. So much of the radionuclide migration is tied up in hydrology and physical transport mechanisms that it is not valid to split them out; one person takes one task, one takes another. You've really got to consider them as a whole in order to make a good prediction for field scale transport processes.

I am going to present a matrix diffusion model which we hope to attempt to either validate or prove wrong through a series of field tests. So either way, that will improve our understanding of the transport processes which are occurring in saturated zone.

It was also mentioned about colloids. Perhaps you could characterize them as two different types of colloid questions. One is radiocolloids which are formed at the waste canister and do they transport? And the other is, do radionuclides which are dissolved as dissolved species in the fluid, attach themselves via sorption to the colloidal material which is present in the groundwater and perhaps give us a mechanism for transport which we are not considering if we simply call it a dissolved species. The colloid is apt to transport in different ways through fractures than is a dissolved species. So we are going to attempt to characterize the mobility of colloids or a surrogate colloid if you will, in the saturated zone at the field scale.

We heard talk about matrix diffusion. What I would like to do is just describe in general terms the type of model which I think that through a series of field tests, we can validate at least to a certain extent or prove that it is not a valid model.

The matrix diffusion model basically says that we have fluid flow predominantly through fractures. And this is pretty widely documented in the saturated zone. That is where the majority of the fluid flow in well tests occur. They correlate with fractured regions to a great extent. So, I think it is a fairly good assumption that fracture flow for the hydrology is going to predominate. However, you do have interactions between the fractures in the matrix that need to be considered. For transport, if we have basically a steady-state flow system, in which there is no fluid flow into the matrix, there still is the possibility that solute or radionuclides will diffuse by molecular diffusion processes into the matrix, thereby resulting in, you can call it a retardation mechanism, but it is really more of a fundamental process which is occurring that needs to be characterized. The reason it is so important is that if one just uses the velocity of a water particle, if you will, in the fracture system to estimate groundwater travel time, that may significantly underestimate the time required for solute molecules to travel that same distance because of this diffusion into the stagnant fluid within the rock matrix.

Now the key here is to be able to in some way show that our field tests can only be modeled with this sort of a model. If indeed this model is a valid one, we need to be able to determine a way of testing the model and determining whether it is valid.

I'm going to present a couple of slides which show potentially why matrix diffusion is so important. And really, it is a porosity issue. If we assume that the porosity is simply within fractures, that will lead to a much smaller ground water travel time and solute transport time than if the tracer or radionuclide is allowed to diffuse into this rock matrix. Essentially, in the latter case you'd be effectively flowing through the entire medium, and the effective porosity for transport is more like a matrix porosity than the fracture porosity which is generally orders of magnitude difference. So, as a first order of effect, we need to be able to characterize in the field whether or not we are seeing matrix diffusion processes occurring.

A couple of calculations that I'll show you in the next couple of slides use this sort of geometry as sort of a first cut at characterizing and doing a parameter sensitivity analysis for matrix diffusion processes.

We've got a series of equally spaced fractures, with equal flow in each one. And one of the model parameters is the flow time of a water molecule in traveling from one end of the fracture to the other in the absence of any matrix diffusion. That is one parameter in the model. The other ones basically are the diffusion into the stagnant fluid in the rock matrix, and what we are going to look at is how important in effect that is for typical values that you might expect within the saturated zone.

This slide shows the concentration versus time for what is in effect a breakthrough curve. If at time zero we have--in the case of radionuclides a release at a constant concentration of a radionuclide or any dissolved species really, what is the breakthrough at some location downstream for a constant concentration injected at the inlet. The phi here is the matrix porosity. So, in the absence of any matrix diffusion, the parameter that I have set in this set of calculations is that the groundwater travel time is ten years. In other words, the break through occurs after about ten years in this model. And without any matrix diffusion whatsoever, the solute arrives in ten years.

When you start to incorporate matrix diffusion into the model for typical values of porosity and diffusion coefficients within the Yucca Mountain tuffs, we start to see that the break through is predicted to be orders of magnitude larger than one would assume simply by saying that the groundwater travel time is the relevant parameter. And so given that we have a process which can affect things over orders of magnitude it really says that what we need to do is to try to test that hypothesis in field testing.

DR. DOMENICO: Is that model by Grisak and Pickens?

DR. ROBINSON: It's a similar model, yeah.

DR. DOMENICO: This is analytical or numerical?

DR. ROBINSON: The one that produced these results was a numerical model.

DR. DOMENICO: But it is similar to the--

DR. ROBINSON: Yes.

DR. DOMENICO: That should be a concentration ratio then on the side should it not, instead of concentration?

DR. ROBINSON: Yeah, it is a dimensionless. The source is C_0 and you are looking at C over C_0.

This shows that for various groundwater travel times what effect it has for a given porosity, a 0.05 porosity.

This was one of the curves that I showed on the previous slide and at larger groundwater travel times it basically has a one-for-one effect in that if groundwater travel time is 100 years in the absence of matrix diffusion, it really brings the curve out in order of magnitude, for an order of magnitude change in the groundwater travel time. And it is only when we get to very small groundwater travel times that we get into the sort of times for breakthrough which would kind of make the saturated zone not much of a barrier. For any reasonable values as long as this model holds, for any reasonable values for these parameters, we start to talk about significant travel times of radionuclides in the saturated zone.

Now to test this sort of concept, you heard about the various types of well tests that are being proposed for the C-wells. The one I proposed for carrying out these sorts of experiments to look at matrix diffusion is a 2 well recirculating tracer test. The reason I would prefer a 2 well recirculating test is that one can set up without injecting any tracer, one can set up more or less, if you wait long enough, a steady-state flow field between the wells and eliminate that as one concern that you have about a given tracer test. If we are able to set up something close to a steady-state in terms of the flow field, then we inject the tracer and measure the concentration time response at the surface in the pumping well. This goes for both conservative and reactive tracer tests that I'll be talking about in a moment.

The reason I went into such detail on the matrix diffusion model is that it has--depending on what conceptual model you use for the flow and the transport of a conservative species, that has a great affect on reactive tracer behavior as well. So we have got to get that right before we can go on and try to predict--

DR. DOMENICO: Can I ask one thing on that?

DR. ROBINSON: Sure.

DR. DOMENICO: I think you've got a problem there. Can you put that up? That model is based on a continuous source which means you are going to have to continuously inject tracer, that is the same concentration until you complete your break through at the other borehole, correct?

DR. ROBINSON: No.

DR. DOMENICO: C over C_0--

DR. ROBINSON: No. The model results that I showed you were for a continuous injection.

DR. DOMENICO: A continuous source, that's correct.

DR. ROBINSON: One does not need to in a general case use a continuous injection in order to validate this concept. It is simply--

DR. DOMENICO: Then you need another model.

DR. ROBINSON: Yeah. You need a model which can handle a pulse injection.

DR. DOMENICO: No tracer tests that run with continuous sources.

DR. ROBINSON: That's right. Now, you wouldn't want to do that. The curves that I'll show you in a moment are simulations assuming a pulse injection. And that is one of the reasons to go to a numerical model over the Grisek and Pickens sort of approach. You can model things like injections of pulses of tracer. And that is what these are.

What I am showing here is a dimensionless or really a normalized concentration versus the produced volumes since the time at which you injected the pulse of tracer. Okay, so this is a more traditional break through curve that one might expect to see in a well test such as this.

What I am showing here is curves at different values of the flow rate, the steady state flow rate that you set up between the wells. If the matrix diffusion model were not appropriate and a traditional course medium approach were valid, then these curves would fall on top of each other regardless of the flow rate, if you assume that the flow field between the wells is more or less the same in the three tests.

One would expect a similar break through curve in each case because you are not really changing any fundamental parameters within the single porosity type of model. What I am showing here is that matrix diffusion if allowed to occur for a longer period of time which is basically what you see at ten gallons a minute at the lower flow rates, more material is allowed to diffuse into the matrix. You get a pretty dramatic attenuation of the signal for ten gallons a minute versus fifty gallons a minute.

Recall again these are versus produced volume, so there would be no difference in the curves if there were no matrix diffusion. However, I would think that various amounts of matrix diffusion can be detected in a series of experiments like this at different flow rates.

I mentioned that the reactive tracer testing also relies on us being able to characterize the conservative tracer tests. And this is sort of an example of that in a simulation of a breakthrough curve of a conservative tracer and also a reactive tracer. And I'll explain what these parameters are in a moment. They are retardation factors, but in a dual porosity or matrix diffusion type of model, you need one for the fracture and you need one for the matrix.

So, in one case I am assuming that there is no adsorption on the fracture wall, at least in comparison to the sorption within the matrix.

In this case, I am assuming that you have a retardation factor that is the same regardless of whether you are in the fracture or the matrix. And I am claiming that the difference in these curves is going to allow us to characterize how much--it is going to give us another handle on the amount of matrix diffusion which is occurring. If matrix diffusion is valid, then when the tracer is in the fracture itself, it doesn't have sufficient surface area to really do much adsorbing and delay. So for this curve it is only within the matrix that the sorption is occurring, and you get much less of a delay in the breakthrough curve than if there is sorption occurring on the fracture faces and in the matrix as well.

This points out how important it is for us to characterize the amount of matrix diffusion that is occurring within our system with a conservative tracer before going on and trying to predict the sorbing tracer behavior.

So far I have said nothing about the tracers themselves. The tracer we are looking at right now as our first candidate for absorbing tracer experiment is lithium, injected as lithium bromide in the injection well. And lithium plus ion is the tracer that we are proposing as a sorbing tracer and characterizing the sorption properties in parameters in the laboratory. The idea is to take a model result from a series of laboratory experiments of surface concentration versus the fluid concentration, correlating it with an isotherm parameter model and using those parameters in our study of the field tests. Our models in the field are capable of incorporating both linear and non-linear absorption isotherms. So we are going to be able to take the data from the laboratory and test it against the actual data in the field without any additional adjustability. So if we have the conservative tracer breakthrough curve, we should be able to without any additional fitting of the parameters match the sorbing tracer response in the field, unless we've got the wrong model, and that will tell us whether or not our model is appropriate.

DR. LANGMUIR: Bruce, you've got one sorbing tracer there which is fairly unusual geochemically. What are the plans with respect to other sorbing tracers to be used in your tests? Which sorbing traces as analogs for radionuclides for example? Can you tell us what they are?

DR. ROBINSON: In general terms what we've tried to do is split it up on the basis of mechanism. This first cut at at a sorbing tracer experiment was intended to be about the simplest thing you could imagine in terms of the sorption reactions which are occurring. So it is an electrostatic absorption mechanism and without very many complications in terms of the triple layer theory or any of that kind of stuff.

The idea here is to try that one first, really focus on that one first. If we can then go to later on try to characterize tracers which sorb by a different mechanism.

DR. LANGMUIR: I guess I am asking you what you think they are going to be?

DR. ROBINSON: Right. We've tried to look at the possibility of boron as a tracer which would have different sorption characteristics based on the pH of the fluid.

DR. LANGMUIR: It may actually not sorb at all in or work on tuff.

DR. ROBINSON: Or it may not, that's right. It may not at the types of conditions that we would see. And it may require--that is part of the characterization. It may require either a higher pH in which case you would probably throw it out or either attempt to do a field experiment in which you did something with the pH.

DR. LANGMUIR: Are you going to pick some--I presume you are going to pick some ions which are good analogs for radionuclides. Boron is not.

DR. ROBINSON: Pardon me?

DR. LANGMUIR: Boron is not.

DR. ROBINSON: Right. Boron wouldn't be--boron would be trying to focus on something which chemically sorbs. As far as direct analogs--

DR. LANGMUIR: I suggest you might want to look to look at a thesis by a student of mine named Ann Lewis Rush, which suggests that boron does not sorb on the tuff.

DR. ROBINSON: Okay. It presumably at a high enough pH it probably would, but--

DR. LANGMUIR: But you are not going to maintain those pHs in the system, more likely in the real system.

DR. ROBINSON: It could be done, but it wouldn't necessarily be an ideal test.

This part of the test is basically to get our feet wet with lithium. Elsewhere we are trying to develop analogs which are more appropriate to radionuclides as opposed to just trying to make the step from the lab to the field. We want to do it with lithium first and then elsewhere within Los Alamos they are working on various other tracers which may be good analogs for the radionuclides themselves and I don't remember what they are at the moment.

This is an example though of the result that one obtains in the laboratory in a series of batch sorption experiments, gets the model parameters for adsorption isotherm and then uses those parameters in a simulation of sorbing tracer behavior in the field.

Another way that we can get at the amount of matrix diffusion that is occurring, and also just to look at colloid transport in general and how important it might be at Yucca Mountain is to try to do a test in which we inject in this case polystyrene microspheres of a given size or a range of sizes in order to see if they have the ability to transport over great distances between two wells which are say, 100 to 200 feet apart. We intend to do laboratory tests in fractured core to look at something on a small scale. That doesn't really get at the question of whether they can be transported over the large distances that one is really interested in for radionuclide migration.

The principle here though is that in its simplest form is a microsphere or colloid particle may not have access to this matrix material. And preferentially channeled through only the fractures or even the biggest portions of fractures and thereby transport even faster perhaps than a conservative species which even in the absence of matrix diffusion you may bet enhanced transport due to the colloid migration if the colloid contains radionuclides or has radionuclides sorbed on them.

The only way that I can really see to test that out is to actually do an experiment which you simulate colloids. I mean, you can come up with all kinds of pros and cons as to whether not colloids are really going to be important, but I think the only way to really go about it is to test it in the field, and we intend to do that with the microspheres of different sizes to look at effective the size and in an inner well setting. So it would be a tracer experiment with microspheres as opposed to a dissolved species.

DR. LANGMUIR: Bruce, the microspheres are not going to exhibit the electrochemistry that a real colloid would.

DR. ROBINSON: True.

DR. LANGMUIR: Which maybe a reason why it is going to be retarded, so you are only looking at the physical aspect of the problem, not the whole problem here.

DR. ROBINSON: Yeah. There are a lot of aspects to the colloid problem. The idea here is to try to give them the best possible chance to transport, and that would be by tailoring their surface charge and making it negative so as to really repel the microspheres from the rock surface as much as possible. And if they don't transport in that sort of an admittedly contrived setting, then perhaps we've gone a long way toward eliminating our colloid transporters as a mechanism, because, as you say these other mechanisms which would tend to filter the colloid out or actually result in them sticking to the rock wall wouldn't be present in that case. So that is the philosophy behind it.

DR. DOMENICO: I think it would be very difficult to let people believe that colloids do not transport radionuclides. I think that has been pretty well established, and maybe the bigger problem is to determine whether or not colloids are in the saturated zone at Yucca Mountain. I think if you came up with data that demonstrate that colloids do not transport that would be looked at very, very carefully and very, very closely.

DR. ROBINSON: I guess my opinion on that is that although there have been field tests which have shown transport by colloid mobility, they haven't been done at Yucca Mountain. And one needs to perform the test in as close as possible to the setting that we are interested in. Having said that, I believe that we are also embarking on a kind of parallel path approach to this looking at the actual quantities of colloids and how sorptive they are to the radionuclides and that sort of thing. Let's try to come at it from the other direction as well. Maybe one or the other will give us our best case for or against colloid transport.

My final slide is just to update on the current status of this work. The slide I showed you a few slides ago on lithium sorption, the preliminary analyses have been completed. It was on a material which is not the C-wells material and we are going to go back having learned from those experiments on Prow Pass material from P#1. We are going to run the tests on C-well material and hopefully do a series of experiment which can also reduce the amount of scatter that you saw in that figure. And basically, redo that figure for something more appropriate for our tests which is at the C-Wells, in fact in the Bullfrog member, which is where we are planning right now to do our recirculating experiment.

We've obtained the C-Well core samples by this time now. And we are in the process of setting up to do the isotherm experiments in the laboratory. The isotherm measurements and also other types of measurements to try to characterize in a little bit more detail what something about the sorption mechanism for lithium, although we do anticipate that it is probably fairly simple to characterize. We do want to do a series of experiments in addition to simply measuring isotherms so we have a little bit more confidence in our isotherm parameters.

There is a component of modeling involved in all this and right now we are developing software and carrying out the sorts of parameter sensitivity analyses that I showed you on the previous slides. In terms of field experiments we are working with the USGS in order that we can combine experiments and really get them both done more or less at the same time, using the same equipment. The packer systems that they are developing are certainly appropriate for our tests as well. So, we are going to take advantage of that and also take advantage of the fact that they are doing all this complex hydrologic and conservative tracer testing. Now we'll be able to use that data in order to better plan for our tests.

We are going to be also in the near future performing design calculations to answer questions like how much time do we have to wait before we inject the tracer to set up something that is more or less a steady-state flow field and that sort of thing. And then doing pre-test predictions I think is important to actually make prediction before the test rather than just doing modeling after the fact which is often what is done.

We are going to attempt to make a prediction, at least based on a conservative tracer response, we are going to them predict the sorbing tracer response, to give us a little bit more credibility rather than always tending to backfit and come up with parameters. I think it is a little bit more valid to try to make a prediction beforehand, and that is what we are going to attempt to do here.

I'd be happy to address any other questions.

DR. LANGMUIR: If I might, we are right on schedule. There will be an opportunity after the last speaker to question all the speakers for the last two days. And I'd like to postpone questioning of Bruce at this point for that purpose and proceed with the next speaker.

Claudia.

MS. NEWBURY: Thank you.

Our next speaker is Bill Steinkampf from the USGS. He'll be talking about hydrochemical characterization of water in the saturated zone.

MR. STEINKAMPF: The compulsory introductory slide, having seen that, this is a somewhat different approach because there hasn't been a whole lot of work done with regard to saturated zone hydrochemistry in the project. There is an extant base of data which derives from work from the 50's up to about 1984 with various and sundry bits of information derived since then.

But, there hasn't been anything, since I've been on the project in 1987, any new work or work as reflected in the SCP or relevant study plans carried out.

What the plans are though for groundwater chemistry are to first, as one might anticipate describe spatial variations that exist in the saturated zone with regard to the chemistry. Strictly a descriptive mode to provide information to define, actually define is not appropriate. That is an error. It should be refine conceptual models of the geohydrologic system. And also to provide a base of groundwater chemistry data for numerous uses by various investigators throughout the program, both within the survey at Los Alamos, Livermore and Sandia.

The data that we hope to accumulate will be that which results from examination on two general scales. The regional scale which I'll talk about first, and the existing base of information that we now have derived from two sources, two main sources. This reflects primarily USGS data and also encompasses this which is slightly small scale. This is part of a map that John Czarnecki put up. You'll recognize the large hydraulic gradient here at the mountain, but it extends out several counties. We've got California and part of central Nevada in there. This is Ike Winograd's head map, essentially.

But this is the region, the general region of interest from Gold Flat in the north to Chicago Valley, in the south. And we don't have to worry about time, Don, because I've got a little timer here that will let us know what is what.

This is an existing observation network that the EPA uses. I think this means long-term hydrologic monitoring program. I am not sure. I zipped this out of one of the annual reports. But you can see that they surround the test site--I'm not sure what the rationale for selection seems to be some orientation with regard to structure there, sort of falls within the valleys or right along the valley walls. But, again this is a base of information. In both data sets this comprises about 230 something sites that the data of which range in comprehensiveness from just a couple of parameters to fairly comprehensive analytical suites.

The function of each site was varied and so the intent of the investigator seems to have determined largely what was to be analyzed.

The regional sites will be somewhat sparse. There will be some revisitation as will be planned, and also which will reflect to some extent an opportunistic approach in that as things like mining company holds become available or recognized these things will be visited and if possible information will be procured from them.

One other aspect of the regional system that we'll look at is in some detail is in conjunction with the National Park Service. We'll try to some sampling of a lot of springs that have not been sampled in the past. Quite a few have been addressed by people like Clausen and Winograd and others. But in talking with the people at the Park Service there are quite a few that aren't on the maps and I think they kind of squirrel that information away themselves as to the location and access these sites. So, we'll go in and try to catch these sites also to amplify, or augment the regional picture.

These will give us a little bit more insight with regard to the boundary areas, particularly with regard to Death Valley. The boundary areas of the flow system.

On a more local scale or site scale, some of this has already been discussed. The locations perhaps were not put up. Again, EPA sites on the NTS, and again these vary in the amount of information that has been collected and is available. They range from water sampling to air samples to I think bird samples, reptile samples; there is quite a bit of diversities.

Here we have the exiting water table holes at and adjacent to Yucca Mountain. There are 14 of these. These wells were drilled in the early '80s and have never been sampled other than one or two of them by investigators from the Desert Research Institute. I think four or five of those were sampled in '88. But these wells were essentially drilled, logged and left. They penetrate anywhere from 44 to 99 meters into the saturated zone and provide an opportunity to examine the uppermost part of the flow systems. These are Dick Luckey's water level monitoring sites. Some of the numbers are familiar. I think he showed WT-6 and WT-2.

In addition to the existing holes there are as again have been alluded to by previous speakers, additional data collection sites that have been proposed. John mentioned some, John Czarnecki mentioned some, as did Luckey. Here we have eight additional planned water table holes, again, no more than about 100 meters into the saturated zone. These locations are identified in the SCP. Some for John's studies; some for Dick Luckey's study. These are to be drilled by, hopefully by, non-contaminated methods. The plan is to use some sort of reverse air or a dual wall drilling method that uses no fluids other than air. I think that the drillholes, these are the Fortymile Wash drillholes that John has in his part of the SCP and his study plan. These will be drilled by the same methods. Again, down to the water table, but not significantly far into it.

In addition to these holes there are also a series of boreholes that were planned to be drilled in the Fortymile Wash, again by Czarnecki. I noticed on John's slide, he had 20 to 30 FMN neutron holes. These are fairly shallow holes. It is likely that only these in this area here (indicating), will be completed in the water table. I'm not sure how far down we could expect to see samples that would be useable for hydrochemical samples or sites that would be useable.

We've got ten here so I am either ten or 20 short. I am not sure how to address that. We'll be stumbling over these things if we put that many in.

In addition, after some consideration amongst members of the saturated zone staff, it was recognized of the need for additional boreholes in the northern part north of the mountain where it was desirable. And again, John had pointed to these three 25, 6 and 7. This is on the divide north of Yucca Wash. This is kind of up in Beatty Wash and this is over near Divide on the northern part of Crater Flat. Again these are additional sampling sites that we will be visiting.

We've talked about where; let's say a little bit about what we will try to do at each site. I've got two screens here, one for the chemist and one for the rest of the people. I thought this was a great slide. You can put all kinds of stuff in the periodic table and you can talk all day.

These are the dissolved inorganic species that we will examine or analyze for in every sample. If you don't see one you like there, please let me know and I'll be willing to argue about why it shouldn't be included. The ones that are not shaded are the ones that will be analyzed. Stuff like this is called Iridium. We are going to stay away from that.

And this is essential to rationale for the selection of the species have indicated here. I always swore that I would never go to the lab and just give them the periodic table and say check this out for me, but in this case I don't feel so bad about it.

From the cations and anions and neutral species that we analyze for, we should be able to come up for a means for spatial description, both areal and to some extent vertically. The information combined with field data that will be collected on the site will enable thermodynamic calculations; they will provide a means to estimate the extent of contamination from well construction and conceivably testing that goes on. We should be able to say something about groundwater flow path, possibly about mixing of in member groundwaters, and we should be able to make some statements about the evolution of the groundwater chemistry.

DR. LANGMUIR: Have you got some sort of a field vehicle or design intended for sampling in the field so you--you've got that coming up.

MR. STEINKAMPF: A few slides down.

Those were the dissolved inorganic species.

Now we will talk a bit about another aspect. We'll look at some gases that we plan to sample for and analyze for. These gases will be sampled for both in the UZ and in the groundwaters. In some cases not in the groundwaters. I doubt if we see much hydrogen dissolved in the saturated zone, but we'll be able to look at carbon species like CO_2, we'll look for methane. The sulfur species, the reason sulphur is up there is because of sulfur hexofluoride that's used as a drilling tracer in the air stream. The rest of them are fairly apparent. I should have put fluorine in there for the freon species. The gases should again contribute to the capacity to make some sort of a spatial description.

Again, contamination because of the fact that we have both anthropogenic traces that are introduced and anthropogenic traces that are not introduced in the drilling stream intentionally, but exist. If we are successful in looking at the noble gases, and I see no reason why we should not be, we can come up with a temperature which reflects the temperature of recharge at the water table, not a real recharge temperature, but knowing or having some idea of what that temperature is, we can conceivably back up the ground conditions and make some fairly crude statements perhaps about climatic conditions. And these should also provide some means of looking at fluxes through the UZ to the water table.

DR. LANGMUIR: Bill, I presume you are going to use the noble gas solubilities as a means of backing temperature out?

MR. STEINKAMPF: Yes, sir. We'll have a good control and those are well documented and we'll have a good control on the total solute load at each site so we can make any kind of correction that needs to be done. Most of those have such a flat curve anyway, except for the lighter ones. But, that is indeed the intent there. So those are the gases that we plan to look at both again in the saturated zone and in the unsaturated zone.

I apologize here. We are going to have to make a small correction on your handouts in that this should not be radioisotopes, this should be stable isotopes, stable isotopic ratios, just the title is incorrect. It should be the same as on this slide.

These are the elements whose stable isotopic ratios we will examine again largely in the groundwater. There will perhaps be some done--carbon will certainly be done in the gas phase and perhaps the noble gases. I am not certain about that yet, but that is certainly feasible. It has to be worked out yet with the person I'm integrating with.

The isotopic ratios have again several uses of other parameters due. One provides a means of looking at spatial variation, and this will give us some insight to the plumbing of the groundwater system. It will give us some insight for some of the ratios to sources of solutes in the groundwater. It will enable us to say something about the processes that have taken place in the evolution of the groundwater chemistry, and hopefully it will also again give us some idea about flux through the UZ. We can look at the isotopic ratios in both the vapor phase and in the fluid phases and say something about fractionation. Hopefully that is where we can draw something about fluxes.

Now we will come to the radioisotopes indeed. And again I ask you to make the appropriate change to the title on the overhead. There was a slight QA break down here, but as Alan Flint would say, I think we are still in good science.

The radioisotopes of interest, not too much different from the previous slide. Nothing out of the ordinary here, tritium, carbon, chlorine, krypton perhaps is a bit unusual. The intent with regard to krypton is to look at krypton 85. It has a half life similar to that of tritium and the atmospheric concentration and changing concentration over time is fairly well documented at the test site. The increase with time is well established. Krypton 85 conceivably, or it is my intent to try to use that as a means to indicate when we have satisfactorily developed that part of the unsaturated zone that I want to try to sample.

The rest are pretty much just the K products with which I would think most of the geochemists are pretty well familiar. Strontium 87/86 is something that has not been used extensively in the past in hydrologic systems. It has largely been a petrologic tool. But there has been a fair amount of work done in Sweden on surface waters by a fellow named Yura Noberg. There has been some work that has been over the last few years in survey by people in Zel Peterman's shop. We are starting to accumulate a baseline of information about different water types around the NTS region. And it appears to be a potentially very useful item with regard to looking at things like hydrochemical evolution and groundwater flow paths.

In addition, something that really didn't quite fit on a periodic table, is we will also attempt to look at dissolved organic carbon species, not so much species but generic classes of compounds in saturated zone groundwaters.

There was some work done on this by someone at Oak Ridge on some samples that were collected by Al Ogard back in the early '80s which indicated detectible concentrations of fulvic and/or humic compounds. And Ellen Murphy did some of this when she was in Arizona and is continuing to do this working PNL. There was also some work being done by Burt Allard at the University of Lynkoping in Sweden. He's also working I think up at Segol lake in addition to the work that was done at Aspo were the Hard Rock Laboratory is going in.

But the DOC here will conceivably give us some idea again about spatial variations, but more so probably with regard to paleoclimate and sources of carbon that are in the groundwaters, in that there is a fairly distinct source separation based on the classes of compounds.

DR. LANGMUIR: Bill, I just wondered if anybody was working on the complexation abilities of the DOC in the laboratory with regard to radionuclides since that is what they are likely to run into if there is any kind of breach.

MR. STEINKAMPF: That I don't know, Don. That is part of the reason I tried to stick this in and want to try to do it just to provide the information, should that become a significant issue. If we see significant amounts or organic carbon fractions and I don't think we will, I know we are going to have problems isolating the two fractions. One will come out fairly readily on the X-88, but the other is going to be a problem. Ellen has had difficulties in satisfactorily obtaining, I think it is the low fraction.

I don't know. I am not doing it. I am not sure who is or would be examining the complexation--the potential for the complexation of the organic feeder. I would think that is something that is going on outside the project, but I couldn't point to it. It seems like something that somebody at the University should be or is in involved in or interested in.

We'll also try to look at the carbon isotopes in these fractions if we can isolate enough. Jerry Leenheer with the survey is looking at dissolved organic fraction in surface waters and has done some groundwater in the past. And, Jerry has a methodology that seems to be feasible for concentrating organic carbon fractions from low to extractable or visibly extractable masses. It's not a pretty business or an easy business, but it is something that I am looking at. It seems to be more quantitative and more reliable than the ultra filtration type stuff that Burt Allard is doing. Because of the pH he works at I think that sulfate is a problem in some of his extractions.

That's what we want to do, or what we want to try to get out of the waters. This is kind of a how, I suppose here. In the water table holes, remember we have existing holes and we have new holes. I am going to give you a scenario first for the testing or the sampling we would like to do in the new boreholes that are to be drilled. Again these are going to be dry-drilled. The plan is to stop somewhere as well as we can predict above the water table 20 meters, 10 meters--I am not sure what it is going to be. In some places I think we can pin it down much more readily than others, and start to collect dry core down approximately to the water table. The plan is to squeeze this core, via Al Yang, and look at the matrix waters in a position that is much closer to the water table.

In addition to squeezing the core, the thought would be to have certainly the mineralogy and the pathology done and if we could talk June into it, perhaps look at the Chlorine 36 on not a uniform bases with regard to all the samples, but perhaps with some small percentage.

Prior to drilling on into the water table, the plan is to set some sort of a packer somewhere above the water table. I don't know where and try to extract rock gas or rock atmosphere from above the water table. The plan here is to try to collect water vapor, CO_2 and also look at the concentrations of the gas species that are present.

How feasible this is, I don't know, because we are going to be talking about some sort of a variable saturation. I have talked to some of the UZ people and they haven't really been very encouraging as to how successful this might be, because of the fact that somewhere near the water table you are going to get 100 percent saturation, but at lower potentials how readily we can make gas, how readily we can develop it, we don't know. This is where we will use the Krypton 85 as one indicator of how good a job we've done in cleaning things up.

After the gas samples are collected, we will continue to core into the water table 15 or 20 or 25 meters. These cores would be gravity drained or perhaps centrifuged in air atmosphere and then also squeezed to look at the matrix water, just to see if there is a noticeable difference between the matrix water above and below the water table. And after this, the wells will be drilled to planned depths and sampled.

We will also try to do, probably on some extent on a prototype phase, this gas sampling at existing water table holes. Now these things have been, like Ed Weeks is using 6 and 6S have been blowing and sucking since they were drilled and left. You can go by there and some of them are whistling in and whistling out. It varies depending on the conditions. I don't know how successful that will be but again this will be largely a Methods Development phase and will also give us some insight as to what we can expect, what sort of problems we can expect from the sampling and give us some feel for where we are going to have to set packers and if we can do it in five meters or ten meters or one meter above the water table.

I've addressed pretty much what we are going to look at here, with regard to the samples. Again the fractionation between the vapor and the liquid phases based on the gas samples that are collected and the water that is subsequently pumped from the hole when we do the sampling.

Now one of the problems, one of the other problems that we have is that water levels range from anywhere from 300 to 700 or 750 meters below land surface. This is not a trivial consideration in trying to get a representative water sample from the saturated zone to land surface. It is not difficult to get a pump into one of these holes and pump the water out, REECO says how much water do you want? Do you want 20 gallons? Do you want 100 gallons if the well will make it? That's fine. That's not a problem. The problem is they will probably heat the water up ten degrees in bringing it up and so there is some concern there that we want to try to minimize or obviate any alteration in the water chemistry that might derive from the production.

Well the Swedes have a real nice package of equipment that I've looked at and I find no alternative for, no other available source for similar equipment as far as collecting the water samples. What they use is essentially an umbilical system, a big hose with a bunch of tubes in it on a very large reel, trailer mounted, send this down the hole into the zone of interest and they use a submersible pump to bring the samples up. They have used it as deep as a kilometer. The catch is that their water levels are never more than about 20 or 25 meters below land surface. So, they have a buoyant factor that really makes life a lot simpler as far as putting this equipment down and getting it back up. So we'll have to change the scale of the construction of the umbilicus to some extent.

Essentially you have a control unit linked to a mobile lab, some sort of a field lab that you can run samples into for your sample collection, for your data collection, and any kind of on site analyses that need to be done.

What we are looking at is something that corresponds to the Swedish system, be it SKB's equipment or not, in conjunction with another equipment string that we'll have hanging in the hole. I've got like one section of it here that amounts to just tubing be it 2 7/8ths or 4 inch tubing. The Swedes work inside 54 millimeter boreholes. Packer zones, I don't see more than two or three in these WT-holes, because these, we've got fairly short penetration. Again, some sort of a sliding screen that we can access with a wire line tool to open discrete zones. An in situ hydrochemical tool that sits below the pump and the water comes up through it. And, we get some in situ parameters that will probably be better collected here than at the surface in some attempt through a flow chamber.

DR. LANGMUIR: Which would they be? Are you going to tell us about those?

MR. STEINKAMPF: Yes, sir. We are going to look at this on a finer scale now.

This is just a blow-up of that part of the hole. Again the packers, the sliding screen port, the positive displacement, it's an air drive pump. A fellow by the name of Bob Bennett makes them in Abilene. It's the only pump that I've been able to identify that will lift water satisfactorily or comfortably about 650 or 700 meters at a flow rate of about anywhere from a half a liter to a liter and a half a minute.

Here is the cross-section of the--a hypothetical cross-section of the umbilicus multi-conductor cable for signals from the hydrochemical tool, pH, Eh(3) electrodes and the thermistor. The Eh electrodes are glassy carbon, gold and platinum. It is nice to have the three to compare, they are never the same, but they tend to approach some sort of a similar value. Still no faith in those. We are going to look at couples. We will probably have the oxygen couples and maybe some nitrogen couples that we can look at and perhaps a sulfur couple, although I am not sure about that.

DR. LANGMUIR: How about dissolved oxygen, because these are likely to be oxygenated anyway, in which case Eh doesn't mean much.

MR. STEINKAMPF: Right. But if I didn't collect the Eh data there would be 11 people in the audience saying excuse me, there is no Eh. I agree. I think perhaps in the deeper zones if we have the opportunity to get into the new and I certainly well have the opportunity to get into the new hydraulic hole that Dick Luckey will be drilling up on the crest. Conceivably we will have the opportunity to get into and do some deeper sampling. And maybe we will see some less oxidized waters there. I don't know.

DR. LANGMUIR: Dissolved oxygen needs to be part of your probe down the hole.

MR. STEINKAMPF: Dissolved oxygen is not part of the probe. That is something that we are having to address now. The plans are currently to look at it through a flow chamber, bring it up under ambient pressure which with a 700 meter lift is going to be something like 80 bars coming out of the pump.

What the Swedes have done, is bring it into that trailer under a constant temperature and monitor it there under a closed system. And that seems to be the most doable initially. I cannot see--I don't think it is feasible to try to develop an O_2 measuring capability on this tool. The Canadians have started to do that and money kind of went away and priorities were changed and that was dropped about four years ago and they haven't revisited it. And the stage they were on based on discussions with Jim Ross at AECL, is that it would still be several years away. I am not sure that--I don't have time to do it.

DR. LANGMUIR: Well, you can presumable take a sample of water without gas all the way up to the top and use a wet chemical technique which would be even more accurate. There are some new techniques like this for trace oxygen.

MR. STEINKAMPF: Yeah. Art White has a really nice one that you can just send a package down the hole, but I don't think it will work at the depth that we have here.

The noble gas samples that we collect will be using an oilfield sampler which is essentially a real fancy Nanson bottle with some remarkable O-rings on the end. So as you bring it up the O-rings seal tighter and tighter, and I think that that will work. This is the last one I have Don.

The DO as it stands now will be done at the surface in some fashion, some sort of a simple flow cell or in some sort of a thermally controlled closed system in the mobile lab. And we can monitor that sort of data.

I think that I have touched everything here. One thing that we want to talk about briefly is that I borrowed this idea from MJ's shop, because I sort of sat in and looked over the shoulders at some of the meetings. What I plan to do is to have essentially two sections like this available for the WT holes, so we'll have sliding port here, another packer down here with a sliding port, and also another sliding port below the bottom packer so we can look at the bottom of the hole, the middle of the hole and some place else. I think we can pick spots based on the caliper logs and possibly televiewer logs that are fairly smooth to set these.

In addition, we are going to have transducers in each zone that we sample. We are not going to stress the heck out of these things pumping it at half a liter to a liter and a half a minute, which is attractive for several aspects. But, we will monitor pressure changes probably with some sort of a differential transducer because I would imagine that the changes that we induce will be quite small.

So we get some sort of pumping tests here. It is something that conceivably will be useful to both Dick Luckey's people and to MJ's people.

DR. LANGMUIR: Bill, we probably need to wrap it up here pretty quick.

MR. STEINKAMPF: I'm pretty much done. The logistic problems are the toughest thing we have in this study. And, I would only close with something from Henry Bent. I got this out of a thermodynamics textbook. Until Kirk Nordstrom and Jim Munoz wrote this, this the only textbook I had ever seen, a thermal book that had a cartoon in it. This is the way we feel sometimes, particularly with some of the logistics that we have to overcome.

DR. LANGMUIR: Thanks, Bill.

Claudia.

MS. NEWBURY: That concludes our presentation. The summary will be done by Dan Gillies from the USGS.

MR. GILLIES: This is the warning for anybody that's been asleep that you have only got 15 more minutes, and then according to Don we are going to have some open discussion on at least today's topics and probably some of yesterday's topics, so start making your notes and preparing your questions. We should have ample time for some discussion.

As the slide indicates, my name is Dan Gillies. I am the Associate Chief of the Hydrologic Investigations Program at the USGS. Until very recently as a collateral duty, I was also section chief for saturated zone studies. And at one time the studies that we call paleohydrology. I am no longer in that capacity, but I think that is the reason why I was asked to do the saturated zone summary. So, that is what I am going to do.

Before I do that however, I want to take just a minute to acknowledge someone who has worked very hard to help all of us prepare for this meeting and she is seated here in the front row. I think most people know Candy

Biddison, but I would like to thank her on behalf of the USGS and the other participants for the fine work that she and her crew did in preparing all the visuals for this meeting in a period of less than two weeks.

I am going to summarize as quickly as I can the last six or seven talks that you've heard on studies in the saturated zone. I'd suggest that maybe a good use of this summary would be to help jog your memories or jog your recollection of something that you wanted to ask earlier but didn't. This will be your opportunity once again to kind of flag that item and when we finish walking through this, there will be another opportunity for questions and discussions.

The first actually two studies that we talked about, presented by John Czarnecki, were those studies of the regional saturated zone involving two studies. One which is the data collection effort, and the second one which is the synthesis and modeling efforts. So there are really two SCP studies involved here. And in general the objectives of those studies are to refine what is already known about key hydrologic variables to continue to develop and use some tools like models that you saw quite a bit of, to allow comparison of our current understanding of the system and some alternatives as far as the system is concerned. The study involves obtaining hydrologic, hydrochemical and geophysical data, ultimately to help support models that will be used to help determine magnitude and direction and flow.

Another objective is to synthesize data from the regional hydrologic system with these models at what we call a regional level and also at a subregional level. The models then will be the principal tools applied in order to consider in other parts of the program various scenarios of future climatic or tectonic phenomena that may affect the regional saturated zone as John pointed out in some examples this morning.

Some of the principal uses of data from the regional saturated zone studies will as I mentioned to determine flow paths and velocities for radionuclide transport in the saturated zone. They will also be used as a basis for establishing initial and boundary conditions for more detail site scale models of the saturated zone. And, then as I mentioned as a basis for assessing possible future climatic and tectonic changes.

The important aspects involving geometry and hydrologic properties of the regional saturated zone as John Czarnecki presented them, we have a current concept that there is recharge at the mesas north of Yucca Mountain. We have southward flow, generally southward flow through the tertiary volcanic rocks beneath Yucca Mountain. There is also generally southward flow through tertiary sedimentary rocks, underlying the Amargosa Desert, and we have discharge as evapotranspiration of Franklin Lake Playa.

As a part of the whole regional system, there is we believe also a deeper northeast-to-southwest flow in paleozoic carbonates, and this accounts for spring discharge at Ash Meadows and at Death Valley. Although there are some alternatives to this concept as John Czarnecki discussed this morning that are under consideration.

Based upon the data that has been collected over a period of years and analyzed to-date, there are some major uncertainties in the regional saturated zone. And these uncertainties have been identified in a major way from the preliminary subregional groundwater flow models that have been developed in the past. And I think John made this point that those tools, particularly the subregional model have been used to solve the inverse problem. We need to understand that there is a lot that we don't know about the hydraulic properties of this large regional system, and part of the modeling was to impose some boundary conditions and fluxes on the system and use the model as a tool to help us calculate the hydrologic properties. Given the density of information available in the regional system, and given the prospects of increasing that density, this work on the inverse problem is probably going to continue to be important for the duration of the project.

The inputs though have some uncertainties, and the modeling showed this through sensitivity analyses that have been published. And that is basically where most of these came from. There are some uncertainties about sub-basin boundaries to north of Yucca Mountain. There are some uncertainties as to the continuity of flow from those high areas north of Yucca Mountain down to Yucca Mountain as Czarnecki pointed out in both the potentiometric data and hydrochemical data.

There are also uncertainties concerning the relative amounts of total recharge to the regional system from the things you see listed here; the Mesas, Fortymile Wash, upward flow from the paleozoics and some possibly residual paleorecharge.

There are also uncertainties about the nature and significance of the large hydraulic gradient, and John presented one concept of what may be causing it and what the consequences of that might be this morning.

There is also something that needs to be quantified, is what I am calling for lack of anything else, the early distributed discharge by ET at Franklin Lake. Czarnecki and others have a pretty good handle on the mechanism and on the weights, but need to do additional work to determine the actual areas where this discharge flux occurs and quantify it. Candy, that is the only mistake I've found.

There are studies planned to resolve these major uncertainties. And these have been mentioned. There are plans for additional test holes north of Yucca Mountain to do a better job of defining the potentiometric surface and to eliminate some of the uncertainty there. There are plans for additional hydrochemical sampling and analysis to determine sources of groundwater at Yucca Mountain and flow paths.

There are detailed studies planned for Fortymile Wash which John mentioned this morning. There are plans elsewhere in the site characterization program for test holes and geophysical surveys to investigate this large hydraulic gradient. John said a little bit about that. There are plans for some surface geophysical surveys involving gravity and magnetics that may help us get a better handle on what structurally or in terms of rock properties produces the large hydraulic gradient.

Another thing that is going to continue is model simulations of hypothesis and scenarios, similar to what you saw John Czarnecki illustrate in his talk this morning. In terms of Franklin Lake Playa, John mentioned that there are plans for a number of piezometer nests to measure vertical gradients and Bowen-ratio stations to measure ET at specific sites and the phreatophyte mapping.

Next you heard about the site potentiometric level evaluation. The major objectives of that work as you recall are to define the upper-most potentiometric surface, to look at and analyze long-term trends, to analyze shorter term water-level fluctuations, to determine their cause, and also to use those water level fluctuations as a basis for calculating hydraulic properties. All of this information of course will provide some input to travel time calculations.

I want to make a couple of points about the availability of data to follow up on what Dick Luckey told you. We've made what we feel is a great deal of progress in cleaning up the backlog of historic water-level data, roughly ten years worth of data, getting that data published or close to publication. Spent a lot of work in the last two or three years doing that. We are almost to the point now where the preparation of data reports can be done concurrently with the collection and reduction of data, and of course, that is where we would like to be.

Some key aspects of the site potentiometric-level network, as you recall in what we call the periodic water-level network, we have monthly measurements since about 1981 in selected wells. There are 19 wells currently in this network as of June. Water levels are measured with steel tapes. Dick, as you recall told you that he felt the data was most useful for determining long-term trends, and for travel time calculations because these are the data that will contribute largely to the preparation of maps of the upper most potentiometric surface.

Generally speaking, from this network we see that water-levels are very stable and there are no long-term trends based upon the data collected over the last ten years or so.

In the continuous water-level network, which is the other piece of this, we have hourly measurements since about 1985 in selected wells. Currently there are 12 wells; 19 zones. The water levels are measured with pressure transducers and recorded with data loggers. This network as Dick mentioned is adaptable for high frequency measurements down to one second intervals if desired. This network is also equipped with what we call satellite data collection platforms, something that has just come about within the last year. This allows near real time access to the data from Denver or any other location in the country for that matter.

Dick mentioned that this data was most useful for determining hydrologic properties and I've sort of added this, providing some insight on the stability of the potential repository site. For several years there has been a lot of interest in some of the excursions or parent excursions in the water table, and there has been a sense among some people that that information somehow said something about the stability of this site. And I think as you saw this morning, when that data is picked apart, there are a lot of pretty rational sort of non-doomsday explanations for a lot of what we see going on.

Dick described short-term water-level fluctuations. These are things in terms of short-term that occur over a period of several days as opposed to years or decades. And generally speaking, these short-term water-level fluctuations have been shown to coincide with normal and expected fluctuations of barometric pressure and earth tides.

Excursions--change this to transducer-output excursions on Dick Luckey's suggestion this morning. You heard Dick describe in some detail what we have observed and what we have tried to do to explain these things and some of those explanations. You recall that generally these things are investigated, considered important and are investigated when the occur in multiple wells or in multiple zones of the same well.

The excursions have been classified based upon their amplitude, whether or not they are "expected" and their concurrence with predictable phenomena like barometric pressure change. Dick mentioned that we have established what we call a set of alert procedures to verify excursions, and he described with some examples the methodology for that using the satellite data collection platforms as sort of the real time warning that something is going on that we need to try and verify manually.

I took the risk of throwing some numbers in here and if they are wrong, I'll apologize in advance. Dick mentioned that some fairly dramatic changes in water-level on the order of about 0.3 of a meter have been positively correlated with dramatic changes in barometric pressure because of the passage of storms.

He also indicated that some of the high amplitude excursions on the order of several meters have been shown unlikely to be real water-level fluctuations, and based upon the analysis that he showed you this morning, that those phenomena, those excursions have been attributed to erratic behavior of the transducer itself.

We have some low amplitude excursions, positively demonstrated not to be water-level fluctuations. We have other low amplitude excursions that remain unexplained. And as you recall, one possible explanation, one thing that we are continuing to look at is the possibility of fault creep.

Future plans for site potentiometric levels, as you recall, we want to continue the hydraulic properties and trend analysis based upon water-level fluctuations. There are plans to drill a number of additional wells as you've seen in several talks. We want to continue to investigate these transducer output excursions as they occur, and we also would like to initiate strain monitoring in order to investigate the relationship between strain changes and water-level fluctuations.

Gary Patterson described for you some work that has been underway for two or three years now involving the analysis of strain-related water-level fluctuations. This is incorporated within the cited section of the SCP. The objectives of this work are to determine whether or not this method is truly applicable for obtaining acquifer properties, and if so to use the method, series of methods to obtain acquifer properties before the start of well testing and also at locations where for various reasons well testing isn't impossible.

The advantages as Gary pointed out are that it can be done where well testing isn't practical. We can also obtain data at scales considerably larger than well tests. He also pointed out that it is relatively inexpensive because much of the data to do this analysis is being collected anyway.

Disadvantages involve the plumbing, the casing of the wells, something which is certainly correctable in new wells. It is questionable whether or not it is correctable in the existing wells. The methods of course also assume porous media and that is possibly a limiting consideration for this approach.

As you'll recall Gary Patterson indicated that the atmospheric loading analysis which is the first in this series of analyses each that feed information to the next that the atmospheric loading analysis yields barometric efficiency, hydraulic diffusivity and pneumatic diffusivity, and I think the questions that Domenico was raising concerning the comparison of those two or something certainly worthy the additional consideration.

The earth tide analysis, the next in the series yields matrix compressibility and areal strain sensitivity, and also porosity and specific storage. So, it is a way of getting at storage properties.

You will recall that in the next set of analyses that involve the use of the stress created from seismic waves, from earthquakes and UNEs. At the risk of being long, I also included some numbers in here, but this is what I got out of it that from UNEs we have observed water-level response of about 60 millimeters and closed-in fluid-pressure response of about 1.3 meters in wells at Yucca Mountain.

We also have information from a California earthquake that produced fluid-pressure response of about 140 millimeters and of course the distinction between the closed-in pressure and the actual free water-level response in a well is an important distinction that we need to keep in mind.

The analysis yields peak dynamic strain and estimates of transmissivity. And on that basis, future plans are to expand the fluid-pressure monitoring to include additional boreholes. Again, we would like to be able to initiate on site strain monitoring to include the analysis so that we don't have to sure textbook values or bring values of parameters in from other places.

We also intend to install additional strip-chart recorders so that we can get a complete record of some of these things. It is kind of interesting that this is a good example of how some of the old fashioned equipment works better for certain things than some of the new stuff. It's kind of like a drum and an ink pen, I guess.

The next thing you heard about were the multiple-well interference testing. I'll take the responsibility for this title being different than it appears on the NWTRB's agenda because I suggested at the last minute that Gary change that, and he did, so that is the reason. This is a title that corresponds pretty closely to the SCP activity that includes this work.

These tests, these multiple-well interference tests of course are intended to determine hydraulic properties for quantitative evaluation of flow, determine the applicability of various conceptual models to the site such as anisotropic porous media, or fracture network and also as Gary indicated, another objective is to examine the scale dependency of flow parameters. I was a little puzzled by the statement that said we weren't going to use any of this information for site characterization purposes and maybe when I am finished we can talk about that, because, I guess I though we were.

I understand the Methods Development part of it, but if it works, it seems to me that there wouldn't be any reason why we couldn't use that information.

As a part of the multiple-well interference testing at the C-Hole complex, you'll recall that there are different types of tests. One of them are the cross-hole tests, and these will involve various permeations of pumping and monitoring at the C-Hole complex using a system which we are building in cooperation with the U.S. Bureau of Reclamation in Denver that will allow pumping and monitoring from any one of five different zones in each of the three wells. And each test will be of relatively short duration, three days or so, depending upon what happens, monitor recovery.

MJ mentioned that we'll select these test intervals based upon data such as the cross-hole seismic surveys, temperature logs, tracejector surveys and the previous well-performance tests that were conducted when these wells were drilled back in the early to mid-80s.

One important part of this that hasn't been done yet are these cross-hole seismic surveys. That is something we very much would like to see done. As MJ indicated we are prepared to do and haven't been able to do it yet.

Another important aspect of the cross-hole test is that there are intended to determine spatial and directional variation in hydraulic conductivity, and as MJ mentioned to examine vertical connection between stratigraphic units.

Another type of test that involved the C-Hole complex or the so-called large scale pumping test and I am not clear at this point whether there is only going to be one of these or several. I think it kind of depends upon what we find out, but if they only take 30 days apiece, we have plenty of time to do whatever seems appropriate, it would seem to me.

This type of test will involve pumping one of the C-Holes for a longer period of time, approximately 30 days, and monitoring in more distant wells as MJ described. We can determine, hopefully hydraulic properties at a larger scale at this type of test, and hopefully also get a handle on the hydrologic significance of features like the Bow Ridge fault.

MJ indicated that the analysis of the multiple-well interference test would proceed using a philosophy and strategy that would start with analytical and numerical solutions based upon the simplest set of assumptions that seems to work. And then would proceed to more complex kinds of assumptions. And some of these that MJ discussed are listed here once again just in case you forgot or lost your previous handout, I guess.

An important aspect of these tests is to compare the results of the multiple-well interference test with what we are able to do in single-well tests, and of course that is important, because, even though the C-Hole complex is capable of testing a relatively large volume of rock, it is still small in comparison with to the volume of rock that needs to be characterized. And if it is possible to get an adequate understanding of hydraulic properties from single well tests, then that is something certainly that we need to know and on the basis of that would make decisions concerning additional testing in single wells or additional testing in another or maybe more than another multiple-well complex. And for those of you who have looked at the SCP, you know that there is an activity that would encompass a second multiple-well complex, but there is also a decision point somewhere in our schedule that makes that second multiple-well complex a contingency.

Testing of the C-Holes with conservative tracers, a major objective of this work is to determine storage and transport properties of the saturated zone, and compare various techniques for interpreting the information, various conceptual models like porous media versus fracture-network. And again make the comparison between multiple-well tests an single-well tests. Scale dependency of transport properties is another goal of these tests.

Some aspect of the conservative tracer tests was mentioned that the idea is to use multiple organic tracers since there will be a number of tests conducted in relative close time proximity to one another, each of those tests presumably would be fingerprinted with a unique tracer and that is the reason for having multiple tracers.

Some question as to whether or not these organic tracers would be conservative for the period of time involved in these tests and very much appreciate Don Langmuir's suggestions and words along those lines.

The conservative tracer tests are of several types; injection-pumpback tests, two-well recirculating tests, and multiple-well convergent tests. The analysis of these conservative tracer tests would proceed in a manner analogous to the interpretation of the multiple-well interference test.

Next, you heard about the reactive tracer testing, principally at the C-Hole complex. This is another SCP activity as indicated here. Some of the objectives of that work, could it demonstrate whether or not the lab sorption data is applicable to the field and prove understanding of the actual transport behavior and also evaluate the mobility of colloids.

The reactive tracer testing as Bruce mentioned would be based upon the two-well recirculating type test. This would allow hopefully evaluation and validation of the conceptual model involving fracture flow with matrix diffusion.

The tracer that they are looking at right now is lithium bromide. There was some discussion that was much appreciated on other possibilities.

There are lab and field tests to determine the sorbing behavior of the tracers and the sensitivity analyses planned for matrix diffusion and Bruce pointed out how important the matrix diffusion is to calculations or groundwater travel time.

I think Bruce talked about this, about the work being doing to look at colloids. And I recall that he talked about the size of a colloid being critical to predicting matrix diffusion, or fracture dominated flow, and so there is a plan to engineer colloids of various sizes, test them in the lab with fractured cores and also in the field at the C-Holes.

As Bruce indicated, status of this work is that lab isotherm experiments for the lithium sorption have been designed. They are waiting for core to really run these experiments. They are also developing software to predict the sorbing-tracer behavior prior to conducting the field tests. They are in the process of performing design calculations and also coordinating with us at the USGS on the design and construction and testing of the testing system itself.

The final presentation that you heard from Bill Steinkampf was on the hydrochemical characterization of the saturated zone. This is a study in the SCP that contains several activities. In general, the objectives are to describe the chemical composition of the system and how it varies spatially. Also, to identify chemical and physical processes that influence groundwater chemistry, and to aid in the identification and quantification of fluxes, to, from and within saturated zone.

Bill mentioned that at present there are about 230 sites in the regional study area where hydrochemical data to varying degrees is available. He also mentioned the EPA monitoring for the weapons program that should be helpful in this endeavor. At Yucca Mountain there are somewhere between 14 and 15 existing WT holes. I was making a count and I counted 15, but maybe one of them is on there twice. I am not sure of that.

There are some additional WT holes planned. There are also the existing H-holes B and P holes that can be sampled, as well as holes planned for Fortymile Wash. There are also some other opportunities for sampling in the regional study area that involved existing wells and also springs. National Park Service is involved in monitoring efforts throughout this area, particularly in the Amargosa Desert and over towards Death Valley. And all the activity of the mining companies also creates opportunities for sampling that we wouldn't have otherwise.

Bill ran through the constituents that would be looked at and why they would be looked at and what sort of information could be gotten out of those. And what I did here was simply to pick out those various classes of constituents and list not all of the things they can do, but at least those for which they are uniquely tailored, specialized. And for the inorganic cations and anions that is just composition variation, evolution of the water and carbon flux, if we include the organic compounds in there as well.

We intend to look at gases for the reasons listed. Isotopic ratios, this will help determine recharge temperature and source, and radioisotopes as a way of determining age and the possible mechanism of flux from the unsaturated zone.

And then finally, Bill talked about the logistics of sampling and described what is planned in terms of gas sampling just above the water table. Water sampling from isolated intervals below the water table extraction of water from rock cores, from the new holes both above and below the water table, hopefully this will give us an idea of what sort of fluxes are occurring right at the water table between the UZ and the SC.

Bill described the logistical difficulty in collecting samples and some of the equipment that is intended hopefully to pull this off. Actually, I was glad to hear Don Langmuir say that we ought to have dissolved oxygen on that, because at least he didn't say we didn't need that piece of equipment. And I don't think Bill said this, but that system is very expensive. But, I figure it this way, if the Swedes got one we probably ought to have two.

And with that I will turn the meeting back over to Don Langmuir and hopefully nobody went back to sleep.

DR. LANGMUIR: Thanks, Dan.

Let's open it up right now to the audience as well as to the Board. I think the Board has had plenty of chance during the day to ask questions. Whoever gets to me first gets to ask the first question in any case.

Don Deere.

DR. DEERE: Have you given any thought to taking advantage of the access ramps or the exploratory drifts that will be into the Calico Hills to be a little closer to some of the zone that you are interested in for doing additional testing that might be a little easier to carry out from drilling alcoves, etc.?

MR. GILLIES: Are you thinking about the saturated zone, hydrochemical sampling--your question is in the context of the saturated zone.

DR. DEERE: That you are closer to it and that you can reach down at various levels. I don't know if this would be an advantage or not. And it would be farther along in the program when you already have some information from your first deep holes and you might know a little better what you would like to do different.

MR. GILLIES: I guess I'll have to say that that is not something that has been looked at in detail. Generally speaking, the people that work in the saturated zone studies are not for whatever reason, are not closely associated with the plans for the ESF. But it sounds like a reasonable thing to do. And with that, I'll ask Bill Steinkampf to address that.

MR. STEINKAMPF: I'll address that briefly. The attractive thing about the ramps which are relative to what are done in the field level pretty new information, is that what we seem to be getting more and more like the Swedes' program here in some of the things that are being done. And there has been recently prepared a proposal for some cooperation with SKB with regard to some testing in the ramp at the HRL that is going on at Aspo. And it is a natural. It would certainly provide the opportunity to do the same sort of things that we would like to do there.

DR. DEERE: Yes. If I could follow on with that just to make a statement. In our trip two weeks ago up to visit the

Canadian program, we certainly were impressed with the amount of testing that they were doing in the vicinity of the shaft, in the ground workings and from the underground workings. They are really taking advantage. Because, now they are down at 420 meters.

MR. STEINKAMPF: That's right. You are right there at the site and you alleviate a lot of the problems with getting the sample out. There are other inherent problems, but it does make life much simpler for getting something that is more easily useable and probably more reliably representative.

DR. DEERE: Yes. Because there is going to be a very extensive underground exploratory facility available, and we really should take maximum advantage of its being there in particular that it will be a little bit later in the program than some of the early work from the surface drilling.

MR. GILLIES: I was thinking about that this morning and I almost jumped up to ask the question, probably out of ignorance about what is going on in the ESF. But when Tom Buscheck this morning, he made a statement and said we need to get underground and look at some leaking fractures or something to that effect. And I was thinking about the business of having alcoves strategically located, for example beneath--I guess somewhere in the upper part of the Topopah Spring, but beneath the Paintbrush Tuff under this assumption based upon what we heard today that the Paintbrush Tuff, the base of that Paintbrush Tuff may be a place where ponding, perching would occur. And where there might be an opportunity underneath an area like that to observe that water draining into fractures in the welded unit beneath and being able to observe first hand which fractures were leaking and which that weren't. And I was thinking about what Tom said about a very small percentage of the fractures present actually being fractures that would leak and produce flow. I don't know if there anything like that in the plans.

DR. DOBSON: I think there is something very much like that in the plans and in large part it is USGS investigators that are doing those.

Don, I guess I would like to ask one clarifying question I guess. Certainly in terms of hydrochemistry we will take as much advantage as we can of getting sampling from the underground in all kinds of different settings, in matrix setting in fractured rock and in different stratigraphic units. I guess the question I am asking you is are you suggesting that we should initiate essentially an underground drilling program or something into the saturated zone, independent, or are you suggesting drifting down into the saturate zone?

DR. DEERE: No. I am not really making a suggestion. Just, that you should be flexible enough to see the advantages, even though it may not be in your SCP to gather some very pertinent information that might become accessible to you.

DR. DOBSON: I think we do have a fairly extensive set of drillholes into the upper part of the saturated zone, at least. There is probably some question about whether there might be some utility to bore deep holes in the saturated zones than we have. But as far as getting water samples from the top part of the saturated zone, you have pretty good areal coverage that should allow you to see any major kinds of gradients that are happening at a site scale anyway.

DR. DEERE; I would only suggest that we keep this in our mind that there could be an opportunity that we might want to take advantage of at a future date. That's all.

DR. LANGMUIR: Question from the floor?

MR. WILDER: Dale Wilder. I'd like to follow up if I could. We have been considering some real opportunities that are opening up as a result of the ramp versus the shaft. And I think Dave Dobson had alluded to that when he showed the various places where we can do testing. But in terms of the comment about looking for those fractures which may be making water or weeping as it may be, we are also looking at changing some of the aspects in our study plan to allow us to look at this sigma values that I talked about that Dwayne has been looking at. And we might not be able to do it by merely observing water. We may have to do some tests, but the important thing is that we are going to have to get some judgment as to how representative those are. And so the long ramps give us great opportunity to look at fractures in many areas within the repository area. So that is currently being folded into our revised study plans.

DR. LANGMUIR: Carl Johnson had a question or comment from the floor.

MR. JOHNSON: Carl Johnson with the State of Nevada. I have been sitting here quietly for two days, which is generally not the way I usually am. So Don if you pardon me, I am going to take this opportunity to ask a number of questions.

The first question and it relates to a series of questions that Roy Williams asked of June Fabryka-Martin, and that had to do with the drilling fluid in UZ-1 from G-1. And I've got a question for June if she is still around. The question relates to, you made a conclusionary statement that you believed that the fluid in the bottom of UZ-1 came from the hole G-1. Do you have some analysis supporting information or something that documents that conclusion that you made?

MS. FABRYKA-MARTIN: I made a conclusion. I didn't think scientists were supposed to make conclusions. What was found in the bottom of UZ-1 was fluid that contained the drilling polymer that was used in G-1. And so I think it is safe to conclude that the fluid in the bottom of UZ-1 had at least some component of the water that came from G-1. Now whether it was 100 percent that or whether it was mixing with perched water or water from another source, one can't say that. But I think the thing to do would be to look at the UZ-1 final drilling report for one thing, or else talk to Rick Whitfield who is the expert, the local expert on that.

MR. JOHNSON: Well the point that I was trying to get to was that whether there was analysis conducted, that came to the conclusion that polymer material was found in UZ-1. Because, we have tried for a number of years to get the report or whatever that analysis has been and have been told consistently there never was an analysis done.

MS. FABRYKA-MARTIN: It is mentioned in the UZ-1 drilling report, just in a single sentence though.

MR. JOHNSON: Well, could I make a request that somebody get us the analysis of that?

MR. DOBSON: We will get you what we can find, Carl.

MR. JOHNSON: Thank you.

My next question and it came about as a result of hearing this summary made by Barney Lewis of the unsaturated zone hydrology program. I don't know if Barney is still in the room or not. Well maybe somebody else can answer the question then. Most of the discussion today and even yesterday which related to geochemistry focused on matrix water. I think it is fair to assume that fracture water chemistry may be different than matrix water chemistry. Could somebody describe in brief terms what is the department's plan for collecting and characterizing fracture water chemistry.

DR. DOBSON: This is Dave Dobson. Let me take a quick crack at it and then I will defer to at least one other person that I see here which is Dale Wilder.

If you look in Chapter 8 of the SCP now, essentially our plans are to characterize all the kinds of water that we can get our hands on. So we have a program attempting to characterize the compositions of unsaturated zone pore water. Obviously, you heard Bill and others talk about the program for characterizing saturated zone waters.

If we find water in a fracture that we can collect, we will most certainly characterize it as well, and you can see that in the plans for the underground exploration in the perched water characterization program and things like that.

I guess from a bigger perspective though, from the perspective of performance assessment, what we need to understand is how important it is what the different compositions of water and different pH's and Eh's and things like that, how that would affect radionuclide transport.

The reason I said I might defer to Dale is that for example from a waste package perspective, we need to understand what it would mean if you had a water of the given composition. And certainly when doing the performance assessments, we'll be looking at the potential effects of a range of compositions of pH's and chemistries of water. And so I think it is not our goal to uniquely define the one and only composition of water that could occur at Yucca Mountain during a post closure period and address its ability to dissolve radionuclides. But more to understand what kinds of waters, what different sorts of compositions. You know it is presumably the water that is volatilized in near-field environment when it is heated up, if it is a boiling environment, it is not going to have the same ionic strength as the water that is in the matrix there now. It would be presumably rather lower ionic strength.

Similarly, if you had somehow a scenario where you got saturated zone water up into a repository horizon, the composition of the matrix bore waters wouldn't be all the relevant either. But we feel like we need to understand all of them because we need to really to understand the range of chemistries and characteristics of water that would be likely to be important from a performance perspective.

MR. JOHNSON: I would agree that there is a need to understand the range of chemistries and especially for input into performance assessment. But without specifically collecting and analyzing samples of fracture water, I don't see how you are going to know what that total range is.

DR. DOBSON: Well, I guess all I can say is if we find water in a fracture in the unsaturated zone we will collect it and analyze it.

MR. JOHNSON: Well, I am also asking what the plans are. Your strategy is--I think you've just portrayed it there as sort of an opportunistic strategy, if you find water in fractures you are collect it. But there also could be some strategies developed to enhance those opportunities to collect water in fractures if some kind of recharge infiltration event occurs.

MR. GILLIES: I'm not sure any of the people from the UZ are still here, like Alan Flint and Al Yang, but Tom is here. But, one of the things that we have done is attempted to sample water from some of the neutron holes that we believe got there via a fracture pathway. I can't give you any details off the top of my head on what holes have been sampled. But that is an example of the sort of thing I think you are asking about is what sort of deliberate strategy is there for going out and finding water that has gotten to wherever it is via a fracture pathway. So that is being done.

Bill Steinkampf also mentioned I believe centrifuging water from cores from the saturated zone, did you not?

MR. STEINKAMPF: This is Bill Steinkampf. The plan there is to look for the possibilities of differences between water that we pump from the well which is going to be essentially if not completely fracture water with water that is squeezed from the core of the saturated zone after gravity draining or centrifugation. So there is a comparison there for the saturated zone. I can't address the UZ, other than the neutron holes that are sampled whenever they are observed to be filled with water, or to contain some waters. And that is usually in the case of getting out there after a winter rain or snowfall and get some significant runoff or melt.

But again, that is an opportunistic scenario. But I used the words opportunistic in my study plan.

DR. LANGMUIR: Dale Wilder has been asking--he has been trying to get up here to answer one of Carl's questions. Go ahead Dale.

MR. WILDER: Well what I wanted to do is respond in terms of Livermore's perspective looking at the waste package. And of course that doesn't answer all the questions and certainly USGS and others will be looking at characterizing the water in the fractures in the overall mountain.

But because we do not know what water will contact the waste packages, we do need to look at a wide variety of possibilities. And one of course would be the vadose water chemistry which you've heard discussed, and the other is the fracture water that can come and get in contact with the waste package.

We have a study plan which addresses the change in water in chemistry which may be induced by such things as man-made materials. And so we are looking at ranges of chemistry there, not specifically sampling the fracture water.

We do have a effort ongoing within our geochemistry in which we are looking at using models, EQ-36, whether the water is in equilibrium with the rock, because from what Tom has shown we may have rather fast episodic events and those events may or may no be able to come into equilibrium with the rock. But the rock water interaction work that Bill Glassley and Kevin Kanouse and others have been doing as well as EQ-36 modeling are addressing whether or not water coming down a fracture could be expected to be in equilibrium.

There is also a report out that you may be aware of, I don't know it is just recently been published in which we looked at the water that has been taken from the saturated zone, but never the less represents water that is going through fractures in some extent, and trying to look at whether or not that water, J-13 and other waters could be representative of what we would expect to see.

So there are efforts, not just opportunistic efforts, there are some efforts that are looking at whether or not the water would be in equilibrium geochemically.

DR. DOBSON: Let me add one other note that just occurred to me, and that is in terms of looking and having a strategy for finding places where there might be perched water, I think that is at least part of the rationale for the kind of testing that we are planning with the radial boreholes and characterizing all the contacts in the ramps as we go down.

If our conceptual models are any indication and observation is, then there may be dramatic changes in saturation values across the welded unwelded contacts, and we think that those are good targets, good areas to look for existing fracture water. So the drilling of things like the radial boreholes and actually excavating the drifts in those kind of places will give us an opportunity to test areas with a higher likelihood of finding fracture waters.

MR. JOHNSON: I've got a few more and I don't want to keep the opportunity from somebody else who wants to talk here.

Relative to the discussion that we had on tracers and this is going to be a question directed to Dave Dobson. As the Department knows that whenever tracers are used to inject into waters of Nevada, a permit is required by the

State of Nevada. The Department has filed for that permit. As part of that it was requested that they define the list of tracers they intend to use at the C-Well location which was the intended location. However, in the presentations that were made today, it certainly is clear that not all the tracers that are intended to be used at the C-Well complex have been defined as yet. And so I would just like to have you comment on why the discrepancy is to what has been provided, is my understanding, to the regulating agency in the State and what is actually going on in the program.

DR. DOBSON: Carl is absolutely right. We require permits for all the tracers. And there is ongoing developmental work as you have seen some indication of. But certainly the Department will not use any tracers at the C-Wells or anywhere else for which we don't have permit. And so if the Department comes up with tracers that it thinks might be good tracers, it will submit them in amendments to the permit application to the State prior to their use. And certainly nothing would be used that had not been approved by the State.

MR. JOHNSON: So you intend then, possibly in the future to amend that permit?

DR. DOBSON: It may be, Carl. I am not familiar in detail. You are correct that we have made some--we submitted something and I know that the State Engineer--is it the State

Engineer that responds?

MR. JOHNSON: No, it is the Department of Environmental Protection.

DR. DOBSON: I think they already told us that one of the ones on our original list was not acceptable and that was fine. It came off the list and if there are additional ones that we develop we would file an amendment of some sort. But, you are correct, there is an application in now with some number of potential tracers.

MR. JOHNSON: You might want to make the Department aware that you may want to amend that in the future, just a comment.

DR. DOBSON: Sure. Thank you.

MR. JOHNSON: The second part of that dealing with tracers and it is for John Czarnecki if he is still here--there's John. In your discussion, you mentioned that you were going to be putting in a series of holes down Fortymile Wash and you were going to be conducting infiltration studies. Could you elaborate a little bit more on the fluids you intend to use for that and whether tracers are going to be used as part of that?

DR. CZARNECKI: The intent is to use water as the tracer.

MR. JOHNSON: What kind of water?

DR. CZARNECKI: Likely, J-13--J-12 or J-13. And we haven't selected a tracer as such, but something like lithium bromide or lithium chloride could conceivably be used.

MR. JOHNSON: Then John you are aware that you are going to have to work with DOE for a permit?

DR. CZARNECKI: Yes.

MR. JOHNSON: Okay. Last question and it is for Bill Steinkampf. As he remarked in 1988 DRI sampled, took water samples from seven of the water table holes on Yucca Mountain for the purpose of getting some information on water chemistry. They obtained those samples and did an analysis and Nancy Matuska who is the principal researcher on that produced a report which I think most of the organizations in this room have a copy of. The question though relates to at the time of that sampling, the USGS requested and received splits of those water samples in the field. Bill, I would like to have you talk in three or four minutes about the analysis that the survey had conducted on those samples and what the results were.

MR. STEINKAMPF: You mean laboratory analysis.

MR. JOHNSON: Laboratory analysis, correct.

MR. STEINKAMPF: We requested essentially duplicate samples and in large part they were duplicates except for those collected for Carbon-14 and C-13. We had them analyzed, you said seven wells were sampled. I think only five were successfully sampled. I haven't seen the report from Nancy and I was out there in the field with her.

MR. JOHNSON: We can provide you a copy of that.

MR. STEINKAMPF: Great. Thank you.

I talk with Nancy off and on about this to kind of track it through time. We did not do any sort of significant analysis other than to look at the results, compare them with the results that Nancy provided us with and our results were provided to Nancy for corrobative purposes.

It is my opinion that the samples that were collected were not representative of the formations that the wells penetrated. The samples were collected from inside 2 and 5/8ths ID tubing that Dick Luckey monitors water-levels through. The wells as I indicated earlier were drilled, logged and left. They were never developed or instrumented for hydrochemical sampling. And I do not have a great deal of confidence in the data that derived from those samples.

MR. JOHNSON: Well the--my question really was getting at and what I am interested in is you have done an analysis. We in the State and DRI had never seen that analysis so could you provide us a copy of those analyses?

MR. STEINKAMPF: You mean the lab reports?

MR. JOHNSON: The one that the survey has done on the samples that were collected.

MR. STEINKAMPF: Those were provided to Nancy Matuska.

MR. FORDHAM: John Fordham from DRI. I thought that there were some that were incomplete at the time she finished her report and her work for us. And I never saw the rest of the analysis.

MR. STEINKAMPF: Some of them were incomplete. I know that one of the C-13 samples was broken in transit and the only complete samples that we have and that we received were for WT-14, 15, 12 and I think 10 or 11. WT-7 was satisfactorily sampled; WT-4 could not be--

MR. FORDHAM: Yeah, there were some problems trying to get--

MR. STEINKAMPF: Indeed.

MR. FORDHAM: Using that Bennett Pump is not so easy.

MR. STEINKAMPF: Not in the situation to which it was applied. But I can go back and look and see what has come in. I know that over a period of eight months I sent the stuff the Nancy as it came in, because we don't get all of our results back because of the dispersion of the samples. And some go to Reston and some go to contractors and some go to Lakewood.

MR. FORDHAM: I think what Carl really wanted to know is if we had received everything that was done on that.

MR. STEINKAMPF: I think you did, but I can certainly check to make sure.

MR. FORDHAM: That is really all I wanted. I want to go back and make a strict comparison to her analysis.

MR. STEINKAMPF: We can do that.

DR. LANGMUIR: Any further questions?

MR. MIFFLIN: Marty Mifflin. I've got a question for you, Bill.

In your sampling plan, as I understand it you were assuming that the drilling would be some type of air like dual tube reverse circulation. The question I have, have you considered that you will be blowing both cuttings and water to the surface while you drill once you hit the saturation? Are you familiar with this type of drilling?

MR. STEINKAMPF: In a cursory fashion, yes.

MR. MIFFLIN: So, this also goes for any perched water.

MR. STEINKAMPF: But I think that the coring that will be done will not--will be done using a wire-line core tool.

MR. MIFFLIN: Well, the question is, are you going to drill with dual wall recirculation or are you going to core in a traditional fashion?

MR. STEINKAMPF: We will drill dual wall recirculation and core with a wire-line core cutter. That is my understanding.

MR. MIFFLIN: Okay. Well the point I am trying to make, have you considered that when you do traditional down the hole hammer dual wall reverse circulation drilling, you get back water and cuttings?

MR. STEINKAMPF: Yes. That is why we are going to stop drilling above the saturated zone, core through the unsaturated zone to the end of the water table, and use those two suites of cores.

MR. MIFFLIN: Why not try to get a water sample from your first saturated zone just by blowing it to the surface?

MR. STEINKAMPF: I don't think it would be a very good water sample.

MR. MIFFLIN: It would be better than none, which is what you have now.

MR. STEINKAMPF: I'd rather make hypothetical guesses than base something on bad data.

MR. MIFFLIN: This leaves me with some other questions I have.

Drilling that way, you realize that you are using air and you are blowing air back into the formation, and I don't know how this would affect your gas sampling. Have you considered that problem?

MR. STEINKAMPF: Remember I noted Krypton 85 as one of the checks that we would use to assess the time to sample from the unsaturated zone for the gases. The other things that we will use will be relative compositions, gas ratios, we'll look at the absolute CO_2 concentration. We've got some rough idea of what that should be.

Conceivably we will look at the tritium and use that as an indicator as how reasonable it is to assume that we've got a representative sample. So we will take steps to assure the goodness of the samples that we collect.

MR. MIFFLIN: You will drill with air?

MR. STEINKAMPF: Yes, sir.

MR. MIFFLIN: From the land surface?

MR. STEINKAMPF: That's correct.

MR. MIFFLIN: You have not considered drilling with nitrogen or something like that?

MR. STEINKAMPF: I see no need to.

MR. MIFFLIN: Okay.

Another question I have with respect to your sampling program is the problem that may exist in terms of the cross-communication from one fracture zone to another. Once you open up a borehole there is evidence in some of these other boreholes that you have different fluid potentials with depth.

MR. STEINKAMPF: Significant depth.

MR. MIFFLIN: At different depth, yes.

MR. STEINKAMPF: Significant. Much, much deeper depths.

MR. MIFFLIN: What is that?

MR. STEINKAMPF: The higher heads that were noted were associated with much deeper depths. There is a great head difference over a great vertical difference.

MR. MIFFLIN: That is where they have been measured.

MR. STEINKAMPF: Yes.

MR. MIFFLIN: But they exist in systems over much shorter distances too.

MR. STEINKAMPF: Like 44 to 99 meters?

MR. MIFFLIN: Yes.

MR. STEINKAMPF: Okay. Well, that is a possibility.

MR. MIFFLIN: So the problem you can have some circulation between fracture zones.

MR. STEINKAMPF: It is certainly conceivable. As I indicated we will monitor the heads both within the sample zones above and below using some fairly sensitive transducers in the context of sampling the WT-holes. And that is the only thing that I can think of that will give us some indication of a bypass to the packers.

In looking at the caliper logs of the WT holes, there are some significant intervals with less than one inch or half inch or radius differential over 5, 6 or 10 meter intervals. So I would feel very comfortable that zones can be selected above and below desirable zones for packer situation. That is something we'll have to see as it develops.

MR. MIFFLIN: I have one more comment/question and I forget who it was from yesterday's unsaturated zone drilling in a sampling program. Perhaps, Dave, you could answer this. When I heard a description of the monitoring program, maybe a year or so ago, two years ago, with the downhole packages and so forth, there was also a program of geophysical logging, et cetera, that suggested that those holes would be open for quite a period of time prior to the emplacement of the instrument packages. Is that still part of the plan?

DR. DOBSON: Yeah. I am not sure in any kind of detail about what the schedule is Marty, but they will be open for some period of time. I mean there is--I don't know if we have anybody who is in detail familiar with the drilling schedule, but after the holes are drilled and sampled, there is a period of time in which they are logged geophysically using a variety of different kinds of logs that meet the needs of a bunch of different people. Of course, that brings up one other note I also made which is, in order to get good gas samples as you noted earlier, you can't just kind of go down and take a gas sample, you need to pump the air, you need to pump the gas samples for awhile too. So there is some period of time prior to the installation of the monitoring.

MR. MIFFLIN: My question is this, and the reason I am bringing this up is that as I recall there was a comment made a year or so ago when I asked the question off the record that maybe those holes might be open for several months while all the different logging procedures would occur. And, my question is or my comment is, is it wise to design a program where you are trying to look at both the gas and the liquid phases in the vadose zone, leaving a large diameter hole like that open for a length of time prior to you might say shutting it into your instrumentation. You could considerably change the dynamics of that system--you've already got some holes out there that are changing it now obviously, based on Ed's work, and it seems to me like you might want to rethink whether or not you want to keep changing all of that vadose zone before you really understand it.

DR. DOBSON: Well, I guess I agree that drilling a hole in the vadose zone is definitely a perturbation on the pre-existing dynamics of the system. And certainly you have to have a strategy that gets the most that you can out of the hole, and loses the least data. And so if you'd be interested, I'd be happy to get somebody who is more familiar with the details of the drilling schedule to get in touch with you. And I don't know what the length of time is frankly, but we do have a schedule that you try and get out samples that are as pristine as you can get, and get them put away so that you can analyze them. You try and get the information you need out of borehole logging, and then you try to get the monitoring equipment in as quickly as you can, but there are limitations on each.

MR. MIFFLIN: Well, again a general comment. The planned tests sound very good. They are very detailed, very elaborate, but my own opinion is is that almost in every case you are trying to do too much with a borehole. And that in the interest of completeness there is a real question as to whether you are modifying your systems to the point that you are getting the data you want. Dedicating a hole for one purpose might be more useful until you better understand the system. That's my comment.

MR. GILLIES: Dan Gillies. I had a sense from a combination of Al Yang's presentation on the unsaturated zone, the hydrochemistry, particularly the gas sampling and also from Joe Rousseau's presentation on the UZ borehole monitoring, that they were fairly confident that we would have indicators of when the holes had returned to a state essentially equivalent to their undisturbed state. And one way that I recall was Al Yang mentioned that through some of the work that has been done at Apache Leap experimenting with the SF_6 by using that as a tracer in the gas during drilling, that some amount of time would be required to pump those holes and observe the concentration of that tracer coming back out of the hole and based upon that it had a sense of when essentially pre-drilling conditions had returned with respect to gas.

Joe Rousseau I think said that he felt that conditions with respect to gas flow would return to essentially pre-drilling conditions fairly soon. He was more concerned about settling down of the holes with respect to moisture. But I thought he also said he thought that they had a way of monitoring that, that that was part of the strategy for the three to five years of monitoring to allow sufficient time.

DR. LANGMUIR: As I understand it, the atmosphere has an ambient freon level because of the world's pollution with freon. And it is easily detected at those levels anywhere in the world. And that could be a basis for identifying any air pollution that remained at depth as you were pumping out your system. Once the freon is gone you are back to the ambient bore gases. That's at least one way to do it.

Any more questions from the table or from the floor?

DR. JONES: I have two questions I would like to ask Bruce Robinson.

DR. LANGMUIR: Bruce left, I'm afraid.

DR. JONES: Okay. Maybe I can talk to him later.

DR. LANGMUIR: Well, I want to thank everybody on behalf of the Board and the Panel on Hydrogeology and Hydrogeochemistry, the presenters and DOE for their efforts in presenting the Board with a very informative two days of talks. And with that we can adjourn.

Some of us are going to meet tomorrow again. Don Deere, would you like to talk about that?

DR. DEERE: I just wanted to make sure that you say the best is yet to come tomorrow. Tomorrow is the rock mechanics.

(Whereupon, the meeting was adjourned.)