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I N D E X

CRITICAL DATA NEEDS FOR MODELING FLOW AND TRANSPORT IN FRACTURED UNSATURATED ROCKS

I N D E X (Continued)

P R O C E E D I N G S

DR. DOMENICO: Can we take our seats, please?

Yesterday's discussion dealt largely with some case histories and some information on waste isolation in arid regions. Today's discussion, we'll talk about what will be involved with the critical data needs for modeling flow and transport in fractured unsaturated rocks, so if we're looking at critical data needs, we obviously have to turn to the laboratories; Los Alamos, Lawrence Berkeley, and Sandia, so that's where our speakers will be coming from in the first part of the session.

And then, after a break, we will then get involved in the question, I think the most important question is: What are the consequences of all of this? What does all of this mean in terms of performance assessment? And, from that, we'll look at that probably from the modeling perspective.

And, as usual, we will have a round-table discussion when this is all over. I'd like to encourage all the speakers to sort of stick to the time frame that we have set up here so that everyone gets a chance, and, of course, at the end of the session, we will also open this up for public comment or for anyone who is urging to say something of some consequence.

So, our first speaker for this morning is June Fabryka-Martin, who's going to talk to us a little bit on isotope dating of ground water at Yucca Mountain.

DR. FABRYKA-MARTIN: I think my introduction was done very well by Dr. Domenico, so I'll just pass right on through.

What I'm going to be talking about today are the radiometric studies being done to try to come up with estimates of ground-water travel time at Yucca Mountain, and so, the outline of my talk will be, first, I'll briefly go over what the objectives of the radiometric studies, which include tritium, Carbon-14, and Chlorine-36.

Secondly, I'll review the sample collection protocol very quickly, and then most of the talk is going to be presenting data, and I'll be presenting, also, the chloride concentrations of pore waters. Even though that's not an isotopic study, these are very valuable data, very simple-minded, but I think, nonetheless, justified technique for getting at least a qualitative indicator of infiltration rates and infiltration paths.

Mostly, I'll focus on the isotopic results that Al Yang and I have measured for borehole samples. First, I'll present the results for the shallow boreholes, the neutron boreholes, the upper 100 meters, and then, secondly, two deep profiles, UZ-16 and UZ-14.

And then, what I'm going to show is that--this may be somewhat disappointing, but the data are not unambiguous at all, not in the least. Maybe you already knew that, but what I'll present is two alternative interpretations of the data that are 180 degrees apart. I don't think we can choose between them at this point in time with the data that we have.

Then, finally, there'll be some conclusions. There are several strong conclusions that one can make from the data, and then I'll remind you what those are at the end.

The objectives of the isotopic studies are similar to that of Alan Flint's, and I sort of view this, to some extent, as providing corroborative evidence for the conclusions he presented yesterday.

One is to estimate net infiltration. For example, in the case of Chlorine-36, one can estimate net infiltration rates based on the distribution of bomb pulse, how far down bomb pulse Chlorine-36 has moved.

Secondly, more and more strong, a better application of data is to evaluate infiltration mechanisms to identify where fracture flow may be occurring as opposed to matrix flow.

Thirdly, estimate spatial variability in ground-water travel times, and, here, the radiometric techniques start to show their real potential, because there's really no other way of getting at such data or such estimates; and, finally, to identify evidence for fast paths in the unsaturated zone. And, here, I want to remind people that the fast paths that are identified by looking, for example, for bomb pulse nuclides are not the same as the fast paths that one talks about for the repository. All we can identify is a fast path from the surface to the point of measurement.

That really may or may not have anything to do with the fast path from the repository disturbed zone to the accessible environment, and that's an important point to remember because there may be other pathways by which the bomb pulse nuclide can get to the point of measurement that does not pass through the repository block at all.

The geochemical tools that are available to us are being measured in the program are the radiometric tracers of ground-water travel time--and here, I've listed them again. They all have drastically different half-lives, and, as a result the applicable dating range is quite different. All three are bomb pulse nuclides, for one thing, and so that means that they are sensitive for showing the presence of ground water that's less than forty years old, but the timing of the signals are very different, and that may be an important point in doing the interpretation.

Secondly, Carbon-14 has a half life of 5700 years, so, theoretically, under ideal conditions, its applicable dating range would be about from 500 years to 40,000 years. Chlorine-36, with a half life of 300,000 years has a theoretical dating range, if anything went perfectly, which it never does, of, say, 50,000 to maybe up to a million years.

Now, the other geochemical tools that I won't be talking about so much, there's a variety of other isotopic tracers that are being collected within the project; Strontium and uranium isotopes, and the stable isotopes of water, as well; pore water chloride concentrations, which I will be touching on briefly; and, finally, a whole slew of other pore water geochemical characteristics are being collected under Al Yang's program.

Now, who are the users of these data? There's two general groups; the hydrologic flow modelers, for one. They use them to validate, or they intend to use the data to validate or calibrate their models, especially the conceptual models; evaluate boundary conditions; and to provide corroborative evidence for the calculations; and then the solute transport modelers use the data as well, primarily for the corroborative information.

Now, the boreholes for which we have isotopic data I show in here. This is somewhat out of date. It's a slide

I got from Ed Kwicklis, which no one has updated yet, but it'll give you at least a general idea that the boreholes for which we have sample results surround the repository block. There's seven holes for which we have Chlorine-36 data. Six are shown here, and one is N-11, way up in the north part, off the map. The number of holes that we have, I guess, five holes for tritium within that, and, oh, I don't know, four or five holes for Carbon-14. There's only two holes for which we have all three isotopes, and that's UZ-16 and UZ-14, which I'll be showing later on.

Now, the other thing I wanted to say here is that all the holes are dry-drilled, for which we have samples. The sequence is they core the holes. That's what provides the sample for Al Yang to do the squeezing and extraction of the fluids for his analysis. I get the corresponding ream bit cuttings, so we're getting samples from the identical intervals, but they're still somewhat different in the size and the point at which they're collected.

Now I'll talk first about the Chloride pore water concentrations. The Chloride pore water concentrations are an indication of the amount of evapotranspiration the water has undergone before it becomes infiltration, and this simple schematic is to show that idea.

When the rain water comes down, it's very dilute. It has extremely low Chloride concentration, say, one milligram per liter or less, and then, as the water passes down through the root zone, the concentration increases, until finally, when it passes beneath the root zone, then, theoretically, if it's just one-dimensional downward piston flow, the concentration will stay constant, and Bridget, I think, mentioned yesterday that more commonly, one actually sees a bulge and it starts to decrease, which might be due to climatic variations in the past affecting the rate of evapotranspiration, or possibly, instead of piston flow, we're having some fast path leading from the macro porous flow.

Now, infiltration is the inverse of that, is inversely proportional to that, so when the rain water first falls on the surface, it's like an instantaneous infiltration rate, but then, as water is extracted from the root zone, effective infiltration rate drops until once it gets below the root zone. Then, by definition, that is infiltration rate.

And so, what one does then, if the assumptions are met about knowing a constant Chloride deposition rate, and so forth, is by comparing the pore water, one can calculate from the pore water what the effective or apparent flux is, and here we have done that with a number of holes.

I've broken it out by different units here so that in the alluvium, we have three holes for which we have pore water concentrations, and they range from 200 up to almost 6,000 milligrams per liter. The corresponding apparent flux that one would calculate from that is far less than one millimeter per year, and this is backing what Alan Flint was saying yesterday about thick alluvium being an effective barrier to infiltration, but one gets a completely different view when you look at the pore water chemistry of the non-welded units, the PTn, for example.

Now, these are Al Yang's numbers, and the concentrations here range from 35 to 94 mg/L, much more dilute than what one sees in the alluvium, and the corresponding apparent fluxes range from one to three mm/yr, so, apparently, the water that's getting down into the PTn does not get there by way of the alluvium, but by some other pathway.

It's similar for the Calico Hills non-welded. The average pore water concentrations are fairly dilute compared to what it is in the alluvium, again, suggesting fairly fast apparent fluxes.

Now, the next point I want you to pull out of here is that these apparent fluxes, in some cases, are greater than the saturated hydraulic conductivity of the overlying matrix. For example, the Calico Hills non-welded is overlying by Topopah Spring welded. The saturated hydraulic conductivity at that unit is about 2 mm/yr, so here we have apparent fluxes of three to six mm/yr, so this is, again, evidence that the pathway by which the water reaches these units is not by matrix flow through the overlying welded unit, but, rather, by some other faster pathway.

Next, I'm going to turn my attention to the Chlorine-36 profiles. These are all from the upper 100 meters of the mountain, and what I've tried to do is contrast these two slides. One, these four holes are all from channels and terraces where the alluvial cover is fairly thick, ranging from, oh, let's say five to ten meters or so in thickness. The next one, which I want you to contrast this with, is where the alluvial cover is thin, or absent altogether.

Now, the important thing to look at for these data is all we're looking is where is the distribution of bomb pulse Chlorine-36 at this time? So, what we want to do is compare where the measured Chlorine-36 here, and that's plotted here in units of x 10^{-12}, where that measured Chlorine-36 is relative to the present day meteoric background, which is a ratio of .5 x 10^{-12}.

And, what should jump out at you is that, with one possible exception, there's early bomb pulse in the alluvial cover, in the alluvium. There's no evidence for bomb pulse penetration down below the Tiva Canyon, well into the Paintbrush non-welded from these particular holes, and so here, again, is yet another indication that the alluvial cover acts as a barrier to fast downward movement of the water.

But you get a dramatically different perspective when you look at the wells where the alluvial cover is absent. Now, again, I've had to shift the scale. Before, the full scale went from zero to 1.5. Now, to make sure I could fit all these points on, I've had to expand the scale to zero to 5, and even up to zero to 30, in this particular case.

The common feature here, again, is the meteoric background, which is .5 x 10^{-12}. Now, when you look and see what data points are above present day meteoric background, every single point is, so, apparently, we see bomb pulse penetration, definitely in these two holes, getting through the Tiva Canyon, well into the PTn.

Before I move on to the conclusions here, the ratios are actually so high, there is a possibility of contamination of the sample at the time of collection, which, they may have used a contaminated piece of equipment. I'm checking that out now, and we should have the results within two months, because if this were true, it would only be the cuttings that were contaminated. I've got the corresponding core. We've processed those samples, and we should have results by end of August to see whether or not these ratios are still so high.

But, regardless of that, these data are fine, and I think what I want you to get from these two slides is that the bomb pulse Chlorine-36 penetrates into the PTn under the side slopes in the ridge top locations, where the alluvial cover is thin or absent, but not where the alluvium is fairly thick. So, the conclusion is that initiation of fracture flow is most likely where the alluvial cover is thin or absent.

Now, what happens, though, once it reaches the PTn? I mean, these results so far shouldn't have surprised anybody. I'm thinking of not even analyzing any more samples in the upper part of the profile, because I think that's fairly conclusive, but what happens deeper?

Here I show the profiles. Actually, this is an historic occasion. This is the first time that tritium and C-14 and Chlorine-36 have all been plotted on the same graph. I really like this. Anyway, let's talk through this now.

These are the profiles of tritium, shown in the black squares; Carbon-14 for the aqueous phase, shown in the red and the gas phase shown in the blue; and, over here, Chlorine-36, the chloride ratios. This is a function of depth from zero down to the water table, which is slightly less than 500 meters.

The tritium first. Fortunately, Al Yang's in the audience, I think. Al, are you here?

DR. YANG: Yes.

DR. FABRYKA-MARTIN: Oh, okay. Good. So, correct me if I'm wrong.

When we look at the tritium signal, what we want to do, basically, is look to see if it's above or below about 10 to 20 tritium units; right? It's only when it's above 10 to 20 tritium units that it's unambiguous indication of bomb pulse. Am I stating that accurately? Okay.

And so, as a result, when you look at this profile, you see most of them are at or below that threshold, with a couple exceptions. No surprise up in the PTn, possible evidence for bomb pulse penetration of tritium. I'm not sure what one would say about this particular point. I think it's about 30 tritium units, so that might suggest bomb pulse into the TSw; and then, finally, two peaks down here, one extending up to about 105 tritium units, so there's some suggestion that there may be bomb pulse penetration of tritium down to this depth.

Carbon-14, one gets sort of a different picture. It's about a thousand-year Carbon-14 from the aqueous phase up in the PTn, and then zero to 5,000 years travel time--these are uncorrected ages--down in the Calico Hills non-welded; and, finally, the gas samples, about the same, one to 3,000 years up in the upper part of the profile, 11,000 years down in the lower part.

This, I don't think is coincidence, but the ground water at this location is probably on the same order of about 11,000 years. Is that right, Al?

DR. YANG: That's above the ground water.

DR. FABRYKA-MARTIN: Right. This is above the ground water table, but I believe the ground water itself is about the same activity, and I think that's an important point that I'll come back to.

Now, let's look at Chlorine-36. There should be three things you should pull from the Chlorine-36 profile. One, you've seen in the upper part of the profile before, a bomb pulse in the alluvium, no question about that. Then, apparently, one would estimate older ages through TSw, but I'll come back to that. That may not be a safe conclusion. Apparently, younger ages in the elevated signal in the Calico Hills non-welded, and then older ages again in the Prow Pass welded formation.

One thing I want to say right away, though, this may not be bomb pulse. A lot of people are saying it is bomb pulse. It's true that it's elevated above present-day background, but that's a totally different thing from saying bomb pulse, because there's another explanation, too, that can't be ruled out, and that's that that ratio, that initial meteoric ratio, may have changed over time, and there's two lines of data or two sets of data that support that, as well as some theoretical arguments.

Here is one set of data. These are packrat midden samples from Pyramid Lake, around Reno, Nevada, and they were dated by Carbon-14, and then the Chlorine-36 to chloride ratio was measured. This is from a master's thesis that should be done this month, actually, by Mitch Plummer down at New Mexico Tech.

And what you can see here is that the ratio for the past, oh, 10,000 years is about half to maybe even a third of what it was, say, 15 to 35,000 years ago, so there's evidence that that initial meteoric ratio may have changed even over the--well, may be changing over time, and that could happen from one of two ways, at least one of two ways.

One is if the Chlorine-36 production rate in the atmosphere has been changing; for example, due to geometric field strength variations. Evidence for that, actually, is also beryllium profiles in ice course shows that that's the case, but it also may change if the chloride deposition rate has changed; for example, due to storm tracks coming along different pathways, or other mechanisms.

Mitch is continuing this work for me this summer, this time using samples that have been archived by Desert Research Institute that were collected in the vicinity of Yucca Mountain, and his first set of samples is going to be two suites, one clustered around, say, zero to 1,000 years old, another clustered around 30,000 years so we'll have two populations and we can see whether or not the differences are significant.

There is a second data set that supports the variation as well, and that is Chlorine-36 profiles from deep alluvium at Area 5, Frenchman Flat, and they, just similar to these data, they also show a maximum ratio. Scott, what did you say, it was like 750 was the maximum ratio; is that right?

DR. TYLER: About 800.

DR. FABRYKA-MARTIN: About 800, and here the maximum ratio I saw is, I think, 675, something like that.

Now, the other thing that I want to point out about that profile--and here I show it again, the Chlorine-36 profile--is that when one looks at these points, knowing that the half life is 300,000 years, the first thing that will occur to people is, "Well, geez, this water travel time must be extremely old. I mean, this suggests travel times of, oh, 400,000, 500,000 years or more." That would not be a correct statement, because when these samples are collected, as you can imagine, as the reaming bit goes down there, it's breaking open a rock fluid inclusion and releasing dead chloride that dilutes the signal, and it gives you a falsely old age.

The way I'm trying to adjust for that is by looking at the bromide/chloride ratio, and the idea is that the bromide/chloride ratio of the fluid inclusions is significantly and predictably different than that of the meteoric halides that we're trying to use for estimating travel times, and here I've plotted our measured bromide/chloride profile for this hole, and what I've shown here is my best guess at the present time of what that end member is for the rock bromide/chloride, and the meteoric bromide/chloride.

What you can see is that it suggests that the samples from the non-welded units probably don't need much of a correction for this effect, but the correction from the welded units could be fairly massive.

Now, normally what you'd expect me to do is next throw a plot on there that shows you what the corrected ages are. I'm not prepared to do that as yet because there's a lot of uncertainty about what that meteoric bromide/chloride ratio is. It appears to vary depending on how long the halides hang up in the root zone, and so what one would estimate this line to be for a fast path, where it doesn't hang up, and it passes through the alluvium very quickly and doesn't hang up in the root zone is dramatically different than what one would get if one looked at halides that have come down slowly through the alluvium.

You can see that right away by seeing all the alluvial samples cluster around a ratio of about 5 x 10^{-3}, but you can see if I assumed that was my meteoric ratio, then a lot of these samples fall in the so-called forbidden zone, where they don't make any sense, but this ratio is about the average that applies to the surface soil sampling I've done in the area.

Next is a profile for UZ-14, where we see some similar features; first, looking at tritium. One would say that based on that guideline of 10 to 20 in tritium units as being the threshold for unambiguous bomb pulse, that there's no evidence for bomb pulse tritium anywhere below the PTn; that those are all within the noise factor.

Carbon-14 is somewhat similar to the previous case, where there's really no difference between these two populations of samples from the PTn and from the Calico Hills non-welded. They both suggest ages of no older than 3,000, and then gas, I think, is indistinguishable from the aqueous phase as well.

Chlorine-36, also, one sees no indication for bomb pulse Chlorine-36 in the ream bit cuttings, except within the alluvium, or no evidence in the cuttings. Perched water is another case. Here, the Chlorine-36 signal, the hole was baled several times, and then pumped several times, and then baled again, and with one exception, it pretty much monotonically increased over time, and the last few samples were all here, which happens to be the same ratio, same peak ratio as what we saw on the Calico Hills non-welded for the UZ-16 profile. I don't know if that's coincidence or not.

Well, what can we say about these data? I had suggested earlier that there are two dramatically different interpretations, and here they are: This Interpretation No. 1, I'll call the face value interpretation. You look at these data and immediately you say, "Well, okay, no question. Fracture flow is transmitting bomb pulse nuclides below the PTn under some conditions. There's young water, less than, say, 10,000 years throughout the unsaturated zone, based on the Carbon-14 data, and the Calico Hills unit contains a significant component of bomb pulse, based on the elevated tritium, the young C-14, the elevated Chlorine-36."

The arguments against that are not impossible ones, they just seem unlikely to some people. One is it seems that it would be unlikely to have a continuous pathway from the surface extending all the way down to the Calico Hills non-welded unit, because the PTn itself has a fairly low fracture density, as does the Calico Hills unit.

Secondly, it would require precipitation capable of initiating fracture flow during those peak years for the global fallout of tritium. However, as Alan Flint alluded to yesterday, those years were unusual, were drier than average, so it's not impossible, but it makes it difficult to occur.

And, finally, most importantly, it implies that the interaction between the fracture and matrix fluids is negligible; that there is no matrix imbibition to dilute the signal down and retard it, and this is a critical solute transport issue, even more so than a ground-water travel time issue.

Now, I think there is an alternative view, although it, itself, has problems, and at this point, Al Yang asked me to say that these are all my own thoughts, and he doesn't necessarily agree with them, as he will make clear. Is that okay, Al? Okay.

So, Interpretation No. 2: That is there's no question that there's bomb pulse water reaching the PTn in some locations, but it makes sense that the PTn would be a barrier to any additional downward fracture flow. We think, based on the Chlorine-36, that the water in the TSw unit may well be greater than 50,000, maybe even hundreds of thousands of years. That's still a possibility, and it is a second interpretation.

Now, here's another important point where it differs from the previous interpretation, that we expect that there'd be a higher vapor flux through the fractures that would be driven by temperature and pressure variations in the unsaturated zone. In contrast, Interpretation 1 assumes that gas flow is only by diffusive flux only, no advective flux.

Then, also, water in the Calico Hills unit is younger than that in the overlying Topopah Spring, but it doesn't contain any bomb pulse. Instead, the elevated radionuclide concentrations are due to a higher meteoric Chlorine-36 to Chloride value in the past; and, secondly, isotopic exchange of the pore water CO_2 with CO_2 in the gas phase.

And finally, C-14 in the gas and the liquid phases above the water table are maintained in equilibrium with C-14 in the saturated zone. Now, this interpretation also has its problems, and if I don't say it, Al will, so the arguments in favor of it is it accounts for the elevated C-14 and Chlorine-36 in the Calico Hills. It's consistent with our expectations about the gas/liquid isotopic exchange for C-14. It doesn't require a continuous fracture pathway extending from the surface down to the Calico Hills unit, and it's qualitatively consistent with predictions of flow models, as Andy will show in his talk.

The arguments against it are pretty strong, but, one, it doesn't account for the two high tritium values in the Calico Hills non-welded; and, also, Al asked I add this: That there's no strong field evidence supporting that isotopic equilibrium does exist for the carbon isotopes in the gas and liquid phases in the unsaturated zone, and this is sort of interesting, because Al and I look at the same data and come to different conclusions.

Now, let's see the conclusions. I was going to slip one more overhead in there, by popular demand, after Alan Flint's statement yesterday.

Okay, conclusions. I mean, despite the fact that the interpretations are dramatically different, there are some things one can take from the data, even at this point. One is that the infiltration fluxes through the thick alluvium soil cover are low, and that's based on the high chloride pore water concentrations, the limited depth through the bomb pulse Chlorine-36 has penetrated, neutron logging of moisture, changes in moisture content.

Secondly, there's no question that fracture flow into the PTn, through the Tiva Canyon welded into the PTn does occur under some conditions, getting both bomb pulse Chlorine-36 and tritium from other holes that you haven't seen occurs in the PTn, low chloride concentrations in the pore waters. The bromide/chloride ratios themselves act as a tracer to infiltration processes, neutron logging, and field observations.

Thirdly, there does appear to be a fast flow path into the Calico Hills unit at UZ-16, at least, but whether or not it's fracture flow through the Topopah Spring welded, or lateral flow from a location with a higher recharge rate is an open issue, and Andy will address this more in his talk. The evidence for the fast flow path are the bomb pulse tritium, elevated Chlorine-36, very young C-14 ages, and low chloride concentrations in the pore water.

Thirdly, there's an apparent radiometric age reversal when one looks at the Chlorine-36 data for UZ-16, where you have apparently younger water in the non-welded units overlying older water in the welded units. Did I get that backwards? No, this is right; younger water in non-welded units underlying older water in the welded units, so the water in the PTn appears younger than that at Tiva Canyon. The water in the Calico Hills appears younger than that in the Topopah Spring. Whether or not that still holds up when the age correction method gets worked out is still an open question.

And, finally, in case no one picked up on this already, the radiometric data suggests it's a very complex flow system. You have multiple flow paths contributing moisture to any given point, and that's shown by the highly variable radiometric and geochemical signals that, in places, seem disconcordant or at odds with one another.

So, the lessons learned from this so far are, first of all, that we ought to expect disconcordances among travel times indicated by different radiometric methods, and, in fact, they're inevitable. They have to be, even under an ideal situation, and here I've shown just one reason why, and that's because as soon as one has mixing of waters of different ages, you get disconcordant radiometric travel times.

I've tried to make that point here twice; once, just with a conceptual model, and, secondly, with a simple calculation. This conceptual model is meant to show three water age travel time distribution, two of them just bell-shaped curves, but with different sigma values. This blue one would represent, say, a mixture of fast-moving fracture water with slow-moving matrix water that's much older, and the idea here is that you could conceive of these three populations having the same identical average--whatever that means--travel time, but they would have dramatically different radiometric signals, radiometric travel times.

Then I tried to make that point again by going through a simple calculation, and here, the idea is you have, of the sample that you're looking at the radionuclides in, 1 per cent of it is fracture flow that's young enough it actually carries bomb pulse nuclides. 99 per cent of that volume is old matrix water of four different ages, and I have four cases here.

So, you imagine, you take this 1 mil poured into 99 mils, so what do you get? Okay, if the matrix water was 5,000 years old, then the C-14 signal would be about 55 per cent modern carbon. It'd be about 1 per cent at 125 per cent modern carbon, plus 99 per cent at about 50 per cent modern carbon, and so the age that you'd get would be about 5,000 years.

But for Chlorine-36, here you'd have, adding 1 per cent bomb pulse Chlorine-36 with something that's essentially undecayed from the present day signal, so you'd still get a signal that's slightly above present day, so you'd look at that and say, "Ah-ha, bomb pulse, modern."

Now, let's say what if the matrix were 50,000 years? Now, the bulk of the C-14 in your sample is coming from that 1 per cent fracture fluid, but the matrix fluid itself is essentially dead, and so the apparent radiometric travel time, based on C-14, would be 35,000 years. With Chlorine-36, 50,000 years is just a night, a single night. It doesn't decay very much, say, 8,000 years. Actually, you'd look at it and you'd say, "Present day, but pre-bomb."

What if you had 100,000 years? Well, now, the only source of C-14 is brought in that 1 per cent fracture fluid, and so one would calculate an age of 36,000 years, and, in fact, it doesn't matter if it's 500,000, a million, a billion years, you're always going to get that 36,000-year-old apparent travel time from C-14, but the Chlorine-36 increasingly more closely reflects the hydraulic age of these older waters.

So, the point here is to say that it's a very--it's almost impossible to imagine that one doesn't have mixing of waters of dramatically different ages, and as soon as that occurs, one is going to get these disconcordances like this, and Andy's going to reiterate this point with his particle tracking results from his flow model.

Second lesson learned is we need multiple radionuclides, and we need to continue, because you get a different view of the mountain if you looked at just one of these, or even just two of these than you'd get by looking at all three of them together.

Thirdly, one can't separate out the hydrologic flow aspects of this issue from the solute transport aspects, because the physical and geochemical processes are controlling the transport, even though the so-called conservative tracers, such as by matrix diffusion and dispersion, so it's not just a hydrologic flow model that one needs to use to look at these data, but, also, the site transport model.

And, finally, no site model is going to be able to reconstruct all the radiometric signals, for no other reasons than difference in spatial scales, and you can see that the tritium signals are varying over centimeter scales, and our blocks, our model blocks on the scale of 100 meters on the side, tens meters a side, so there's no way we're going to be able to reconstruct those signals.

With that, I'll stop and ask for questions.

DR. DOMENICO: Thank you very much, June.

You know, the Board has been extremely supportive of this kind of work. It's prima facie evidence of movement through that mountain, so I think your presentation is very timely, and I'm certain that there's some questions here that some of the Board members would like to address to you.

Don Langmuir; Board.

DR. LANGMUIR: June, you posed two interpretations. What has to be done to take that down to one, and can it be done?

DR. FABRYKA-MARTIN: I think the answers are there. I think the--

DR. LANGMUIR: In the existing data, or additional data you'd need?

DR. FABRYKA-MARTIN: I think Al Yang's data holds the answer. It's just a matter of more people becoming familiar with those data and studying them, and spending as much time with them as Al has, for one thing. I mean, the major problems, more than anything else, is, I guess, the issue of gas/liquid exchange. To me, that's one of the most basic questions there is, and so that's one thing that needs to be resolved. I think with his data on Carbon-13 isotopes and pore water chemistries, that the answer does lie in that set, but few of us have had the luxury, or, I shouldn't say luxury, the exposure to the data that he has.

The other thing is the tritium issue, too, with what those high peaks are. I honestly don't know how to resolve that issue, because it's hard to come up with an independent way of checking those data, so the analysis itself destroys the sample. Is that right, Al? Once the water's extracted, that's your only shot, so it's just a matter of following careful QA procedures and...

DR. YANG: Al Yang, USGS.

Now, for that tritium pulse, this afternoon, not this afternoon, maybe for the round-table, I can put more data up on the chemistry, and a showing of the ground water would correct it. It shows some of the data from near the surface, calcium bicarbonate-type water, and it's in the ground water. That's evidence of some of the fast paths, so that date, if you want me to show now, or maybe this afternoon?

DR. DOMENICO: In order to be timely here, we've got, according to my calculations, about seven minutes, but I'd like to give June the courtesy of any further questions from Board members or staff.

DR. LANGMUIR: Can I ask one more?

DR. DOMENICO: Certainly.

DR. LANGMUIR: Is this information that you're obtaining and Al is obtaining going to give us any read on lateral versus vertical movement?

DR. FABRYKA-MARTIN: Oh, yes.

DR. LANGMUIR: I mean, in the Solitario Canyon, are you collecting the kind of data that will provide insights into flow directions interbed, and that sort of thing?

DR. FABRYKA-MARTIN: Say again?

DR. LANGMUIR: Interbed flow, for example, from the Solitario Canyon area.

DR. FABRYKA-MARTIN: Well, okay, I should back up. First of all, the data cannot say anything about pathway. All it says is that you see this at that point, so it's in conjunction with the modeling that we can rule out, or at least rule as one scenario being far less likely than another, and I think that's the point Andy's going to make in his presentation as well.

Does that address your question?

DR. LANGMUIR: In part, yeah.

DR. DOMENICO: I don't think there's any question that the occurrence of bomb tritium in-depth at Yucca Mountain has got to be explained at some point in this program. I don't think there's any doubt about it, in the sense that that does represent prima facie evidence.

Any questions further from--Cantlon; Board?

DR. CANTLON: Recognizing your caveat that fast pathways for these radioisotopes are not relevant necessarily to movement of radioactive materials from the repository, with hindsight, would it have made better sense to pick a site with much deeper unconsolidated materials over the footprint?

DR. FABRYKA-MARTIN: I don't think so. I think any site that is studied as intensively as we've studied this is going to come with its own complications.

DR. CANTLON: But the movement of materials, if you have evapotranspiration of very sizable amounts is going to be very different than where you don't have a high level of evapotranspiration.

DR. FABRYKA-MARTIN: But even aside from that, ground-water travel time is just only one issue, too. I'm sure there's other problems that would be outside my--I would be kind of brash to actually offer an opinion on that at all. It's funny, because I got that question before.

DR. CANTLON: We like brash people, June. Thank you.

DR. FABRYKA-MARTIN: Yeah.

DR. PALCIAUSKAS: June, could you put on your sort of hat and look into the future, let's say, knowing where the program is as it is today, and the work you will be doing and Al Yang will be doing, and also, the modeling that is coming in line? What do you envision as being, in terms of understanding, where do you expect that we will be in about three years in our status of knowledge of the flow system that you work with during this?

DR. FABRYKA-MARTIN: I think in three years, these are the issues that I would want to see and would assume that we will have resolved by then; one being the nature of the gas/liquid exchange process for C-14, which, as I pointed out, is not only a ground-water travel time issue, but also a solute transport issue, and I think that Al is well on the way, if he doesn't already have it, to having the data to evaluate that.

Also, similarly, whether or not the vapor fluxes-- there are, indeed, high vapor fluxes due to temperature and pressure variations. I think data are being collected that should answer that question through Joe Rousseau's program.

I think we heard discussion yesterday, such as from Scott, for example, that there is some hope that there may be a physical model capable of explaining the bomb pulse tritium at depth by looking at the nature of how the fracture coatings affect the matrix fracture interactions. That's one possibility, anyway.

I hope, within the next year, that they have a correction method so, at a minimum, we'll have an estimate of how large the uncertainty of those corrected ages are, and whether or not those corrected ages, given the large uncertainties, are useful to the project, or whether or not we should just back off on that.

We're further along--and maybe by sometime next year--deconvoluting the chemical and the isotopic data to try to estimate relative volumes of water transmitted by different pathways. I think it's going to require us to cross our study plan boundaries and work together more closely than we have heretofore.

And, finally, all of us are working--the data collectors are working closely with the modelers. There's been a dramatic increase in our interactions, even over the past year, and I think that's going to continue to ramp up over the next couple years to make sure that the geochemical and isotopic data are not inappropriately used, that they're used knowing the caveats and the limitations; you know when something's firm and when something's sort of ambiguous.

Does that answer your question?

DR. DOMENICO: June, I'd like to see Al's data if we could at this point, and before we go on. Al, would you do us the courtesy of presenting some of that information to us on this topic? We're departing a little bit from the program here, but we'll try to stay with the time constraints, but we may run a little bit over, but I think it's important enough that we see this.

Thank you very much, June.

DR. YANG: Okay. First of all, the Carbon-14 show the same peak on aqueous, too. It's by the tritium and by the

Carbon-14. It's young ages.

DR. LANGMUIR: Al, excuse me. This is squeezer data from the matrix?

DR. YANG: Yes. So you can see this Carbon-14 is almost 100 per cent --, so it's a very young water at the Calico Hill. That's one thing.

This is the pore water from UZ-16 I was talking about. I'm not sure this is dark enough to see it. Can everybody see this? Okay. So, you can see the crossing here, all these are the UZ-16. The water, when we hit the water, we didn't hit the water at that time, so then water come up after we finished it. Then, without pumping, we collect the water up and then using for those chemistry changes.

Now, you can see all this below 1200 feet. It's all Calico Hill at UZ-16. Now, above it, 1600, it's in here now. You can see these cross. It's right here. All these three crosses somewhere in here and in here and in here, and here at one crossing here, too.

DR. LANGMUIR: Al, can you define the corners of your diagram so we know what you're doing here? You're talking about calcium concentrations, aren't you?

DR. YANG: Right. You can see here, and all these things in here is from the saturated zones, water. Now, those Carbon-14 was dated. It's conducted about 7,000 years, non-corrected apparent ages. If they are corrected, it would be younger than 6,000 years, and from these path of diagrams, you know, this is show you where the young water is, when it forms. When they finish at the Calico Hill, then they show up in here.

However, the crosses up here, up here, and one even up here, showing you the young water chemistry, and that's what is apparently near the top of the Calico Hill. So that's another area. If it's all older, should be all four around here, but this water table water has shown some of the higher one that the Calico Hill, and one even up on here, near the very top chemistry of the water. So that's showing you some of this rapid flow.

DR. LANGMUIR: Al, I don't think the audience knows what a piper diagram is. Maybe some of us do, but can you explain what the corners represent and what the trends are?

DR. YANG: Right. Here is the calcium, sodium, magnesium major irons. This is bicarbonate, chloride, and a sulfate. So, now, all this from here and from this intersection of every element in here, and this diagram show that evolution of the waters, it's coming from young water from near the surface. I have another one on pore water in here. It's in my briefcase here. Near the top water is all formed around here. Topopah Spring, formed around here, Calico Hill, all formed around here, and that's where the perched water is formed around here.

So, when the water is formed around here, it's showing you it's near the top of the young age water, and apparently this water shows up in the saturated zone water. That's the point I'm driving across.

DR. CORDING: The points on top are Calico Hills points with the Xs; is that right?

DR. YANG: This X is not Calico Hill. It's ground waters. It's pick up from the ground waters.

DR. LANGMUIR: Al, what you're saying is the water types are--

DR. YANG: This is not the pore water. These are the ground waters.

DR. LANGMUIR: The water types are consistent with the ages; is that the essence of this?

DR. YANG: Yeah.

DR. LANGMUIR: If it's a sodium water, it's a certain--they tend to be one age; if they're a calcium bicarb water, it's another age?

DR. YANG: Right, something like that.

DR. LANGMUIR: And they're consistent with the ages, the chemistries?

DR. YANG: Yes. Age is young, you know, 6,000. If you go back and look, mostly--youngest is J-13, about 9,000 years. The old one is about 13,000-17,000 years old, and this is the first time we see on the Calico Hill, on the UZ-16, the water table water is only about 6,000; if it's corrected, even younger, maybe 5,000, 4,000. I would go into it more for the age corrections, because depending on what caliche is dissolved into this water, and that's caliche. Right now, it's very, you know, it is caliche. It's not marine. There are some ages. It's ranging from 20,000 years to about 50,000 years on the caliche.

So, depending on how much of this is dissolved, then you have to age correct for that, and that comes from the Carbon-13 ratio, and it's very hard to see, you know, there is no--there is a deposit on top of it, and you hardly can separate these out, so the Carbon-14 age corrections, it's very hard to do. Right now, I only give the ranges, maybe from 6,000 to about 2,000 years. How are you going to get exact ages? I don't know even if we can do it. I don't think we can do it. We just can't tell age. It's likely like this, but you cannot get because it is so confusing, setting to a ratio in the calcite, when they dissolve near the surface in the caliche, and we can't get very exact ages out and I don't know how to solve it.

That's how it's complicated, because they layer and they layer and they layers, and they cannot separate these layer out to dating it. They try to see the young ages by the -- group, and I talk with them. They say they cannot separate those young water out. When you take it out, it's all mixed. Old water and young water is all this. If you don't know the caliche ages, you know, it's hard to correct for this Carbon-14 age in the water.

So, you know, we can say it's less than how much, but exactly how much, we cannot tell, and I'm sure we can resolve that problem in the future.

DR. DOMENICO: I'd like to take this up at the round-table if we can. Do you have another slide there, Al, or are we--

DR. YANG: I'll have more maybe this afternoon.

DR. DOMENICO: Well, yeah, but I think probably later. We better go on in the interest of time. Thank you. Thank you very much.

The next presentation is modeling of fast pathways, conceptual models and data needs, prepared by Bruce Robinson and Andy Wolfsberg, but presented by Andy from Los Alamos.

DR. WOLFSBERG: I'm not sure where this title came from that is in the agenda, but it's not the topic of my talk, and it's interesting, especially after Gilles' tirade last night, that we set the record straight, but what we're doing is we're looking at the migration of solutes in unsaturated fractured rock at Yucca Mountain, and we're viewing it from the solute transport perspective, and in this talk, I want to talk about the mechanisms and measurements and the models that we're using.

Fortunately, I don't have to spend a lot of time talking about the mechanisms, because one of the speakers last night did such a nice job at really going through the whole complex process that we have to deal with, and so, as I get into the simulations that we're doing, I just want to draw attention to the fact that we're dealing with aqueous and gas phase solutes.

It's a multi-phase system that we're dealing with, and we're looking at the driving forces and the barriers to movement of the solutes that we're looking at; the driving forces being, primarily, right now, what we're interested in is the infiltration of the aqueous phase solutes. There were some other driving forces that were brought up, but, for our purposes right now, the infiltration is the key component of getting to the bottom of that transport process, and for the gas phase solutes, we're looking at temperature and pressure gradients as driving forces for those.

And, for the barriers, again, we have aqueous and gas phase solutes that undergo different processes. It's key to address the fact that these are important physical and chemical processes that affect the movement of the solutes, regardless of what's happening to the moisture as it moves through the system.

The issues I'll be talking about are molecular diffusion and sorption of the aqueous phase solutes, and, in effect, the same processes for the gas phase solutes. We have diffusion into the rock matrix when we have gas flow in fractures, and reactions of gas phase solutes which--at least, partitioning between the gas and the aqueous phase, but it looks like a retardation, a sorption-type process.

Now, the measurements that are so key these studies, I've just put the four most important ones up there. There's plenty more, but infiltration is absolutely important, and I was really pleased to hear Al Flint's talk last night, because as they make more and more progress with defining infiltration, the top boundary for this system, which is going to be so key to the floor modelers, and it's actually key to the transport modeling as well, we'll be using that information, and you'll see as I continue through this talk why better knowledge of that information is going to be important to our work.

Now, when you're seeing the Chlorine-36 and the Carbon-14 measurements of June and Al--and that's, in a sense, to calibrate our transport models; that is, to make sure we have a handle on the transport processes that are occurring in Yucca Mountain, but the problem at hand is not the movement of Chlorine-36 or Carbon--well, Carbon-14 actually is a radioisotope that is potentially released from the repository, but the answers we need to know are how are the releases going to move.

What we have, though, is we have data on infiltration, and the movement of these to help us understand the system, so we can move in the direction--and I shouldn't have put Kd. I should have put, you know, something important like neptunium, because that's one of the radionuclides we're interested in. Kd is the measurement that's being taken, but we move from this set of measurements to understanding of the system, to this set of measurements, to predicting what's actually going to happen, which is going to be so key to the performance assessment people as they evaluate how the potential repository's going to behave into the future, not the past.

I apologize. Most of you don't have a lot of colored pictures in your handout. I couldn't make that many. I tried to make them for the Board members, so I just--these don't Xerox well, and I'm not a good enough graphics person to figure out how to make them in black and white, so I just didn't.

I just want to briefly describe the modeling process we go about. We very recently received the three-dimensional hydrostratigraphic model that's being used by the USGS, LBL, and we're now using that in our site transport modeling. I'm showing this stuff in two dimensions because it's easy to display that way.

We take the hydrostratigraphic model. It composes 18 units. Roughly, the red units are the PTn--I'm sorry, the Tiva Canyon welded unit. The very thin yellow unit through here is the Paintbrush Tuff non-welded unit. Then we have the thick Topopah Springs, and then the Calico Hills below that. Those are going to be the four units that we focus on as we go through this system.

We can take these hydrostratigraphic units--it's a three-dimensional model--and extract a three-dimensional, two-dimensional grid at whatever resolution that we need, which is a key component to being able to effectively do these modeling studies.

Now, what I want to start out with first is the work that we're trying to do in support with June. I've recently started working with her on trying to use the modeling to work collaboratively with the Chlorine-36 measurements.

We're starting this out with two-dimensional simulations. By this summer, we'll be doing the three-dimensional simulations. As we get the areal map of the infiltration, you'll see why it's going to be so important that we go to these larger simulations to try to get a handle on what's happening in the system.

We're taking a grid, which represents an east/west cross-section through Yucca Mountain. We call it approximately the Antler Ridge cross-section, and, first, we solve the flow problem, and then we solve the solute transport problem.

We're looking at the effect of non-uniform infiltration. Alan Flint so well stated that that's an important aspect of what they're measuring, and where it's an indication that there was high infiltration on the west slope of Yucca Mountain, which would be up in this area up here, so appointing a high infiltration over there, we generate a system where we're simulating the Chlorine-36 movement through this cross-section, and under the area of high infiltration, we do have fast movement down through the Topopah Springs into the Calico Hills, but then significant lateral movement underneath.

So, right now, what we don't have in here is we don't have the fault. We'll be putting that in as we get the fault properties from the USGS. Depending on how those properties are assessed, that'll be a key component to what's happening to the lateral flow. There's a lot of issues that we need to deal with here.

Mechanistically, what we see is that knowing where the infiltration is high is an extremely important data point, boundary condition that we're going to need towards understanding how these solutes move, and what we get is by modeling the Chlorine-36--I've normalized it from zero to one, one being atmospheric concentrations--we see the concentrations decreasing, but we still have younger water represented by a higher Chlorine-36 signal underneath older water, which, mechanistically, supports one of June's hypotheses that lateral flow is one process that can get younger water underneath older water, rather than viewing it as a pipe basically flowing straight down from the surface.

DR. DOMENICO: Excuse me, Andy. What's your lower boundary condition; the flow model?

DR. WOLFSBERG: Right now, this is the water table. It's at no flow boundary.

DR. DOMENICO: No flow. Okay, and the blue is the old water; the red is the young?

DR. WOLFSBERG: Yeah. The scale goes from older to younger. On the surface we have a constant input source strength of background going through, since we don't have any bomb pulse in here at all, and so, this is 500×10^{-15} Chlorine-36 to chloride ratio, and as we go from red into the blue--this is including the 300,000 year half life, so the water is getting older as you get further down, and so, this is not 50,000-year-old water. I mean, this is substantially older at .5, which is about where the green is. That would be one half life. That's 300,000 years.

So, this does not describe the system, but this is, you know, we're starting to get a handle on the processes.

This is modeled with an equivalent continuum model, and we're now putting permeability into the system, and we'll see if, in fact, that does get this infiltration down to the Calico Hills more quickly, so that it can begin its lateral movement and maybe these ages will actually move into the Calico Hills more rapidly. I can't say that for sure right now. I'll be talking about dual permeability a little later on.

DR. CORDING: Your vertical/horizontal permeabilities are--you say you're using different ratios, are you?

DR. WOLFSBERG: The vertical permeabilities are those that are used in the LBL model right now, right here. They're uniform across the strata, varying substantially in the vertical. So, we have much higher permeabilities in the PTn unit and the Calico Hills, low permeabilities in the Topopah Springs and Tiva Canyon.

DR. CORDING: So this is matrix permeabilities?

DR. WOLFSBERG: Yeah. We have fracture permeabilities as well. The equivalent continuum model uses both fracture and matrix properties, and a continual property is calculated from that. We'll be moving into the dual permeability model, and I'll show some results from that later. We haven't done that for this type of simulation yet, but it models a fracture continuum, a major continuum, and the interaction between the two.

DR. CORDING: Right. But, for this case, you have a single permeability, and each layer has a single vertical and horizontal permeability?

DR. WOLFSBERG: That's correct. They vary from layers.

DR. CORDING: But when you come down to the bottom line on it, whether it's fracture or matrix, you're picking one value, a smeared sort of value. Is your vertical permeabilities higher, lower than horizontals, or what?

DR. WOLFSBERG: The horizontal and the vertical permeabilities are the same for this model.

DR. CORDING: Okay.

DR. WOLFSBERG: In each unit.

DR. CORDING: All right.

DR. LANGMUIR: Is this comparable to the composite porosity?

DR. WOLFSBERG: That's exactly--

DR. LANGMUIR: And you're going towards the WEEPS model when you're going towards--

DR. WOLFSBERG: The equivalent continuum and composite porosity, it's the same.

DR. LANGMUIR: Right, okay, and the WEEPS model is where you're headed, or something analogous to it?

DR. WOLFSBERG: No, the dual permeability.

DR. DOMENICO: In the interest of time, I think we better move forward and save these for after the presentation.

DR. WOLFSBERG: Now, in working on these Chlorine-36 simulations, June could have alluded to the Chlorine-36/chloride ratio as not being constant over time, and there's some references to the Chlorine-36 being--cosmogenic production being related to the geomagnetic field intensity over time, which they have a record of, and I've written clearly these are estimated variations in Chlorine-36/chloride ratio as related to the geomagnetic field intensity, but for the purposes of this study, it's important--the important aspect is, is that here's where we are right now, and that's the current ratio of Chlorine-36 to chloride ratio, and going back through time, it's been--there's a potential that it's been actually stronger, and so we've taken this function and put it into our transport model, and again, there's no bomb pulse Chlorine-36 in here. We've deliberately left that out.

And I'm comparing the simulation where Chlorine-36/chloride ratio has been constant over time with the case where it's with the variable input strength, and so, for that reason, you see the scale on the bottom graph going up to 1.1, because, in fact, it's been stronger in the past.

And we see, at depth, instances where, due to the brighter red color, there's stronger Chlorine-36/chloride ratio than, certainly, in the case where we didn't have the variation, and, in fact, it's underlying, you know, we basically see younger water underlying older water by an analysis based on a constant production of Chlorine-36, when, in fact, that's not true. This is all the water just came in with a stronger source.

So, what that means is, this aspect of the project needs to be evaluated to determine how it can be used in these studies, and what ramifications it has on the measurements they're taking. If, in fact, we have--due to the whatever infiltration process we use, you know, faster movement in Calico Hills, and then lateral movement, we may see this signal from the past stronger Chlorine-36/chloride down here that would actually look, you know, make the water look apparently younger than it is, so I wanted to address that component of this study.

The other thing June talked about towards the end of her talk was the importance of the mixing of waters. That's why I've got these three points located, the .1, .2 and .3. With our particle tracking simulations, we can actually look at the distribution arrival times of those locations, and these arrival times give us some indication of the mixing of the water.

Now, at the first point, which was just below the high infiltration zone, it's not surprising we have a very thin distribution of arrival times. They're all coming in the same flow path and arriving at about the same time, all the particles that are measured there.

As we move into the lateral flow in the Calico Hills, we start getting a component of water that's not coming in from that fast vertical flow, and as we get further out there, this red line indicates a bimodal distribution where the first hump is the fastest water that's coming in laterally, and then this is the arrival of the vertical infiltration that's coming down through the top.

So, we're mixing relatively young water with relatively older water. If we just looked at the Chlorine-36 ratio in that sample, what would the age be, and would it actually represent the transport processes that led to the mixing of fluids at that location?

Now, let me move on to the Carbon-14. I'm glad Al Yang is here, because we're looking at the movement of C-14 in the gas phase as a fast flow path, a fast mechanism of transport of C-14. The initial conditions for the simulations that we do involve computing a saturation profile in the mountain, and then holding the moisture content fixed at that saturation, and we're only going to be modeling the gas flow. Eventually, we'll be doing both gas and fluid flow, but, for now, for the purposes of this demonstration, it's just a gas flow model.

We also have a temperature profile, which is based on a gradient from lower to high, as well as a temperature profile across the surface of the mountain.

Then we simulate the gas flow, and we get a flow path, and this is consistent with the work Ed Kwicklis has done, and other people at USGS. We have flow coming in the valleys, and out the chimneys. You have lower pressure and lower temperatures up there, and that leads to that. Because of the stratification of the system, we have a flow, a low flow zone, and a weaker, but very extensive deep zone where the gas comes in from here, flows down, and then up.

Now, the big question is, so what? Just because you have gas moving, CO_2, does that have any implication on the CO_2 water samples that are taken?

We model the full chemistry, the full carbonate chemistry for this system. I have the overhead if anybody wants to see what the equations look like, but there's a partitioning between the gas phase and the aqueous phase, and we view that as a potential, very important chemical mechanism for basically tagging the water samples with an age that's relative to the movement of the gas, and it doesn't have to be related to the movement of the water at all.

What I've tried to show here are two end members for this type of analysis; no chemistry, that is, the gas dismissed through the system and through fractionation, puts a radiometric signal on the aqueous, on the water, which, in this case, isn't moving, and the case where we have a pH of

8.4, which implies substantial partitioning between the gas phase and the aqueous phase.

The radiometric ages here, due to the flow path that I showed before, range from very young to old, and here, we only have 2200-year-old water all the way down to the water table. As we increase the chemistry that we're putting into the system, the movement of the C-14 in the gas phase is retarded. It's a retardation mechanism, and we see an increase in the travel time and, therefore, the signal on the water samples.

Now, as a last little analysis, the last thing I did was, I said, "Well, wait a minute. The water table is something on the order of 10,000 units." So we just put a constant signal of 10,000 units down here, and even for this massively-retarded system, we shifted the time scale of the system from 36,000 units which is the maximum age you're ever going to measure with Carbon-14, and which June described in her talk. We shifted it down to 20,000 units, so that's, you know, that's about a third of the scale that's been reduced, and this is still for a pH of 8.4.

Our next step is to include the calcium carbonate dissolution chemistry, which is going to actually determine, or it's going to be an important component of what the pH is to the system. It's going to differ in the different units, depending in the mineralogy, and then we'll have a varying pH and this graph will change and we're expecting to see something between this and this. But, again, I reiterate that this is assuming that there's interaction between the gas and the aqueous phase.

I don't completely understand Al's argument that there isn't, but I know it's an argument that he makes, and--

DR. LANGMUIR: Which of these is closest to what's been measured so far in the mountain?

DR. WOLFSBERG: It's something between. Al has measurements between three and 10,000 units.

DR. LANGMUIR: In terms of these three?

DR. WOLFSBERG: Yeah. It's in between this one and this one. I mean, Al's measurements range in age between three and 10,000 years, and so, I mean, our initial conditions for calculating the flow field were very rudimentary. It was just a pressure and a temperature profile across the surface. We need to find out the barometric connectivity through faults and fractures at depth, which will be an important driving force. That may, in fact, reduce the age of--I mean, this is an outflow of gas, and so, you wouldn't expect to see 20,000-year-old gas at the surface, but because it's coming from depth, it'll move on a very slow outflow path. That's why that's measured.

But, anyway, something between this one and this one would represent the measurements that are currently taken, and it's going to be a pH-dependent problem, and it's going to require better resolution of what the initial boundary conditions for the gas movement, not to mention there is aqueous--I mean, here we have no fluid movement. We have stationary water. Once we get the infiltration and all that working, we'll have the movement of water as well.

What this gives us is something that we can actually turn over to performance assessment that'll be used in their analysis of the movement of radionuclides from the repository, and I'm not going to go through the whole process of computing Kds and retardations, but we come up with a map of retardations of Carbon-14, which are based on pH and the saturations of the system, and, you know, that's a usable tool that will be beneficial in their type of modeling that they need to do.

Okay, I've got to move very quickly here. We've talked about the Chlorine-36, we've talked about the Carbon-14, two very different transport paths, rather than one single pipe that gets to them, but I could build up to the worst case scenario. If we're measuring these elevated Chlorine-36, Carbon-14 and tritium at depth, what kind of a pipe would it actually take to get those from the surface to depth?

And so, this is a model that Cliff Ho from Sandia National Lab has used and presented, and it's a conceptual model that helps us look at these systems. He's looked there from a flow perspective. We're now looking at it from a transport perspective.

It's a vertical column. You don't have any lateral flow out of the PTn, so we've eliminated that as a barrier to solute migration. If it comes in through the Tiva Canyon, it keeps going through the PTn, so we no longer have any buffering effect of the PTn.

We have matrix and fracture characteristics for each of the four different units, and we're going to look at varying infiltration rates into the system. We're going to value the sensitivity to the infiltration rate; the diffusion, a physical process that affects all solutes, regardless of whether they're reactive or not; and a fracture/matrix connectivity. This is modeled with a dual permeability system model that solves a fracture continuum system and a matrix continuum, and then couples the two through a connectivity term.

The first thing we do is we compute saturations for both the fractures and the matrix, and, not surprisingly, as we increase the infiltration, the saturations go out most remarkably in Topopah Springs, and the fracture saturations go up, too. It's important to note that, because as fracture saturations go up, there is more volume in the fractures, and so if you have any kind of movement of solutes, there's more carrier volume to bring them with.

I'm going to start moving towards the extreme now, and I'm only going to look at infiltration rates of one millimeter per year and four millimeters per year. Now, previous studies have looked at infiltration rates of, you know, .1, .01 millimeters per year, but with the new information we're getting, it seems very reasonable to start looking at the high infiltration rates.

There have been three typos in this talk. Here's a very important one. The diffusion rates are in meters squared per second, not per day, and I apologize for that, and I hope nobody misconstrues this information.

What we now do is we look at the case of no diffusion and increasing the matrix diffusion, because that's the process that gets the solutes from the fracture into the matrix.

First of all, for this case, we're looking at breakthrough curves in years, and so we're at about 10,000 years, and with no diffusion, you get a different breakthrough curve for the fracture and the matrix flow. The diffusion, it turns out, as we increase it, provides the buffering mechanism to slow the solutes down in the system.

Now, as we move to a high infiltration rate, we see that the relationship between the diffusion term and the flow rate in the system becomes more profound, and at a diffusion rate of 10^{-12}, we still have a slightly different breakthrough between the fractures and the matrix. We're expecting the diffusion rates for the system to be somewhere between 10^{-10} and 10^{-12} m^2/day (sic). That's why these aren't just arbitrary numbers. This is indications we get from Ines Triay's group at Los Alamos.

But this still, even at a high infiltration rate, shows that the diffusion is an extremely significant process that retards movement of the solutes in the fractures. Even if you have fracture flow, what I really need to do is to show--I probably should put that graph on this one to show breakthrough curve in the fracture without diffusion, and breakthrough curve in the fracture with diffusion. So you have ground-water travel time arriving early, and yet, with the diffusion of the solutes, it's a retarding mechanism.

The next system we're trying to evaluate was discussed in the panel last night about fracture coatings. What if the connectivity between the fractures and the matrix is substantially less? We reduce that connectivity by an order of magnitude, so through some sort of coating, consistently distributed throughout the entire fractures--I'm not going to worry about the saturations--we have less imbibition from the fracture into the matrix.

And so, you do start to see a very different behavior in the breakthroughs of the fractures with no diffusion in a very early one, with this intermediate diffusion turn, you see a double breakthrough curve, and only at this higher diffusion rate do we actually see the fracture and the matrix breaking through at the same time, basic full retardation due to the diffusion.

If we can come with some justification for why we need to reduce the fracture/matrix connectivity, I don't have that physical argument right now. I wanted to look at a worst case scenario, and, again, for the increased infiltration of four millimeters a year, you see the same process.

DR. DOMENICO: Andy, you're going to have to wrap it up in a minute here.

DR. WOLFSBERG: All right. All these studies are to support the work that we're doing in the movement of radionuclides, as I'm charged with putting this up. There's a whole other parameter that needs to be considered, and that's the adsorption of the radionuclides in the Calico Hills, and I just need to make the argument that as we understand the system more, and the four new fractures, this figure will be revised, because this is for a uniform infiltration rate, but we're just looking at breakthrough curves of neptunium from the potential repository.

Under the worst case, very thin Calico Hills, and relatively high infiltration rate in the matrix, and we see a massive retardation of the neptunium, and we haven't talked at all about adsorption and chemical processes in the Calico Hills, a very important natural barrier to radionuclide migration.

All right. So, in summary, to summarize this, the dipping stratigraphic beds may provide a reasonable pathway for Chlorine-36 from regions of high infiltration to the locations of measurement, and we need to do this in a full site-scale map, and that's what we're moving towards. The new infiltration data from Flint & Flint, and three-dimensional site transport models are going to help us understand this.

The gas phase movement of C-14, in my mind, is a very important process for considering as far as how is that C-14 moving through the system, and how do we compare the C-14 measurements with the Chlorine-36, since they're two very different transport mechanisms.

The high tritium signal isn't explainable. There's two data points at depth that we're concerned with. They're right next to data points that have background, so that needs to be addressed, and I finished up with the dual permeability simulations that look at what types of processes occur within the fractures and matrix that might actually lead to that.

I'm sorry for going over.

DR. DOMENICO: No, you didn't go over. Thank you. We gave you five minutes that we took away from you and someone else, but thank you very much.

I'm going to, in the interest of time, I'm going to hold questions of this one so we can go forward, so we're running only a few minutes behind. Thank you very much, and thank you for adhering to the time constraints.

The next presentation comes from Lawrence Berkeley, of course. Yvonne Tsang will talk to us about characteristics of flow and transport in highly heterogeneous media, and this is a theoretical study.

DR. TSANG: I'm going to talk about the flow and transport in highly heterogeneous media, and I will talk about advective transport only in this talk.

When you have a strongly heterogeneous medium, that means when you make a measurement, that your measurement would depend on where you make the measurement, so there is very strong spatial variability, and that, of course, is manifested in the flow channeling and the so-called fast path.

So, if the numbers you get depend on where you do the measurement, that means there is a lot of uncertainty because of spatial variability, and so we would like to know how this would influence your prediction of the performance of your repository.

So, in this talk, I will try to address how the site-specific data should be assimilated into the performance assessment process to reduce the uncertainty of predictions given your finite amount of data that you can collect, and I will use a site-specific example, which is not the Yucca Mountain.

So, the outline of my talk is: First of all, I will describe to you our conceptual model, a stochastic, three-dimension model of a fracture medium, and this is using the site-specific hydrological data from SKB's Aspo Hard Rock Laboratory. This is an island in southern Sweden, and it is in the granitic fracture medium.

I will show you some calculation on the transport predictions, and show you that it is sensitive to the structures of heterogeneity, and, also, I will show you some calculations to quantify this uncertainty due to the spatial variability, and one point is that we show, also, you see, usually, when you do a site characterization, you can do your experiment in the very small scale. You don't have the time to do a very large scale experiment.

However, I will show that, actually, in these very highly heterogeneous medium, Fickian limit is not reached, so that when you get your parameter out at a smaller scale, you really cannot--this does not represent what is on a larger transport distance. So then we have to think about what is this implication, then, to inference from your small scale measurement to very large scale predictions, and then I will summarize.

So, this work is related to the SKI Site-94 Project. SKI is the Swedish Nuclear Power Inspectorate, and the Site-94 project is part of their strategy for developing integrated performance assessment as a licensing tool for the nuclear waste repository.

What they do is they have, in this strategy, is they have alternative geological, hydrological transport, geochemical conceptual models, many models from different scientists, not just one model, but all based on the site-specific data, and the data is based on what we call the pre-investigation. Right now, already, the hard rock laboratory, which is 600 meters below the Baltic Sea, is built now, and now they are beginning to carry out underground experiments, but all the data are based on the pre-investigation from 1986 to 1990, and there is a wealth of data, borehole and surface based, geological, geophysical, hydrological, and geochemical.

And so, using these data, then, the strategy is to do a performance prediction, and so this is a dress rehearsal, I will say, from the site characterization to the performance assessment, using the site-specific data, but not a repository. This is just a laboratory.

So, let me just give you a little bit of background for those of you that are not familiar with Aspo. This is the island, and you can see there's many, many boreholes, and some are cores. Most of the core boreholes are in the southern island and down to 600 meters deep. A lot of these other boreholes are just percussion, maybe down to 100 and 200 meters. Most of the hydrological data is in the southern Aspo Island, so my 3-D model is actually only covering the southern Aspo Island. The scale here, you can see, is about 500 meters.

Okay. So, the hydrological data I'm going to use is for these very, very deep, 600-meter, boreholes. There are seven. They pack off every three meter section, and they do injection tests, so you get a local hydraulic conductivity, lots and lots of data. Then they do the interference pumping test. I'm going to use those point data to construct my model. Then I'm going to calibrate my model to the interference pumping test.

So, these local hydraulic conductivity, thousands of points, data points, is plotted over here, hydraulic conductivity on a log scale. You can see that it ranges over seven orders of magnitude, because it's a fracture medium, okay. Seven orders of magnitude, and this is a distribution of the hydraulic conductivity. I just divided them out into six classes, about one order of magnitude for each class, and in the next graph I'm going to show you how these hydraulic conductivity data are distributed in space. I will color code it in a rainbow color. The highest will be red, and down to the blue.

Right now, I would like to say the highest hydraulic conductivities only cover about 10 per cent of all the data they collect.

This is a three-dimension rendition, so you can see the contour of the island on the top. This is the north, and you can see these boreholes. This is boreholes, and the red, the high conductivity, you can see those really cover a very small percentage, and most are actually very, very low hydraulic conductivity. Remember, all were seven orders of magnitude here, and if you can see some of these short lines, orange short lines, these are the bracket where the spinner test shows very high conductivity, and they're very consistent with the injection test data.

Now, I took these data, and then I also look at the correlation, so I plot out variogram for all the different classes. In your handout, you will see this variogram, and I don't have time to show, and what they show is it's mostly the variograms show no effect. That means there is no long range correlation. They are all very short correlation.

On the other hand, when you look at the radar measurement, the borehole fracture mapping, and geological measurement, they show very, very extensive fracture zones all over, and in the southern Aspo Island, where I'm going to concentrate, they actually have the set in the nor-northwest and the nor-northeast, very steeply dipped, very extensive fracture zone.

So, what I'm going to do in my model is I'm going to use the hydrological data as my hard data to condition my model, but I'm also going to use these data to take into account that they are fracture zones in the medium.

So, this is my model, then, stochastic continuum model. We will use geostatistical generation of a three-dimension hydraulic conductivity field. That means I will take a block of 500-700 meters, and 600 meter deep three-dimension block, discretize it into 10 meters by 10 meter blocks, and in each one, they will have a different hydraulic conductivity.

They will be conditioned to the point data, so everywhere where there's a measurement, the hydraulic conductivity will be exactly the same as the measurement.

Now, I already mentioned that the variogram shows no--only short range correlation; however, because the nature of the data are all these borehole data, so they're very closely spaced in the well, but then they are widely spaced. So, in fact, even if your long range correlation--and I prove that--you really cannot tell from the variogram. So, then, I would have the option of incorporating the geological information of these major fracture zones; that means, they are very transmissive structures with long correlation length, so this is my hypothesis:

Because they are extensive zones, there might be long correlation length, and I call that the soft data. This does not contradict the hard data. It is consistent with all the measurement.

So, what we have, then, is a single continuum representation, but I will have all the fracture and the matrix. Then, of course, I will do the flow calculation, and then I will do the transport, and back to transport, by particle tracking.

I was very fortunate that just before I started this project, I learned about the method of Gomez & Hernandez of a non-parametric technique. That means you don't have to assume any kind of normal distribution. You just take the data as it is, like what I've done here, and separate into different classes, and it turns out to be very useful in the sense, in this method, I can actually assign a different correlation length for the extreme classes.

So, what I have done is, I take this very high conductivity and I assign them very long correlation length, on the order of 200 meters in these orientation of the fracture zones; whereas, for all these other ones, I allow them just to have short correlation length.

So, in this way, so that means in extreme values of hydraulic conductivity, I give a very large correlation length, so the generated field can have a very long range connectivity for only the very high conductivity, and this allows the concentration of all the large conductivity in the specific planes of fracture zones, so this is my way of incorporating fractures in a continuum model.

So, what I'm going to show you next, then, is my generation, and first of all, I'm going to show you--only single out the high conductivity, the red, and, remember, these are three-dimension pictures, so I orient on the computer screen until I have it in so that I can look at it exactly on edge, and you can see very clearly the high conductivity as we want it to be, has these very long range--they sort of align themselves in this orientation; whereas, if you go down to this lower conductivity, say, in the yellow or in the blue, they are just isotropic. They will fill up the whole medium, as you see here.

So, now, then, let's now look into one of these fracture planes, then, in the fracture zone. When you look at one of the fracture zones--this is a continuum model. That means I have hydraulic conductivity over the whole three dimensions, so what I show here is just highlighted in one of the planes, and you see, of course, in the plane you have all seven orders of magnitude of the hydraulic conductivity, except they do--the larger ones do seem to have a long range and they congregate here.

So this is quite different from what you see in the discrete fracture model, when you have one plane and they have only very high conductivity. I believe this is actually more realistic. This is probably what you will see in the few. They tend to concentrate there, but you have all seven orders of magnitude of the hydraulic conductivity.

So, then, that means my conceptual model looks like this schematically. When people talk about fracture network model, that means you only assign very high conductivity in these fractures, and nothing over here. In my model, I have a continuum. That means I have hydraulic conductivity everywhere, except I assign some kind of long range correlation, and if you look at one into the fracture zone, you still have all hydraulic conductivity, but more high conductivity.

What is the consequence, then, if you do a pumping test, when you do a fracture network, if you do a pumping test, they pack off in these holes, they pack off one section, they pump, and then they observe at all the other packed off sections. In this model, unless you have both your pumping well and your observation well on these fractures, you will see no response. That's a characteristic, and, remember, no matter how long you pump.

In this picture, if you, initially, when you pump, because you have this high conductivity, if you have both your observation and pumping well on these lines, you will see a very fast response, but if one of these were over here, you probably will see very little response in the beginning. But if you pump longer, you will, because mine is a continuum, you should see response for all packed off sections, and, actually, this is exactly what is seen.

Most pumping tests are done for--they pump for three days and then they look at the response in all the packed off sections. Then there are two long-term pumping tests. One is for 30 days, and one is for two months, and, indeed, they show that kind of a response, and I don't have time to go into--we have also done transient calculation to show that the short term, only a fraction response, some at very long distance also respond, and there is only a fraction at the very short distance of response, but when you pump a longer time, almost everything in the near field responds. It's in your handout, some of the data, but I won't go into it now.

DR. PALCIAUSKAS: Yvonne, the 200 meter correlation of that thing, what is the size of the grid block compared to that?

DR. TSANG: Only about 600 meters, but this is a spherical model for the correlation, and so when I say 200 meter, really, data only correlate within 200 meters. When you use a different co-variance model, say, exponential, which is very common in most cases, then when you talk about 200 meters, they are really correlated over, maybe, 600 meters. We have look at it for maybe when a correlation down to 100 meter or more. It's not very sensitive.

Okay. So, now, once we have the model, now, this is just schematic. We have this block of 500, 700 x 600 meter block, a three dimension model, and I'm going to do a very simple case, close boundary on all side, just a linear gradient, okay, so then let's consider that the bottom is our repository level and the top is our land surface, and you have transport going up.

So, let's consider, then, a release from the entire repository level here, the tracer release. Now, we will do the measurement up in the land surface, and I am going to now divide the top up to 100 meter by 100 meter block for observation, okay? So, what I'm going to show you on the next graph is you see a picture of 5 x 7, 35 blocks, divided 35 blocks, okay?

What you're going to see is, first of all, you're going to see at the top surface, and I do particle tracking by tracer, so every time there's a particle come up, I give it a dot. So this is your poppyseed plot on top. What we are seeing is 500 by 700 meters, so you see very clearly that if you were going to do your measurements somewhere over here, and here, you probably will wait for a long, long time before any stuff will come out, not surprising at all. When you have heterogeneity, this is always what's happening, and, also because of these long range correlation of these nor-northwest, nor-northeast feature, you can see that stuff are coming out so over here, and maybe over here.

So, this is just an illustration of the spatial variability. Depending on where you do the calculation, you get very different observation. Now, of course, when we do a transport experiment, we do tracer breakthrough. We look at how the mass is coming out as a function of time, so in the next graph, I'm going to show you for each observation area, I'm going to give you a plot of the evolution of the tracer mass coming out as a function of time, and they're all plotted on exactly the same scale, and here it is.

Okay, so illustrate again. If you are doing measure, measure, measurement here. You will say, no transport, nothing, you know, very, very safe. On the other hand, if you look at some of these, you can say this is a very, very early arrival, and a lot of times you see these multi-peak structure, which is very characteristic of a transport in a heterogeneous medium.

This is done when I included the long range correlation. Of course, as I said, my hard data actually doesn't show any long range correlation, so I can just go back to do a calculation where I forget about the long range correlation, and just consider everything to be isotropic. What kind of a result do you get?

If you look at a spatial distribution, indeed, when you take away the fracture zones, you have a much more evenly spaced--not evenly spaced, but if you average over this different area, essentially, if you go to different areas everywhere, you will see something, okay, so the spatial variability is a little bit reduced, and, again, if you look at the breakthrough curve, now you can get something from every block, although, still, if you just make a measurement over here, that is not characteristic of the medium, because you can make measurement, whatever parameter you get out at different ones will be particular to where you do the measurement.

Since we cannot really go out and do all these measurements, let me now just do a spatial average. Let's average over everything, and just get one breakthrough curve for each system.

What I'm going to show you, then, now, is each one of these breakthrough curve is when you are looking at collecting it over the entire surface, and what you have is these four are four different realization of these field where you have the fracture zone in a nor-northwest, nor-northeast, with a dip angle 80 degrees. These two are the isotropic case, no long range correlation whatsoever, and here are with different dip angle.

They are plotted on the same scale. All I want to show is you can see now, of course, when you have the long range correlation, when you have fracture zones, you would expect that the arrival is a little bit earlier than the isotropic case, not surprising, and a longer tail. But, in general, you can see now the amount and that the breakthrough curve are actually qualitatively very similar. The results now are stable, which is not surprising at all. I just spatial average over everything, so I just sweep everything under the rug. I mean, my knowledge has not increased at all. I just have integrated a way.

So, the remarks on these result is that the spatially integrated solute arrivals are much less sensitive to alternative heterogeneous system. They are similar order of magnitude in solute arrivals if we do a spatially integrated thing.

So, that has the implication on the choice of performance measure, or the quantities that you choose for the prediction. If you want to choose a point quantity, then there is a very, very large variation, but if we choose a spatially integrated parameter, then you have more stable result, and that kind of prediction, I will say, is more commensurate with our ignorance of the system. We really don't know enough about the system to ask this kind of a question, you know, where is the stuff going to come out? You would never know when it's going to come out.

Now, here, still, I'm talking about tracer release from the entire repository. Usually, of course, when we do a tracer measurement, you're talking about a release from just one point, so now let me do a calculation from just one canister, release from one canister. But I've already stated that you have spatial variability, so, depending on where your canister is, your result will be different. How do we get around this?

That means we have to repeat a calculation for hundreds of randomly selected sites of our tracer source, so for each calculation, you will obtain some kind of parameter that will characterize a breakthrough, maybe the v, mean velocity, and the dispersion coefficient. So then you have to repeat all these calculation to get a distribution of the transport parameters, and that distribution is your measure of uncertainty, and that's what we need. We don't have one value. We need a measure of uncertainty.

And so, that means in this calculation now, I'm just going over and select randomly different source sites, and then look at the breakthrough at the top, and for each of these calculation, typically, what you will see is this histogram, your breakthrough curve; typically, this kind of multi-peak histogram, and so what I do--this is for three dimension transport. Then we will fit each of these curve by one dimension, advective/dispersive equation. The fitting is just so that I can get out a v and a D. I want to get out some parameter.

And I do two things. One is, I can fit this entire thing by a single peak, and get a v and D, or I can go ahead and be a little more complicated, and fit these by each peak, so I call them single peak and a multi-peak fit. So, for each calculation, now, I can get one v and one D, and you do hundreds of calculation. Then what you will get out is this.

On the top, this is v, and this is D. This on the top are the single peak fit, and the bottom are the multi-peak fit, so now this is plot in log. You can see again it is a few orders of magnitude and this is the measure of uncertainty. If you are asking, what is the transport properties of the medium if you have a single canister release, you need a result like this.

If you make any measurement, it can fall anywhere in the distribution. It's highly probable here, less probable there, yet you might get a measurement over here, so this is just a measure of uncertainty, and we did that for all the different kind of a structures, all kind of a models that is consistent with our data, and see how sensitive it is to the different model.

And let me just show you one example here, then. I put it in the table here. Remember, I get the mean velocity and I get the dispersion, so I can get a mean of the distribution, and also a standard deviation from it.

What you see here is, here is the case with dip angle 80 degrees, dip angle 90 degrees, isotropic and dip 40 degrees, and this is the number of peaks that are fitted. What I want to call your attention to is you look at the mean velocity. They are really not too dissimilar, but, actually, the thing to concentrate on is the sigma. It's this value. This is the measure of uncertainty, and this is what--this is showing you how the different conceptual model, what kind of uncertainty is introduced by a conceptual model.

Here, in this calculation, I still am talking about transport over 600 meters in distance. Of course, we don't have the luxury when you do site characterization to do such a large experiment. Usually, you would do experiment with a transport distance at a much shorter distance, so what I'm going to show you now is I did the calculation for transport distance at 100 meters, 200 meters, all the way to 600 meters, and show you now how these parameters, v and D change, how does the transport change.

So, what we're going to do is that computer tracer breakthrough curves from a single canister source with transport distance of 100 meters, 200 meter, up to 600 meters, and we will fit the transport parameters, v and D as a function of the transport distance, so this is how it come out:

On the top curve is v, the parameter v; on the bottom curve is a dispersion coefficient, D, and this is a calculation at 100 meters, 200 meters, 300 meters, 400 meters, to 600 meters. The circles are for the isotropic case, where there is no fracture--fracture zones are not taken into account in my model. The squares are where the fracture zones are taken into account.

What I would like to call your attention to is this curve, the top curve here, D over v, your usual dispersivity. You can see that for the isotropic case, indeed, the Fickian limit is reached. If you do a measurement at 100 meter, 200 meters, this is exactly the same as you go to larger transport distance for the isotropic case, where no long range correlation is included.

However, when you include the long range correlation--remember, I'm only really putting the top 10 per cent of the hydraulic conductivity, giving them a long range correlation. You think it's a very small percentage, but what you see is here, certainly, the dispersivity is changing as a function of distance. The Fickian limit is not reached. So, when you do a measurement at this scale, can we use that as a prediction to a large scale?

When I'm doing simulation, I have the luxury of doing all this, but when you are actually doing a real whatever, exercise, I mean, you really have to go to the data, site characterization. How do you carry that to performance assessment?

So, to summarize, I have shown you a stochastic continuum model. It is a non-parametric sequential indicator simulation conditioned to the hard data. I included long range correlation structures by using the geological soft data to account for fractures, and I use all kinds of different heterogeneous structures, all consistent with data in order to evaluate what is the uncertainty introduced by using different conceptual model.

I should emphasize different conceptual model a lot of time can introduce more uncertainty than the uncertainty of your measurement, you know, because, well, and, secondly, I talk about the choice of performance measure. If you choose a point quantity as a predictive quantity, then this is--your prediction will have very large uncertainty, and I don't think you would ever have enough data to narrow your uncertainty if you choose that performance measure.

On the other hand, if we choose spatially integrated parameters, then this kind of a choice would be more commensurate with our ignorance of the heterogeneous system.

And I've also shown you transport from single canister source releases. I did a three dimensional flow and transport, and I fit it by 1D just to get out these parameters, and in order to assess the variability or the uncertainty, we have to do lots and lots of calculation to get out--to bracket that uncertainty.

I think I have demonstrated an approach to go from site characterization data to performance assessment in my theoretical study, but I also point out that when you do a measurement at a small scale, because the Fickian limit is not reached, then you actually cannot infer from the small scale measurement to large scale predictions. When I demonstrated approach, I was doing the simulation.

And, lastly, then, I will end my talk with a cautionary note that when you are using the predictions, we really have to keep in mind that there is an inherent ignorance of the strongly heterogeneous system. We have to remember about the spatial variability and model uncertainty, so when we look at the number, you have to keep in mind of the bracket of uncertainty, and not to say this is how the repository is going to perform.

Thank you.

DR. DOMENICO: Thank you very much. We have five minutes, so we can entertain some questions.

I have one right off the bat that I'd like to formulate intelligently, if I can. With regard to your last slide and the conclusion that the Fickian limit is not reached, in the study someplace you actually ran the model, three-dimensional model, and presumably got three-dimensional data and fitted a one-dimensional advection equation to that. I think that whenever you fit a one-dimensional equation to three-dimensional data, or two-dimensional--data obtained on a two- or three-dimensional scale, you automatically build in scaling of the dispersion coefficient, because you're using one value to do the work that's being done in the system by spreading in three directions, so you automatically build scaling.

Does this affect that conclusion there? In other words, was that conclusion an artifact of the way you handled it?

DR. TSANG: I think this conclusion is, in fact, the way I handle it. Let me first answer this. We are doing a three dimensioned calculation, so now you have this block, and you have a three dimension heterogeneous system. You have the release from a single canister release, and we are doing particle. So, in fact, when you do a particle tracking, this particle release get there and it go up to the top. In fact, it is a one dimension problem. The particle really doesn't know it's a three dimension.

So then the distribution, what I get out of the breakthrough curve is all these particles going up, and I get out this histogram, and the fitting of one dimension advective/dispersive equation is really just a method to get out the measure of distribution. It has no, you know, I'm not putting any more physical in it. In my previous work, I avoid using dispersivity, and I use a different measure, the spread and things like that, but because in the literature people seem to, you know, more familiar with this term, that's why I use these terms.

But, no, my conclusion still holds with my calculation. Yeah.

DR. LANGMUIR: I'm just wondering how all this would work in an unsaturated zone study. This is saturated work.

DR. TSANG: This is saturated.

DR. LANGMUIR: Where you have a lot of data from boreholes, and continuous fluid somehow in the system. How would this apply to Yucca Mountain, and the other thing that comes--comment related to that, you argue that point quantities, you're going to have large variations if you're trying to predict point behavior in the system, whereas, if you're willing to accept an integrated behavior for a block, you have better chances of predicting that performance on a larger scale.

But if we're dealing with fast paths, how much do we have to know? Do we have to know more than that in a case like Yucca Mountain? Do we have to identify where the fast paths are, in fact, in a system like Yucca Mountain?

You haven't worried about it here. You're looking more at average behaviors on different scales.

DR. TSANG: The whole focus of this project actually is not--it's not even the model or the system, it's really the, how do you go from data? Everybody look at the same set of data and we come up and interpret data, and then we make a prediction. How do you carry all the uncertainty from the site characterization to the performance? So that is more the emphasis on the methodology, but let me come back to the difference between saturated zone and unsaturated zone.

There are two things, also. When you are saying looking for fast path, there might be some physical features that give you a fast path. Then I will say you can find it, you know, and you can maybe engineer around it. But if these fast paths are due to heterogeneity, the small--when I say small scale, it's not this large, small scale heterogeneity, then you will always have these kind of a fast path, and you really can't identify where.

All we can say in my calculation is, yes, there will be fast paths, and in that system, it is more likely; in the other system, it is less likely, but where, I cannot tell you, and that is my idea about don't look at the point measurement, so I think it's these two.

Then how does it carry over to unsaturated? In a lot of calculation, of course, the heterogeneity will be even more pronounced in the unsaturated zone. There are different processes. In certain things, maybe they will go in different directions, but, in most cases that we have looked at, the heterogeneity is far more pronounced in the unsaturated zone.

DR. LANGMUIR: That means you'd have to have more data from the unsaturated zone system to use your model?

DR. TSANG: Actually, I didn't come to that point. Actually, I think when you go to--the reason I also go through this exercise is to show what kind of data do you need to use. Actually, I don't think it's more is better. I don't think it's, you know, get more and more. When I unpack my boxes on this project, I have about three boxes of 40 or 50 reports. Out of all that, eventually, I come up to use what I can use in the model, so I don't think it's go and collect more data. I think this is why we need to go through this exercise of how do you utilize the site characterization data?

I think that's why you have to ask your question, what do I really want to predict? In this case, I want to predict this. Then I only need this kind of a data, so I think that's why we need to go through this exercise of what is a performance prediction, what do we need to predict, and what kind of data we need, and then we go back and see whether it's useful, and what give us the most uncertainty.

In my case, it seems the structure is giving us the most uncertainty, and so this is something that we will have to look into more.

DR. LANGMUIR: Is there any isotopic information from the Swedish site that you tied into in your opinion?

DR. TSANG: Yes. My responsibility in this is, actually, I was doing the advective transport to feed it into their chemistry model. So they were using my advective transport to do the full chemistry. Yes, there is some underwater geochemical chemistry.

DR. DOMENICO: In order to have a full break, which is important to me, I think we'll probably conclude this. Come back at ten-fifteen, and we have then some presentations on what basically all of this means to performance assessment.

(Whereupon, a break was taken.)

DR. DOMENICO: Moving right along, the next two presentations will consider the consequences of what all this means, if we could take our seats, so the next presentation by Rally Barnard will deal with the consequences of what we refer to as fast pathways for Yucca Mountain.

DR. BARNARD: Thank you, Pat.

I'm going to talk about the importance of fast paths to site evaluation at Yucca Mountain, and for the purposes of this presentation, there are two main aspects of fast path evaluation that we need to be concerned with. The first is ground-water travel time, and the second is what we can call performance assessment, or the total system performance assessment.

And one thing to keep in mind is that these two evaluations consider vastly different time periods for ground-water flow. Ground-water travel time, the requirement specifies that the fastest path of likely and significant radionuclide release should not be less than a thousand years; whereas, for total system performance assessments, we're interested in times up to, perhaps, a million years if you consider doses.

So, we have a very large range of times that we have to consider, and it's quite possible that the implications and meaning of fast paths for those two cases are different.

What we're doing so far to investigate fast paths falls into three different categories. I have refrained from bringing that infernal diagram of the PA pyramid here, but if I did, you'd find that the top item there, the phenomenal logical, I call it--it's probably the bottom of the pyramid.

It's where we look at process models, and we're looking theoretical and experimental studies of flow channeling, for example. That's work that's going on by Bob Glass and Vince Tidwell at Sandia, where some of this type of work is being done, and some of the work that Andy talked about, I think, would fall into this category, also.

Then, the next level up is the ground-water travel time work that I have been doing some of the technical management for, and, in this case, what we're doing is attempting to model water particles, orange water, or something like that, in both the saturated and unsaturated zone, but only looking at flow modeling. We are not considering any geochemical or other retardation processes, nor are we considering any aspect of mobilization of radionuclides from a source term in this. It's just how water would flow.

And, finally, wrapping the whole mess up is TSPA. The hydrologic component is what is applicable to this. Bob Andrews, in the next talk, will be discussing exactly where the fast path issue will fit into a total system performance assessment, and at the risk of stealing some of his thunder, I'll just mention that he looks at the geosphere and represents it as different barriers through which the fast paths that we're interested in must penetrate.

The thing to notice and to remember about all these different types of models is that they emphasize different aspects. There are different degrees of abstraction here between the phenomenalogical down to the TSPA, and there are certainly different scales in which the modeling is being done.

Well, I will now give you something of an update on where we stand on GWTT-95, the 1995 ground-water travel time work. What this will require, to a certain extent, is dredging back in your minds, remember what we talked about for GWTT-94, because this is to build on that, and due to the lack of time, I'm not going to be able to make contrasts between the two.

But, to start off, I'll make a contrast between the two, and that is these are the model domains that we are using for GWTT-95, and in 1994, two of the model domains--A and B, shown here--were the transects that we used, essentially to the west of the Ghost Dance Fault, but now, you see we've added two more.

The Transect C, practically north/south, lies along the path of where the ESF main tunnel will go, a little to the west of the Ghost Dance Fault, and then Transect D runs down Drillhole Wash, and the importance of that is that, as you can see, we are attempting to take data from drillholes that are near the transects to condition our geophysical simulations, and Transect D had a multitude of data points that were used for our conditioning data.

There are a number of faults included, not only the Solitario Canyon and the Ghost Dance, but the Windy Wash, I think, is the other one that is also included in there. Oh, and the other thing to notice is the color coding on the drillhole identification shows which data are available for us to use for conditioning.

To model the hydrologic properties on the two-dimensional cross sections that we used, we have developed a slightly different technique from last time; whereas, previously, the work involved first identifying rock types by means of a geostatistical simulation, and then developing heterogeneous porosity and hydraulic conductivity properties on there, we've now gone straight to developing the--simulating the hydrologic properties such as porosity and K-sat directly on the cross sections.

For example, for matrix porosity here, where we have conditioning data available, passing through a transect, those data are used directly, and where they are not available directly, we have a geological framework model, the LYNX model, which is something being developed by Chris Routman and Bill Zalinski, which is characterized by the porosity values and some lithologic values to identify where the different distinctive units are. So, that is used for areas where values are not available directly.

And then, K-sat is calculated by what Sean McKenna calls a co-regionalization analysis, which provides a correlation between the porosity value and the K-sat value. There is a regression relationship between porosity and K-sat which is used to give a, hopefully, a more meaningful heterogeneous distribution of K-sat values across transects like this.

But, what you can see in this case is that there are a few locations of quite high porosity. The Topopah Spring basal vitrophyre is this dark blue layer; in other words, the layer at which we have some of the lowest porosities. It's reasonably continuous. Another thing to notice here, which is a major difference from the '94 work, is that we have included faults in here. You can see an offset here, and an offset here. Here's the Solitario Canyon offset, and everybody can see that one, so, if you couldn't see the rest of them, believe me.

In addition to the matrix properties which I discussed for the previous slide, the fractures are also being modeled somewhat differently this time. We have isotropic randomly-oriented fractures representing the cooling fractures, and then some vertical fractures which are representing tectonic effects, and so, from this, we can generate some idea of the hydraulic properties of faults. We can model them in a parametric fashion by varying the density of vertical fractures.

I don't have time to go into too much of the detail, but I do have an illustration of that if we have a chance to look at it later, but what we're doing is combining a deterministic geologic framework model--this time, in contrast to what we did last time--with geostatistical simulations of the material properties within the units.

This differs from the '94 work, where we did not have a deterministic geological framework model, and so the unit contacts varied all over, and this time, they don't do that so much.

Yesterday, we heard from Alan regarding the shallow depth infiltration studies that he's doing, which are what provide us with the boundary conditions for our flow models. The thing to realize is that I think the issue started to come up yesterday as to what the boundary conditions should be for a model at depth, for example, at the depth of the repository.

Well, we may not be right, but we're certainly positive about what we're doing in this case, because we are modeling the flow from the surface, where we have some data, down to the level of the repository before we attempt to use those values for the actual flow which might carry radionuclides to the accessible environment.

Alan's stuff is, as we learned, he uses both the neutron-hole data and surficial saturation, and, most importantly, as we use these data, we include the geography; in other words, a north slope is going to have a different behavior than a south facing slope.

What we're planning to do with this work is to look at the effects of transient boundary conditions, but this will be a sensitivity study in our work, so that from this we'll see some short-term, local increases in infiltration and see what the effects are.

And, as June and Andy have talked about, in our work this time, we're going to review the data and the modeling that they have described from the isotopic dating studies, and we'll see if we can use that as a check on our model.

What we have completed so far this year are a set of boundary--excuse me, not boundary conditions, but benchmark studies on the UZ flow models, and for that, TOUGH2, FEHMN, and DUAL have all been compared in some benchmark tests, and the four benchmark tests, plus a reality check as the fifth item, are described here.

Unfortunately, I don't have the time to go into much of the detail on it, but suffice it to say that when we use the dual permeability model, which is being done with TOUGH2 and with FEHMN, we've been able to model fracture flow in heterogeneous and homogeneous materials with less than saturated matrix conditions, which is not a profound surprise, but it is certainly something that we're happy to be able to do, in view of the restriction that is imposed by the composite porosity model for generating fracture flow.

And, again, I can emphasize that in GWTT-94 work, where we used DUAL, which is only a composite porosity model, the fracture flow that we got first required that we had saturated matrix conditions.

Let me illustrate some of the results that we're seeing from these benchmark studies. This is a comparison of the composite porosity and dual permeability models for a heterogeneous, two-dimensional volume. Here, this is 1,000 meters by 500 meters. The black glob there is the location of where saturated conditions, ponding for one hour, was turned on. This is the modeling after one hour, and the important thing to note here is that both the composite porosity, matrix saturation, and the dual permeability matrix saturation are essentially the same, and at the bottom down here you can see saturations of about one at the location where the water table would be.

Here, we see the fracture saturations, which is where the big difference occurs. In the composite porosity model, the only place where we got any fracture saturation at all was immediately underneath the area where the ponding was introduced, and, in contrast, now, we have a column of fracture saturation, very high up here, and somewhat decreasing, but the heterogeneity shows that it is quite variable throughout this model domain.

So, we really do have a handle now on being able to generate fracture flows which are necessary for particle tracking that we will be doing for the ground-water travel time.

In addition to having to model the unsaturated zone, we need to model saturated zone flow to be able to cover ground-water travel time all the way to the accessible environment. Again, we are starting from the GWTT-94 work, but enhancing it in several regards. The model domain that we're using--excuse me. We didn't do this in GWTT-94, so this is actually starting from the TSPA-93 work.

That total system performance assessment model was what was developed by George Barr from the USGS regional scale saturated flow model, but this time, the work has been enhanced from what George originally did by reinterpreting the geological contacts in the volume beneath the water table that we're modeling, to make sure that fault offsets and other observed characteristics from the data are properly represented.

Again, as we did in TSPA-93, we have used the STAFF-3D code, but, also, we are writing the problem using FEHM. This is important, because STAFF-3D is isothermal. It cannot take into account variations in the temperature of the water, whereas, FEHM is going to be able to, so we will be able to include the observed temperature anomaly in water associated with the Solitario Canyon area.

The STAFF-3D model, and FEHM, as is going to be used, is an equivalent porous medium model which implies a ready ability to exchange water between the fractures and matrix, and so, this is what is going to be used for the particle tracking, the SZ particle tracking, and the UZ particle tracking is based on the flow field described previously from TOUGH2.

So, the UZ particle tracker will be based on the dual permeability conceptual model. The particles are going to be tracked in both the matrix and fracture continua, and, of course, it will allow for an exchange of particles between the fractures and matrix so that, again, in contrast to GWTT-94, we won't have essentially a single shot at transporting a particle when it enters the fracture continuum to the water table and out of the problem, but it'll be more realistic, we hope, in that particles will travel sometimes by fracture advection, sometimes by matrix advection, then possibly back to fracture, and so forth like that, more like what one would expect to happen. The SZ particle tracker is going to use an average value flux.

What remains to be done for this work is to link the UZ and SZ distributions that we're going to develop, and there's probably a lot of wrong ways to do it, but, hopefully, we'll come up with a right way, because what we're going to have to do is include both spatial and temporal factors in there.

Clearly, if you have flow over here, you don't want to attempt to link it to flow in the unsaturated zone to flow in the saturated zone over here if there's no conceivable communication between those two points, and so forth.

Well, a couple of other issues that are slightly off the track of the direct modeling that we're doing for GWTT-95 is to address the scenario selections that we are doing. It's important that we, as well as everybody else, understand all the processes, the features, events, processes, and so forth that could possibly occur that we need to take into account.

We need to make sure that we are as close to being exhaustive in considering--maybe not in modeling, but in considering the processes that could be occurring as mechanisms for ground-water flow. This is important because when we make our recommendations, our report to DOE, who, in turn, makes their reports to the regulators, we have to make sure that we don't have any glaring omissions or oversights in the analyses, and so, by looking at the scenarios that are descriptive of what's going on, hopefully, we can be reasonably complete in our selection of what we have modeled, and also have a good feeling about that which we have not modeled, but have been able to dismiss for good reasons.

Another issue is something that has developed--I'll talk about it in a minute, but the NRC staff and we have been negotiating on some issues regarding ground-water travel time, and one thing that they have suggested is that instead of looking at a disturbed zone, which is a post-waste emplacement concept to be applied to a pre-waste emplacement ground-water travel time--which is something of a metaphysical concept, if you ask me--we're looking instead at two different ground-water travel time calculations, a post-waste emplacement calculation, and a pre-waste emplacement calculation, and then you compare the two.

And if the two are the same, then that would tell you that if you had evaluated a disturbed zone, it will be really tiny. You wouldn't have to really consider it. If it isn't, well, that makes the problem slightly different, but at least we're on the way.

Now, getting back to scenario selection, it's quite likely that post-waste emplacement scenarios describing the fastest paths will be different from the pre-waste emplacement scenarios describing fastest paths. So, there is an ample opportunity to make orthogonal calculations here. If you don't make the right calculation, you cannot make the comparison. This is one of the most important things that scenario selection will do for us, to make sure that we look at consistent paths, consistent mechanisms for the fastest potential paths.

The ground-water flow for this analysis is assumed to be controlled by thermal features, events, and processes, and examples of that are a condensation zone, for example, or heat pipes, and it's also going to be controlled by geochemical features, events, and processes, and examples of that are alteration of the Topopah Spring basal vitrophyre, or deposition of silica in the tuff aquifer from hydrothermal flows.

Now, the assumption of which of these features, events and processes apply, and to what degree they apply are going to be very much dependent on the thermal history, which, unfortunately, gets us back to the question of what thermal loading should we be using for our calculations, which we need to get from the design people, and then we have to decide on the time at which the calculation occurs.

You want me to quit? How about one more slide?

DR. DOMENICO: There's always room for one more.

DR. BARNARD: Always room for one more, yeah.

I alluded to some of the work we're doing with the NRC staff. This has turned out to be extremely valuable, this ongoing interaction with the staff, because there are a number of issues which we know separate ground-water travel time from anything as easy as making a TSPA. This is a real hard one, you see, and it's the guidance of the staff and the interaction with them which is helping to, I think, make this a little more reasonable, and a little more systematic.

We've talked about, for example, alternative approaches to evaluating the fastest paths. There is the evaluation of post-waste emplacement versus pre-waste emplacement ground-water travel time, and the last thing is the evaluation of the appropriate times when we should make the calculation. If you make a calculation in the first thousand years, that's a completely different thermal regime from making the calculation after 10,000 years, when the repository could be cooling down instead of heating up.

Well, with that, since I can see the hook coming out, I will quit, and--

DR. DOMENICO: I'm going to have to hold off any questions, but I have a very special question for you during the round-table, so...

DR. BARNARD: Sure.

DR. DOMENICO: The last presentation before a light break and a round-table discussion comes from Bob Andrews of INTERA, and further deliverance of the consequences of fast pathways for Yucca Mountain.

DR. ANDREWS: Thank you, Pat.

As Rally said, we're now going to look at the very top of the pyramid. In other TRB presentations, it sometimes starts with PA and looks down at the bottom, you know, what information needs are there. We've kind of started from the bottom and looked up during this meeting, and I think that's probably appropriate. There's a lot of new information that's been presented during this meeting, yesterday and today, that are directly relevant to evaluation of performance of the total system.

As an outline of what I'd like to talk about today, I want to first review, just so we put it into context, that we have a whole system here, not just a geosphere component, which has been the focus of the last day, and, of course, we do that because when we talk to the package and EBS people, we're very careful to point out the contributions of the geosphere, so when we're talking to mostly geosphere-related attributes, we want to acknowledge the impacts of the geosphere on the package EBS environment, and how that controls the total system performance.

One look, just very briefly, at the attributes that we're asking of the geosphere in the total system, the potential significance of some localized fast flow paths and, therefore, transport on total system performance, look at a couple of results, some sensitivity studies when these things have been included in total system performance, both in TSPA-

1993, which have been reported to the Board earlier, and in some draft work being done in support of the Calico Hills Systems Study, look at what we're doing to include this phenomena, or these phenomena in TSPA-95, and then have some summary discussion.

Putting it into context, what we've been talking about for the last day is essentially here and here. Rally brought in the fact that we actually do have saturated zone to consider in ground-water travel time, but the rest of total system performance is all the other little bubbles in there, and all of those contribute to the performance of the whole system. They're not disconnected. There's a very important arrow here from the unsaturated zone flow feeding into that near field environment, which, therefore, then, controls the waste package EBS performance and will affect the total system performance.

A very quick, not necessary slide, probably, but just for completeness, to realize that it's the ambient environment, this hydrologic environment, and, of course, the thermal perturbations imposed on that environment that we're asking to be consistent with proposed EBS waste package designs, so they work in a tandem, and there are ways to optimize the performance of the system, if you will, by acknowledging that. Of course, other programs do that in the saturated zone, and we've evaluated those in the unsaturated zone as well.

One that we've focused on a lot here is the barrier component that we ask of the geosphere; i.e., increase the radionuclide travel time from Point A, being the repository, to Point B, being the accessible environment.

A third very important component of the geosphere is it disperses things and mixes things and dilutes things over time and over spatial domains that we're interested in for those nuclides that are released from the package EBS.

I've broken up the next three slides in looking at the localized fast flow phenomena into package EBS environment and performance, and then on the geosphere performance itself.

First--and these are just general statements--the significance of the localized flow first depends on what is, in fact, the spatial distribution in quantity and in time, even, of that localized flow; i.e., does it hit packages or not? A very key issue. Clearly, if it hits packages, it's a little more significant than if it doesn't hit packages.

It depends, of course, on the efficiency of any emplaced materials, including engineered materials, engineered backfill kind of materials, as they can have an effect on distributing moisture; i.e., liquid moisture, in this case, if we have capillary barrier effects designed into the drift system. It clearly affects the per cent of waste form in contact with liquid water, as do a number of other things, like thermal environment, but the predominant one is on the next slide, is that it increases the likelihood and the amount of advective transport, or potential, even, for advective transport from the package into the EBS; i.e., the invert material sitting underneath the package, and through that invert material, back into the geosphere, into the host rock environment.

Clearly, the magnitude, the relative magnitude of advective releases from the package EBS environment versus diffusive releases from the package EBS environment are controlled by the magnitude of that advective flux; the magnitude of the effective diffusion coefficient, which is also a function of that advective flux, because it's a function of the saturations in the near-field environment; and the percentage of the waste package area available for diffusive and, in fact, advective releases.

The potential significance on geosphere transport is predominantly the first one. We've talked about that a lot. Clearly, if we have fast flow paths, we have much higher advective velocities; therefore, much lower travel times for those species that are either unretarded due to water/rock interactions, or that do not have very much matrix diffusion, the point that Andy talked about earlier today.

The dispersive effects in the geosphere depend on how localized those heterogeneities may be, and the dilution effects clearly depend on how much water is in those local features and the networks.

I'd like to next switch gears and look at some sensitivity studies when fast flow and transport have been considered in some TSPA-type analyses, and I'm going to look first at some CCDFs of the normalized cumulative release, so the old EPA standard, over 10,000 years; look at three separate cases: One, a composite porosity versus the WEEPS model, the WEEPS model presented to the Board before. Essentially, it's a discrete fracture model where water only flows through the fracture, there is no fracture/matrix interaction whatsoever. The results become controlled by the likelihood of that localized flow intercepting the waste form itself.

And then, two, composite porosity with matrix diffusion; one case where it's increased fracture flow or reduced matrix diffusion, and another case where it's and reduced matrix diffusion.

For completeness, I want to show some results, also, of the 10,000-year peak individual dose for those same assumptions, and the million-year peak individual dose.

This part, the Board's already seen. This is from TSPA-1993, comparing the WEEPS and the composite porosity model for aqueous releases only, no gaseous releases in this particular case being considered. You can see that the WEEPS model; i.e., the discrete fracture model, if you will, has generally poorer performance at the 50 percentile case than does the composite porosity, essentially looking at reduced travel time from the packages to the accessible environment; to the water table, in this case.

Looking at the effects of composite porosity with increased fracture flow, and, in this case, the way fracture flow was increased was by reducing the saturated matrix conductivity of all the units; in this case, by 10^5, so, essentially, forced that percentage of water to flow through the fractures.

And we've also, in addition to decreasing the matrix conductivity, done three things here which effectively reduce the matrix diffusion, so the diffusion of nuclides from the moving waters in the fractures into the matrix, either by increasing the fracture spacing--that decreases the effect of diffusion coefficient--by increasing the matrix tortuosity, or increasing coating factor, essentially to look at coating-type effects on the ability of nuclides to diffuse into the matrix.

And you can see, if they're all treated somewhat independently, there's not that much difference in the results over the 10,000-year normalized cumulative release measure. However, when the increased fracture flow, which is, as I said, accommodated by decreasing the saturated matrix conductivity, is combined with reduced matrix diffusion, you see some significant effects. So, clearly, the nuclides in the 10,000-year time period here are coming out in a greater amount, if you will, than they were under the baseline TSPA-1993 assumptions.

You see the next slide, a very similar result for the peak dose to the maximally exposed individual. This is that individual at five kilometers, the accessible environment, the EPA accessible environment predicted, and I should say that the package EBS environment for the cases where we decreased the saturated matrix conductivity, or decreased the matrix diffusion were the same as in TSPA-93. There was no modifications on the package EBS environment, which we've talked to the Board about, I think, in April, what changes were being made there.

Clearly, there's a significant impact from the case where it's composite porosity with large matrix diffusion, or where there's very limited fracture transport.

Looking at the million-year peak dose to the maximally exposed individual, you see a significant difference now between the WEEPS and the composite porosity; very simple. If the WEEPS model only intercepted a certain fraction of the total inventory, probably on the order of 1 per cent or less of the total inventory, just because of the localized flow, only went through a limited area, so the WEEPS cumulative peak doses are much less than the composite porosity model.

Looking at the composite porosity model with increased fracture flow and reduced matrix diffusion, the important point is, here, when you're looking at a million, when you start looking at longer and longer time frames, when the peak dose is always dominated by neptunium--at least in all the simulations we've done so far, always dominated by neptunium--you essentially, no matter which representation you use, get very similar results. I mean, these factors of three at the 50th percentile, or ten, even, at the 1 percentile of the predicted releases are generally not that significant in comparison to all the other uncertainties that are imbedded into that analysis. So, the bottom line here is over a million years, the conceptualization, in fact, makes little difference.

What I'd like to do next is move on and talk briefly about how some of these localized fast flow paths are intended to be incorporated into the next iteration of total system performance assessment. I have three word slides, but probably the best thing to do is to put up this figure and talk to it and the accompanying table, because it's the same information, and, desiring to save a little time, it's probably easier to start here and, I think, try to bring in some of the discussions of the information you heard yesterday, the interpretations of new information that you heard today regarding fast paths.

So, let's start with the fact that we know there's precipitation and we know, in fact, precipitation--I think Alan pointed to the fact that precipitation, in fact, is distributed spatially. However, let's, for the sake of argument, assume it's uniformly spatially distributed, but, clearly, it's temporally distributed, and in our assessments, it's temporally distributed in very long time frames due to climate change.

The infiltration rate, as Alan pointed out yesterday, varies in space and is quite uncertain. It's quite uncertain with various conceptualizations, various assumptions of degree of ET, the fracture/matrix interactions, et cetera, but, clearly, it varies in space because of the heterogeneous nature of the outcropping units, heterogeneous nature of the thickness of the alluvium, et cetera.

The percolation flux--now we're looking here. For the sake of argument, I've called that at the base of the PTn, or the top of the TSw--is based primarily on the results of the averaging that the hydrologic models, whether those models are the LBL/USGS models, or whether those models are the LANL models that you heard this morning. Both of those are going to average and spatially distribute that percolation flux at the base of the PTn, and that spatial distribution will be driven by the conceptualization, be driven by heterogeneities. It'll be driven by the spatial distribution of q_{inf}, infiltrating water amount.

We, in performance assessment, not only need the spatial distribution over relatively large areas, which might be driven by these conceptual uncertainties and spatial distributions at the surface, but we, in fact, need it distributed from package to package. We acknowledge that from package to package in the drift, there is variability in the percolation flux, always with the average, if you will, being driven by the average of a particular larger volume of rock, but, locally, we expect it to be variable, and, in fact, probably Dwayne will talk about some observations, in situ observations in other tunnels, in saturated systems where that variability is log-distributed, may be log-normally distributed.

Given that local percolation flux, we then have to decide how much stays in the matrix, if you will, and how much could advectively drip or weep, whatever word you want to use, onto the packages. Well, that is done, in this particular case, as it was done in TSPA-93, by the way, by a comparison of the local percolation flux, not averaged percolation flux, but local percolation flux, with the local saturated matrix conductivity of the TSw unit.

So, that will give us the likelihood, if you will, and, in fact, the amount and distribution of flux onto the packages. Given that, and we now have advective or diffusive, as the case may be, released from the package EBS once the package EBS--the packages have failed, and water has come in contact with the waste form, et cetera, we then have flow and transport in the remainder of the TSw, the Calico Hills, and, if appropriate, the Prow Path into the water table, and then laterally out.

Here, we have a distribution. I think we've heard enough over the last little bit to say there's some component of flow. We don't know exactly what, but some component of flow that seems to be preferentially flowing through local fractured systems, fractured network systems.

This distribution here, we say, is very uncertain, clearly, and is highly variable, and that variability and uncertainty is caused by variability in the hydrologic properties. It's variability and uncertainty in the conceptual models, and all of that variability is rolled up, and uncertainty to evaluate the per cent of flow percolation flux, if you will, through the matrix units beneath the packages, and the fractured units beneath the packages.

So, in summary, I think the information you've heard yesterday and today seems to confirm that there might be fast paths, although alternative interpretations probably exist, and the fact that they may exist necessitates their direct incorporation into TSPA-type analyses.

We expect--and I think probably everybody would agree--that that natural percolation flux beneath the root zone is going to be highly variable, and it is clearly uncertain, and we are trying to accommodate that variability and uncertainty in the TSPA analyses.

We have, in the past, evaluated the fast flow transport phenomena. Clearly, it's total effect depends on some of the inherent assumptions when you do the analyses. It can have a positive effect if you really limit the per cent of the waste packages that see advective transport. That's a pretty positive effect, as we saw up there with the WEEPS model, even if you decrease the effective travel time from a total system point of view now. I'm not talking about from a ground-water travel time point of view, and it has, also, potential negative effects, which depend on the transport properties, you know, the matrix diffusion, surface retardation of whatever material might be at that fracture wall, et cetera.

We will continue these sensitivity studies in TSPA-95 to evaluate their potential effects, using a more representative EBS environment, which we talked to the Board about in April, but incorporating the variability and uncertainty in this localized fast flow.

So, with that, I will conclude my discussion, Pat.

DR. DOMENICO: Thank you, Bob. We do have a few minutes, if there's questions.

Langmuir; Board?

DR. LANGMUIR: Bob, early on, you mentioned that the potential significance of the fast pathways would depend upon the efficiency of any designed in-drift capillary barriers. Those are the words on your overhead.

Have you played around with this; in other words, assuming capillary barriers were present. You realize that that has effects on the thermal loading consequences, insulating effects, and so on, but what does that do to the CCDF results you obtain if you put in different kinds of materials with different diffusive properties as backfills?

DR. ANDREWS: Yeah. That's the work we're doing right now, in fact. I mean, it has multiple effects. I mean, it has that thermal effect, clearly, and that will impact the package and the degradation of the package, and have a significant impact there, and on that local scale hydrologic environment, which would also impact, you know, waste package EBS releases, you know, is it sufficient or significant? I can't answer that right now. We haven't done those yet.

DR. LANGMUIR: You haven't run any simulations yet? We'd love to hear what the results might be. That would be very interesting.

DR. ANDREWS: I think we have a Board meeting in October to talk about results.

DR. DOMENICO: Ed Cording; Board.

DR. CORDING: The composite flow model and the assumption regarding fracture flow, is it being assumed that there's a uniform distribution of fractures, say, in a given stratigraphic unit?

DR. ANDREWS: Yes.

DR. CORDING: And it changes depending on--

DR. ANDREWS: From unit to unit, it changes.

DR. CORDING: It changes from unit to unit, and that encountering the waste packages is basically--there is no positioning of waste packages to avoid a fault zone. You're really doing a uniform fracture analysis on that?

DR. ANDREWS: Yes. So all the packages see the potential for some localized or fracture transport.

DR. CORDING: Then one other question here. You indicate on a couple of the graphs here, you indicate we're looking at the EPA some, complimentary cumulative probabilities. You indicate an increased fracture spacing, and that's showing an increased probability. Is that increased spacing of fractures, or increased frequency of fractures?

DR. ANDREWS: Yeah, that's increased spacing, and that's only--not out of respect to the flow, or the likelihood of intercepting packages. It's only used in there as affecting the matrix diffusion, so if I increase the distance between fractures, my effective matrix diffusion is reduced.

DR. CORDING: It's only influencing matrix diffusion?

DR. ANDREWS: Yeah; not flow or--

DR. CORDING: Not encountering waste package or anything?

DR. ANDREWS: No.

DR. CORDING: Okay. Thank you.

DR. PALCIAUSKAS: Just a brief question to see if I understood your statement here. Essentially, if you hold everything constant in your performance assessment computations, then focusing the flow from a composite porosity to a more focused flow basically doesn't really affect--makes things worse over the million-year criterion, but could make it worse over the 10,000 years now, so that's the general characteristic.

But what about the degree of focusing? How does that affect performance assessment? In other words, whether you use a very, very broad distribution, or if you use, for example, narrower distributions if there was a characteristic like a sigma that Dwayne keeps talking about. How would the degree of focusing affect your computations?

DR. ANDREWS: You mean, the amount of water in a particular feature that might have focused flow?

DR. PALCIAUSKAS: Right. The total flux brings the same.

DR. ANDREWS: Flow flux is the same.

DR. PALCIAUSKAS: Yeah, but the number of fractures being a million, or maybe ten, on a continuous distributions.

DR. ANDREWS: Oh, that will have a difference. That definitely has a difference. I mean, right now, from the TSPA-93 results, that probability of intersection was, in fact, a distribution based on--that was directly tied to the total amount of water going through the system.

DR. PALCIAUSKAS: So it is an important parameter?

DR. ANDREWS: Yeah.

DR. DOMENICO: Thank you, Bob.

Actually, we're just about on time, probably due to my skill and cunning as a moderator. What the program calls for here is a ten minute break, at which time the group here will reconvene, and we're going to be joined by Dwayne Chesnut, who's going to give us his ideas on incorporating Chlorine-36 data, and we're going to start out with Dwayne because it's always good to start with someone whose views are non-controversial.

We'll be joined, also, by Chin Fu Tsang, views on what fast pathways mean to Yucca Mountain. We're going to ask Al Yang to join us, even though nothing will be asked of him, but in the event someone wants some clarification of the information that he presented, and, finally, we have a question for Jeffrey Pohle of the NRC. How does the NRC view fast pathways, are they a problem from the conceptual and modeling viewpoint, and now that Jeffrey has been warned, I suspect he's looking for a fast pathway out of the building.

So, we will reconvene in ten minutes.

(Whereupon, a break was taken.)

DR. DOMENICO: We have reconvened the people at the table, as well as the invited people.

At this stage, I'd like to ask Dwayne Chestnut. My sheet says he's going to talk to us about some of his ideas on incorporating Chlorine-36 in alternative performance assessment modeling, but, Dwayne, do whatever you like.

DR. CHESNUT: As most of you know, I've been looking at ways of taking some of the more complex models that we have, and the more complex data, and then putting it into what I call a macroscopic view.

The idea is to try to roll all this stuff up into a very small number of parameters, which captures things like spatial heterogeneity in some way that we can relate it directly back to something that we can measure, so my whole purpose in this stuff is to try to complement some of the things that are going on with the more complex modeling approaches.

Much of this has come out of international program work. I've been working with the data from the SKB Aspo Hard Rock Laboratory, and also have some involvement with NAGRA.

I just want to start out, throw this diagram up on the screen. I showed this to Victor yesterday, and he said, "Oh, that's Yvonne's stuff." It's not. This is an experiment. This is real world data, an experiment by Drury and Butters several years ago at Riverside, and what you're looking at here is breakthrough curves on 20 m^2 centers, a uniform pulse of lithium bromide working its way down through a homogeneous soil.

And what this tells me is that heterogeneity is there even when we go to great lengths to make the system uniform. We don't have to worry about fractures. We don't have to worry about all these other complications. We see this kind of behavior, even under conditions when we've got extremely uniform soil properties.

In terms of the log normal model that I want to talk about, the sigma here is about .4. You will rarely find anything that uniform in natural rock.

The other picture I want to leave in your mind to start with is Chin Fu Tsang and Luis Moreno. This is a simulation run in an extremely heterogeneous, three-dimensional cube, with steady state flow from top to bottom, totally saturated system, and what you're looking at here are the flow pathways that account for 90 per cent of the total flux.

If you look at this thing, this is a 20 x 20 x 20 cube, the inlet at the top has about 400 places where flow can start. What you see is that most of the flow occurs in about 10 per cent of the channels. These things don't interact much with each other, almost independent channels, so all this stuff about diffusion and all these other things, the interaction, mixing, et cetera, mixing is not a very good mechanism in a heterogeneous system, and we have to take that into account.

Now, what I'm showing here is what happens when you increase the degree of heterogeneity in the system, going from a sigma of one for the input distribution, to a sigma of four. Sigma is the standard deviation of the log normal permeability distribution that was used to generate the 20 x 20 x 20 cube, so as you increase that, you get more contrast between adjacent elements of the calculation.

There is a modest degree of long range correlation in here. Correlation length is about a tenth of the edge of the cube, but the point here is this is a pictorial idea, and think about what happens if I drill a borehole down through a system like this, and I sample it for Chlorine-36 or tritium or bomb pulse stuff.

I could easily drill into very young pathway and drill out of it back into a very old pathway. I could do that several times in a single borehole, so that's one picture I want to leave in your mind.

Now, in terms of repository performance, what I've been working with is a convolution of a source term, a release from the EBS, and some kind of a ground-water transport function. Under reasonably broad, but not necessarily completely general conditions, we can represent the release to the accessible environment in terms of this convolution integral. Then our problem is, what is the function of time that represents release from the EBS, and what is the function of time that represents transport through the geosphere out to the accessible environment, and how is that affected by all the things that we know are important.

So, what I've been specifically working with is using a log normal function to represent that ground-water transport function, and see where it leads us in terms of implications about repository performance, and how we might tie this back into data.

So, let me show you some conclusions from some of this stuff. By the way, nothing about this depends on log normality, particularly. I can use another distribution function if you'd like. You can do bimodal, or however many things you think you can identify in terms of data.

I'm saying that the release can be bounded. Now, the calculations I'm going to talk about most, I have assumed a regulatory EBS, and that is the generic requirement. So, if I can then identify the site characteristics that I have to combine with that generic requirement, then I have an argument that I can use for how the ground-water travel time function affects radionuclide release.

So, we assume that we have that, and then we have a separate problem of showing that the EBS, in fact, is bounded by that function, and we have a lot of work still to be done that those bounds can be met, but if you will give me that kind of an assumption, and then I'm going to use this ground-water transport function.

And what I need, I need retardation factors, which we already have from Los Alamos's work, heterogeneity parameter we don't know. We can try to get that from cosmogenic data. Mean ground-water travel time, which basically comes from infiltration studies, percolation measurements, because this model says all this porosity forms a continuum. It may have seven orders of magnitude difference in permeability, but the porosity is all connected and interacts to some degree with each other.

I think this is an important point here. Chlorine-36, Carbon-14, possibly some other things, provide the only data we have on the spatial and temporal scale that we care about. We have got to be able to use this data in some way to constrain our transport models, and I'll show you how this works with the log normal transport function, and some of this, I think, will be familiar to some of you.

This is what happens if you do a three-dimensional calculation. This is Chin-Fu and Luis Moreno's work. As sigma increases, you go from a sharply-peaked breakthrough curve all the way--well, a sharply-peaked breakthrough curve at very early time, and the mean in every case is one. So, you have not changed the mean breakthrough time. You've merely changed the peak arrival time. The same behavior results if you use an analytic function for a log normal distribution in terms of sigma and the mean.

So, what we see from that is that we can get the flavor of fast pathways by increasing sigma. We get progressively earlier first arrival, earlier peak arrival, less effective dilution, because now we're getting stuff coming through at a higher concentration, and this is important: a decreased sensitivity to the retardation factor, because we have less contact efficiency between the contaminated fluid and the rock.

So, if we put this together, what does it mean for PA? Bob was seeing this curve. We take a list of radionuclides, we take their inventories, we take the retardation factors, combine all this stuff with an EBS release function, do a convolution integral. I've used a sigma of 2.2 and a mean ground-water travel time of 70,000 years, just for an example.

The blue curve shows what come out to the accessible environment as a function of which radionuclide. The red curve is what left the EBS. This is a measure with the geosphere in force, and you'll notice that if I normalize on the EPA sum, what comes out of the EBS is about a thousand, and what reaches the accessible environment is about one, for this particular set of parameters. So I can use this to look at individual components of the model, and see what the sensitivity--and one of the things, in this context, we're talking about is mean travel time and sigma.

So you can map out a compliance region in the context of this model, everything above the green curve has an EPA sum less than one. Everything below the green curve has an EPA sum greater than one, and would violate the total system release standard. Everything below the red curve would have an EPA sum greater than ten. So, where are problem is, to establish with a probability of .9, we're up in this region, and a probability of less than .001 of being in that region, and that's our compliance problem.

What I've plotted here is the mean ground-water travel time versus sigma that will give you exact compliance with the EPA sum.

DR. DOMENICO: Dwayne, can we do that on just a point? A question, really. As the heterogeneity increases, the ground-water travel time, of course--

DR. CHESNUT: Right. I have to have a much longer mean as the system gets more heterogeneous.

DR. DOMENICO: But isn't it also true that the more heterogeneous the system, the more spreading of the mass that you get?

DR. CHESNUT: Right.

DR. DOMENICO: And lower the concentrations, which may be far more important to ground-water travel time.

DR. CHESNUT: You don't get lower concentrations. That's the point of this guy. At some point, your concentrations start to increase again, because only a small percentage of all the possible pore volume is actually contributing to flow, and so you get less dilution. You go through a progressively increasing dilution. It smears out a whole lot in this intermediate region, and then starts to really climb again, so there is a limit in a heterogeneous system to what you can accommodate from dilution.

DR. DOMENICO: How are you using the term dilution?

DR. CHESNUT: Well, in other words, this is the normalized concentration, so if I'm seeing a peak value up here that cites ten times what it is here in this middle region; in other words, I'll have no dilution. I'll have a normalized breakthrough of one, and, in this case, my peak value, which is my maximum concentration that arrives, maybe even approaches one.

DR. DOMENICO: But you're using dilution and dispersion synonymously?

DR. CHESNUT: No, no. I'm not even using dispersion. I think dispersion is an outmoded concept. I think it's really dominated by spatial heterogeneity of the flow pathways, and dispersion is just one way of trying to look at the data. So, what I'm proposing, basically, the sigma is the parameter we should be looking at, not the dispersivity. But, basically, you don't get, when you get into these strong heterogeneous systems, you don't get the help from dilution that you might think you would have.

Just to show you how these things combine, this upper curve shows the mean travel time versus sigma that I need for compliance. This one shows what happens to the median, the 50th percentile, which is much less sensitive, and the lower one is the mode or peak arrival time, and notice the peak arrival time can get very small, ten years, and that still, again, within the context of the model, can show you compliance with match release.

Now, here's where the interesting thing comes in. What do we care about the fast pathways, and how does this relate to the ground-water travel time subsystem standard?

What I've plotted here is the fraction of the water that would arrive in less than a thousand years as a function of sigma, with a mean ground-water travel time that complies with the total system performance.

So, for relatively homogeneous systems, I can't tolerate much breakthrough in less than a thousand years. As heterogeneity increases, I get up above 30 per cent that I could actually have come through the system in less than a thousand years, with the retardation factors that we think we have, and I can still demonstrate compliance. And then, you get a little fall off as you get to more heterogeneous systems. So, this is, I think, an important point.

The fact that we have a significant fraction of the ground-water travel time, less than a thousand years, is not fatal to the repository standard.

Now, here's another interesting one. How does this relate to bomb pulse? We know we've seen some bomb pulse, or at least, if you set aside the objections about sampling problems, and so on, we think we've found bomb pulse at various depths, so I'm looking here at a fraction of ground-water travel time of less than 40 years from the surface to the water table, and, again, plotted as a function of sigma, with mean values taken off of the compliance curve.

And you'll notice, if I have a sigma less than one, I cannot explain the bomb pulse occurrences and comply with the total system performance. I have to have a fairly heterogeneous system, and by the time I get up into the sigma of around two, I level out to about 1 per cent chance of seeing bomb pulse if I sample, and I think that's kind of the order of magnitude we're looking at.

That kind of shows how this connects with performance, and the other question was: How does this connect with cosmogenic tracers? And this kind of takes off on some things that June was talking about earlier. This is a little different way of looking at it, but amounts to a similar thing. We're looking at--when we sample, we're mixing things, and we're seeing things that arrive from different pathways to get to the point where we sampled it, and there's a bunch of parameters in here that, I don't have to go into detail, but the heterogeneity, the sigma.

But, on looking at the sampling at some depth at below the surface, the volumetric water content averaging about 10 per cent for Yucca Mountain, average infiltration rate--and I've just simply taken Alan's site-wide average of 1.4 mm/y, retardation for Chlorine-36 and Carbon-14 is more or less one. We could argue about that a little bit, and we're assuming cosmogenic input at a constant, steady rate, which we've seen some data. It may not be a good assumption, but you can do this for any signal that you want.

What you find is that the two parameters that matter are sigma and this parameter I call beta, which is the ratio of one over the mean travel time times the decay constant for the radionuclide species, or you can look at it as a ratio of decay time over advective transport piston-like system, and this is what gets interesting.

Here, I've plotted these things up as histograms. If I were sampling and putting the concentration, the normalized concentration measurements in a bunch of buckets at .05 intervals, so I have everything that would fall between .95 and 1 assigned to this bar, and so on, and I look at what I would observe if I were to go sample, systematically, at 20 meters depth, the parameters I've put in give me a mean travel time of a little over 1400 years.

The Carbon-14 is fairly sharply peak, and it's around B^{-1} over beta for Carbon-14. I have piston-like flow, for all practical purposes. I don't see much on Chlorine-36, because nothing's decayed, and as I go deeper, the Carbon-14 pulse moves to the left, as you would hope, and I start barely getting into the point where I might be able to see something on Chlorine-36, but not really. And then, finally, I get to the long time of the order of 50,000 years. I can start to see some decay.

Now, that's what would happen if the system were homogeneous, or relatively homogeneous. What happens if I put in a typical value for heterogeneity in a porous medium?

This happens to be a number close to what I've seen work for water-flooding in petroleum reservoirs. You don't have fractures, just have heterogeneous permeability and matrix permeability distribution.

Now look what happens. My Carbon-14 age, if you want to look at it that way, or per cent modern carbon spreads out all over the place. I have a much broader distribution of apparent Carbon-14, and at 20 meters depth, I still don't see much from the Chlorine-36, but as I go deeper, I start to get broader distributions, and I can start to see some overlap in these things if I continue to more heterogeneous systems.

And here's what happens if I put in a value of sigma that is characteristic of the STRIPA inflow experiment with a log normal of about 1.6, and look what happens to my Carbon-14. I have almost equal chance of finding any Carbon-14 age, and I would even see some water that is old enough to have decayed significantly for Chlorine-36, even at 20 meters. And as I go deeper, I get broader and broader distribution.

So, the point of this is that there's a chance, by using some form of this analysis, to go back and start systematically looking at Chlorine-36/Carbon-14 data. In addition, this simple two-parameter model on the last 100,000 years of geology and climatology, and so on, and use that for a forward prediction of repository performance.

DR. DOMENICO: Questions from the Board. Don Langmuir?

DR. LANGMUIR: Dwayne, doesn't this argue for--June may not appreciate this, but doesn't this argue for lots more data? You don't have enough information with spots here and there to generate a distribution of a--just to suggest this model is working unless you've got lots of data; right?

DR. CHESNUT: Yes. If you wanted to use this approach to try to get parameters that you could apply on a site scale, you would need a lot more Chlorine-36 and a lot more Carbon-14 and other kinds of measurements. It doesn't mean we necessarily need more samples. June still has a backlog of several hundred, but I think it does mean that we would need data and analyze it carefully.

Now, one real opportunity for this is in the ESF main tunnel, because there we're going to be essentially at a constant stratigraphic level, and we could go systematically down the tunnel if we can find a way to get around the contamination from the construction water, which is a question that has to be looked at.

DR. FABRYKA-MARTIN: Could I add, also, this not only requires more data, but corrected data in the case of Chlorine-36.

DR. CHESNUT: Yes.

DR. FABRYKA-MARTIN: I mean, Al's data, even uncorrected, are useful enough because I don't think the correction factor is going to make that dramatic of a change in interpretation on a qualitative scale, but for Chlorine-36, it could make a major difference.

DR. DOMENICO: For the record, that was June Fabryka-Martin. We have to identify these things.

Well, thank you very much.

DR. LANGMUIR: Could I ask one last one, Dwayne, that requires one word or so?

DR. DOMENICO: Langmuir.

DR. LANGMUIR: What is your judgment now as to the sigmas that would apply, or the range of sigmas that apply to Yucca Mountain?

DR. CHESNUT: Okay. The only thing I have is a large-block test, which is about 3.2 from permeability measurements, which is a very small scale. I have the saturated zone pump test data from the Calico Hills, which is a sigma of 2.2, and that's about all we have, except for the data that Gary LaCain has on air permeability measurements, which may have a problem with drilling damage and skin factors, so we're not too sure at this point.

My guess would be, my gut feeling is we're looking at a value of somewhere in the neighborhood of two.

DR. DOMENICO: Is sigma Fickian? That is, is it--

DR. CHESNUT: I do not know, and that's the other thing about--this never approaches Fickian limit in this model. It gives you stuff that is a lot like what Yvonne was talking about, where dispersivity goes, continuously increases as you get larger scale.

DR. DOMENICO: Andy, did you have a point here?

DR. WOLFSBERG: Andy Wolfsberg, Los Alamos.

Dwayne, you stated that the diffusion for this type of model is not a significant component for mixing, and I can see from your cube model why you say that, but do you factor diffusion into these retardation factors?

DR. CHESNUT: That's a good question. What I've found in the past, where I've tried to take into account effects like diffusion, is that they can be accounted for by slight adjustments of the parameters, either in the retardation factor, or in sigma, and possibly in the mean. So, what you can do is take the complicated model, with all the right physics, put in the spatial heterogeneity, like Yvonne has done, look at the breakthrough curves, back-fit this thing to get the parameters for the simple model, which is sort of the same process Yvonne used, only she was fitting the advective conversion equation.

In fact, the same process I went through in a paper on high-level waste last year, of showing how you could take a breakthrough curve and fit it with either model, but it's a good point, because you can, I think, start to get some judgment about how important these other effects are.

DR. WOLFSBERG: It seems appropriate to me that you wouldn't put the diffusion into your sigma. I mean, that sigma function is complicated enough that I don't think you want to, in a sense, contaminate it with that. But I guess you could put it into the retardation factor, but then for your Chlorine-36 and Carbon-14, it may change drastically from the number one that you were citing.

I mean, this is the first time I've really thought about this thing, so...

DR. CHESNUT: I think the only way to answer that question is to start doing some detailed models with the physics in there, plus a heterogeneous spatial distribution of permeability and see what it does to breakthrough curves. That would be my suggestion.

DR. DOMENICO: Thank you.

DR. TSANG: Yvonne Tsang.

I want to add to this matter of the matrix diffusion. I think there are two factors. I don't think it's a matter of sigma. If you do not allow the interaction between matrix and fracture, you escape matrix diffusion altogether, that's one thing. But if you do have matrix diffusion, matrix diffusion will wipe out heterogeneity in the sense of the tailing. It's a much slower process, but it would not interfere with the spatial variability.

DR. CHESNUT: You're looking at it from a different point of view. I'm looking at it from, what does it do to the breakthrough curve, and what do I have to do to match the breakthrough curve?

Now, the only real experiment I can tell you about is in a case of water-flooding, where I have a stratified, layered system, but I allow cross-flow between the layers. The analogous process is capillary imbibition from high water saturation to low water saturation, with imbibition of, or counter-flow of oil back into the water layer.

When you look at that, what it does is make the system look more uniform, reduces sigma, so I think that's what I would expect.

DR. DOMENICO: As the moderator, I'm going to make one final statement and close you off, my dear.

Every bit of hydrologic information collected in the field is, by its very nature, three-dimensional. There's no question of three-dimensional movement. Things spread in three dimensions, and I still say that when you apply N dimensional data to an N minus one dimensional model, you're going to get non-Fickian behavior, or what we call scaling effects that never reach--

DR. CHESNUT: Okay, I would agree with that.

DR. DOMENICO: I don't know if that has any relevance to what we've been talking about here.

DR. CHESNUT: But I would also point out that field data look like they're non-Fickian.

DR. TSANG: But in my calculation, Fickian limit is reached when you don't have the long-range correlation. When I have a isotropic heterogeneous system, Fickian limit is reached very soon.

DR. DOMENICO: That may approach the one-dimensional assumption.

Okay, let's move forward. Chin-Fu Tsang is going to talk to us, according to my sheet here, about what fast pathways mean for Yucca Mountain. Have I got that right? Or data needs and characterization.

DR. C. TSANG: Thank you. I think Dwayne gave a very good talk. There's a need, really, to look at sigmas, and also, concerning the retardation, there's a couple of recent work which look at retardation on the local level and see how they relate to transport through a heterogeneous medium as a whole. It is not a simple translation from a local retardation to a global one, where you have heterogeneity. There's a paper by Norquist, and co-authors, and also one of --.

I prepare only one viewgraph. I have to apologize to Rally. I was writing my viewgraph when Rally was talking. I just want to make some general comments.

First of all, I will say I would like to make comment on long reactive flow in transport. I sort of like to divide this into long reactive and reactive, because reactive is very complicated, but it's not so good to divide flow and separate from transport. They really go together. You cannot divide them.

Then, I would like to make comment, when we talk of fast path, there could be several kinds of fast paths. They are not the same. The first one is that fast because you have fractures or layering in --. These are kind of dramatic effects, and so you worry about the fracture flow and spatial flow features of that, and this is something where you look at a geological system and say, "Ah-ha, here is a path going through."

The second one is the fast path due to heterogeneity. That's what Moreno and I, and also Dwayne were talking about. Even though when you look at the block of geological material, you cannot see particular cracks going through, but, nevertheless, when you apply a potential difference across the heterogeneous block, the flow will look for the path of least resistance, and that will form channeling, and that was a result of Moreno and myself.

Finally, there is a difference between saturation fingering, which means when you, say, have a ponding water on top, and how the water go down your unsaturated system. A lot of experiments, such as Bob Glass experiment at Sandia, shows strong fingering of such, in fact.

On the other side is even if the flow is not fingered, you know, say you have a unsaturated block with variations of .3 saturation or .7 saturation, the flow is spread all over. Nevertheless, when you look at a tracer transport, you will see channel, in fact, and characteristic which will occur at a sharp rise and a long tail, so it's useful for us to distinguish among these.

Then the next question is what are the prediction needs? I thought maybe we should indicate the prediction needs, and then the next one would be data needs.

Of course, one is very interested in the frequency or channel spacing. One is interested in the quantity of water in tracer coming in, and one is interested in breakthrough curves, which is very different from the usual breakthrough curves. Then, the next thing is that one has to get used to the concept of probabilistic predictions. It is no longer a simple prediction of the mean value.

Like Dwayne was demonstrating, a number of applications, you really need to indicate a prediction with the uncertainty range. I think Yvonne, in her talk, also indicated that, and so one would be interested to know how the mean transport, as well as the uncertainty depends on conceptual model, depends on parameters, depends on everything else. So, it is a important concept.

In that respect, also, then, what is the predictive quantity you want to look at, whether it be point or integrated quantities. I think Yvonne demonstrated a big difference in the two.

As far as the fact that we do not know the medium in great detail in a deterministic sense. There is some intrinsic ignorance. We have to take that into account. If we know everything in detail, you'd be poking Yucca Mountain like a Swiss cheese. That does not help the system. So, we have to know what is the prediction measure for quantities.

So, now, some comment about data needs. For the fast path related with fractures, then, of course, we need to know the geometric factors; where are the fractures, where are the major faults? Of course, even that does not really tell you flow and transport, because, as we all know, that most fractures are really not conducting. Usual, in the saturated rocks, only about 20 per cent conducts, and so even in the flow system, you still have heterogeneity you have to worry about.

Then, for the second kind of fast path due to heterogeneity, you need to know the distribution of the permeability of a system. You need to know the sigma, which is within this distribution, and you also need to know the permeability curve that's a function of saturation, and also the porosity variations, and all the other characteristics for capillary effects. So you need to know them, and our hope is that by studying this, one hopefully can come up with some lump parameters.

The usual unsaturated flow, when you have a characteristic curve, you should assume a homogeneous porous medium. Now when you have heterogeneous medium, it's important to relate the local properties with the global properties, how do you put them together.

Then, of course, a sensitivity study on conceptual model characteristics, we need to know what is important. One interesting thing from Yvonne's talk is that if you are looking at the predictions of a integrated quantity, actually, it's not very sensitive to the fractures, the 10 per cent of the fractures. The breakthrough curve in the integrated quantities are all in the same range of arrival time, and so on, so maybe details different. So that kind of study is very important for us to know what is really needed measured.

The last one is that I think it's quite useful to study the saturated cases and that would teach us how to handle unsaturated cases. One cannot just study unsaturated by itself, without understanding how the saturated system goes.

We are just doing some investigation, for example, when you have saturated system, the channels, the solute flow paths search for the largest part of the permeability curves, and then we try to study a different saturation value, is at 1, .7, .5, .3 in liquid saturation. It's quite interesting, because, like Joe Wang pointed out before, when you have unsaturated system, the liquid flow try to avoid the larger porosity, so the sampling of the different saturation is different regions in the permeability distributions, so we try to quantify that. So, it's a interesting--what we learn can be applied.

Just one more thing I want to add here, is about data needs, is since the system heterogeneous, it is extremely important to have a proper measurement of boundary conditions. If you look at the infiltration over Yucca Mountain, if you only have a few measurement, but can that represent the average infiltration over the whole surface in Yucca Mountain. If the system is heterogeneous, your measurement might not be able to give you the average value, so it's important. And then the next question is, what is the uncertainty of the boundary conditions?

Finally, I think there is an absolute need for observations. One would think, "Look, there's a lot of data needs," but with the ESF, we'll have a great opportunity to satisfy the data needs. ESF represents a tunnel, cover the whole mountain of tens of miles, so there's a lot of surfaces exposed to us, and there are many experiments. So, the important thing is to look at the ESF at different experiment and study plan, not as individual pieces, but together, and say, with all the experiments, can we give a feeling about the mean and standard -- system.

Then you go and look at the observation directly and say, where are the channels coming out, and does that correspond to the spread that you expect from the model calculations? And then you can ask the next question, the points versus integrated quantities that Yvonne was talking about.

Instead of trying to predict individual points where the flow comes out, can we just map a ceiling of the ESF as a integrated quantity, say, 100 meters by two or three meters diameter. And so, one can make that kind of study, and, of course, the ESF is going to Topopah Spring, and the ESF at Calico Hills under the repository horizon. Joe Wang and Nevill Cook have given a lot of thought to a design of an experiment in that system.

The only other point I want to mention is that since the ESF is there over a period of a few years, let's try to see whether the fast flow change over time. Suppose you have a lot of rainfall one year, how would that change? I think that extremely important and interesting information.

That's all the comments I have.

DR. DOMENICO: Well, thank you very much.

Is there a question from Board members, or the people at the table; staff?

DR. PALCIAUSKAS: I'm not sure if it's a question, but a certainly a comment on one of your points here. In fact, it's closely tied in with what Rally had put on Slides 10 and 11. I was going to ask him, and it's the fact that you need both the median and the sigma.

As Rally showed quite nicely, the long range travel times are very well defined, but the very short travel times really depend on the correlation of the highly permeability units, and that really means knowing sigma to a very accurate degree, which is probably the least defined part of the ground-water travel time computation. Yet, it's the one that is being regulated the most.

Rally, you showed, basically, there were two conceptual models, both give very different short travel times, but they both showed very similar distributions. So this is a very, very important part of the ground-water travel time. The conceptual models, as you intimated, lead to very, very different distributions of ground-water travel times. How are you going to approach that problem?

DR. C. TSANG: I think I'll give it a try, and then you can pick up my pieces.

One of the hope I have is that if one know the permeability distribution and sigma, and some information about the correlation length. We hope that we can give a probabilistic answer to that.

Now, then, in this whole system, there is a very interesting similarity between scales. It probably behaves better than the usual situation. In other words, if you look at transport and flow transport block of Scale L, then the chance of the correlation length within that scale is probably about 10 or 20 per cent. If you look at a bigger thing, the correlation length is still about 10 or 20 per cent. This is sort of related with Shlomo Neuman's universal scale, all the fancy things.

Then, the other parameter is the variations of it. Now, so, hopefully, if we can understand our system in a smaller scale, say, even ESF scale, which is by no means small. ESF scale is probably a factor of two or three smaller than the performance assessment scale. Suppose you can understand that, I have fairly good confidence we can scale up in the same probabilistic sense.

DR. BARNARD: I think we need to keep this in perspective, Victor, because what this is, is clearly a model, and a model is not reality, and the model is an abstraction--I should say that our models are abstractions of what other people are doing, which may be even more detailed.

So, what I have shown are two different examples, one of which may be intellectually more satisfying than another for the purposes of evaluating fast paths, but I don't think we can say which one is right, even. We can't even say that the assumptions that we made about this; namely, the heterogeneity, for example, that was used, is correct.

However, all we can do is to use these and recognize that they are covering a range of behaviors, and hope that without making any judgments about which one of the models is the right one, if there is one, that we have covered sufficient amount of the range of behaviors that we can have confidence that what we are modeling is going to be reflective of reality.

Susan, did you want to say something, also? This is Susan Altman, one of the PIs working on the ground-water travel time effort at Sandia.

DR. ALTMAN: I mean, for now, with the ground-water travel times, we're going to running both advective continuum and dual permeabilities, and we're going to have two separate distributions, and until we do multiple realizations, we can't really, you know, we'll be looking at -- and the sigma for each separate conceptual model of multiple realizations.

In terms of the hydrologic properties, we are incorporating sigma for each separate unit, and adding that to the heterogeneity of our models.

DR. DOMENICO: Thank you, Susan.

On ground-water travel time, let me ask a question of Rally before we ask a major question of Jeffrey. I think I heard you say we don't know what pre-emplacement ground-water travel time is and a lot of folks never did like that, but we know what it was introduced to. We had nine sites and you had to make some distinctions, but the concept has stayed with us, and now I think I heard you say something about working with NRC on post-emplacement ground-water travel time.

It seems to me that that could give a lot of credibility to the concept of a ground-water travel time that we really don't need, I think, and it would be good if we could shed even the idea of a pre-emplacement. Is this an NRC request of you, or is this--how do you feel about that?

DR. BARNARD: Let me make a few comments, and Jeff can correct me, then.

The concept of pre-emplacement ground-water travel time, as I understand it, was an attempt to be the suspenders to go with the belt of the total system release concept; in other words, it was a secondary, supposedly independent measure of the ability of the site to contain radioactive waste. And so, for that reason, the point is, well, once you've made your system, you attempted to satisfy the system regulation, there was then this subsystem regulation.

There was concern that the subsystem regulation could be misapplied because of the process of putting in a repository that could change in such a way that you weren't making a valid measurement of the ability of the site to contain waste, and so that's where this disturbed zone came from. As I say, it's a very metaphysical concept when you look at a post-waste emplacement construct on pre-waste emplacement regulation.

So, the NRC staff, at the December technical exchange that we had, came up with kind of an innovative idea, and that is to say they, too, don't like the concept of the disturbed zone, so why don't we make a comparison between a post-waste emplacement ground-water travel time with a pre-waste emplacement ground-water travel time, and if they're the same, then there is no disturbance, no disturbed zone that you have to consider.

The ellipses in that sentence is if they aren't the same, what are you going to do? And so, that's part of the problem that we have been looking at.

Whether ground-water travel time, as a subsystem regulation, is something that we should pursue is not for us foot soldiers to say at all, but, if people decide that it is not a worthy regulation to continue, I don't think that there is very much work that we've done which will go down the great conduit in the ground. We've really done a lot of work, and it will be appropriate for total system performance assessment aqueous analyses, and so, from that standpoint, there won't be much lost.

DR. DOMENICO: That's a good answer, that's a good answer.

DR. BARNARD: Good.

DR. DOMENICO: But the point is, when I heard post-emplacement, good heavens, I mean, I thought that we were taking the concept of the ground-water travel time a little bit too seriously, but I understand.

DR. BARNARD: Yes. It's only to avoid trying to explain away some very difficult conceptual concepts for pre-waste emplacement when you have to worry about thermal effects, and that sort of stuff; chemical effects, and a lot of stuff that becomes very nasty to try to evaluate, especially in terms of what the likely processes are that are a consequence of thermal effects that aren't necessarily appropriate to pre-waste emplacement processes.

DR. DOMENICO: Okay. Let's get to Jeffrey and ask, how does the NRC feel about the fast pathways, if we now know what they are, and are they a problem from a conceptual and modeling viewpoint? This is Jeffrey Pohle, NRC.

DR. POHLE: Let me rephrase your question into something that, perhaps, I can answer.

Our interest, I think, in fast pathways is somewhat like your meeting has been organized here this week; that is, one, from a performance assessment perspective, and how fast paths relate ultimately to a dose calculation or some release calculation based on the EPA standard, and our other interest is from the site characterization perspective, which Tom got into some of our research in that area yesterday.

So, at this point, it seems in the presentations I've seen, that fast paths could be a factor in dose computations. It may be positive, it may be negative, perhaps, and it remains to be seen, and, other than that, there's not much one can say. You know, what is the answer for Yucca Mountain? I have no idea, but I just don't know how else to attempt to answer your question.

DR. DOMENICO: You say it may be significant with dose computations, so, does that mean that--

DR. POHLE: I was referring to, I think, the presentations given here today that said that there could be a sensitivity to it, depending on a number of other factors. Being that that's a question, it's a question that would need to be addressed. That would be the NRC's ultimate interest.

DR. LANGMUIR: Pat, could I ask a related question, which, Rally, in his overheads, on the last page of the overheads, I believe it was, the evaluation of post-emplacement and pre-waste emplacement travel times suggested by the NRC staff as a method to avoid calculating in disturbed zone.

How can we avoid calculating in disturbed zone if it's in the regulations that we must define one and use it as the starting point for travel time calculations?

DR. POHLE: I wish I would have brought that with me. I can supply you with the presentation we made at the ACNW. Essentially, a point of view I gave both at our last technical exchange with DOE, and to our own ACNW was that it seemed to me that we didn't need to emphasize trying to draw this line in the sand, that this is the disturbed zone here, and, somehow, that conditions beyond this zone are unchanged as a result of repository construction and the effects of the emplaced waste heat.

I think the effects may be very widespread, but they might not be significant, and significant is an important word in the very definition of the disturbed zone, so what I set about to do is, essentially, to try and define what a significant effect was.

And so, my thinking was that if we evaluated ground-water flow in a post-waste emplacement environment and looked at the travel times, and if they were not significantly reduced from the pre-waste emplacement state, that there is no significant disturbance, or no significant disturbed zone in the context as it was defined in Part 60, as related to ground-water travel time. It doesn't make any difference. It didn't negatively affect the site in terms of ground-water flow, so, hence, don't worry about it.

DR. LANGMUIR: Then you're defining your travel time, ignoring the existence of a zone called a disturbed zone, because you're going to start the calculation from the waste; right?

DR. POHLE: From the edge of the repository, not ignoring the disturbances that take place, but merely saying if those disturbances are not significant, they do not significantly affect ground-water flow via the definition of the disturbed zone, we can say it does not exist, or it is coincident, for convenience, with the edge of the repository.

DR. LANGMUIR: I think I, for one--and I think some other Board members--would love to see the disturbed zone go away and have a calculation of travel time be taken from the waste.

DR. POHLE: Well, if your focus is on the effect on ground-water flow of the repository, in effect, you know, that's what happens. You're looking at the processes, and not to some arbitrary line in the sand.

DR. BARNARD: This is Rally Barnard again. Let me just amplify on Jeff's position.

We consider that we would defining a disturbed zone for the purposes of meeting a regulation by performance, you could say. If the performance is such that there is no effect, then you have defined the disturbed zone. It happens to be coincident with the edge of the repository.

If, on the other hand, as a result of our scenario analysis, and as a result of our calculations, we determine that there is a finite volume that we have to consider, such as even the mechanically-disturbed volume caused by driving a tunnel-boring machine through, then that would be what we could define as a disturbed zone; again, based on its performance that it has on the pre- and post-, particularly the post-waste emplacement calculation.

So, it avoids making an arbitrary decision, drawing on expert opinion, if you will, to come up with something a priori, but to find out what the results are as a result of your calculation, and kind of backing your way into it for regulatory purposes.

DR. DOMENICO: Bob, I have a question for you. In a sense, you're also doing performance assessment, but are you happy with the fact that fast pathways have emerged as something that we should be concerned with in performance assessment? In terms of the modeling that you've been doing over the years, we haven't been too concerned about fast pathways before, but we're concerned about mass release, we're concerned about concentrations.

What does this do to performance assessment in terms of trying to address everything now that some people think becomes very, very important in the program? You know, next year, it may be something else, but how does that change your approach to performance assessment?

DR. ANDREWS: I don't think it's a new concept at all, even in this project. I think it's been recognized, even in SCP days. I think one of the speakers, even yesterday, alluded to that fact. The identification and quantification of it, clearly, I think everybody would acknowledge, is very uncertain, and probably highly variable, and the predictive ability of that will also be uncertain.

I think the consequences of it--one of the things I tried to point out, clearly, is it's somewhat dependent on the time frame that we're looking at, which we don't really have a good handle on right now. We're still, of course, waiting for the NAS panel recommendations to come out, and EPA to decide how to implement those. That makes a difference.

You know, over the million-year time period, there is no significance of fast paths. What comes out, comes out, if you will. We don't have million-year packages. We don't have million-year travel times, even in anybody's best estimates, so the time period is clearly important.

The type of standard, I think Yvonne alluded to it. You know, when you start integrating over space, perhaps you have a little more robust solution than when you look at a discrete point in space, which would be a concentration-type standard or a dose-type standard. That makes a difference, clearly, also.

But in terms of how we do performance assessment, and how we incorporate localized flow or fast flow in performance assessment, it's not a significant issue. I mean, clearly, it impacts performance, but it's not significant in how we approach and do performance assessment.

DR. DOMENICO: Are there any other questions from the Board? Ed Cording?

DR. CORDING: To just go back to some comments previously, Dwayne was talking about the Chlorine-36 observations or measurements in the ESF, and I was just wondering if, at this point, you have--you're satisfied with the program, if you have a program where you're going to--it's set up so that you're going to get the type of samples or type of data you need as you go through the ESF so that you can get great sections across various structures and away from structures to get some feel for the distributions there; Chlorine-36, degree of saturation, those sorts of...

DR. CHESNUT: I think I have give that question to June, because I'm not involved with the actual experimental investigation.

My impression is that we could use more data of that kind if we believe this has some merit for getting us a site scale transport model.

DR. FABRYKA-MARTIN: There is no question about it. We do need more data, and for the ESF sampling, what's currently in the sampling plan for the ESF is every stratigraphic unit gets sampled at least twice. Where the unit is thick, there's a sample to be collected--I forget if it's every 50 or else every 100 meters, and, also, sample every feature that--every contact and every feature that might potentially be a transport path or flow path for water, such as fractures.

And I might say that the Bureau of Rec, who's collecting the samples, is going at the sample collection with a vengeance. Even in the first, oh, I guess, what is it, 1600 meters they're into the mountain, they have well over 100 samples--well, I would say on the order of 100 samples, I would guess, for Chlorine-36, although I'm not sure on that. Just Bow Ridge Fault alone, they collected--that's a two meter wide feature there--they collected 15 samples for me.

DR. CORDING: Are they collecting them so that they're getting them with some distance away, so you can see some of that variation that may be taking place away from major structures, as well?

DR. FABRYKA-MARTIN: Right, right, both intact matrix, as well as the feature of interest. Where they do collect at a possible flow path, such as a fractured region, I've asked them to collect a sample from the unfractured matrix block in the vicinity of that sample.

DR. CHESNUT: I think there's a couple things that come out of my analysis. One is that it's important to look at the ones that don't have any signal, because those are part of the statistics. The other thing is, sampling that we would see with the frequency of about 1 per cent may be important to discriminate. In other words, we need enough samples to be able to tie down 1 per cent tails of some of these distributions, and that, I think, needs to be looked at as a statistical problem, what sample frequency would be required to achieve some accuracy in the parameters.

DR. CORDING: I think in looking at those things, you know, people discuss why do we need detailed mapping of fractures, but you need to get some perspective of the characteristics of the fracture systems and stratigraphy as it applies to your measurements, and I think that helps you develop a model, a better model for the site, and however you apply the statistics to that, I think, is you start to-- you're going to start to see some patterns that, I think, are going to be extremely helpful in trying to look at the site scale behavior.

But the other part of it, I guess, was the question on the quality of the samples. Is the quality of the samples such that you're going to get measurements you feel are good measurements, uncontaminated measurements of these conditions, or--

DR. FABRYKA-MARTIN: Have you been reading my monthly report?

DR. CORDING: No, I haven't, but are you getting the sort of thing that you feel really give you what you need, or is there something, some other modifications you're making to the process?

DR. FABRYKA-MARTIN: I did have to contact the Bureau of Rec. So far, I haven't done any Chlorine-36 analyses of the ESF samples, but I did do bromide/chloride analyses to look for any evidence for tracer contamination of the samples.

What they do is, they spray the walls down with what's supposed to be a fine mist of water before they map the walls, and that water is J-13 water labeled with about 20 part per million bromide, and the natural background for bromide would be far less than one part per million.

The first samples I asked for from them was the first suite they collected across the Bow Ridge Fault. When I leached those samples and measured bromide/chloride, it was clear that at least half those samples had strong evidence of a tracer presence, so I warned them about that, and they went back and collected a second suite, this time, digging back farther into the wall, so I haven't received that suite yet from the sample management facility, so, yes, we do have to modify.

It's very important, I think, for all the PIs to go out there occasionally, meet with the sample collectors in the field, and walk through the procedure with them so there is no misunderstanding. In fact, not only did I modify the collection criteria for that particular set of samples, but I modify and clarify a lot of the criteria that I had thought originally were unambiguous, of course, but there's always room for uncertainty.

DR. CORDING: There may be some modifications to the way they wash the walls down, whether they wash them down before they take a sample. I know they're trying to map as they go, but, also, whether you go in and do some dry coring to some depth, where you can get something that's less disturbed.

DR. FABRYKA-MARTIN: Dry coring might be my preference, but...

DR. CORDING: Those are all things that, I think, need to be thought of, because, to me, this is a very major part of why this ESF is there.

DR. FABRYKA-MARTIN: Right, and it's very timely, I think, that you even bring up this issue at all, because as I understand it from a conversation I had last week, in the interest to save costs and budget, the whole collection procedure, the consolidated sample collection plan for the

ESF is being looked at very closely, and there's concern that PIs are asking for too many samples.

DR. LANGMUIR: I have a concern, here, too, practically, and my understanding is that you've got tremendous backlogs of samples to do Chlorine-36 on from the surface-based testing work sampling cores.

I learned from Dwayne the other day that Livermore has the capability of doing the same Chlorine-36 analyses that you can do. Is the issue one of money? Is there just one of you doing the work? Would it help to get more money and more people involved in the analyses so we could have the data in a timely fashion?

DR. FABRYKA-MARTIN: Yes.

DR. LANGMUIR: Whether it be for you, or--

DR. FABRYKA-MARTIN: I would say about a third to half of my resources this year have been spent trying to come to terms with that age correction method. It turned out to be more complicated than I had initially thought it would be. It never occurred to me that the meteoric bromide/chloride ratio could be so variable, and that's thrown me off. That's not something that's going to continue. One way or another, I'm going to just cut bait, and say, "This is the best we can do," without making it into a research project, so that's going to be a matter of, oh, well, next fiscal year for sure.

At that time, then, I can go back to processing samples in a timely fashion. Right now, I'm limited by storage space at Los Alamos. I can't bring any more barrels of samples in until I send some back, and I don't want to send them back until I'm sure I'm done with them, so that's been sort of the thing there.

But, Livermore, okay, right now, there are two facilities in the United States who can make the Chlorine-36 analyses, and the Chlorine-36 analyses themselves are the most straightforward part of the whole project. Livermore is one, Purdue is the other. I put out a bid for the analyses, and--this is proprietary information, so never mind, but, anyway, there's no problem making analyses. They both have a good turnaround time, although they differ on costs.

But it's the sample processing that's time-consuming, just the simple leaching of the samples and preparing the samples, not the analysis. The analysis just goes like that.

DR. LANGMUIR: Have you got a backlog of stuff that you have done the leaching to that simply hasn't been analyzed yet?

DR. FABRYKA-MARTIN: No. That goes through pretty fast. Actually, the most difficult measurement is the bromide/chloride analyses. That's the slow-down, and the reason is that the leachate is so extremely low in bromide, in particular, we're down to part per billion levels, and

I've been very demanding in the precision with which those measurements are made, and making sure that what that bromide/chloride ratio is measured is known within, say, 10 per cent or better.

That's the real slow-down, particularly since it's only in the past, I'd say, two years that we finally had our ion chromatography system up to where we could perform with high precision at part per billion level for bromide, so now I have a couple years of data that are just lousy as far as bromide goes, and I ought to re-leach those samples and repeat those analyses.

And I think, when you saw my bromide/chloride curve, I think that's what some of those outliers are, so I think, after thinking about this for a week, I think what I need to do is re-prioritize the sample processing. As I mentioned in my talk, I think it's pretty clear cut that there is fracture flow through Tiva Canyon into the PTn. There's not much more to be gained from continuing, my continuing on measuring Chlorine-36 in those shallow neutron holes in the upper part of the profiles.

I might just do three or four analyses of PTn samples from some of those holes, but, other than that, don't even go at it anymore, although we would have samples archived, I would hope, so that it would always be an open thing, but, instead, focus on those deep boreholes that extend to the water table, particularly repository horizon down to the water table, and also focus on the ESF samples. Then the issue of the backlog isn't nearly so pressing; and, then, secondly, continue to give highest priority to where Al Yang, if he does see tritium bomb pulse, but where he does see those, those would be top priority, and we have that understanding between us.

DR. LANGMUIR: Yeah. I wanted to get to Al at this point, and I had a couple of questions for him. I know he's been impatient to get up there, but see if I can say what I thought he was trying to get across earlier on.

The critical thing for both of you, of course, is not only what does the sample represent, but is the ager getting a true age, or just simply an apparent age. You're having to correct it, and the corrections are very complex in both chlorine and the carbon case.

My understanding of Al's problem was, as far as the fluids in the system go, that when you do a Carbon-14 correction, you have to get the Carbon-12/13 ratio out of minerals in contact with the water that might have equilibrated with it to correct the age to a true age, and my understanding was, Al, that you had, in your rock, you had various zonations of calcium carbonate which, in themselves, had different 12/13 ratios, and trying to decide which of those zones in the carbonate rock had equilibrated with the water complicated your interpretation and your age corrections, giving only a range of values, rather than a well-defined age.

Is that basically correct?

DR. YANG: Yes. Can I explain?

DR. LANGMUIR: Yes, you may.

DR. YANG: Now, I assume you are talking about how much confidence you have in your ages. Now, what we date, this is the thing dissolving in water. That's what we date, and what's the reaction going on. This is a soil CO_2. The CO_2 dissolve from respirations, and the difference in the caliche near the surface that reacted with the water, and rain water, and dissolved this and reacted with this, and it formed in this, and these are what we are dating on the Carbon-14.

So, the Carbon-14 from here is come one from here, one from here, and this caliche has a range of--right now, the primary--this is changing, because they have dissolved it from 20,000 years to 40,000 years, all the caliche, and they sit on top of each other, and all these constant dissolve, dissolve all of them at once, or, you know, all this complication comes in, so I don't know the exact ages of this.

If this is marine, it's dead, easy to correct. Now, if this is marine, zero, and this is one model that is 50 per cent to start with. The carbon just form--you start with already 50,000 years. Then you need to correct these kind of ages.

Now, on top of that, the CO_2 in the biosphere out at Yucca Mountain, it's changing from years to years, depending on the wetness and all this, so this is not 100 per cent. Sometime, it changes. On the 13/12 ratio, we want to know if the--assume this was a constant, but at Yucca Mountain it's not. It's already been measured at Yucca Mountain from the soil sample coming up. In the wet years, this is more negative, -25. Sometimes, drying year, this is only about, you know, -16, something like that.

So, if this is not constant, and this is, you know, we have to know how much come from here and how much come from here, and this combination, and that's what the population is, how to correct the Carbon-14 age, and this is a problem we have.

Another bicarbonate can be formed from this process, you know, as the silicate dissolve with this acid, then, in that case, it's easy, you don't have to correct for it, but this is very, kinetically, a very slow process, so not much is going on and a lot of this going on, and that's what the problem with Carbon-14 age corrections, but on this order of magnitude, you can say, well, this is less than how much, and we can say that.

So, you know, should I show how the perched water--

where we go from there?

DR. LANGMUIR: Yes. I would personally like to hear about it. My understanding is you've got some preliminary age dates for the perched water, and they vary around the mountain.

DR. YANG: Yes.

DR. LANGMUIR: And I think that's interesting information, since it's well down below the repository horizon. I'd like to see it, personally. These are, again, apparent ages?

DR. YANG: Apparent ages.

DR. LANGMUIR: These are apparent.

DR. YANG: Yes. Now, the most important thing is this most recent data for the perched water on the SD-7. Now, you can see the Carbon-13 ratio for all these very consistent. Carbon-14 is--the first one is on the March 8th. This is where we hit the water, then we collect the water in the dating of those, and in those, after March 16th is the pump test, and we collect the sample as a function of time.

Chin-Fu has said, you know, that changing as a function of time is important, and that's what we did in here, as a function of time, and see what the change is, but the first time since the younger--seems we have some of the modern water or something, more recent water come in. May not be recent, but it's younger compared with those. Those are very consistent in the Carbon-14, so these are about 10,000 years, 11,000 years uncorrected, and you can see all isotope, these are supportive of the ages, because we know, in the last Ice Age, it's about 10,000 years ago, this value will be about -120, something like that.

So, these stable isotope data help us to incorporate the ages, too, so from these data you can see there is no Ice Age water, and if this is corrected for the Carbon-14 ages from this, this will become about 7,000 years, something like that. I cannot give the exact number, but it will be less, so it will become 7,000, or something like that, from this number we see here.

And how confident are this, you know, from this data, from isotopic data, it's telling me this is not Ice Age. If there's water from 10,000 years, 50,000 years ago mixed with modern water, this result is more negative, and we didn't see that. All this water, after 7,000 years to present water, so our complication is not as much we expected, so very old mixed water and young water mixed together. That's sure from this isotopic data.

DR. LANGMUIR: If you apply correction to the Carbon-14, independent of --, which suggests temperature recharge, if you apply the corrections to the Carbon-14, then, using your dead carbon or carbonate within the ranges you'd expect--

DR. YANG: Yes. So that's why I'd say, from this range from 2,000, something like that, to about--

DR. LANGMUIR: The perched water could be as young as 2,000 years?

DR. YANG: No, no, no, no, just for this, really, between, I don't know, 5,000 to 7,000, 8,000, somewhere between those, if you even correct for them, so it can't be go over that range. I think it just pull some age reactors to correct for it, but I cannot pinpoint, you know, how old that is. There is some range on this based on this volume. That's why this is very important, to have a 13/12 ratio for every sample you measure.

DR. DOMENICO: I'm confused on the depth. I see 16020. What does that mean in terms of the depth of this?

DR. YANG: These are all the same depth. We pump test, so the pump is down there, keeping pumping up the water, so it's a big, big volume of water reservoir there, and we are analyzing. Every day, we take two samples and analyze for it and see what the change is about, and from this data, it doesn't seem to be any change. It's the same body of big, big water.

DR. DOMENICO: Are you certain that you're not in the saturated zone there?

DR. YANG: No, this is unsaturated zone. It's still at the Calico Hill. This is still at the Calico Hill formation, above the water table. So, because of the pump test, we didn't have that much pump test, only on the two, UZ-14 and SD-7 we have all these tests, and that show--this was not quaternary. UZ-14 was quaternary, but this is not, so it's pretty good confidence. It's a big body.

Now, talking about air/water exchanges, now, you can't affect this water. This is a big, big body of water. The gas cannot change this, and we show these ages similar to Calico Hill pore water Carbon-14 ages, and that give some confidence. You know, we are talking about that same ball park on the Carbon-14 ages, not talking of the very, very old ages.

So, from this data, and how the water is getting there, you know, this is another issue. You can see these Yucca Mountain precipitation lines, and this is the ground water, saturated zone ground water. These are the one--it's some of them pretty light, you cannot see it, but it's all near these precipitation lines. What that tell you is the water come right in, instead of runoff the surface for evaporation. If evaporated, then this data will show something like this, away from this line here.

DR. LANGMUIR: June Fabryka was suggesting yesterday, from her results, that most of the infiltration took place on slopes and hillsides, as opposed to alluvial areas, and so on--this morning, rather--and is this consistent, this sort of thing, with those being the kinds of sources of this water as well? Do we know enough about it? Do you have Chlorine-36 data on some of it? You don't have any, actually, on this. This is perched water. You have no Chlorine-36?

DR. FABRYKA-MARTIN: I do. Al has been collecting water samples for me for Chlorine-36 analysis, and so I had the full suite. I do not have the results yet for SD-7. I do have results for all the other occurrences of perched water, which are around--under the Drill Hole Wash area, and, actually, it's rather interesting, because in UZ-14 perched water body, they collected samples each time it was bailed, each time it was pumped, so we had a suite of maybe ten samples, and C-14 and Chlorine-36 are anti-correlated. It's a very nice--I can show it up on the overhead. I don't know what to say about it, but it is interesting, anyway, because it's not expected. I'm not sure whether it makes sense to me yet or not.

When I was talking about the infiltration occurring where the alluvial cover was thin or absent, that's when we're talking about an area, you know, just about the behavior in general. For all we know, the source for the pathway leading to the perched water might be along a fault structure, in which case, it's not really the same thing, so I don't think I would know enough to say, and I think probably the geochemical data ought to be an interesting--

DR. YANG: Yes. I have some more geochemical data, you know. You're asking about fast flow paths, are the water coming out to feed the perched water coming through the fracture, or coming through the matrix. That's your question; right? How many passing and coming through the matrix and coming through that?

Now, if I show you some of this data here--

DR. DOMENICO: Excuse me. We're going to have to allow some time for public comments, so I'm going to have to turn the meeting over to Don.

DR. LANGMUIR: Do we have time for one more illustration and just a minute or two?

DR. DOMENICO: And then we'll call it a day.

DR. LANGMUIR: I think I'd like to see how--you brought some insights that many of us have not heard, where the general chemistry should be consistent with the ages. I'd like to just make sure I understand that.

DR. YANG: Now, this one is perched water.

DR. LANGMUIR: Now, please, help me, because I'm not following this, and if I can't, I think we have general problems.

Define for me what the chemical trends are. You were looking at major cations and anions.

DR. YANG: Right.

DR. LANGMUIR: On the top of that diamond.

DR. YANG: This is all the perched water around here.

DR. LANGMUIR: Okay.

DR. YANG: This is in the Calico Hill unit, all the perched water from just above the Calico Hill unit, or in the Calico Hill unit.

DR. LANGMUIR: What does that mean? It's a calcium bicarbonate water, or what?

DR. YANG: Yeah, sodium. It's near the sodium bicarbonate water.

DR. LANGMUIR: It's a sodium bicarbonate water?

DR. YANG: Yes.

DR. LANGMUIR: Okay.

DR. YANG: Now, here is Topopah Spring. Only UZ-14 perched water show up in here, because this from in the Topopah Spring unit, so what I'm trying to show here is you look at the size. This is the total result, you know, how big the circle is. All this perched water is very small. Now, if you compare this with the perched water--

DR. LANGMUIR: I thought the first figure was the perched water.

DR. YANG: Right, the first showing the perched water, not the pore water.

DR. LANGMUIR: This is the pore water?

DR. YANG: Yes. Now, you can see all the pore water is, total result is a lot bigger, so what that mean is this water coming down doesn't go through the matrix. The perched water just runs through the fracture or something, less interaction with that, and if all this big circle is pore water, so that compared with the perched water, apparently, the perched water is running through some fractures and don't go through the matrix, and giving such a very small, say, compared with such a baby, how many times, four, five times bigger on the total result side, and it does show you not much matrix fractures contribute to those perched water bodies.

And the question you were asking about, you know, those perched water body formed. How much come from this matrix? How much come from fracture? From this, the same as Gregg Davidson said yesterday, are most likely coming from-- we can do some of the -- again on this kind of data.

DR. LANGMUIR: That sounds like--let me--one last thing, I guess, June, and we'll close it out.

DR. FABRYKA-MARTIN: Oh. If I could back up Al, I'm just looking at the chloride concentrations alone. I haven't seen the SD-7 results, but I know for the other occurrences, it's the lowest chloride concentration seen anywhere by Al in the mountain in an unsaturated zone, about 10 mg/l, or even less in some cases, and, remember when I showed the chloride pore water results yesterday? The only other lowest thing was, what, 18 mg/l in some of the Calico Hills non-welded squeezed pore water, so there's no question it's coming down by some fast pathway. It's not even hanging up in the root zone hardly at all. It's just going straight from rain water down to percolating water.

DR. LANGMUIR: Sounds like you two have to sit together some more, and we heard yesterday about NETPATH. It sounds like there needs to be some analysis of how the waters get where they are, both reaction-wise and age-wise, to wrap this together, and, of course, the corrections are critical, to the extent you can make them and limit the uncertainties in the ages, so you can feed the hydrologists some fundamental information for their models.

I think I have to close it off here. Everybody's been very patient. It's been, for me, very edifying. I've enjoyed this thoroughly, and I think most of us, or all of us have enjoyed it thoroughly, and appreciated the speakers' presentations and the education we've all obtained. Thank you. Everybody on the panel, thank all the speakers. I think we've adjourned.

(Whereupon, at 12:40 p.m., the meeting was adjourned.)