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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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151ST MEETING

ADVISORY COMMITTEE ON NUCLEAR WASTE

(ACNW)

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TUESDAY, JUNE 22, 2004

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ROCKVILLE, MARYLAND

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The Advisory Committee met at 10:00 a.m. at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Michael T. Ryan, Acting Chairman, presiding.

COMMITTEE MEMBERS:

MICHAEL T. RYAN	Acting Chairman
JAMES CLARKE	Consultant
ALLEN G. CROFF	Invited Expert
GEORGE M. HORNBERGER	Member
RUTH F. WEINER	Member

ACNW STAFF PRESENT:

JOHN T. LARKINS, Executive Director
NEIL COLEMAN
HOWARD J. LARSON, Designated Federal Official
MICHAEL LEE

GEOSPHERE TRANSPORT WORKING GROUP:

JAMES DAVIS, U.S. Geological Survey
RICHARD PARIZEK, Pennsylvania State University, NWTRB member
DONALD SHETTEL, Geoscience Management Institute, via videoconference
INES TRIAY, U.S. Department of Energy

ALSO PRESENT:

ROBERT ANDREWS, U.S. Department of Energy
BILL ARNOLD, Sandia National Laboratory, Bechtel SAIC Company
PAUL BERTETTI, Center for Nuclear Waste Regulatory Analysis
ANDY CAMPBELL, NMSS
KEITH COMPTON, NMSS
TIM McCARTIN, NMSS
JAMES WINTERLE, Center for Nuclear Waste Regulatory Analysis

I-N-D-E-X

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

(9:01 a.m.)

ACTING CHAIRMAN RYAN: The meeting will come to order. This is the second day of the 151st meeting of the Advisory Committee on Nuclear Waste.

My name is Michael Ryan, Vice Chairman of the ACNW. Chairman John Garrick is unable to attend.

The other members of the committee are present: George Hornberger and Ruth Weiner. Also present are consultants Allen Croff and Jim Clarke.

During today's meeting the committee will continue the working group on the geosphere transport of radionuclides at the proposed Yucca Mountain high-level waste repository.

Neil Coleman is the designated federal official for today's initial session.

The meeting is being conducted in accordance with the provision of the Federal Advisory Committee Act. We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. Should anyone wish to address the committee, please make their wishes known to one of the committee staff.

It is requested that the speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so that they can be readily heard.

Without further ado, I will turn over the meeting to our working group meeting chairman, George Hornberger.

MEMBER HORNBERGER: Thank you, Mike. As Mike said, we are going to continue our discussion on the geosphere activities. Yesterday we heard a lot of the detailed presentations from both the NRC people, DOE people, and, of course, we had Jim Davis' presentation to kick us off. So we talked a lot about the geochemistry and the hydrogeology of the site, and today we will continue hearing from several other people.

Our first presenter, though, is on the NRC performance assessment and the risk perspective, which is I think a somewhat broader overview of how these things -- how this scientific information gets fed into an assessment. And Tim McCartin is going to do that presentation.

MR. McCARTIN: Center, you may want to consider going on mute.

(Laughter.)

Yes. Today I'll be giving a perspective on the performance assessment and risk from NRC's and the Center's standpoint. I would like to point out a couple of things before I begin. I would like to acknowledge Sam Nalluswami, who helped me do some of the computer simulations that I'll be talking about.

Before I begin, I would like to make two introductory statements. One would be that, as I alluded to yesterday, this is a -- in the last couple of years the NRC has done a lot in terms of trying to communicate our results and a very important aspect of the performance assessment.

Yesterday Center staff presented some of the technical aspects of the modeling. What we've learned in the last two, three years, or so, and have tried to improve is our capability to use performance assessment as a tool to understand the processes and important features related to Yucca Mountain, but also communicate that to other people.

And I think that's an extremely important part, that oftentimes I think prior to that we would come in, and maybe we'd present dose curves, we'd present a lot of information. And I'll attribute George Hornberger a couple of years ago making a very simple statement to us. I see all of that. I see the numbers. What does it mean?

I don't think we had a good answer to that very simple justified question, and we've been trying to present information in a way that not only for our -- internally, for ourselves to assist our review, but externally to other people to help get suggestions back. And over the last couple of years in various presentations, we've gotten a lot of helpful suggestions, be it from ACNW, NWTRB, the National Academy of Sciences, where we've presented some of this information. And this is an evolution.

Some of you will notice some of these slides are repeats. And I'm just giving a background to show how we got there. There is additional information that we haven't presented before that I think will be useful to see this evolution. But I would say -- and then that gets to NRC's independent role.

And I think our independent role has at least two primary factors to it. One was what you saw yesterday -- development of models for the performance assessment code, understanding the processes from a very technical standpoint.

The other part today -- I won't really be talking much, if any, about the models as much as presenting the information. What are the models telling us? And it's what I would prefer to look on using performance assessment as a tool, and that really is why we have our independent role in developing a performance assessment is to help us probe and review DOE's performance assessment.

In regards to independence, I would hope -- and I'm going to say this just because it -- no one should get the impression that, well, gee, maybe all DOE has to do is take NRC's model, because, oh, that's what the NRC believes, run that, and if it shows compliance they're done, because we're already saying, "Oh, here is the way to do it."

Nothing could be further from the truth. What we develop in the performance assessment is a way to help us probe DOE's models. We aren't saying this is the right way to do it. It is a way we've done to help us probe DOE. DOE has to demonstrate the safety, and we will use this tool to help us understand what DOE is doing.

Clearly, we're putting in our performance assessment code, scientific formulas, processes that we believe are credible for Yucca Mountain. But there's nothing in our performance assessment code, as both Paul Bertetti and Jim Winterle pointed out, that should denote regulatory acceptance. And all of that work, be it the development of the models, understanding of results, as you'll see is a way for us to get ready to do our independent evaluation of the Department of Energy.

And with that, let me go to the first slide. And as way of an outline, I will talk first about -- give some idea of what we mean by risk-informed approach, talk about the performance assessment for the saturated zone, and then understanding the saturated zone in the context of the risk insights the way we -- one way to look at the results.

There are many ways to look at the results. Here is one way that we're -- for today we think is useful for understanding and putting into context some of the features, events, and processes related to the saturated zone, especially with respect to retardation.

Next slide.

In terms of the principles of a risk-informed approach, clearly we first start with a quantitative understanding of performance. Certainly what can happen, how likely it is to occur, and what are the consequences if it occurs? And that's related to the requirements for post-closure safety. The requirements for post-closure safety are multiple barriers, the dose limits in groundwater protection.

With respect to the dose limits -- everyone, that is the quantitative measure we have. And I would say early on the most disappointing part in our -- of getting comments back on our proposed rule for Yucca Mountain was an implication that we would merely run a performance assessment. The dose is either -- coming out is either below 15 millirem or above. Below 15 millirem you get a license; above 15 millirem you don't. And that's it.

That is not our role. I would say -- and I've said this before in a couple of different forums. The performance assessment will produce a dose. Let's say, whatever, it's two millirem. I have absolutely no basis for knowing whether I should believe that number or not. Two millirem. No one goes out in the field and measures two millirem. It's a future dose estimate.

I don't have a sense of why I should have confidence in that number. I believe that gets to multiple barriers.

And I know -- going back to yesterday, I know Atef Elzeftawy brought up the issue that NRC had walked away from certain requirements in Part 60. There were subsystem requirements, a limit on groundwater travel time, a limit on the waste package lifetime, and a limit on releases that were quantitative limits. Those limits were there to provide confidence that, indeed, that overall dose -- or at that time an integrated release number was met.

Over the years of trying to understand how to implement those subsystem requirements the NRC found they did not give confidence that the overall performance objective was met. And, in fact, culminating in the National Academy of Sciences report on the Yucca Mountain standards, they advised against the imposition of subsystem requirements.

Well, how do we get that confidence? It's through the multiple barrier requirement. And the regulation, while not giving a quantitative value, requires the Department of Energy to identify the barriers, discuss their capabilities, and present the technical basis for those capabilities. And it's in the context of the barriers that you're understanding how these barriers contribute to safety.

And as I go through my talk, I think I'll show how understanding how the various barriers the Department of Energy is taking credit for gives us that confidence and understanding that indeed that dose limit is met. And that's very important, and that I can't stress enough.

We may not have a quantitative limit, but I believe that the understanding of multiple barriers and their contribution is how you replace that meeting a quantitative limit with the -- providing you that confidence that indeed the dose limits are met.

In that regard, there's a variety of analyses to assist that understanding. The overall performance value, that dose limit is certainly calculated. As I said, it is calculated. You certainly want to understand how that dose varies with different assumptions, etcetera.

But ultimately I think the intermediate results, things like how long is the waste package estimated to survive, what is the transport -- the delay time of certain radionuclides, understanding the different pieces of the repository system that ultimately provide that confidence in the overall performance.

And I'll talk quite a bit about that, and there are certainly sensitivity uncertainty analyses to allow you to understand, point to what things really matter. Where do I want to bore in in my review?

Next slide?

The objectives of that risk-informed approach where we use the overall performance, the intermediate results, sensitivity uncertainty analyses, we want to identify those parameters, models, and assumptions that are most relevant to meeting the performance objectives. We certainly want to identify important uncertainties. Where is the variation in performance most significant?

And we would focus our review in key areas. We certainly want to look at risk dilution. One, it's not -- as you saw Paul show with the -- some of the Kd's, with the more recent approach, the variation in Kd narrow.

There is a concern, of course, with -- one might say for sake of conservatism, "I'll make my Kd range very broad" and run the performance assessment with a very broad range as a conservative approach. That may be prone to risk dilution. You tend to spread out the peaks. And in terms of what the overall mean dose estimate is, you could be arbitrarily making it lower by increasing that range.

And so there are issues that we would look at in terms of the performance assessment to understand how assumptions relate back to that final dose estimate.

Certainly, there is a possibility for inappropriate conservatism, where you say, "I'll be very conservative in this particular part, this particular subsystem." And, actually, the net effect is that it improves performance, and so you're really not being conservative.

And I'll give a -- and I'm not suggesting anyone is doing this, but I'll give an example of -- let's say, "Well, let me make the infiltration 1,000 times higher." I get a meter of water per year going into the repository. Okay. What that might do in terms of the water chemistry on the waste package, you are now flushing off any deleterious salt deposit or everything. You have a very clean waste package.

So it lasts forever. Yes, it's conservative with respect to the infiltration amount, but it may inappropriately give you a result that indeed you'll never have any significant salt deposits. You have nice water -- washing off the waste container. And I'm not suggesting anyone is doing that, but the performance assessment has a lot of attributes. There's a lot of submodels.

And part of the NRC review is understanding how these models interact with one another and making sure there aren't assumptions made in one part that lead to non-conservative results.

Next slide.

With that, that was a little bit on the -- sort of what risk-informed -- our approach will entail in a very succinct way. With respect to the risk insights, Keith Compton talked to these earlier in terms of our current understanding that we've put forward in our publication. We had retardation of the alluvium as a high significance. This is with respect to the saturated zone.

Matrix diffusion, colloidal transport, and the length of the alluvium flow path were all of medium significance. And I'll talk a little bit more about that in the subsequent slides.

Next slide.

In terms of understanding saturated zone performance, one of the things we've noted, that in terms of getting risk insight it's extremely important to look at the inventory involved, then look at the identification of the barriers, but that also is in relationship to the inventory. You don't want to lose sight of that, and certainly consider the uncertainties. And all of that you'll see in my subsequent slides.

Next slide?

In terms of the radionuclide inventory, why is it so important? If I look at this slide, I realize there is a lot of numbers. However, if I look at the percent of the inventory for americium-241, you can see it's 54 percent of the inventory. Now that's at 1,000 years. It will vary over time, obviously, because of decay.

Plutonium, 25 percent, 18 percent. You can quickly see that a large fraction of the inventory of the repository is tied up in a few radionuclides.

There are other radionuclides. Technetium, .7 percent; iodine, .002 percent. If we looked at performance assessments today, generally it's iodine and technetium that cause the dose. These are mobile radionuclides. They are generally considered to be unretarded in geologic media.

But if you look at the percent of the inventory, it is a very small fraction of the inventory. If you look at the inventory weighted by its dose conversion factor in that how much -- what's the effectiveness in causing radiological harm, a nuclide like technetium is even far less a percentage in terms of radiological harm.

You're looking at for both iodine and technetium less than a thousandth of one percent. So in terms of, yes, iodine and technetium, we do see those radionuclides. It's not the bulk of the inventory.

From an NRC review standpoint, we would be looking at a very -- we would not be doing our job, in my opinion, if we focused on iodine and technetium, less than one-thousandth of one percent of the radiological hazard that's there. Doing a good job just on technetium and iodine doesn't really say much about safety -- the safety of the repository.

There is what's happening here with these radionuclides. It's an important part of the review. You want to make sure, in general, these never get out. Well, that's comforting. But I think in terms of our review, when looking at the potential radiological hazard, you sure want to know, well, why are we assuming they're not getting out?

What are the processes affecting those? Not so much we want to -- we see iodine and technetium getting out. Why aren't they getting out? And that's part of this risk-informed. You want to understand the hazard. You would not want to just focus on iodine and technetium.

Next slide. In that previous slide, I had 20 or so radionuclides that are commonly there. If you'll notice, one of the radionuclides -- and it might have been the americium -- has a half-life of 430 years, a relatively short half-life. I was asked if, indeed, that particular display of the percent of the inventory was being skewed inappropriately by short half-life radionuclides.

So I opted to do -- here's another slide depicting the same amount of information, but I have excluded from the inventory everything with a half-life less than 10,000 years. So these are the nuclides with half-lives greater than 10,000 years. And as you can see, actually it's fairly dominated -- well, it is dominated by one plutonium. In fact, the radiological hazard is 99.5 percent.

Technetium is a little more significant in terms of the inventory. The radiological hazard is still just two-thousandths of one percent. Neptunium got a little higher. But in general, you're seeing a similar kind of behavior that actually plutonium, even for the radionuclides with half-lives greater than 10,000, which is still a very dominant aspect.

Next slide.

And why is that important? If we look at -- and this is just a -- a somewhat typical plot. If we looked at releases from a waste package, you can see that indeed the americium, in terms of what gets out of the waste package, is relatively large relative to iodine/technetium. Neptunium is a little bit larger, but you can see the plutonium/americium/neptunium, in terms of what gets out, is significantly larger than iodine and technetium, which is no surprise given the relative amounts in the inventory.

Go to the next slide -- the question is: what's released from the geologic setting? I used the same scale just because it -- I didn't want to distort things. But you can see the neptunium/americium/plutonium -- you can't -- well, you can't see them. But, trust me, they're all zeroes. They don't get out.

They are getting out of the waste package. They do not get out of the geosphere. Something is going on between the repository and the geosphere in the geologic setting that's resulting in that -- those significant releases that you saw before not getting out.

The iodine and technetium -- there is a little rise there. They aren't delayed. Generally, they -- the releases from the waste package, on the order of, I'll say, a thousand years or so make it to the geologic setting.

Next slide.

In terms of trying to capture this behavior for that inventory, one of the goals in my mind for performance assessment is try to present information to our technical staff as well as other review committees, committees in general, to give an idea of that understanding.

And what I've done is created a table -- and this is just a prelude to that table -- and you'll see a bunch of L's and a bunch of D's on the table. And for things that limit release, I calculated the releases from a waste package. And if indeed -- if the release from a waste package, if that release was instantly transported to the accessible environment, if it would result in a dose that was 10,000 times less than the standard, then I gave it three L's; 1,000 times, two L's; 100 times, one L -- to look at its effectiveness.

Why 10,000 times, you ask? Well, there's approximately 10,000 waste packages. So what this would say, where you see three L's, that means every package in the repository leaking at that rate, those releases could be instantly put to the compliance location, and it still would be below the regulatory limit. And so that's how I came up with 10,000 -- as a way to display some information.

There are other things -- aspects, barriers of the repository -- that delay releases. And for there the delay release is a little simpler. I looked at the time it took to transport to the saturated zone. If it was greater than 10,000 years, it got three D's; 1,000, two D's; 100, one D. And 10,000 years is the regulatory compliance limit. So if it took greater than 10,000 years to get to the accessible environment, that means no radionuclides would get there within the 10,000-year time period.

And with that, I'll show the next table. And what this is is a way to look at different features of the repository system, have the waste package, waste form, solubility limits, and solubility limits plus limited water are all aspects of the release rate, so you can see there the L designation is used. For the waste package it's relatively simple. It's just the delay time, the time to that initial defect in the waste package.

And then, more relevant to today's discussions is transport in the geosphere. And we have transport in the fractures, in the fractured rock, the delay that is evidenced there, and transport in porous media, the alluvium. As you can see, there is a definite difference depending on the radionuclide. And certainly not too surprising, the americiums, plutoniums, are significantly delayed. A nuclide like technetium that is generally unretarded is not delayed much.

But I think in terms of what's going on, and where do we want to look, once again, from an inventory perspective the americiums and plutonium that make up the majority of the inventory is really an important aspect to see more than 10,000 years delay for those nuclides is an important aspect.

I presented this table not that long ago. I've used it in a number of places. I know I presented it to ACNW. I also presented it to the National Academy of Sciences, and this table is based on mean values. And they made a suggestion that we also were part of this evolution over the last two years. You really would like to make this an uncertainty table, that, okay, that's the mean value. But what's the variation in that behavior? And we've done that.

And, next slide?

In terms of the saturated zone characteristics, there is a relatively flat gradient between the repository and the compliance location.

That's one of the reasons the transport time is relatively slow, even in unretarded space. But once you add some retardation -- on the order of 50, 100, or more -- you get some significant delay in the alluvium.

Certainly, there is the porous -- the sorption properties in the matrix versus the fracture where in the fracture there is matrix diffusion. And I will talk a little bit about the difference -- why we see what we do in our model with respect to matrix diffusion, possibly a difference with the Department of Energy.

Not to say one person is right or wrong, we're aware of these differences, and it's part of this understanding. We want to understand the limitations and the different assumptions being made in our model, etcetera, but it's all part of gaining this understanding.

Next slide.

And so in looking at that variation, in terms of getting to this uncertainty table, where I'll look in more detail at the behavior of the saturated zone, certainly in the alluvium the retardation factors can vary orders of magnitude for certain radionuclides. For certain radionuclides there is no variation. Example: iodine and technetium are assumed to be unretarded. There is no variation there.

The length of the flow path is uncertain. In fractured tuff, there is -- matrix diffusion depends both on the sorption properties of the matrix and the extent of fracturing. And I think I'll -- maybe this is as good a time as many, but there is a little difference in how we got to the approach we have in the TPA Code.

At one time, we had the -- well, we still have the capability; we do not use it -- to do matrix diffusion in the unsaturated zone and saturated zone. We only invoke matrix diffusion in the saturated zone.

We have the exact same model for matrix diffusion in the saturated and unsaturated zone. We thought the parameters for that model would be very similar, both -- fractured tuff in both cases. And so in looking at that, when you're doing matrix diffusion in the saturated zone, as I mentioned, a very flat gradient. Travel times are relatively -- travel velocity is relatively slow. The flow path is 10 kilometers or more.

This is an opportunity for matrix diffusion, a slow process, to occur. And so that is

-- it is not a very dominant effect, but it is a non-trivial -- it does have some significance in that context.

In the unsaturated zone, the flow path is on the order of 300 meters, decidedly less than 10 kilometers. The gradient is one. The velocities are rather high. And so the travel time from the repository to the water table is relatively quick. Trying to simulate a slow process like matrix diffusion in a very rapidly-moving fracture is incredibly time-consuming. It can break the bank, basically, in CPU time.

We years ago did offline calculations. And, once again, we're using the exact same model. If that model is the same, we can simulate it. It takes a hellaciously long time on a computer. We noted that it did very little. There just wasn't enough time to -- for matrix diffusion to occur that you would get a significant amount of diffusion in that -- from that fracture.

And so we don't have it in there, because we have the same model. My understanding of the Department of Energy -- they actually have two different models for matrix diffusion in the saturated zone versus the unsaturated zone. And so they have a different approach. And, obviously, that's something we will -- we are looking at. We're not saying our model is right or -- but that's why we merely use it in the saturated zone.

Next slide.

If I go -- here is the uncertainty table or variation table. If I expand that table that you saw just for the geosphere transport -- this is just looking at attributes of waste isolation in the saturated zone -- and I now have the variation of the alluvium distance. I'll set the alluvium distance to its shortest value and to its highest value.

The alluvium retardation -- I'll set it to its lowest and its highest. I'll combine, let me make the alluvium distance and retardation both at its lowest and highest. And then I turned off matrix diffusion, and I'll explain why I did that in just a minute.

But what you see -- and I realize there is all of these D's everywhere, and it's not intended to be an eye chart.

(Laughter.)

But what you're -- if you -- stepping back, at the broad-scale view, what you're seeing is for the americiums, the plutonium -- indeed, it doesn't matter. Our lowest -- our shortest distance in the alluvium, which I believe -- and I had it down on one version of my slides, and I -- I apologize, I did not bring the right version. But I believe it's two kilometers.

Also, the shortest values for -- the smallest values for retardation, we've still got over 10,000 years of delay for all those radionuclides. I think that's a very important aspect that, once again, an expansion of -- you're trying to give information. And I know, Dr. Ryan, you talked about, where do you want to expend your resources? Gee, when I look at this, I want to look at -- I can just look at the lowest value.

Do I have confidence in that as a lower bound for retardation? And it helps me. I mean, if it has more, that's fine. I'm getting even more performance. But it lets you know, am I worried about the retardation if it varies between the range I have? It's not so much the range. Is that low bound the right low bound?

And I know Paul or Bertetti brought out for some of these radionuclides even the low value is fairly significant. But, indeed, it gives you a sense of, how is that variation changing things?

It also shows why, in terms of sensitivity analyses, you always want it -- and this is the part -- using performance assessment as a tool to help your understanding. It does not drive your understanding. I would never use a performance assessment result to do my thinking for me. It's a way to help my thinking.

And, gee, I might -- if I did a 10,000-year dose assessment, I would never see the retardation factors for americium or plutonium ever show up as important. It is always zero. Also, the length of the alluvium distance. And that's why you use the tool to get -- to pull out the information, and that's what we're -- this is information for our staff.

I'd like to think it helps other people get a better sense of, well, why is the repository operating the way it is? Why should I have faith, or confidence, not faith -- confidence that the dose limit is what it is? I don't ever see americium and plutonium getting out. Well, this tells you why.

Now, on the other spectrum, there is a few other radionuclides. They don't change either. Technetium, carbon, iodine, they are all single D's. Well, not surprisingly. As I said, the retardation value is one for those. It doesn't vary. But it clearly gives you an idea of why -- why do you see iodine and technetium? Well, that's why.

Now, I'll say it also -- there is always things -- when you do this, you never know what to expect necessarily. And, I mean, there's a couple of things I'll point out. If you look at the alluvium distance, there is absolutely no variation. If it was one D, it stayed one D; two D, stayed two D; three D, stayed three D. As Jim Winterle pointed out, we really don't see a lot of significant as long as you have at least a couple kilometers.

If I look at neptunium, there's a few nuclides there that -- it's always good to have something that forces you to scratch your head and think. That's the beauty of performance assessment is produce information that forces you to think, why should I believe this result? What's going on here? And at first, you might say, "Something isn't going on right."

Here is one D for alluvium retardation. When I kept both low, the distance and the retardation, I ended up with two D's. It got better when I made both low.

And, you know, at first I said, "Oh, boy, something is not right here." But it's actually very understandable, and we did another run. Well, let me take out matrix diffusion in the volcanic rock. And, indeed, when I did that, I got to the one D.

What's going on is that if you take away an unretarded or a low retarded -- well, neptunium actually is unretarded at its lowest value. So if I remove the -- I'm not when I said "its lowest value," obviously, if I take out alluvium it isn't that it's gone. I replace it with volcanic rock. I mean, it's still a flow path from repository to the compliance location.

If I remove an unretarding alluvium and replace it with a matrix-diffusing volcanic rock, it actually is better. And that -- and it made perfect sense after I did that. The retardation in the volcanic rock we did not vary. That was kept to a relatively low number, but it was kept -- so actually matrix -- some matrix -- matrix diffusion with some retardation for neptunium is better than an unretarded alluvium.

And so, once again, that's the benefit of looking at this information. I think it's -- for me, it's a very useful table to force your thinking. And like I said, that is -- the risk-informed approach is you want to think and understand what's going on. You want to focus your review on where are things really mattering. One might argue americium and plutonium.

Like I said, let's -- how sure are we about the low value? What does come out, though? Where does neptunium -- a key radionuclide, relatively soluble, somewhat mobile, not a huge fraction of the inventory but a non-trivial amount of the inventory -- and you can see that the variation in retardation is one D to three D.

And so there are processes going on with respect to the neptunium that I think what this chart -- one of the things it's telling me is that neptunium is an important thing to understand what's going on in the geologic system with respect to retardation in the saturated zone, be it either matrix diffusion or retardation in the alluvium.

And with that, I mean, that's sort of the -- I'd like to think this is part -- a key part -- of the risk-informed process, trying to convey what you understand and what you're going to review. It also -- I mean, I think for some of these things you go back and you say, "Okay. Well, what's the evidence for the retardation factors for these radionuclides?" And you can start piecing together the entire picture of, "Here's the technical basis. Here's why I have confidence in the dose estimate." There is parts of this that, if I can support the delay times here, I have confidence in that final dose estimate.

Next slide.

In summary, that was it. I mean, and that was the biggest part of my -- as we continue in this process of developing information to assist our review of the Department of Energy -- one thing I did mean to say. That was done using our results. Obviously, it's easier for us to take our code and develop that.

However, do not despair, DOE, we are doing that with your results. And we are certainly in the process -- as I mentioned, we are significantly ramping up our review of the DOE TSPA, the GoldSim results, and we believe we can cast some of their results in a similar kind of thing, to give us some feel for what's going on.

And are we going to compare it to ours? I don't think so. I mean, it's -- but it's more you create a table like that. Why do I believe the delay times there? What's in their model? What retardation factors? And it's a way of probing their analyses and actually -- although this was the NRC results, we're rapidly moving to doing actually more with the DOE results rather than NRC's results.

It turns out the risk insights, in summary, what we want to do is we want a comprehensive understanding of the performance assessment, what's going on and why. We want to identify important models, parameters, and assumptions, consider the uncertainty that -- how that variation might change the result. And it provides for what we would call -- that is the informed -- when you say risk-informed, it's an informed and focused review that we would do on the Department of Energy's license application.

And with that, I'd be happy to answer any questions.

MEMBER HORNBERGER: Thanks very much, Tim.

MR. McCARTIN: Yes.

MEMBER HORNBERGER: I think that what I'd like to do is actually invert the order in which I call on people to ask questions.

(Laughter.)

Give people a fair chance.

(Laughter.)

The last shall be first.

(Laughter.)

I'm not sure -- Don, are you there in Las Vegas early in the morning?

DR. SHETTEL: Yes.

MEMBER HORNBERGER: Do you have questions?

DR. SHETTEL: I have one that I can think of this early in the morning.

(Laughter.)

MR. McCARTIN: I am impressed anyone is there.

DR. SHETTEL: Does the model include in-growth of radionuclides, Tim?

MR. McCARTIN: Yes, it does.

DR. SHETTEL: Okay.

(Laughter.)

MEMBER HORNBERGER: Great. Since it's so early, if you think of another question later, Don, just let me know.

Dick?

DR. PARIZEK: I'm looking at Number 12, and some are blanks. They neither have D's nor L's. What does that mean?

MR. McCARTIN: It was below the bare minimum --

DR. PARIZEK: For either.

MR. McCARTIN: -- of a single L or a single D, right. So, for example, for delay time it was less than 100 years, or for the release it was less -- it was more than 100 times the -- no, I mean, more than 100 times less than the standard.

(Laughter.)

DR. PARIZEK: Page 15, is that like a one-on and one-off analysis? As you were describing it, it almost kind of came across that this is --

MR. McCARTIN: No.

DR. PARIZEK: No?

MR. McCARTIN: It's just looking at the behavior of that particular aspect of the system. And it -- you would have -- that delay time is there, whether there is a radionuclide to be transported in it or not. Now, I obviously released some radionuclides into there, to get that delay time, but it's -- this is more what we call the capabilities of the barrier, and you don't -- the saturated zone has this capability, regardless of whether there's a leaky container. And --

DR. PARIZEK: You took out the alluvium's role, and then you got the benefit from the -- in the case of the tuffs. So it sounded like you were almost making trades there that --

MR. McCARTIN: Right. Now, that was in terms of the variation of what the alluvium can provide. In our code we have -- the length of the alluvium flow path is a variable. And all I did was set it at the lowest value in our code versus the highest value. And so the alluvium I'll say varies between -- and this is a guess, it's approximately right -- between two and 10 kilometers in length.

And so it's not so much a neutralization in that it -- well, what if it was actually at its lowest value? What if it was at its highest value? And that's all I was doing with those was just spanning the -- rather than we typically, when you see our mean dose curve, it's sampled over all of that. This is just setting it to, how significant is that low value? And --

DR. PARIZEK: When you were describing the significance of each of these points, that's part of the narration of what the TSPA result means, or how it came about.

MR. McCARTIN: Yes.

DR. PARIZEK: It's not independent lines of evidence that you might be offering to add another reason for feeling good about the results, right?

MR. McCARTIN: Right.

DR. PARIZEK: This is -- you're just describing them, and that requires transparency and, you know, clarity in terms of --

MR. McCARTIN: Absolutely, yes.

DR. PARIZEK: -- data and analyses, all the rest of --

MR. McCARTIN: Yes. Yes. Absolutely, yes. Independent lines of evidence would be --

DR. PARIZEK: Something over and beyond.

MR. McCARTIN: If we carried this further -- I mean, the logical, in my mind the thinking process that you would like to go to is here is -- I have this depiction of the results. And we'll just use that saturated zone table. Okay, here is what's going on in the saturated zone. Now, next comes, well, why should I believe that range of Kd for neptunium?

What evidence do you have for it? And you would have some experimental evidence. You might have some additional information on, say, flow paths or retardation. The best example I can give is you might -- after you have that technical basis for those parameters, you might have some natural tracers as was alluded to yesterday in the geosphere that you can use to, geez, it's -- your unretarded travel time is, say, around 500 years or something.

What are geochemical tracers? And maybe you have some that are -- tend to have some sorption.

That would be what I would consider another line of evidence beyond, say, experimental information on heads and Kd measurements, etcetera.

MEMBER HORNBERGER: Okay. Jim Davis?

DR. DAVIS: I want to see if I understand Slide 15. Also, I -- in the same line of questioning, you have your results. And, as I understand it, you're looking at the -- this is sort of a non-quantitative sensitivity analysis, the expression of those results, where -- and you might use it to prioritize where your most important parts are to focus on.

But in that analysis, you seem to take it for -- in some ways, it seems to me you're taking for granted that you have your arms around the whole -- everything that needs to be known. And to me, in listening yesterday and today, there are still some things that are maybe not as well known as they should be. And I don't see that showing up now in these D's.

DR. PARIZEK: Right. Yes, that was my conceptual error. Are there areas of concept here that would blow this apart in some detail, or do you know it all? And as a result, I would say like alluvium, one kilometer versus two, DOE had I think as little as one yesterday, you have two. What if it's none? It's all straight south in the faults.

Well, okay, there may be something that's alluvium-like down on the southern tip of Yucca Mountain, but what if it's all faulted flow? And if it is, this is a conceptual error. It might have a big consequence. And so these are the -- this is the conceptual error thing that was -- a question I would pose. And you're sort of asking the same thing. What don't we know?

DR. DAVIS: Well, you know, I was thinking about the mean value for the neptunium retardation in the alluvium. Is that really the mean? It's the mean based on what has been measured so far. So have all of the appropriate -- have the right measurements been made? And --

MR. McCARTIN: Probably. Yes, very valid points. And that's why I'll say what this -- what risk-informed, at least in this context, is trying to display -- what you put -- you've put some concepts into your performance assessment -- be it models, parameters, assumptions. Where do they show up in terms of relevance of estimating the performance measure and delay time?

You're right, there is -- and I'll say an inherent assumption that we have our hands around the problem. But, first, you want to -- in my mind, you want to start with, well, let me understand how these assumptions have affected my result.

Now that I understand how they're affecting the result, I mean, really, to me that's -- this is the starting point for the NRC review. It is not the NRC review, by any means. Now where I'll say I'm more of a performance assessment person, here is where the discipline people come in. And we work as a team in terms of, what's the science behind the retardation factor, the length of the alluvium? What's the technical basis?

And this is -- just gets us starting. What do we really want to hone in on? And we don't want to -- there are many, many different aspects to Yucca Mountain. Trying to go at all of them, without some kind of prioritization of what does it mean, and that's what this is about. Here is what -- you have put all of these assumptions into the code. Here is what it resulted in.

And now I can go in and start attacking the technical basis. Gee, would this make a difference? And, you know, an example I give with alluvium -- or the americium and the plutonium. It looked like, well, gee you know, gee, at the lowest value of Kd, they really have a lot of delay time.

Well, maybe I'm not that interested in the distribution or the upper bound, but what's the basis for that low value? Indeed, it can't be lower. And so there's ways of thinking about the problem. Rather than having to look at everything, there is ways to focus your thinking. And that's all this is.

And you're right. I mean, why should I believe the performance assessment? Clearly, not because I can display it. And, gee, see, I understand the performance assessment. And I didn't mean to imply that, okay, now I believe it, because I understand it. No. There is all of this technical basis review that has to be built up. That's where the multiple lines of evidence, for the things that are more important, you would like to see multiple lines of evidence. Why do I believe that part?

For some of the other things maybe it's not as critical, but that technical basis really, combined with an understanding of how it affects the performance assessment result, is what ultimately gives you the confidence in saying we understand the DOE analysis to be X.

And, yes, I mean, it's -- you're absolutely right. I mean, there is -- this is presented in a way that I believe it. And I -- I'll say I believe my understanding, but that isn't -- that doesn't say that all of the models backing it up are necessarily believable. That's part of a very detailed review, and that's the three to four years that NRC is going to take to understand the technical basis behind those models. That may include the result in changes to models and parameters.

MEMBER HORNBERGER: Jim Clarke?

DR. CLARKE: Tim, excellent presentation.

MR. McCARTIN: Thanks.

DR. CLARKE: And, by the way, you answered my questions from yesterday on matrix diffusion. So thanks for doing that -- in the vadose zone.

Just one question. Are you applying this thinking to other subsystem elements? For example, waste package corrosion, waste package lifetime -- using this as a tool for other --

MR. McCARTIN: That's a goal to lay out, and we're turning the crank in terms of working as hard as we can in preparation of getting a DOE license application to provide -- and you can see, at least I think for a lot of us, this provides us additional understanding. And that's what you want to start into the license review is, how are we going to depict the different aspects of the -- how are we going to understand what matters?

DOE is required to tell us. I mean, in fact, if you look at our Yucca Mountain review plan, one might ask -- I mean, the first thing we have in our review plan for post-closure is -- tell us what the barriers are, and tell us what their capabilities are. Clearly, DOE does that at the end. I mean, you can't really do that until you're done with everything.

We're asking for it first, because we want to have that understanding. It will be a way to -- now the question is how best to depict that information. The Department is certainly -- has the flexibility to provide us that information however they can.

Part of what we're doing is looking at ways that -- and that's why we're looking at the GoldSim model now. How we can go into GoldSim and extract stuff that may be -- oh, gee, we'd like to look at it this way, and maybe DOE didn't give it to us. And so there's this process of how best to present this information to assist our review, and --

DR. CLARKE: You know, it just struck me this would be a great way to look at the engineered barrier piece as well.

MR. McCARTIN: Yes. Yes. And to date, if I go back 20 years ago, when my beard was red as Mike pointed out --

(Laughter.)

-- I started out doing, well, groundwater modeling and transport modeling in a variety of applications. And so it was easier to -- for me to think through the saturated zone and the geosphere. Certainly, the -- we're looking to expand it into the other areas for the whole repository system.

DR. CLARKE: Thank you.

MEMBER HORNBERGER: Ines?

DR. TRIAY: I also think that this is an excellent way to present data. And you really ought to be commended for trying to present data in a manner that is very understandable, you know, for many people from different backgrounds to come in to look at the whole system performance. So that was very well done.

On page 15 --

MR. McCARTIN: Thanks, on the part of the NRC staff and the Center staff. And the review committees that -- and people have given me a lot of comments. Like I said, we've evolved over two years, and it really -- there's a lot of people involved, and there's a lot of credit to go around.

DR. TRIAY: This is excellent. Let me ask you a couple of questions. On page 15, you have americium and plutonium, and, in many ways, you know, some of our understanding of solubility and sorption, actually when you put everything together it comes through here, you know, which is the biggest uncertainties in uranium and neptunium.

The anions like technetium, or technedate or iodate -- well, clearly, they are going to migrate likely as anions, or they're going to be limited by solubility, if you can evoke some reducing conditions in certain cases.

So, based on that, can I ask you, then, from the NRC's perspective, the result of these analyses that you will concentrate your sorption and solubility efforts on uranium and neptunium?

MR. McCARTIN: No. I don't think that's a fair assumption, in that -- only because there is a lot that goes into the considerations. And so, you know, an example for uranium, it may be -- even at the high end of solubility, it's pretty low. And so there's not that big of an issue.

I would say, in general today, I think neptunium is the one issue. Colloids for plutonium certainly is a way that defeats solubility limits and could defeat some of the retardation. So there is aspects of colloids.

This is -- once again, it's evolving. It should not be taken as the definitive thing. Here is some information, but you have to put it into the context of the whole performance. And what you are saying, though, that I think is correct -- that solubility limits is an important part of this also, because you've got the -- the transport is one thing, but what if the solubility limits were low enough that you never really got any significant amount of that particular nuclide.

That would be another capability, and I'll say in terms of the big picture of the -- assessing the safety of a Yucca Mountain repository, solubility limits for certain radionuclides could be a very important aspect of providing some measure of safety.

DR. TRIAY: So, then, I would like to ask you another question. What is it that you think that you don't know? If -- you know, to go back to the question here. I must admit that I'm having a little bit of trouble visualizing exactly what is it that would make page 15 a totally different picture. I mean, I can see that myself. I don't want to lead you on an answer, but I would like your opinion.

MR. McCARTIN: Yes. Yes. Page 15, totally different.

DR. TRIAY: Well, different enough to matter, right? I mean --

MR. McCARTIN: Right. Well, certainly, as Dr. Parizek brought up, if indeed there was no alluvium, I think the table would change dramatically, if there was no alluvium. That's certainly one aspect.

If I brought in --

MEMBER HORNBERGER: It would change dramatically for neptunium, not for very many other things.

MR. McCARTIN: Well, I --

MEMBER HORNBERGER: Okay. Never mind. Dramatically, okay.

MR. McCARTIN: Yes. For neptunium, yes. I mean, I think americium and plutonium might show some differences also. I mean, I haven't done a zero --

MEMBER HORNBERGER: Okay.

MR. McCARTIN: -- but certainly there is a -- one of the things to add in here is something with respect to colloids. I don't think colloids would change the overall tone much. They need to be considered. But I think one has to look at, you know, the generation, how much, what radionuclides, and the ability to transport colloids a long distance.

And as Paul mentioned yesterday, you know, we are certainly following the DOE work with respect to colloids, and we are -- we are doing some modeling ourselves with respect to that. But I don't believe -- I mean, that's something that is certainly an uncertainty that is not accounted for here. I do not believe it would make a big difference, though.

Other than that, I guess if there was something about the chemistry that would drastically change the retardation values in the alluvium -- I am not a geochemist. I would be more than happy -- I know there is at least three of them over there, and maybe Paul would be -- as a speaker from yesterday, would be -- I don't -- my layman's understanding of the geochemistry, I don't believe you would see a radical change in the chemistry of the saturated zone. But I -- I'd be happy to turn to Paul. I mean, that is not my area of expertise.

MEMBER HORNBERGER: No, that's okay. That's okay. I think Ines' question was more general than that.

MR. McCARTIN: Yes, okay. Okay.

MEMBER HORNBERGER: Allen?

DR. CROFF: Excellent presentation, but Jim got there before me, so --

(Laughter.)

MEMBER HORNBERGER: Michael?

ACTING CHAIRMAN RYAN: Thanks, George.

Tim, I think a lot of good questions have been asked already, so I won't repeat them. But one thing that strikes me that I think is a power of this tool, if for example we decided neptunium, which, based on all of the discussion and questions is a key radionuclide, there's no reason you couldn't set up a table that said, "Variation in waste isolation and saturated zone for neptunium," and then down this left column look at all of the parameters that were of interest or people were discussing as important and create the same table.

MR. McCARTIN: Yes.

ACTING CHAIRMAN RYAN: So I guess my point -- and I think I talked to you about this the last time we talked about this general approach in tables, is you can drill down systematically -- and I think that's the important thing -- systematically to look at it for a radionuclide or a water -- range of water chemistries, or whatever the subject is, and then, you know, systematically bring that analysis back up to the overall risk-informed kind of question.

So I think part of it is not answering all of the questions today, but it is now a systematic tool, so that if I did it in a room, and you did it in a room, we'd probably come up with something similar if we, you know, used the tool in an appropriate way.

So to me that's the power of the tool, and it's interesting that you've now, you know, kind of taken to this next step from your, you know, presentation at the Academy. It's great work. Thanks.

MR. McCARTIN: Yes. And I'm glad you brought that up, because, I mean, it really is an approach that we're developing. The numbers, while they're interesting, they're not DOE's numbers. And those are the ones that matter.

And -- but it's more or less, when I did this, do you get information that's useful? I think yes. And so now it's a matter of looking at the DOE performance -- and here are some ways we can help communicate our understanding among ourselves and our -- to aid the review. And that -- yes, exactly, it's an approach.

MEMBER HORNBERGER: Ruth?

MEMBER WEINER: I'll bet you thought all my questions would be answered.

(Laughter.)

And I almost though Ines was going to ask them.

First of all, I want to add my thanks for a really excellent presentation and for your making the point very well that performance assessment is a way to find what parameters the performance is really sensitive, and which parameters don't matter. I think that's the real strength of the method.

Now, why didn't you include plutonium-238?

MR. McCARTIN: We don't have it in our groundwater --

MEMBER WEINER: It's part of the short half-life, high-curie content, but there may not be enough. I was just curious.

MR. McCARTIN: One thing -- when we do the performance -- there's certain time that we know, gee, you're not going to get out in X amount of years, transport is X. And if it's a short enough half-life, I mean, there's many radionuclides we've excluded.

MEMBER WEINER: Yes. It's 87 years, so that may --

MR. McCARTIN: Eighty-seven is pretty short. I mean, that --

MEMBER WEINER: Yes, that may have excluded it. That was just -- just a question, because it was the dominant part of the inventory -- of the curie inventory in WIPP. The dominant part was 238.

My other question is much more general. In the work you've done so far, is it the chemistry of the actinides themselves, or the sorption characteristics of the matrix to which the performance assessment is more sensitive, or can't you tell? Or is it too soon to tell? Or does it make no difference?

MR. McCARTIN: That one I will gladly deflect to either Paul or one of the geochemists over there. I am really not a geochemist, and I'm -- this analysis is based on the retardation factors we have in the TPA Code, but I don't know if I -- I'm not -- that's not my area. I --

MR. BERTETTI: Well, I can try. This is Paul Bertetti from the Center. I mean, I guess based on what I presented yesterday is that I would say that Tim's analysis is dependent on the retardation factors for alluvium. Obviously, included in those numbers are the uncertainties with which we've calculated those values, you know, to begin with.

MEMBER WEINER: Yes, recognizing the two are not -- not independent of each other.

My final question, which maybe Paul will answer also, is, you said plutonium might become important. Plutonium-4 is an intrinsic colloid. So if you have conditions that lead to formation of -- significant formation of plutonium-4, is that something that you can in the future then include in your considerations?

MR. McCARTIN: Well, the next version of the code will have an explicit colloid -- plutonium colloid in it. So we will be able to evaluate it. How -- and when I said it could become important, it really depends on the extent of the concentration of colloids and the ability to transport that concentration large distances, and, you know, that remains to be seen. I don't know if Paul wants to add anything to that or --

MR. BERTETTI: Yes. This is Paul Bertetti again. Yes, what I would say is that our intention from generation of colloids and assumption of plutonium colloids is that they would be associated with plutonium-4, and kind of independent of other things just as a conservative assumption for the first implementation.

MEMBER WEINER: Thank you.

MEMBER HORNBERGER: Neil, I think you had a question.

MR. COLEMAN: Tim, your discussion of matrix diffusion brings a question to mind, especially with regard to neptunium. We normally think of matrix diffusion as a fractured rock phenomenon. But it must also occur in the valley fill aquifer.

The alluvium consists of silt to boulder-sized fragments that radionuclides can penetrate by matrix diffusion. It means a much larger rock volume would be available for sorption, especially by neptunium.

Is this mechanism considered in NRC's TPA?

MR. McCARTIN: No. We don't consider a diffusion coefficient in the alluvium. It's considered porous flow.

MR. COLEMAN: That sounds like a big conservatism.

MR. McCARTIN: I don't know if it's a big conservatism. It is something we haven't explicitly included. I'll ask Paul if he wants to add something to that.

MR. BERTETTI: Yes. This is Paul Bertetti again. I would just add to that that the -- from the perspective of results from the ATC testing that the Department of Energy conducted, their initial push-pull tracer testing -- their results did not provide any indication of that process. So they were unable to verify or determine that that diffusion into alluvium grains did occur during the test.

So although what you have mentioned as conceptually reasonable, the sole test conducted so far did not provide evidence to support that. So that's one of the reasons DOE has made some of the decisions they have made to model alluvium the way that they do.

MR. COLEMAN: Well, of course, that test didn't extend a whole lot further beyond the disturbed part of the well, the disturbed area around the well. Unfortunately, they never had the chance to do the full-size field scale test.

MR. BERTETTI: Correct.

MEMBER HORNBERGER: Tim, we heard yesterday from DOE about the experiments at Busted Butte and in the alcoves, and DOE drew the conclusion that, in fact, matrix diffusion in the vadose zone was an important process. And this morning we heard from you that your model indicated that it didn't make any difference.

What should I conclude? Should I conclude that your model is wrong? Or that DOE's experimental results are wrong?

MR. McCARTIN: It's a good question. Right now, I guess I'm not familiar enough with the test results. But certainly, I'll say experiments don't lie. They do need to be interpreted, and I think we certainly will be looking at the test results.

And, you know, I'm not going to say that our approach in TPA was a correct one. I will say what we saw when we modeled it, there was not enough time for diffusion to occur. We felt that was somewhat consistent with at one time -- and this goes back a few years at least -- that the chemistry in the fractures was different than the chemistry in the matrix, suggesting that there wasn't at least rapid equilibration of the chemistries between the two. And so that matrix diffusion wasn't going on rapidly.

I haven't followed that particular experiment. I don't know if anyone -- the NRC staff or Center staff have any comment, but I think certainly we need -- we will be looking at that information, and maybe we can get back to you at a later time.

MEMBER HORNBERGER: Okay. Good. Thank you.

We are going to take a 15-minute break now. We will reassemble promptly in 15 minutes.

Thank you, Tim.

(Whereupon, the proceedings in the foregoing matter went off the record at 10:15 a.m. and went back on the record at 10:33 a.m.)

MEMBER HORNBERGER: Okay, we're going to reconvene and continue our Working Group session. For the next roughly two hours we have several presentations scheduled. And the 10.3 on your agenda, there's one slight change, but the presentation will go as scheduled.

But first, these are presentations by representations from the State of Nevada and Nye County and the Electric Power Research Institute, EPRI.

First, we have a presentation scheduled from Don Shettel who is with Geosciences Management Institute and is, of course, one of our panel members as well.

Are you ready, Don?

DR. SHETTEL: Can you hear me?

MEMBER HORNBERGER: Yes, just great.

DR. SHETTEL: Okay, I'm going to be reviewing sorption data that we have available to us, since the State of Nevada is not doing any sorption work itself.

(Slide change.)

The second slide is an outline of my talk. I'm going to talk a little bit about solubility, then sorption in the unsaturated zone and then the saturated zone. Then I'm going to talk about DOE sorption assumptions and finish up with conclusions.

(Slide change.)

Slide 3, volubility of neptunium. I have two Eh-pH diagrams here. They are essentially the same except for the temperature. The one on the left is 25 degrees. The one on the right is 95 degrees. The one on the left is appropriate for the saturated zone. And it shows under oxidizing conditions, the neptunium is somewhat soluble. The beige or yellow area there is a solid field. But it also shows -- we have one ppm fluoride here both of these diagrams which is a very conservative number for at least the saturated zone. And under acid conditions you see that there are some fluoride complexes in neptunium.

If you look at the diagram on the right which is the higher temperature one, more appropriate to the in-drift environment, where we may have fairly acidic conditions, depending on the evaporation of pore water and seepage water. We have two fields where fluoride can dominate. And remember, this is still one, only one ppm fluoride so with any concentration in the vadose zone at all, fluoride is going to be a dominant complexer of neptunium.

And as I state in the caption, these diagrams only have one part per million total of phosphate. If we increase the phosphate a little bit, 10 or 100 parts per million, we start seeing the fields where we have neptunium phosphate complexes and unfortunately, I didn't show one of those. But they're just under the surface there. Remember, the fields you're seeing here are just the dominant complexes and you still have all the other complexes that are not dominant or lesser value in terms of activities underneath these essentially the most dominant fields.

(Slide change.)

Slide 4, looking at some of the DOE neptunium solubility data. The one on the right shows a comparison of some of the PNL data. This solution is spent fuel versus some of their model calculations. A few years ago they dropped the solubility from the upper line there which I believe is Np205 solid phase down to the three lines. It also shows the pH dependence. This drop in solubility from the upper model down to the carbonate dominated model at lower solubility is maybe realistic. It is certainly not conservative.

The diagram on the right shows the solubility of neptunium versus pH and partial pressure of carbon dioxide and this model seems to indicate that DOE's only concerned with the carbonate model and has not really looked at any other complexes such as fluoride or phosphate.

Go to the next slide.

(Slide change.)

Still in the unsaturated zone. We're looking at the time versus the fraction release of technetium which is a non-sorbing radionuclide. I see we get -- it takes up to 100,000 years or so to get most of the technetium out of the system to the water table. The problem with this is chlorine-36, the bomb pulse, is also non-sorbing radionuclide. It travels from the surface, land surface to the ESF in about 50 years and that's only in the northern section of the repository. In the southern section it may already have been flushed past the repository level. So here we have a discrepancy of two non-sorbing radionuclides. One is taking on the order of 50 years to traverse not to the water table, but to a significant maybe halfway between the land surface and the water table. The other non-sorbing radionuclide technetium is taking orders of magnitude longer to reach the water table for only -- for about the same distance.

This is a major discrepancy and needs to be explained by DOE. Why haven't they applied their sorption models to chlorine-36 to see how real they may be?

(Slide change.)

Going to the next slide, slide 6, shows some of their experimental data for neptunium for a couple of different rock types. The sorption data versus experiment duration and we see a 2, 3, 4 order of magnitude difference range in experimental results here which really hasn't been explained.

Let's do the colloids. Sample heterogeneity, insufficient sample size. I know they've done some work on size fractions, but crushing of grains generates new surfaces and you can't get around that.

(Slide change.)

Next slide, second one for the saturated zone shows a diagram, I believe, Bob Andrews showed yesterday and which I discussed a little bit in my questions yesterday.

This shows sorption K_ds for neptunium in the alluvial vise from Nye County Drilling Program. The point was only two of these wells, 19D and 2D are in the flow path, the potential flow path from Yucca Mountain. The rest are to the west and really don't apply.

The other problem there's -- 19D -- especially there are at least I believe seven forest zones of flow. Most of the flow goes through one or two of these zones and I can't recall the particular interval they sampled here as the most -- fastest flowing zone or not. The other point is depending on how they treated these samples, the finest grain materials is usually going to be between the more porous units, more porous and permeable units. And this type of diagram doesn't any uncertainties in their experiments or in their individual experimental results, just a mean value. It's not clear how they will be using this data in their models.

(Slide change.)

Moving on to a major portion of my talk involves 10 sorption assumptions that DOE made. The first one here involves microbial activity and how that affects their experiments. It's probably not a major factor in their experiments, but they have not confirmed that microbes have no effect in their experiments.

(Slide change.)

The next slide is assumption 2. And this involves the use of crushed tuff which I think is applicable to solid tuff matrix in the field. They also claim solid rock column experiments are infeasible due to long times needed to elute radionuclides, but that suggests they're trying to force things through the matrix of the volcanic rock and perhaps somebody didn't get the memo that fracture flow is dominant transport mechanism, at least and mostly in the vadose zone. So this assumption needs to be confirmed as well.

Next assumption number 3, the J-13 water and the deep carbonate water are bound to chemistry of groundwaters. First of all, this certainly doesn't apply to the vadose zone and if you look all the data that's been collected in water compositions in the saturated zone, it doesn't even apply in the saturated zone really. The sorption experiment, as Jim Davis showed yesterday, is a function of water chemistry, redox potential and all of that. So this assumption, certainly needs to be confirmed. I believe most of the experiments that were done on sorption were uncontrolled in terms of gaseous phase. They certainly had atmospheric CO_2, but other than that redox conditions, were not controlled.

Next assumption involves transport modeling. DOE has broken down the site rocks into four classifications. These are rather simple. This assumption also requires confirmation, especially perhaps through the alluvial rock type because that can be highly variable rock types and grain sizes and everything you can imagine. And the iron oxide stratum was meant to simulate the corrosion of the waste canister, an engineered barrier, but it ignores all the different metals that are present in the C-22 which we think if there's rapid corrosion in the -- early on in the regulatory compliance period, assuming that stays at 10,000 years. If it gets longer, then it's relatively much earlier than the compliance period.

Next, assumption number 5. They assume that measuring a single radionuclide is applicable when more than one is present. It involves a competitive effect between radionuclides and other cations in the water, for all available sites on mineral surfaces. This assumption certain requires confirmation. It may not be as important in the saturated zone or far-field as it is in the near-field, but certainly it could be important in the near-field in the vadose zone.

Next assumption involves nonlinear isotherms and that sorption coefficients are not constant value for different rock types, assumes variability of sorption parameters is a function of the concentration can be captured by just lowering the K_d to some minimum value so where experiments are above that value. But assumes that a single K_d per rock type can explain sorption behavior under all temperatures, solute compositions and pH-Eh conditions. This -- I don't know how you could confirm this because if anybody does geochemical modeling knows that if you -- even if you look at the diagrams I presented in the third slide for neptunium, you know that neptunium has different behaviors under different temperatures, different complexing ligand concentrations and all that. So this is just kind of silly.

(Slide change.)

Go to assumption number 7, slide 14. Again, we assume or DOE assumes they should say that lowering the K_d to some minimum value is sufficient to take into account moving water and fractures, what have you. This certainly is a dubious assumption in the unsaturated zone because of the possibility for rapid episodic flow. And if fracture flow is dominating in the saturated zone, that needs to be confirmed. We also have a problem with -- I'll get into this a little bit more in a moment, but long time steps in there, TSPA and modeling.

(Slide change.)

Next slide involves some experiments that we have done in our office. Take a little side trip here from the assumptions for a moment. We have made some thin welded disks or welded tuff disks, I should say. These are a few millimeters thick. We have glued them to PVC pipe which I'll show in a moment, put a little bit of head on this system and observed how much time it takes for the fractures in these little rock disks to saturate as well as the matrix.

We'll talk a little bit more about

chlorine-36 in a moment.

(Slide change.)

Next slide, 16, these are some of the cores that we made. One core actually I should say that we made before we could make the rock disks, but it shows and we cored this with water, but it shows -- look at the upper left hand one. It shows the wet area in the middle there is a fracture. Of course, all the excess surface water has drained off and dried off, but the fractures appear to suck up the water rather than the matrix as DOE would have you believe. The lower right hand corner shows core that was taken from the upper rock sample. And you can see very well that the fractures are saturated with water and the matrix is essentially dry.

The time and duration to make this core was on the order of an hour or so, but it obviously shows that fractures can take water and can flow in fractures without saturating.

(Slide change.)

Next slide shows the disk experiments. The far right picture shows a disk glued to PVC pipe, but some of these disks have fractures in them. These fractures can wet up in a matter of hours or less. Matrix can take a lot longer to saturate and this one implication from this is that -- well, the DOE time step in their modeling and some of their -- are the only ones that we could really pin down were on the order of a hundred years and if water is flowing in fractures in the order of hours or so, obviously the DOE time step is too long for modeling.

(Slide change.)

Next slide, sorption assumption number 8 is a big one. It states that sorption experiments conducted under saturated conditions are applicable to the unsaturated zone. This is just not correct as far as we can see for a number of reasons, one being that it would have different water compositions in the unsaturated zone versus the saturated zone and the fact that the rock just -- there's a difference in the saturation state between the two zones. So it's difficult to see how this assumption could be confirmed.

(Slide change.)

Number 9, next slide, number 19, assumes that the characteristic water compositions of J-13 in the p#1 decarbonate water and affluent sorption can be adequately represented by simulated solutions in the laboratory.

The solution that was simulated was the

UE-25-p#1, a decarbonate. It was simulated as a sodium carbonate solution or sodium bicarbonate solution. I think we saw yesterday from Jim Davis that if we're not using the full natural water chemistry, you don't get the right sorption values. And another point I made there was that the groundwater should not be used for sorption experiments in the vadose zone. And even if you look at the groundwater chemistry that's been accumulated so far, you see it actually is outside the bounds of J-13 and p#1 waters.

Last assumption involves the assumption by DOE that decrease in radionuclide concentrations in their experiments is due entirely to sorption and not to anything else such as precipitation of phases or formation of colloids. This certainly needs to be verified. Probably best by electron beam analysis that looks at the complexes can be formed on the surfaces of minerals and possibly -- they have done some autoradiography experiments. It's not clear if these clots of fission tracks that they find are actual minerals or solid phases or just complexes that are on the surfaces of the sample.

My conclusions. There are numerous chemical complexes of neptunium and certainly other radionuclides of interest which may not have been considered by DOE in their models, especially in both saturated and unsaturated, really. DOE needs to reconcile nonsorbing radionuclide transport in the UC between the chlorine-36 and technetium-99. This is what we can see as a major issue here, why there is such a discrepancy in travel time.

It's clear, we believe, that saturated zone sorption is better understood than unsaturated zone sorption, but there's still a lot of questions and one of these involves location of volcanic alluvium contact, uncertainties in sorption experiments and especially for alluvium, the proportion of drain sizes in the most porous and permeable pathways.

Certainly, all of the sorption assumptions that were made several years ago, still require confirmation by the DOE.

Lastly, we have the problem of colloids. There's data on the NTS test site that shows plutonium colloids can travel some distance in a fairly short time. This needs to be better incorporated, considered in models. Thank you.

MEMBER HORNBERGER: Thanks very much, Don. Don has clearly done a good job to point out a lot of interesting and important issues and as we proceed to questions for Don, I just want to caution everyone that Don is not the right person to ask for responses in terms of what DOE has done. And also, if we get to the point of asking representatives from DOE to try to answer all of the questions that Don has posed, we may be here for a long time indeed. So I would urge people to try to concentrate their questions on questions for Don.

Dick?

DR. PARIZEK: I'll pass for a moment.

MEMBER HORNBERGER: Jim Davis?

DR. DAVIS: Some of the assumptions that you are questioning, for example, biological activity redox conditions, solubilities. In some cases, they would seem, that by making the interpretation or assumption that DOE has made that in fact, they've made a conservative assumption whereas if they follow through with some of the questions, they, in fact, show more retardation. Do you have a problem -- do these things need to be confirmed, even if the result of confirming them results in more retardation?

DR. SHETTEL: I don't believe that's the situation. If you look at the experiments where they assume just a decrease in concentration and that's sorption, if in fact, that that is a formation of colloids or another solid phase of the radionuclide, that experiment will give you a false sorption value.

DR. DAVIS: I think there's some other cases though, for example, with respect to redox. If they're looking at the mobility of -- of the transport of the most oxidized form, and that's the more mobile form, and they're assuming that the reduction to say, for example, in the case of uranium, they're not worried about the formation of uranium-4. Well, that's a conservative assumption and we need to worry about whether uranium gets reduced to uranium-4 or not.

DR. SHETTEL: Well, if we take a hypothetical situation, if we consider a sample from alluvium, there is a little bit of organic matter in alluvium and say that the sample that they used for their experiment contained a little bit of organic matter in it. Most of the rock is going to be under oxidizing conditions, that little grain of organic matter may be reduced condition, so in effect, what you're seeing is a mixed sorption coefficient. It may be valid for that bulk rock sample, but does it really tell you about the environment?

It's going to cause -- it could certainly cause additional scatter in the results and make interpretation of the data more difficult.

DR. DAVIS: So if I can understand what you're saying, you're saying in their batch experiments, if there had been some reduction, then that would overestimate the K_d, is that what you're saying?

DR. SHETTEL: Yes.

DR. DAVIS: Okay.

DR. SHETTEL: That's one interpretation.

MEMBER HORNBERGER: Jim Clarke.

DR. CLARKE: I'm kind of inclined to pass as well for the concerns that you mentioned, but I guess one question and I don't know if this is something you can answer now or just would get into a kind of discussion that George doesn't want to get into for obvious reasons, but are there -- you have 10 assumptions that you are concerned about that you challenge. Are there -- is there any priority here? Are there certain ones that you think are particularly important from the standpoint of impact on dose with the compliance part?

DR. SHETTEL: Yes, I believe I could prioritize all of them, but I don't think we have enough time right now. I think the most important one is I believe number 7 where they assume that their saturated zone experiments are applicable to the unsaturated zones. That's probably -- no, excuse me, that's not the one.

Number 8. It says saturated zone experiments apply to the vadose zone. That is blatantly incorrect because the water composition in the vadose zone are different than the saturated zone, plus you have the problem of the different saturation states of the sample. And the fact that really, in static batch experiments, you're dealing with matrix diffusion and sorption in the matrix versus the main transport mechanism in the vadose zone is fracture flow. So there are major discrepancies between unsaturated/saturated zone and the application of results from one to the other.

DR. CLARKE: Thank you.

MEMBER HORNBERGER: Ines?

DR. TRIAY: Yes. In the bullet where you said that the sorption data needed confirmation, what did you have in mind?

DR. SHETTEL: That's not my job. This is your data, most of it, I believe. DOE should be the ones that confirm these assumptions.

MEMBER HORNBERGER: Allen?

DR. CROFF: I'll pass.

MEMBER HORNBERGER: Michael?

ACTING CHAIRMAN RYAN: Just a follow-up on Jim's question on the ranking idea and in answering Jim Clarke's question you talked about the fact that the assumptions were incorrect from a technical point of view based on the science, but is it also highest ranked in your mind because it has the biggest impact on dose, or have you all looked at that kind of impact? Or are you judging each assumption intrinsically?

DR. SHETTEL: We can only judge these in a fairly qualitative manner because we're not involved in doing any kind of TSPA or PA modeling, but obviously, I think number one is probably the least important. Number 8 is most important and I could rank the other ones in between there if you're interested.

ACTING CHAIRMAN RYAN: Having your insights on it would be helpful, but I just wanted to clarify it wasn't on an ultimate dose kind of calculation basis, but more on the intrinsic science of each assumption.

DR. SHETTEL: Oh no. I think number 8 could have a major impact on ultimate dose.

ACTING CHAIRMAN RYAN: Okay.

DR. SHETTEL: So it's not just minor tweaking of the experiments or whatever. I think some of these assumptions can have major impact on performance assessment.

ACTING CHAIRMAN RYAN: Okay, thanks. I appreciate it.

MEMBER HORNBERGER: Ruth?

MEMBER WEINER: Don, on your slide 4, at the bottom you say "DOE's solubility models may be realistic, but are not conservative."

What would you consider conservative and what's wrong with realism?

DR. SHETTEL: There's nothing wrong with realism. I think in a general sense, DOE is always saying that they make conservative assumptions, but I'm just pointing out a case here where they initially made a conservative assumption and they jumped down to something that may be more realistic, but still probably needs to be confirmed because these are based on spent fuel dissolution experiments. And I can't remember looking at these papers recently, but there may be some problems in the experiments in comparing them from the laboratory experiments to the actual Yucca Mountain environment.

MEMBER HORNBERGER: Don, again just to make sure I understood your answer to Mike Ryan's question, if -- I mean and perhaps Ines' question as well. So when you say some of these things need confirmation, if in fact, the Department of Energy did an analysis to suggest that some of these assumptions would not be important in a performance assessment, would that then negate the need to do confirmatory testing in terms of either laboratory or field measurement?

DR. SHETTEL: I suppose that's a fair approximation. But I also believe some of these cannot be confirmed because of the conceptual problems that are involved.

MEMBER HORNBERGER: Yes. I grant that it's a hypothetical. I didn't mean to prejudge that they could do so, but if they could do so, then your answer would be that that would be fine.

Okay, well, thanks very much, Don. Oh Neil?

MR. COLEMAN: I just wanted to make one more realism comment. Several times in this meeting the Benham nuclear test has been mentioned. Colloidal transport plutonium has been reported over a distance of about a kilometer, but that finding, I believe, has very limited application to the study of natural systems. This was a -- if memory serves, a 1.3 megaton blast below the water table, generating a massive pressure wave and creating prompt injection and local hydrothermal circulation for many months. And we can only imagine the process of colloid formation at the point of a nuclear detonation. This is not a natural process. And I would propose colloid studies done under natural flow conditions are much more appropriate for understanding colloid transport at Yucca Mountain or any other site. I just wanted to make that comment.

MEMBER HORNBERGER: Thanks, Neil.

DR. SHETTEL: If I can respond to that, briefly?

MEMBER HORNBERGER: Yes.

DR. SHETTEL: The fact that there was some hydro thermal activity, that could be equated in some sense to the repository near-field environment where you have high temperatures. The pressure wave, I believe it's been shown that the plutonium colloids are beyond the shock wave effect, so it's not a result of the explosion.

MEMBER HORNBERGER: Okay, thank you very much.

DR. SHETTEL: In any event, it's an analog, possible analog of Yucca Mountain.

MEMBER HORNBERGER: Thanks very much, Don. We now have three presenters listed for Nye County and according to my schedule Les Bradshaw is going to go first.

MR. BRADSHAW: No.

MEMBER HORNBERGER: We have a no there. Who is going to go first?

Tom Buqo.

MR. COLEMAN: Let me just mention while they're setting up there that the EPRI talk, which is the last one in the sequence, has been e-mailed to both DOE and to San Antonio, so it is available there.

MEMBER HORNBERGER: Tom?

MR. BUQO: Thanks for the opportunity to make this presentation. This is a presentation I gave earlier this month at Devils Hole Workshop, so Dr. Parisek and Mr. Duncan, if you bear with me while I go through this material. You've already seen it.

(Slide change.)

The second slide is an overview of what I'd like to go over. Nye County has been doing groundwater level evaluations for some period of time now. Over the course of the last year we have expanded our water level monitoring program, especially in Pahrump Valley and Amargosa Desert. We've done some numerical modeling looking at groundwater mounds associated with paleo climates and we've been zeroing on groundwater conditions in the Ash Meadows are, particularly with respect to the depth of groundwater and water level trends.

Why do we bother? We're looking at baselining what the current conditions are and looking at the impacts of our development of groundwater in both Amargosa Desert and in Pahrump Valley because we're concerned that the FEPS process is not looking at future groundwater withdrawals. And I'll be discussing a little later, there's a lot of competition for the available resources going on at this point in time. There's battles over water rights, right to go in and develop on a very large scale. And we are concerned that there's enough effort put into evaluating the impacts of the repository on future groundwater withdrawals and perhaps most importantly vice versa. What are the impacts of those future groundwater withdrawals going to be on the performance of the repository?

(Slide change.)

The next slide is a map that was presented a year ago at Devils Hole. This map was prepared so that we could validate a USGS regional potentiometric map that was put out. We used the water level data. This is a baseline for the winter of 1999 to 2000. We compiled all the available data, our own water level measurements with those from the U.S. Geological Survey, the various DOE groups, both the test site operations and the Yucca Mountain site. We supplemented that in data gap areas with older data. We used spring data in some areas because we had to. There was no other available data. And then in the departure from the USGS approach, we use control points. One of the problems we had in validating the USGS map was that they used an algorithm in a computer program that could not be reproduced if you didn't happen to own that proprietary package. So we used control points so that others can go in and say here's exactly how it was done at this point.

The next slide shows how -- in the previous slide it showed the map area with the potentiometric contours. The area shown in here is the overall data set we used. It went way beyond what is shown on the map itself. We did that because we wanted to eliminate the edge effects associated with the contouring.

Again, we had to use spring elevations in some areas as they represent the only data that's available, particularly in the Panamint Range and portions of Spring Mountains. There's no groundwater level data available.

Somebody asked last year if I didn't think those springs were perched and since that -- in making this map, if a spring had been identified as perched, it was censored from the data set. However, in subsequent work and looking at springs, many of these may be perched. Some of them undoubtedly are semi-perched and some of them are unperched and it would be a massive undertaking to go in and try to figure out which is which. Nonetheless, I think for general potentiometric map development it's suitable to use them.

(Slide change.)

I'd like to talk a little bit on the next slide about what constitutes a data because a lot of Nye County studies are focused on identifying what data gaps are important to the county and how we go about filling them in.

Well, everybody has their own definition of what a data gap is. It's based upon their own interest, the issues that they have, the responsibilities as the data collection agency and their concerns, their scientific interests. Of course, gaps are defining the distribution and the variability of the data. There's the regulatory authority defines gaps in terms of what information do they need for making better informed decisions.

There's environmental concerns. We run into that a lot in Nye County and gaps in understanding cause and effect. We spend a lot of time defending ourselves over the existing water withdrawals that are going on and the potential impacts of those.

(Slide change.)

Or in some cases, your idea of a data gap depends on who you work for. I work for Nye County. So in the next slide, it points out that my interest, issues, responsibilities and concerns focus on southern Nye County, so please limit your questions and issues to this little part of my world, thanks.

Next slide.

(Slide change.)

There are some rather large areas in southern Nye County that are devoid of any groundwater information. In other areas, such as Yucca Mountain and the Ash Meadows area, we on the surface seem to have rather large data sets. So how do we go about filling in the gaps and where are these gaps?

(Slide change.)

The next slide shows that we really can't collect data on a grid. It's an impossible task, so therefore we have to prioritize where these gaps are.

(Slide change.)

The next slide shows Nye County's principal areas of concern. Up at the upper right hand portion of the map we show weights, disposal and weapons testing. Those are concerns up there; Yucca Mountain, of course, a Nevada test site. Through the southwest, the green area is the environmental concerns. What impacts will Nye County pumping in the Amargosa Desert and particularly in the Amargosa Farms area have on the environmental and sensitive areas at Ash Meadows and on the springs to Death Valley.

In the lower right hand corner we have water supply issues. Pahrump is a growing community. It's about 35,000 people now. We project the full build out of 150,000 people with a corresponding demand of 80,000 acre feet a year. We have a water supply problem at Pahrump and the county has instituted a resource stewardship program recently to address that water shortfall and to protect the future resources in the county from development by others.

The little gap shown in between the black and the blue is an area of real concern with respect to resource competition. Last month, there were water right filings, massive water right filings on behalf of a private corporation and just a little bit to the east by the Southern Nevada Water Authority that would appropriate several tens of thousands of acre feet of water from this area. Nye County has previous water right applications in the same area and is quite concerned about the competition for the resources.

I can't discuss all these things, so the next slide shows two areas I want to concentrate on today. One is the Pahrump gaps. In addition to basic water level information, we also have significant gaps in our understanding of recharge. We have almost no water quality information. If you go to the NWIS, the USGS database, they show two water quality analyses for Pahrump. To address this need, the Southern Nye County Conservation District has provided funding to go in and do some comprehensive water chemistry sampling in Pahrump for the first time.

We've got a lot of uncertainty with the amount of underflow. It's the old geophysical joke, what do you want it to be? We don't have enough information to make valid estimates of how much groundwater underflows through Pahrump Valley. We have a gap in the cause and effect. Everybody blames us for every effect, but we still do not see, based upon the data that's available a true cause and effect relationship.

At Pahrump, we also have a problem with separation of the upper and lower aquifers. We see somewhat deeper trends in the deeper part of the upper system, but we don't have any deep system information at all. In the Amargosa area outlined in green, we also have some significant gaps with respect to the amount of groundwater discharge that is going on, the depth to water, the water level trends and again, the cause and effect relationships.

(Slide change.)

The next slide shows in Pahrump Valley from 1999 to 2003, the blue circles represent the wells that Nye County was monitoring at that time. Over the last year, 62 additional wells had been added, so we now have pretty comprehensive coverage at Pahrump. We added the Utilities, Inc. wells which is the major water purveyor in Pahrump. We've added deeper agricultural wells and we've gone in and added wells in all of the data gap areas, particularly in areas with high domestic well and septic system densities. We've got some 30 sections of land in Pahrump that have over 200 individual septic systems in them. So it is a concern for us to go out there and characterize both the water level trends and the chemistry.

To date, measurements have been taken in 170 wells in Pahrump Valley.

(Slide change.)

Next slide shows -- well, that's actually too many. It's quite a burden Nye County to go out and collect data on that kind of scale. We believe that we now have an adequate distribution of existing wells for monitoring the effects of water withdrawals within the developed portion of the basin. I stress within the developed portion because as you see from the map we have a very high density data within that developed area, but once you get outside that developed area, the data is very sparse.

So we're looking at developing transects to reduce this monitoring burden and I show it conceptually here, these transects that work is going on right now. Again, the data is very sparse beyond this area. The water quality data is severely limited. Recharge estimates need refinement. The discharge estimates need refinement and the underflow fluxes are poorly understood, but yet we sit down and we do models and state with certainty or uncertainty how the system is responding and we really don't have some of this fundamental information.

The next slide is similar for the Amargosa Valley area. Again, the blue dots are -- represent water level areas where water level measurements are taken by Nye County. There's a lot of other monitoring that's going on out there. The U.S. Geological Survey, Fish and Wildlife Service and Park Service are also doing monitoring. Over the last year we've increased our monitoring in the Amargosa Farms Area and in between the Farms Area and Ash Meadows.

We've also done some work to go out and look at where these springs actually are. We did a compilation of all the springs that are on the 1 to 24,000 topographic coverage and our field guy that does the water level measurement tries to get out to a couple of springs every month to see if they're still flowing and to further document those springs. Hopefully, at some point in time we will be able to consider doing some actual monitoring on those.

Again, once you get beyond these developed areas, the data is very sparse. Our discharge estimates need refinement and the perennial yield is not well defined, but it is very important.

I'd like to concentrate on those discharge estimates. There were a couple of studies done recently. One was for all of eastern Nevada. I think they looked at 17 bases in eastern and central Nevada and then we had some USGS studies done in the Ash Meadows area and other discharge locations in the Death Valley Regional Flow System. They came up with quite different results. Both of them relied on imagery analysis and ground Et measurements. The results in eastern Nevada found that Et was double the reconnaissance report series values and hence, recharge was also double. The results in Ash Meadows in contrast, found that Et was only 18,000 to 21,000 acre feet per year which was only slightly higher than previous estimates based on spring discharge. So I've always been scratching my head saying what is the big difference? And what I've been told is that well, it's because the other ones were in the northern part of the state and so they had more Et. Well, we have a map on the right from the state map, state report on potential evapotranspiration rates and it doesn't wash with that because that shows that the southern part of the state has much higher PEt rates and we would expect to see higher Et rates down here, rather than lower. So one must question were the original reconnaissance estimates more accurate in the south.

(Slide change.)

Well, we'll get into that a little bit more on the next slide. Both methods relied upon remote sensing approach and used satellite imagery to define the extent of the areas of evapotranspiration. I've got a background in that. I used to work down in Waterways Experiment Station and I did a lot of work with remote sensing down there, so I was able to follow the work that these folks did and I'll summarize that.

(Slide change.)

The next slide is for the recent estimates of evapotranspiration in eastern Nevada, those 17 bases that I talked about. And what you see is the worker here at Nichols went in and defined the areas where he thought that evapotranspiration should be occurring and classified everything within that area. So all land within each Et area was classified and Et was estimated on the basis of plant cover with correlations to depth to groundwater and annual groundwater Et as is shown on the next graph.

Next slide, please.

(Slide change.)

Now historically what we find in the literature is that Et is greater than one foot per year from areas with groundwater depths of 10 feet or less. This is consistent with previous observations. One question is how low can it go? If it's one foot at a depth of 10 feet or less, what is it at a depth of 15 feet or 20 feet?

Well, we've got the Amargosa Research Station up near Beatty and they've been doing a lot of work looking at profiling of chloride ions and also various radioisotopes and they are suggesting now that the groundwater has an upward flux from depths as much as 100 meters. It's negligible. They say at that depth, but nonetheless there is a positive upward flux.

Now we don't know at 50 feet what that flux is or at 25 feet, but it does suggest that there could be an appreciable quantity of water being lost.

(Slide change.)

Well, let's go on to the USGS work in the next slide. And this is the Ash meadows area and what was interesting and I found out why the difference in the values. When -- in this area, the USGS and I think this best exemplified in the little figure down at the lower left which is for Chicago Valley. The dotted line represents the area of groundwater discharge, yet when they did their classification, they only classified about 25 to 30 percent of the area within that discharge area. So all land within each Et area was not classified. They used a similar method of estimating Et on the basis of plant assemblages and densities.

I took a different approach based on our water level data and that's shown in the next slide. I went in for this 560 square mile area and I took our potentiometric surface. It agrees with the previous map by the USGS and I subtracted that from the digital elevation model of the USGS to come up with a depth to water. Depth to water is a very hard thing to contour, so you have to go about it in -- I found that this was the best method. I've looked at just contouring the depth data by itself and found that that was a meaningless exercise because it didn't account for the topography.

But what this shows is that based on a 2,000 baseline data added to that the data that the USGS collected in their Et study of Ash Meadows, I found an area of 58,000 acres where the depth to groundwater would be 10 feet or less and another

45,000 acres where it would be 10 to 50 feet to groundwater.

(Slide change.)

Going on to the next slide to show the significance of that, there's 58,000 acres where the groundwater is 10 feet or less in depth. Of that area, the USGS only classified 12,500 acres, meaning there's some 46,000 acres out there, according to this estimate that were not classified.

I don't put a rate on that because I don't know what the rate is for bare soil in this area. If I were to apply the numbers Nichols used up in northern Nevada which was .4 acre feet per year, that would come out to 19,000 acre feet and it would essentially double the discharge in Ash Meadows, just like the numbers in northern Nevada.

(Slide change.)

In the lower right I say but wait and the reason I say that in the next slide, even though the Ash Meadows area has been extensively investigated and there's long term records available for many of the wells and springs, there is still not, in my opinion enough data to accurately define the depth to groundwater and as I show in the map on the right, the area where I have showing depth to groundwater of less than 10 feet, a lot of it is in data gaps. We really don't know what the depth to groundwater is.

But our fundamental estimates of recharge and perennial yield rely on this knowledge. When we're using groundwater models and we're calibrating to discharge, we should have a good idea of how much discharge is going on.

We hope that the geophysicists can improve our knowledge of this and we can use resistivity or another method to go out there and see if we can't better define what the depth to groundwater is.

(Slide change.)

The last slide, our plans for the future with respect to groundwater evaluation, the evaluation of the upper and deeper water level trends in Pahrump Valley. Nye County has a cooperative proposal with the USGS to do some deep drilling in Pahrump. We've been hocking that to anybody that will listen for the last three years, but we haven't found any takers yet. We're hoping that will change.

We are in the process now of selecting the transects for monitoring in Pahrump and in accordance with our QA procedures for that. We have to inspect each individual hydrograph for each well and throw out wells that are in duplicate areas, maintaining deep and shallow in the same areas.

We're going to continue evaluation of the effects of groundwater balance on water withdrawals. It was interesting listening to the discussions yesterday on the improved calibration of groundwater flow model made by using a few selective interpretations. Some of our preliminary findings are that they also should be incorporating in paleoclimate effects because they have pronounced impact on water levels in the region today.

We're going to continue doing our water level monitoring in Pahrump, Amargosa Desert and the Stewart Valley, Chicago Valley and points beyond as budget and personnel, time allows. We'll continue doing the spring verification and as I mentioned, we'd like to do some spring monitoring in the future. It's nice to go out there and prove that the spring is still there and that gives us some information that is useful. But what we really need to know is is the discharge of that spring going up, down or staying constant.

That's it, folks.

MEMBER HORNBERGER: Thanks very much, Tom. Very interesting presentation.

What I'm going to suggest is that the

panel members all jot down their questions and let's ask Nye County people as a group. We can get all of the questions from Nye County at once.

So if we could proceed to the next Nye County presenter.

DR. HAMMERMEISTER: I'll get started.

MEMBER HORNBERGER: Wait a second. You must not have a microphone on. We're not hearing.

DR. HAMMERMEISTER: Try this one?

MEMBER HORNBERGER: That works.

DR. HAMMERMEISTER: Is that better?

MEMBER HORNBERGER: Yes, thank you.

MR. COLEMAN: Also, the camera on your end is still pointed at Tom.

(Pause.)

DR. HAMMERMEISTER: Okay. Are we ready?

MEMBER HORNBERGER: Please proceed.

DR. HAMMERMEISTER: Okay, prior to the study of geologic samples of Fortymile Wash alluvium that had representative particle size distributions had never been collected, at least in Nye County's opinion. And this coring program Nye County believes provides the first geologic examples that have representative particle size distributions. It's the first accurate picture of layering, textural laying in the subsurface.

This has been funded by the Department of Energy which we appreciate. However, the Department of Energy has directed or in any way influenced the actual study.

Next slide, please.

(Slide change.)

I'd like to quickly just emphasize some key points as I give my talk. I'd like to go over some of the field and laboratory methods, describe significant results and talk a little bit about future work.

At the end I'd like our senior geologist, Jamie Walker to talk about a little different subject and that is some growth faults in alluvium and in underlying bedrock in the south of the repository that may influence flow paths.

Next slide, plese.

(Slide change.)

The points I do want to emphasize from this talk is that we had cored nearly 300 feet of continuous summit core from the upper portion of alluvial aquifer. We've logged it and we've tested some of the core. The core recovery was exceedingly good samples are minimally disturbed. I should emphasize that the porosity and density are disturbed and we'll show examples of that. However, the particle size distribution, we believe, are representative.

Preliminary field and laboratory testing results have begun to fill some important data gaps.

Next slide, please.

(Slide change.)

Our location of the study was along Highway 95 at the alluvial testing complex showing the slide encircled. It's south of the repository of course, and it's north of the Nye County residents that live in Amargosa Valley. It was located at that location to potentially help interpret future cross-hole tracer tests at the alluvium testing complex.

Next slide.

(Slide change.)

The coring method was used with the sonic coring method of vibrations, the method of Brooks of imparting a vibration into the drill strain which in turn causes the sediments to start vibrating and become slight fluid and if you put, apply positive, downward pressure on the drill strain and you rotate it, the sediments move up into the drill strain. After the drill strain is full or the actual lower part of the drill strain which we call the core barrel is full of sediments, it's pulled to the ground surface.

Next slide.

(Slide change.)

And it's removed at the ground surface. It's actually extruded from the core barrel. We see a portion of the core barrel here and the slide, and it's simply vibrated out of the core barrel into a plastic tube or a plastic sock.

Next slide.

(Slide change.)

The core is then -- I'm sorry, I jumped ahead of myself.

We did not core the vadose zone, the unsaturated zone. This was a demonstration project. There was limited amounts of funds. We drilled that rapidly and encased it off. The upper roughly 160 feet shown in green was cored with one size core barrel roughly 6-inch diameter core barrel. And the lower, approximately 100 feet, and showed in orange. It was cored with a smaller diameter of 4.5 inch core barrel.

Next slide, please.

(Slide change.)

The core, once the core is brought to the ground surface, it's moved into a core trailer at the site and for geologically logging and subsampling.

Next slide, please.

(Slide change.)

The logging methods that we used were basically ASTM visual manual logging methods and some of the parameters that we actually measured, we actually logged and described are shown in this impossible to read slide. They include particle size, estimates of gravel, sand, silt and clay, cementation, plasticity and so on.

Next slide, plese.

(Slide change.)

The particle size distributions in the core remain more or less intact and so in many cases we're actually able to see, visually see the layer in the actual core.

Some recent examples here, the uppermost slide shows a transition from cobbles and coarser gravel to finer gravel, about two thirds the way along, going from left to right. You can obviously see a transition in the particle size. Another transition of particle size, the middle core is showing from a finer textured material to a less -- to one that contains less fines and finally the lower slide shows an actual cobble that was drilled through, actually cored through the cobble.

Notice that the material is fluffed up. It's not -- density and porosity has been disturbed and I want to emphasize again, we believe these are representative particle size distributions. We have not messed up and screwed up the particle size distributions.

Next slide, plese.

(Slide change.)

Some additional examples of disturbance, we have to be honest here. The fines in the upper core shown, the fines tend to migrate to the outside of the core. This complicates subsampling we found. And so -- and also it probably defines also probably migrate to the walls of the bore hole, the actual bore hole formation walls and it can cause some problems with sampling and testing in the actual bore hole itself.

Heat generating in the lower slide shows an example of the effects of heat that it is -- heat is generated during the coring process and causes water to accumulate in the top of the core, shown in the far left, a darker color and tends to dry out the core slightly and the lower region of the core shown in the more oxidized colors in the lower, in the mid section of the core.

Next slide, plese.

(Slide change.)

A whole suite of geophysical logs were conducted. However, these are probably the two most important logs because as I mentioned, the density and porosity of the core is disturbed. These are nuclear logs that actually blast the radiation out in the formation. They attempt to see into the formation and the epithermal neutron and gamma-gamma density logs shown here basically trend together which they should. They're expected to and there's a fair amount of character to these logs indicating, suggesting a bunch of things. There's a bunch of casing in the holes that sort of complicate the interpretation, but there are some potential changes. The character of these logs indicates some potential changes with depth in the alluvium.

Next slide, please.

(Slide change.)

Once the logging was completed, it was finished, the hole was complete with a dual piezometer for groundwater chemistry monitoring and also groundwater level monitoring, future groundwater level monitoring.

Next slide, please.

(Slide change.)

A quick overview of some field measurements. We weighed each core segment. We got the wet mass of each core segment in a particular core run. We used this to support calculations of the overall density, dry bulk density of the core run and we sort of backed into the dry bulk density of the core run by subtracting off with laboratory measurements of water content. We got the dry mass of each of these core segments by subtracting off the water content and we assumed a volume of measurements equal to the -- defined by the outside diameter of the core barrel and the length of the actual core run.

Next slide, please.

(Slide change.)

Probably some of the most important laboratory tests. This is sort of a PR slide as much as anything. Nye County now has its own laboratory testing area. Probably one of the most interesting and most important is particle size distribution. And here we have wet sieve and hydrometer test. Wet sieve addresses the coarser fractions and the hydrometer tests looks at the finer fractions.

Next slide, please.

(Slide change.)

Since these samples were disturbed, we really can't run a hydraulic conductivity test on them. We have to reconstitute the samples. We did select core intervals, 15 different core intervals that were representative, that had representative particle size distributions that covered the range of particle size distributions encountered in the core hole. We repacked these 15 samples into a 12-inch long by 6-inch diameter flow tubes that we put into flow cells. These were repacked in lifts and these particular 15 samples were repacked dry in an attempt to approach and I emphasize approach in situ dry bulk densities.

Next slide.

(Slide change.)

This -- we did conduct constant-head conductivity tests. On the left is showing the repacked core, just shown in the previous slide with the actual flow cell caps on them and the actual constant-head conductivity tests being run.

We also conducted hydro conductivity tests on 10 drive core samples, smaller core samples in brass liners. We collected, I believe, five, two and a half foot drive core cores throughout the entire core interval and the bore hole. We did this because this is the only previous method that we had used to obtain core. It's a very expensive and very time consuming method, but we wanted some data to -- we had some previous drive core data and we wanted some additional drive core data from this particular hole.

Next slide, please.

(Slide change.)

Field hydroconductivity tests. There were two types. On the left it shows a constant-head injection test into the completed bore hole that again has two screens, dual piezometer screens. This is a U.S. Bureau of Reclamation method. Basically, just pumped the water out and put it in a tank and then ejected it back in and I believe on constant head.

On the right it's showing a larger scaled pump test conducted in individual isolated screens in a nearby well, located 50 to 60 feet away. Each of the screens were pumped for 48 hours and this is an actual pump test and we were able to get a large scale hydro conductivity value.

Next slide, please.

(Slide change.)

I'd like to turn quickly to the results, geologic results, logging results suggests that there's little evidence of buried soils. This has been an issue brought up that various soils might tend to limit flow. The data doesn't suggest this. There's little cementation. Little or no cementation. In addition, log results show that the colors indicate primarily oxidizing conditions and finally the coarse fractions are volcanic and they've been weathered, some angular, some rounded and clearly it's alluvial material. Next slide, please.

This slide, next slide shows just the cementation data from our geologic logging and note that there's little to no cementation. These are the depth profiles, again on cementation. And HCL reaction is shown on the right and there's very few carbonate layers in the system.

Next slide, please.

(Slide change.)

I include this slide because it's kind of pretty. It's the gravel fraction showing and indeed these are slightly rounded alluvial material.

Next slide, plese.

(Slide change.)

We determine density by a bunch of field methods and laboratory methods. I've already sort of very quickly mentioned one, one of our calculation methods and also mentioned geophysical logging. In general, they were fairly consistent. The actual calculated porosities that we were able to calculate were generally in the upper range of values used by the U.S. Department of Energy. That is, roughly 25 to 31 percent porosity. This corresponds to bulk densities of -- ranging from roughly 1.7 to 2.1 grams per cubic centimeter or 2.0 grams per cubic centimeter.

Next slide, please.

(Slide change.)

This slide, the next slide does show the depth profile of these different density measurements. The blue squares have a tremendous amount of data. It's actual gamma-gamma density logging data. We believe that it over-estimates hydraulic conductivity slightly. The red Xs shown there that are difficult to see are actually the calculated values that we calculated the densities for each core run. We believe that they slightly under estimate densities.

Next slide, please.

(Slide change.)

Probably one of the most interesting and probably one of the most useful results of the particle size distribution data that we collected, the laboratory particle size distribution data was the comparison of sonic depth profiles of particle size distributions from sonic core with profiles in adjacent hole of drill cuttings that have collected using air rotary versus circulation methods.

It's assumed here in this comparison that the sonic core does not disturb, the coring process does not disturb the particle size distribution. So it's sort of a standard by which to compare samples that have been collected by other methods.

The bottom line is that this comparison shows that the drill cuttings are highly disturbed. Basically, they're ground up. The larger gravels are ground into sand and ground into finer material and in addition to that, some of the natural finer material is washed away during the actual drilling process.

(Slide change.)

The next slide shows a result, a comparison of the plots. It's a pretty complicated, messy thing, but the blue in the background are drill cuttings data from again from a hole that's about 60 or 70 feet away from the sonic core hole. The pink and the red are sonic core hole data. The area to the left of the curves, the white are to the left of the curves corresponds to fines, the percentage of fines, the area between the two, for example, the two blue curves corresponds to the amount of sand and the area to the right of the blue curve and the red curve corresponds to the amount of gravel.

This clearly shows that the amount of gravel has been reduced in the drill cuttings and increased amount of fines and also a very large increase in the amount of sand. And I think this is obviously a fairly significant finding. Folks that have used drill cuttings for adsorption tests should at least take this into account.

Next slide, please.

(Slide change.)

We've also used particle size distribution data to delineate layers, textural layers, unified in USCS stands for Unified Silt Classification System, textural layers in the actual core.

This classification system that we apply to our core showed that they're mostly gravels and sands with fines in the upper 100 feet of the core hole and that's the upper hundred feet of the saturated zone. The fines classified primarily as clays, however, some preliminary -- we hadn't finished our Atterberg Limit tests yet and the preliminary Atterberg Limit data suggests that these are actually, if we use Atterberg Limit data that the fines actually are primarily silts.

Again, this classification I'm going to show you here and on the next slide are based purely on particle size and category limit data is not included.

Poorly graded layers predominate and in the lower 100 feet of the hole, they're primarily clay, sands are found.

(Slide change.)

The next slide shows the graphic of the Unified Silt Classification System layers and we've used these to -- we've basically just built our lithostratic or lithographic logs. On the left we use all 13 of the Unified Silt Classification System groups that were encountered in the core hole. And notice that it's a highly layered system if we show all those layers.

The blue layers correspond to the coarser texture. Dark blue is the coarsest textured layers which are, of course, the most permeable. We're talking about gravels and sands. The reds and oranges correspond to the finer textured layers that contain more silt and more clay and these, of course, are less permeable.

So you can see on the left diagram that you transition from the upper portion of the core where it's more coarse grained and you get down in the lower portion of the core hole, it's finer grained. The middle diagram is a simplification of the left hand diagram. In this case, USCS groups with similar particle size distributions were grouped together and we have five groups there that are shown and their textural range is also the percent fines and these five groups are shown in the legend which is impossible to see from where I am.

And in the far right hand side we've grouped similar USCS groups into two larger groups. And we've simplified the diagram even further. Because particle size distribution or texture is related to permeability, these lithostratic graphic logs are perhaps a first step towards identifying hydrostatic graphic units. Clearly, more work has to be done in that area.

Next slide, please.

(Slide change.)

This slide I'll actually skip over, really quickly. This shows the difficulty of sub-sampling core that doesn't have a large volume and we have large cobbles in a large coarse fraction. It's very difficult to sub-sample and to split and to get representative samples in individual splits and, of course, we had multiple uses for this core. And all I'm saying is that the line of this graph should be a 45 degree line and it's obviously not. We have some work to do in this area.

DR. HAMMERMEISTER: Next slide, please. Briefly, I'd like to turn to the hydraulic conductivity testing program, and also there's some transport parameter testing program. In the 15 repacked core samples that I've talked about previously, we're working cooperatively with Los Alamos National Laboratory on th is. Nye County has and continues to conduct the hydraulic conductivity tests on these repacked core samples. Again, they're repacked to attempt to approach in situ densities. Once we've completed these tests, Los Alamos will conduct transport parameter measurements on these tests.

Some of the results of the samples that have been repacked to about 1.7, an average density of 1.72 grams per cubic centimeter. The values are relatively high. They range from 17 to .6 feet per day depending upon the particle size distribution in the repacked core. And as many other workers have found, the hydraulic conductivity of the sample decreases with increasing finds.

This observation is shown in the next slide. We plot finds on the X axis and the log of the hydraulic conductivity on the vertical axis. The triangles are the 13 individual cores, and the squares are the mean values for the five combined unified classification groups that we had talked about previously.

Next slide, please. Well, I guess you might be asking, and I asked myself what is the value of repacked, reconstituted small core samples? What's the value of working with those samples? And what is the relationship between these small laboratory samples and larger scale hydraulic conductivity values that are obtained in field tests?

A number of workers have found a rough correlation between the size of the sample or the volume of the sample being tested and hydraulic conductivity. This particular slide is from a 1999 study in groundwater, and it shows on the left the actual data. On the far left are laboratory core samples. The next sample, which I can't even see the shape of it from here, is a piezometer sample. It's larger in size. It's the larger scale sample. They injected water into the piezometer screen in a bore hole. The next samples were aquifer pump tests, hydraulic conductivity values obtained from aquifer pump tests, and a little bit further to the right are some high volume pump tests.

At any rate, this is from —- this particular material they have plotted here is from glacial outwash material in Wisconsin, outwash aquifer in Wisconsin. These particular authors, their data looks awfully good compared to our data. Next slide, it looks almost too good. I guess I'm not going to show our data yet. This slide just shows that Nye County has conducted hydraulic conductivity on a wide range of sample sizes going from the smallest dry core sample shown as Number 1 on the far —- and the actual relative scales on the far right to field aquifer tests between a pump hole and an observation well.

Next slide. We plot this data, which is Nye County's data, and it doesn't look as nice and tidy as did the data that was published on groundwater. However, the good news is that the core data, the smaller scale data on the left hand side is at least lower in hydraulic conductivity than the larger scale data from aquifer pump tests. It's on the very far right of the slide.

However, the bad news is that the —- in particular, the repacked core data is shown on the second circle from the left. The hydraulic conductivity is actually higher than the larger scale data that was determined in piezometers by concentrated injection tests.

The reason for this apparent inconsistency in the data, there are probably a whole bunch of reasons, but one of them is that part of the porosity, the density of the repacked samples. We did repack the initial samples dry to facilitate speeding up the tests. We were only too able to achieve a density of 1.7 grams per cubic centimeter. Again, we believe the density of the subsurface material is in the range of 1.9 to 2.1 grams per cubic centimeter, so we've actually repacked, and on the process of repacking and testing samples that have a maximum density to be packed in that optimum water content and we received a density of roughly 1.9 grams per cubic centimeter.

If we plotted those data, we have some preliminary data. We don't have enough to actually plot here, but they appear to move that whole data downward towards the regression lines.

We think the piezometer data may be affected by findings accumulating on the bore hole walls during the sonic cooling process. We talked about finds migration in the core previously, and we're going to go out in the field and try to more aggressively develop the piezometer and try to clean up our data set a little bit.

Next slide, please. Let's just skip that slide, and skip that slide. Future work, we have some additional work to complete, and also Los Alamos is going to be running some transport parameters on our repacked core samples.

Next slide, please. Our field work, as I said before, we'd like to go back to the sonic core hole which now has piezometer screens. We'd like to develop the screens further and re-run our injection test. We want to actually this year, actually put in another sonic hole at Site 22, which will be the site of the Nye County tracer test, which will start this fall. We'd like to use the data. We'd like to get this hole in before the actual tracer test, and we'd like to be able to use this data to help us interpret the single-hole and cross-hole tracer test that Nye County plans to do this fall.

In addition, we would like to drill one or two additional exploratory holes in Flat Tire Flat, and the next slide shows the possible locations of these holes. Well, it doesn't show the locations, but Flat Tire 22 is shown on this map. It's located Pacific northwest of the white circle, and again Site 22 would be the location of our cross-hole and single-hole tracer test, and also of our new sonic core hole. Directly, not directly but a little bit to the west and northwest, actually north of the cinder cones shown there is Flat Tire Flat. We'd like to possibly put some additional holes in that area to understand faulting and potential flow paths in that particular basin.

We also plan to do some geophysics, and I did not mention we plan to do some square-array resistivity work this year along the margin of Forty Mile Wash to attempt to get a better handle on the transition from saturated flow in volcanic rocks to saturated flow in Alluvium. That's all I have, and I'd just like if Jamie could spend three or four minutes talking about buried faults and some preliminary cross-sections he's developed that shows the potential effect of these buried faults.

MEMBER HORNBERGER: Okay. Thank you, Dale. So where are we, is Les Bradshaw up next?

DR. HAMMERMEISTER: No, Jamie.

MEMBER HORNBERGER: Okay.

DR. HAMMERMEISTER: Could we have the next slide, please.

MEMBER HORNBERGER: Okay. Please proceed.

MR. WALKER: Good morning. I'm Jamie Walker and —-

MEMBER HORNBERGER: You have to turn the microphone on. We can't hear you.

MR. WALKER: Good morning, I'm Jamie Walker and —-

MEMBER HORNBERGER: Maybe some of the other microphones should be turned off because we're getting interference.

MR. WALKER: Is that better?

MEMBER HORNBERGER: Yes, that's fine. Thank you.

MR. WALKER: I'd like to talk to you today about some work that we've presented at Devil's Hole just recently. I put this slide up as the start of this presentation to show an interpretation of depths to pre-cenozoic basement or thickness of basin fill. We've been working on looking at some of these things in our Phase 4 drill program.

I'm also going to present two new

preliminary cross-sections. I think that Nye County's Phase 4 drilling has better developed some understanding of some of the older underlying growth faults in the flow path from Yucca Mountain to Amargosa Valley.

This figure shows, of course, the thickness of basin fill deposits. You can see the location of Nye County wells, Highway 95, the test site boundary and the location of ESF at Yucca Mountain.

I've looked at this figure for several years and thought about it quite a bit. Now what we've done is we've divided the Yucca Mountain basin into two basins, Crater Flat Basin, and that follows Friedrich's work. I also think that there's a second shallow basin called the Fortymile Wash Basin for now to the east.

We've drilled a series of deep exploratory drill holes in Phase 4 to investigate some of these features. That data was presented by Dr. Hammermeister on the November `03 meeting to the ACNW.

You'll see that in this analysis there's two basins of vast difference in thickness of basin fill deposits from approximately 800 meters to as deep as four kilometers. That's based on gravity data. 2DD does drill through at 800 meters.

There are several recognized major faults that control the thickness of cenozoic deposits. These faults are shown on the map as the pink thicker lines. We see south of the basins are the Highway 95 faults. Of course, these are interpreted differently by Nye County than say by the USGS.

The other large scale features are the Bear Mountain Fault to the west and the Gravity Fault to the east. I've also put in a series of other features or faults, but one that I call a fault is the northeast trending fault that bisects the Highway 95 fault. It was of interest to us. It has both a magnetic and a gravity signature. We've drilled these three holes through there and I'll be presenting a cross-section.

The cross-sections are not labeled, unfortunately, on this diagram. The first cross-section that I'll be presenting is labeled A-A-prime on the next figure. That's the more east-west cross-section, A to the west, and A-prime to the east. And then B-B-prime, which is the north-south cross-section following along the longitude of the boundary there.

We can go to the next slide, please. This is cross-section 8A-prime. It goes across an area we call the Flat Tire Flat, which is directly due north of the Lathrop Wells cone area over the south end of Yucca Mountain and into the Fortymile Wash area, the Fortymile Wash basin, if you'd like, the deeper section.

What we can see is that on the west side in 16P, the bore hole 16P, we have a complete section of the tuffs of Yucca Mountain, and we go across a bedrock or pre-volcanic section in the south end of Yucca Mountain. That's shown by borehole 28 which drills a large section of the Paintbrush group and Crater Flat group rocks.

I should mention that this is somewhat simplified to project these important features that are shown in this cross-section. The biggest feature in this cross-section is that the older rocks, rocks that are generally called TVO and TS on most cross-sections are rotated upward along growth faults that are buried below Paintbrush top rocks.

These rocks, of course, are part of the —- variously referred to as a lower volcanic aquitard, and this would actually form an impediment to flow in a southerly direction based on the orientation of the structure.

It also has an interpretation of the

Fortymile Wash area in the section down at Site 22, which is a little bit problematic, but I think it's consistent with most people's understanding of the relationships.

Go to the next slide, please. This is in cross-section B-B-prime. It's Nye County's interpretation of the thinning of the volcanic tuff section. Realize that this is a north-south cross section basically along Fortymile Wash. It illustrates the Highway 95 fault. The Highway 95 faults are the two dashed lines to the south.

From early in Phase 1 of Nye County's drilling, the geologists recognized the importance of the Highway 95 fault. In boreholes along Highway 95, and especially south of Highway 95, no thick sections of tuff were observed. Rather there are thick sections of fine-grain sedimentary rocks that fill the interval. The age of these rocks are uncertain, although we generally believe that the rocks are part of the older package of volcanics and sediments.

Clearly, there's a rapid facies boundary or fault that juxtaposes the thick volcanic sections against fine-grained basin fill sediments. This cross-section shows that the volcanic section I think in green units bend to the south before they are entirely cut off by the Highway 95 fault, here is interpreted as a growth fault.

Both the Highway 95 fault and the southern —- the fault that I've shown on the other section which I refer to as the southern Yucca Mountain fault decreased the thickness of the upper permeable aquifers, and likely formed barriers or complicate flow from Yucca Mountain into the Amargosa Desert.

In a general sense, I believe that these aren't very well understood. They're only beginning to be understood and they're not reflected in some of the models that has been generated. I think perhaps we've got a little bit more thinking to do on some of these areas, and maybe reinterpret some of the hydrostratigraphic sections. Thank you for your time.

MEMBER HORNBERGER: Thank you, Jamie. And I now assume that we do move on to Les Bradshaw. Again I think that your microphone may not be on.

MR. BRADSHAW: Thank you. Is that better?

MEMBER HORNBERGER: Yes, thank you.

MR. BRADSHAW: I'm going to be brief today. My remarks are not directly on point as far as being of a scientific nature. I wanted to give you some ideas on some broader Nye County policy perspectives on issues having to do with Yucca

Mountain. Specifically, with coordinating water resources information in southern Nye County, southern Nevada, and having to do with Yucca Mountain as sort of the focal point of that concern.

I want to talk to you just very briefly about land-use patterns, growth patterns in Nye County, review the federal activities that are happening in the county, just mention the cumulative impacts of these federal resource management actions, and basically suggest that there could be —- we could all work towards having a coordinated water resources definition development and use plan that would encompass a broader issue of broader needs than just the Yucca Mountain project.

We have to think of Yucca Mountain as being one of many activities that's happening in Nye County. The county is growing exponentially. We expect that growth to continue until the bubble bursts in Clarke County if that ever happens. In our county, we have about three-quarters of our population in Pahrump which is becoming essentially a bedroom community of Las Vegas, so our growth is tied to the growth in the Las Vegas valley.

The next page, please. We are looking at continuing that growth in the Pahrump area in southern

Nye County. Pahrump, Amargosa and Beatty will form a corridor of population where the bulk of Nye County population will reside. Certainly the bulk resides there now. We expect that trend to continue. We expect the communities in northern Nevada, northern Nye County to continue to decline, and for the bulk of the population to be centered in Amargosa Valley and Pahrump, with Pahrump being the centerpiece of that growth.

Go forward to page 5. Eleven million acres in the county. We have to provide our tax base on 2 percent of that which is just as you look at a map showing the private land in the county, there isn't very much. The big bulk of it, a big chunk of it is in the Pahrump Valley and Amargosa Valley. The towns are landlocked. We have a plethora, if that's a good word, or a large grouping I'll say, of federal land management policies, agencies with their policies clustered in Nye County, and each one of them having their resource management plans. And we have to deal with this wide range of resource management plans that are not all particularly coherent, or interlocked, or coordinated with each other.

The next page, please. The federal agencies that I'm mentioning are the usual suspects, and we try to have good relationships with all of them. I think we do with most of them. Certainly, we have the closest relationship with the DOE, and I have DOE listed twice there because the Yucca Mountain project is one of two DOE kingdoms in Nye County, the other being NNSA. But we have a large contingency of DOD, of course, BLM, and then the Forest Service, Fish and Wildlife, and we have a little piece of the Death Valley National Park.

Let's go on to skip page 7. I think we've talked about that, and go to page 8. There is a number of federal regulatory burdens that Nye County is marrying these days, and so put the Yucca Mountain project in the context of these issues. We have federal air quality issues in Pahrump. The folks that have cleaned up the air in Las Vegas now have moved on to Pahrump, having recognized there's some fresh meat over there to work on.

We have tortoise habitat areas in southern Nye County, and the feds that looks after tortoise, likewise, have looked over the hill to Pahrump, and they're going to start looking after the turtles there. A spotted frog habitat in northern Nye, the Amargosa toad in the Beatty area. You may not realize that in Beatty there's a river that flows through town, and some toads live there, so that group will be looking after that, a large group of feds.

We have wilderness study areas, some of them going on 30 years, 30-years old. These are wilderness study areas that are being managed as de facto wilderness areas. I don't have the exact acreage but they're huge areas. We do have two wilderness areas in Nye County, beautiful areas if you ever have the opportunity to go up and look at them.

Areas of critical environmental concern are sprouting up everywhere. The Federal Land Management Agencies are using these as ways to manage habitats where they can't establish a WSA or wilderness itself. And then there's all sorts of ad hoc land management policies about species in habitat, and cultural, recreational, grazing.

From our point of view, we don't see any particular inter-agency cooperation or coordination, and so Yucca Mountain, of course, has –- they're part of this resource management grouping in Nye County. And then the last one, the latest little thing that's bothering us is this federal law enforcement issues where some of the land management agencies in fact believe that they have police authority on the lands in the state. So there's a large grouping of issues.

On the state-level issues, there's the Southern Nevada Water Authority, what we call the water "grab". And, of course, you folks here in Las Vegas think that that's your water. We have to solve a statewide water problem, but from our point of view it's a grab, and so we need to work that out. And we don't have the financial ability to participate effectively in that issue, because the people that sell water in Las Vegas have way more money than we do. Consumer issues, health management policies, over-allocated basins, and water speculators, so that's kind of the context of the range of federal issues in Nye County.

Water, of course, is at the center of most of those. Nye County went out the other day and filed on all basins in the county that are not shared with other counties, and filed on all the water rights that were available to file on, so that really hasn't come out in the normal bureaucratic channel ways. I mean, most people wouldn't know about that now until the abstracts are published, and that will come out soon. But we intend to sort of take charge of water issues in the county, and we actually filed on basins on the south side and on the north range, so we think this is a fairly bold step. But it's all aimed at trying to get a grip on water resource management issues, and trying to be sort of in charge of it instead of just reacting to other people's actions.

Go on to page 11, please. The federal activities having to do with nuclear issues are, of course, the bomb testing that happened for 35 or so years in Nye County, atmospheric and underground bomb testing. The comment that I'm making on Slide 11 here, "Lack of effective Nye County involvement in NNSA groundwater monitoring programs", I mean, some folks might see that as a bit harsh to say, but we, in fact, have no invitation from DOE to be involved in their groundwater monitoring program, and we have no money to be involved on our own dime, so we simply have to sit back and look at what they're doing and hope for the best.

The folks that live around the potential off-site migration areas in northern Amargosa Valley and the northern part of Beatty, are not too comfortable with that, so we're sort of mounting a campaign to tell DOE that we need to be involved in that program. And we suggest to the other DOE, to the ORD DOE, that that issue is of concern to them, and that they should help us have the ability to look at NNSA groundwater monitoring programs, because for perhaps two easy reasons.

One, the public doesn't differentiate between the two. The public doesn't really —- I mean, there has been no defined statement by anybody that Yucca Mountain and NNSA radioactive contamination issues are different, so it's all one big lump to the public. And we'd like to be able to help DOE explain themselves to our residents a little better than we're now able to do. And perhaps the benefit to ORD might be that it would help keep ORD's hands clean as to the NNSA groundwater problem, so we suggest that.

And page 12, that's how a lot of people view the test site. I mean, of course, that's one small area and there are two areas of the test site that have nothing to do with bomb testing, but this is one view of the test site, and people are concerned about what's happening with the groundwater in that issue.

And the next slide, 13 - when you show people that and you go back to 12 and 13, people have the presumption that there's an issue with the bomb testing, and people are asking us as a county government to look at the whole ball of wax, instead of looking at Yucca Mountain as an isolated part of this overall DOE presence just north of the town of

Amargosa Valley.

And then when you look at Slide 14, if you live in Amargosa Valley, you'll have reason to be concerned because that's sort of going down through your town. Just for those that are perhaps from out of town, I'll mention that the town of Amargosa Valley, you can't see it very well, but the town boundaries of that town, they're established years and years ago before Yucca Mountain was a gleam in anyone's eye. It's a large area. It's about 400 square miles, and there's about 1,800 people living there now. It's getting almost as large as Round Mountain and Beatty, so while not large by a lot of standards - and it's going to keep growing because it's one of the largest chunks of private land in southern Nevada right now, outside the Las Vegas Valley, so we've all got to be able to work together to be able to deal with that growth that's going to come. And ORD is just going to get lumped in with this larger DOE groundwater issue and the public perception.

Going on to page 15, federal activities on the test site that our residents are concerned about, the Yucca Mountain project. And we try to tell folks that as to groundwater contamination, that issue is way, way far in the future. That's not something that anyone needs to worry about in the immediate future.

Transporting nuclear waste in the first years of storage at Yucca Mountain, we don't have a groundwater issue, but people don't quite either believe that, or don't understand it, so we've got to be up front with the residents on that.

We are concerned and we have an obsession to define regional and local groundwater flow paths, and since you folks, ORD, and NNSA are spending most of the money these days on groundwater definition in southern Nevada, let's say southern Nye County, that those other significant —- well, let's say not significant money-wise, but significant lines of inquiry from the Park Service. They're very concerned about what's happening with the water in the region because they have their concerns about the pupfish habitat mainly, in our county.

You might not know this, but the National Park Services owns Devils Hole, and so it's their concern. And there's concern about the water levels there, and draw-down, and so on. We don't understand the regional groundwater flow paths, and the local, but the information that BLM and Park Service needs, and that ORD has, and perhaps that NNSA has, it's not flowing inter-agency, so people are just not in their own little programs, Park Service is going to solve the pupfish, ORD is going to solve Yucca Mountain, NNSA is going to solve bomb testing, and it's discordant. It's not being coordinated in a way that is amenable to good decision making based on the best available database.

Certainly, local government officials can't hardly get a grip on it, and I would suggest that the agencies that are working on groundwater in the area probably could speak to each other more than they do.

Let's skip page 16. Some of the reasons that these issues are economically important to Nye County on page 17, there's a large dairy there that pumps a lot of money. The diary is, in fact, one of the top three or four employers in the county, and their operation there - I'm not going to suggest how much of an operation it is every year money-wise, but it's a multi-million dollar operation. The number of cows comes and goes, but it's up in the range of six to eight thousand cows that are milked there every day, and it's milk that supplies a good deal of —- well, it supplies the southwest U.S. I don't know where the markets are, and I'm not going to make a guess on that, but these gallons of milk head down to people that live close by.

Going on to page 18, the basic issue that I'm laying out is that we, as county government, must understand the quantity and quality of our groundwater resources. Without an accurate understanding of that, we can't plan for growth. We can't give ORD the best advice on how to lay out their infrastructure and how to meld their infrastructure into Nye County growth patterns and plans without understanding where the water is, and all about water. Everything is about water, and so we're calling —- I guess what I'm doing is calling for better coordination amongst all of the agencies that are spending money on this issue.

We're in the process of adopting a Nye County water resources plan. I helped to bring this before the Board of Commissioners for a couple of public hearings on this during July and early August, and the idea would be that the county water resources plan would be adopted as part of the Nye County comprehensive plan. And that's important so that Nye County can have a water resources strategic plan, so that when we come and talk together about water, that you know what we're thinking, and we can help you understand —- we can understand what you're thinking and tell how your plans fit into the Nye County water resources plan.

I envision this as a collaborative undertaking. And I've given on page 20, just to wet your appetite a little bit and I encourage you to read a little bit of the Table of Contents.

Page 21, the water resources alternative. Of course, in that plan are no action, just let things happen, just stand back and hope for the best. And we're good at that, we've done a lot of that kind of stuff in the past. The second alternative is an advisory alternative where we would particularly be active in shaping water resource issues, but we would act as an intermediary between the users and the regulators, and that's one alternative. I think what we're going to do is actually on page 23, and my Board of Commissioners is actually being actively involved in water resources planning.

I'm not suggesting that we're going to start up a general improvement district or water planning commission. We haven't quite got that far yet, but we're going to do more than just being laid back and sort of reacting to other people's actions. And as direct evidence of that, is the fact that the board instructed us to go out and file on all the unallocated water in all the basins in Nye County that aren't shared with other counties.

By the way, with those basins we're going to go and perhaps collaboratively —- if the speculators haven't beat us to the punch, we're going to go out and work with the county that we share basins with and do something about those. So we can expect Nye County to be active in its own water resources management planning in the future.

The next page, 24, the resources plan will be adopted soon. And I'm proud of the Board of Commissioners for being forward-thinking in this issue. We can't just lay back. Just like our attitude about Yucca Mountain, we can't just lay back and let it happen and hope for the best. Let's be at the table and be part of the planning.

My closing comments are that let's coordinate our research. There doesn't seem to be - and I'm speaking from sort of a —- I'm a geologist. Maybe today I shouldn't really admit that, but from a county administrator or county official's point of view, there doesn't seem to be a common database that planning commissions and town boards, and developers and others can go to and sort of get the big picture on water issues. You have to go in front of the dime, and then ultimately we have to get expert consultants or staffers to tell us what all the data means. And then even at that, it's sort of a personalized interpretation of what the data means. So we need to have the data more accessible for local government planning, and I suggest also for federal government planning, for joint federal and local government planning.

Competing agency objectives and goals need to be eliminated. You'd be surprised. The BLM has four administrative offices that deal with Nye County. The Forest Service has three, the Park Service luckily just one, wouldn't you know that, but BIA actually has two, and then the DOEs in all the flavors and colors. And if you look in detail at all the resource management plans that are in all the EISs that are floating around out there, it doesn't fit together very well. And there's actually competing agency objectives, and a case in point is the Park Service that, as you know, they forecast everything. I mean, if you want to have a point of divergence for a domestic well —- but magically enough, when DOE is looking for water for Yucca Mountain, actually the Park Service disappeared, so sometimes the Park Service protests and sometimes they don't. It's a little incomprehensible.

Lastly, there are lots of data in people's heads, and we've got to do something, if we can, to retain people that worked for years and years on these issues, and then leave the service of the government. But they're still out there. They're still bright and articulate people, and they need to be part of this data management repository that I'm talking about some way or another. WE shouldn't just them drift off into the sunset and never hear of them again because a lot of this stuff is floating in their heads, and isn't really down on paper.

So a collaborative, non-confrontational coordinated regional planning effort is Nye County's goal. Actually, I tore off the last page of this thing and didn't present it today because I don't really have a clear view of how we're going to reach this objective. I guess I'm just like a lot of people that come before my Board of Commissioners. You come here and lay a problem on the table, and you don't bring a solution with you.

I'm hoping that the people that hear this, and other groups that I'm speaking with, that some way we can formulate a solution, that we can get a way to get southern Nevada, southern Nye County water issues packaged up a little better so that it's a little more useful to local government, federal government, state government. And actually, we can't leave out our brethren down in Inyo County, which is part of —- I mean, economically the eastern part of Inyo is basically tied to Nye County, water-wise they are. So it's a couple of states and it's all the federal agencies and two or three local government agencies and counties that have to learn how to work together. So I leave that with you. I hope that we can spark a discussion and get some ideas on how we could get a more coordinated interaction process as we work together on water issues, and then how that data can be managed in a way that makes it more accessible to local government. I'm concerned about local government, but all of us would have to get together and talk about water resources issues. Thank you very much.

MEMBER HORNBERGER: Thank you, Les. First, let me state for the record that as far as the ACNW is concerned, it's quite all right for a person to admit to being a geologist.

We will entertain some questions now for the people from Nye County. Don, do you have any? Hearing none.

DR. SHETTEL: I had a couple of questions for Dale. The sonic coring doesn't —- I mean, coring almost seems like it's a misnomer. The sample doesn't come out the way it exists before you started.

DR. HAMMERMEISTER: Well, the well isn't tapped. That's important. And we'd probably say for the expansion of the core, would do a reasonably good job assigning depth intervals. I think that's an extremely important contribution.

DR. SHETTEL: Does it come out expanded?

DR. HAMMERMEISTER: Yes.

DR. SHETTEL: Okay. Does it come out warm or hot from the procedure?

DR. HAMMERMEISTER: Because it's below the water table, it's generally not warm, but we see evidence of the heat just by the movement of water in the actual core itself.

DR. SHETTEL: Okay. And the main value of reconstituting the core is the expense versus taking a drill core?

DR. HAMMERMEISTER: The problem is, is that this is probably one of the very few, if the only, method of obtaining core from coarse-grain Alluvium. All other coring methods just simply either wash away the core, or are prohibitively expensive.

In the past, we have been slightly successful using dry core techniques where we just pound the solid tube into the ground and extract the core. This is one of the only methods available that we're aware of, and I think that's an important contribution to the project.

DR. SHETTEL: Thank you.

MEMBER HORNBERGER: Dick.

DR. PARIZEK: Yes. I wanted to compliment Les Bradshaw on a tutorial which gives, I guess easterners, a good sense of the complexity of interaction of governmental groups on all scales, and the competitions on water in a water-scarce region.

I also attended the Devils Hole workshop recently, plus others. I would invite all people who have not gotten in on the ground where the work is being done and meet the public, and basically the people who have to face all these issues. You'll find this an extremely rewarding place to go.

A question about whether or not Nye County's plan includes water for Yucca Mountain. Is that part of the process, and is DOE involved in that plan as you're developing it at Nye County?

MR. BRADSHAW: We're not —- the filings that we just did were not intended to interfere with or supersede, or in any way diminish whatever water rights that ORD or DOE has in regard to Yucca Mountain. The other filings on the test site have to do more with resource damage and various remedies to that. And that has really nothing to do with Yucca Mountain. And Yucca Mountain's water needs are not —- they're significantly, fairly significant, but they're not overwhelming. But in an over-allocated basin, and with expected growth in the future, I mean there's 2,000 people there now. What if there's 10,000 or 15,000 in 2020? I mean, DOE's activities and actions need to be coordinated and integrated with the Nye County's growth management plan. None of our actions are directly aimed at either supplying water, selling water rights to ORD, or interfering with their legal issues having to do with water.

DR. PARIZEK: A question for Tom Buqo. The discussion about really the evapotranspiration estimates, you've implied that there's obviously refinements needed, and yet evapotranspiration was one of the drivers for the regional flow model which also embodies a site-scale model. And if you don't have the discharger's right, you don't have the recharge's right. How do you think this is going to affect the reliability of the present regional model and the site-scale model given the work that you're doing trying to upgrade the understanding of evapotranspiration losses?

MR. BUQO: Well, I think it points to the large amount of uncertainty in the regional model, and by connection to the site-scale model, the uncertainty in that. We don't know what that rate is. We're just saying that it's very important that we do understand that, that the work that's been done suggests that it could be appreciably more. It's all linked together, and if the discharge goes up, then the recharge has to go up correspondingly somewhere else in that system so we can have a balance. And that changes our whole water planning forecast because now we have more recharge to work with.

I think also importantly with respect to the site-scale model, a lot of the input coming into that site-scale, particularly from Rock Valley, is a gross estimate based upon 50-year old reconnaissance reports on underflow coming off the Sheep Range and the Spring Mountains, coming through Frenchman Lake, Yucca Flat, and Mercury Valley. It's not a solid number. It's a very soft number, and that's the sort of thing we need to refine, because if you look at the mass balance for the site-scale model, it all hinges on that one value coming in from one hydrographic basin. And if you separate that one basin out from the others, you get a totally different mass balance, at least from the previous versions that we saw. And we are looking forward to the most recent so-called transient regional model that's supposed to come out in September to see if that's going to provide us a better idea of what our future impacts are going to be.

DR. PARIZEK: Thank you. And one for Dale. The sonic core is a huge improvement over samples that were collected previously. And, Don, I'm not sure whether you've seen that core, but it is a real core in so many ways other than cuttings that just blew out of holes by rotary methods of drilling.

When we look at that core, one sees rock fragments in class which are deeply weathered. Would you have seen similar materials in the other methods of drilling, Dale, or would that rock just break up? And these are pebbles, cobbles, boulder-size materials that were rock, more or less. Do you believe you would see that by the rotary method of drilling, because again that raises a question about diffusion into rock fragments in terms of the groundwater flow field effects. There's benefit to be derived from that if that's, in fact, the way this material is located.

DR. HAMMERMEISTER: No, I think that the rotary method would grind up the more weathered material, in fact, does grind up the more weathered material. And a lot of the off-sites and perhaps some of the clays on the weathered material are not even captured. They're washing away during the rotary drilling. And at least a portion of the finds that we do see, the silt and clay that we do see in a rotary drilling potentially are simply ground up gravels and cobble material.

DR. PARIZEK: And that weathered interval, whether that's really a paleosol or not, I mean, obviously the soils you see, and the buried soils you see in Fortymile Canyon are much younger, but that deeply weathered material implies something about having them transported that way or formed in place as a weathering product, so there may be some soil information hidden down there that may come out of the sonic drilling program.

DR. HAMMERMEISTER: Yes.

MEMBER HORNBERGER: Okay. Jim Davis.

DR. DAVIS: Yes, a question for Dale. In your future work, what's driving where you're selecting locations to drill? For example, you're going to go to the 22 area location. Are you being driven by the understanding of the faults, as Jamie was talking about, or what other criterion are you using to select locations?

DR. HAMMERMEISTER: Tracer test location, but clearly, tracer tests give a much better large-scale estimate of travel times and flow pathways than core do, but we feel that core would help us to understand and interpret the tracer test data, so to date, we drilled out the Alluvium testing complex where we hopefully —- either DOE or Nye County will do additional tracer tests, and the 22 complex. Those are our priorities.

MEMBER HORNBERGER: Jim Clarke.

DR. CLARKE: If we look at Slide 38 of Dale's presentation, could you just tell us which are the wells that will be involved in the tracer test?

MEMBER HORNBERGER: Again, from Slide 38, if it does come up, it's almost due north along the Fortymile Wash channel. If you look due north to the white circle, if you have the handout - it's a little it to the northeast. The first yellow triangle are labeled the 22SA, 22PA, 22PV. That's the location of the Nye County tracer tests that we'll be starting this fall. I guess this slide will never come up.

MEMBER HORNBERGER: Do you have it, Jim?

DR. CLARKE: Yes. For some reason I thought there were other wells involved, as well.

MEMBER HORNBERGER: Well, it's a complex.

DR. CLARKE: Okay. Complex. All right.

MEMBER HORNBERGER: Ines.

DR. TRIAY: I just have one quick comment that I truly commend Nye County for their leadership in trying to coordinate the water resources definition development and use. And certainly, all the other agencies also should be strongly encouraged to collaborate in this effort.

MEMBER HORNBERGER: Thank you. Allen.

DR. CROFF: Pass.

MEMBER HORNBERGER: Michael.

ACTING CHAIRMAN RYAN: I pass. Thanks.

MEMBER HORNBERGER: Ruth.

MEMBER WEINER: I have one for Tom Buqo. I, too, live in a water-short area where we are running out of groundwater, and we have a water management plan. And I'm not on any decision making or decision aiding body, but I know that to get the citizens of Albuquerque, New Mexico, to get the City Council and the County Council, Bernalillo County, to accept the water management plan - we had to have quite a bit of evidence that we needed one. And I was just wondering what did you present to your county to get them to accept your water management plan?

MR. BUQO: Well, initially it was just a dove-tail onto another rural water plan. The State of Nevada in 1999 put out their second state water plan. At that time, they encouraged each of the counties to do their own water resources plan. Since then, that planning department has gone away, but the planning effort has gone forward.

As Mr. Bradshaw mentioned, we have a lot of problems with water in Pahrump, in particular, and Amargosa Valley secondarily. The citizens are all very aware of those. Some of our citizens have had protests outstanding on their water right applications for over a decade now. They simply can't move forward because they don't have the financial wherewithal, so it's not only the citizens, but also their elected officials who are very aware of these problems, so it wasn't difficult at all.

We have a much smaller critical mass in southern Nye County than the folks in Albuquerque do, of course, and probably fewer vested interests. Everybody is interested in their own well in Pahrump, and although initially we did get some opposition, I was yelled at at meetings and that sort of thing - with time and dropping water levels, people came on board. And now those same developers that were yelling at me eight years ago, are now on board as part of ad hoc committees to resolve the problems.

MEMBER WEINER: How long do you expect it to be before you've mined all your groundwater?

MR. BUQO: We can mine groundwater indefinitely. We will pay the consequences of mining it in terms of having to pay more money into the county road budget to fix the cracks in the roads and resolution of disputes between individual well owners. If we have to, we can mine it forever. We've got several thousand feet of available saturated thickness in Pahrump, but we don't see that as being a good solution when there are other sources available.

Same thing, Las Vegas is not mining. They're going to other areas to import water to Las Vegas Artesian Basin.

MEMBER WEINER: Thanks. I have a question for Les Bradshaw also, and comment. We visited the Amargosa Valley, several of us, and the owner of that dairy told me he had moved there in 1995 which, of course, was well after the project at Yucca Mountain had started. And I was wondering, what percent roughly of the 1,800 people who live there moved there since 1987, if you have some idea.

MR. BRADSHAW: I'd say at least maybe 60 percent. In other words, I'm suggesting that in `87 there was eight or nine hundred people in the valley, and there's close to 2,000 now.

MEMBER WEINER: Thank you.

MEMBER HORNBERGER: Okay. Thank you very much. Our last presentation before our lunch break is by Matt Kozak, who is going to tell us about some of the EPRI evaluations of the saturated zone. Fortunately, Matt has a more subdued tie on today so he won't blind us.

MR. KOZAK: Well, this is an enviable position since I'm sure nobody will want to ask any questions afterwards. I am representing the EPRI TSPA team. As most of you probably know, EPRI maintains an independent capability to do TSPA for Yucca Mountain. Ordinarily, at a meeting like this that's predominantly geosphere, we would have Frank Schwartz come and address you, but he couldn't be here, and so you are reduced to having me giving you sort of TSPA flavored geosphere presentation. Next one, please.

Most of the comments that I'm going to be giving you are just notes that Frank sent me last week when I found out that I was going to be here, and so for the past several years, we have been looking at several different issues. Some of the ones that it would be appropriate to look at here is the contributions of the both the UZ and the SZ, and the fact that what we are trying to do with our TSPA is to look at more realistic, as opposed to strictly conservative-type calculations in TSPA.

I'm not going to talk about the UZ and SZ as this seems to be more of an SZ kind of meeting, and so predominantly what I'm going to talk about is the concept of the flowing interval, which Bill Arnold yesterday mentioned was one of their key phenomena that they were concerned with. Next one, please.

I put this in just sort of as a general overview of what we're trying to accomplish and to try to provoke some thought. We go back to 40 CFR 197, and you look at the economic impact analysis for that which is cited down there at the bottom, it's interesting to look at what it is that we're shooting for in the Yucca Mountain TSPAs.

There's a nice discussion in the EIA for 40 CFR 197, and I've only extracted a short bullet item here to kind of get across the idea of what they're looking for. In a nutshell, what EPA said their reasonable expectation meant was if we could get rid of all the conservatisms out of TSPA, that that kind of calculation would be the one that would be appropriate to compare against the numerical standard. Now that's fairly different from what we see DOE, NRC, and even to a large extent the EPRI TSPA analyses trying to do.

For people who aren't really deeply involved in TSPA, it's hard to get across how deeply imbued this notion of conservatism is in the whole process. Really, any time that you're faced with an uncertainty you end up biasing things for the sake of conservatism. And at the end of the day, you have a large amount of compounded conservatism, and yet we've got this standard that says our health-based safety standard is based on something that doesn't have any of those conservatisms in it. So the difference between those two is sort of an unquantified, and to a large extent, unrecognized degree of conservatism that's built into the whole process.

And so one of the things that we try to do in the EPRI TSPA is to look for some of the more kind of Draconian conservatisms and just say well, what if it's not that conservative, how can we back off on that? And what are the results? Is it orders of magnitude, is it not orders of magnitude?

But at the end of the day, we still really don't know. And even in our TSPA, there's lots of conservatisms that we find all the time, and that are there intentionally. There is this undetermined level of conservatism, so when you see those terms, recognize that they are probably some orders of magnitude over and above what perhaps the realistic estimate of what the repository performance might be.

Next please. The other point to make, which I don't think I really need to make to this audience too much is to recognize that this is part of a total system. So when we start getting concerned about individual assumptions, about individual processes, and individual elements of that, we really have to be careful and make sure that we recognize that that's only one part. And even if we were to be completely wrong on one particular assumption, it's probably compensated for elsewhere in the system. And so while we are looking for good performance from the saturated zone, it's not the only part of the system. And even if we have made some incorrect assumptions in regard to conservatism, it's certainly going to be compensated for by conservatism elsewhere in the system. Next, please.

Down to the saturated zone now, and we're back a bit more to Frank's comments. What we think is that —- well, first of all, again in regard to this philosophy, we've tried to avoid excessive conservatism in our representations, and we think that in the saturated zone, for instance, that there has been a neglect of some processes that because they're not well-understood are treated conservatively and so forth, and at the end of the day, we think that the saturated zone analysis is fairly conservative compared to what might be reality.

To a large extent, at least in our program, the saturated zone has played second fiddle to a large extent, because we get a lot of benefit from the unsaturated zone, and the other aspects of the system. And so in terms of setting our own priorities, we haven't put a real lot of priority on saturated zone in the past few years anyway. Next, please.

This concept of the flowing interval has been developed in TSPA to conceptualize the fractured flow in the UZ, and the SZ. The flow and transport occurs within these poorly connected system of flowing intervals, in which the flowing interval spacing is much larger than the fractured spacing. Next one, please.

One of the issues is that these flowing intervals are relatively rare, that they do not appear to have significant inter-connections because of the random distribution. And as a consequence of the assumptions in the TSPA, particularly the DOE TSPA that Frank had reviewed and developed in his work, that the large rock blocks between the flowing intervals tends to minimize the effect of matrix diffusion in the TSPA model.

This is separate from field studies and so forth, because you have to understand that when you do a TSPA, you abstract these field studies, and a lot of times, the TSPA person says well, I don't have the capability, or I don't have the desire to incorporate that, or I'm going to represent it conservatively. So TSPA models are typically a lot more conservative, and don't necessarily represent some of the processes to the same extent that you would see in the C-wells tests. Next, please.

This was some sensitivity analysis that we did looking at the effect of the block size of these flowing intervals, and you'll see that when we go down to smaller blocks, I don't know if you can see the scale which is, unfortunately, in days for obscure reasons, but we get an increase of about an order of magnitude in delay time just in looking at the difference in features of these flowing intervals. Next, please.

And so just a few concluding comments, that essentially in the DOE model, the same sparse network of fractures is being called upon to carry fairly large quantities of water and to function as an aquifer. There seems to be a different model for the UZ and for the SZ, and the way they're put together sort of maximizes the conservative aspects of both, so that the conglomerate of them is actually more conservative than the two taken individually. And so at the end of the day, we think that the TSPA models for the saturated zone are likely to be significantly conservative with respect to reality, and so we probably have lot longer delay times, and probably higher dilutions from the saturated zone than we see coming out of the TSPA models. And that's all I have to say,.

MEMBER HORNBERGER: Thanks very much, Matt.

DR. KOZAK: I tried to be brief before lunch.

MEMBER HORNBERGER: Well, that was good,

and you may be right about not having any questions, but you may not be, as well. Don, do you have any questions for Matt?

DR. SHETTEL: Not at the moment.

MEMBER HORNBERGER: Okay. Dick.

DR. PARIZEK: You picked just the flowing interval as one example of conservatism. You probably have others?

DR. KOZAK: Yes.

DR. PARIZEK: And so you're saying that this is just one place.

DR. KOZAK: Yes. This is one place that in particular, Frank chose to highlight as being an example that we could point out where just by making slightly different assumptions, you can get dramatically different behavior, significantly more benefit.

Now one of the ones that's been talked about quite a bit around the table in the last couple of hours, I think, is the notion of matrix diffusion in the Alluvium, which we don't have in our model either. Certainly, if you start to introduce more matrix diffusion, you're going to get a lot higher benefit, and I think that's something well-worth investigating to see what the knowledge base and the database is to support that.

DR. PARIZEK: Have you actually ranked others in terms of their priority, in terms of performance? Sort of like the risk-based thing that Tim has talked to us about.

DR. KOZAK: Yes, a couple of —- sorry, go ahead.

DR. PARIZEK: That's all right.

DR. KOZAK: Okay. A couple of years ago, we went through something that was a —- I'm trying to think of what the buzz words were that we used for it, but essentially it was looking at each individual barrier of the system individually, and looking at how much benefit you got from each one sequentially.

There were some limitations to the way that we did it, but we have looked at that, and we are seeing a significant benefit from all aspects of the system, is really what the most important outcome of that work was.

MEMBER HORNBERGER: Jim Davis.

DR. DAVIS: Just a comment. I wouldn't be so positive that matrix diffusion in the Alluvium would contribute a lot more, because the batch tests that are normally done with Alluvium are done with grain sizes up to 2 millimeters, and they typically are measuring sorption over many days. And so at least some of the matrix diffusion that you are imaging would be, in fact, already incorporated into the KD measurements.

DR. KOZAK: Yes. And I'm not at all positive. That was just one that struck me from this meeting, that not being a geosphere specialist, that was the first time that I'd heard that proposed as a concept, and that's why I mentioned it. But yet, I'm not at all certain of its —- but it would be interesting to look at what the effect would be, even as a sensitivity study it would be worthwhile doing. And if it doesn't matter, we don't care.

MEMBER HORNBERGER: Jim Clarke.

DR. CLARKE: Mine was answered.

MEMBER HORNBERGER: Ines.

DR. TRIAY: So if you had to summarize anything that needed to be done, if you could summarize for us in your opinion what is it that needs to be done for that independent function that you're trying to fulfill, what would it be?

DR. KOZAK: I think the role that we fill will be in showing sort of the degree or conservatism that is embedded in the models. If we can back off on the conservatism and do so in a reasonable and justifiable way, I fully recognize that in the licensing process it's most efficient, effective, and pragmatic to do things the way the DOE and NRC are doing them, but we do have this gap between what is safe in the EPA sense, and the way TSPA is approaching it.

If we can identify how big that gap is, I think that would help people make a decision about the effectiveness of the repository, and the viability of the repository. Sorry, shouldn't use viability since we passed the viability.

DR. TRIAY: That's right. Don't forget that.

DR. KOZAK: Viability with a small V.

DR. TRIAY: So for instance, just so that I understand, would it be fair to say, and I don't know, so I'm not trying to lead you on. Answer honestly - would it be fair to say that you would be looking at a table like the one that was presented on page 15, with the Ds and stuff like that, and try to see whether you agree with that kind of emulation from the NRC?

DR. KOZAK: Yes, although the way we work is more down at more of a detailed level of we look at a particular assumption and say well, that assumption seems pretty conservative. How can we back off on that and still do something that is reasonable, but less conservative, recognizing that we don't have to stand in front of the licensing board either, that we're not part of the licensing process.

MEMBER HORNBERGER: Allen.

DR. TRIAY: We get all the fun jobs.

MEMBER HORNBERGER: Allen. No. Michael.

ACTING CHAIRMAN RYAN: On your last bullet on your last slide, Matt, you say "significantly conservative." Are you willing to tell me if that's a factor of two, or a factor of a hundred, or somewhere in-between? Do you have any sense of the quantification of that yet?

DR. KOZAK: Yes, I would never use the word "significant" if it were a factor of 2 in TSPA, so I would say an order of magnitude or more.

ACTING CHAIRMAN RYAN: Or more. Okay.

DR. KOZAK: Yes.

ACTING CHAIRMAN RYAN: That's helpful because I think it's an insight, again as you think about formal tools like the one that Tim McCartin presented, that's helpful. So an order of magnitude or more is what you think this particularly saturated zone component might be, and there may be others and so forth.

DR. KOZAK: Yes. Although, again, conservative can be defined in a couple of different ways in the context of the rule because you have a time-dependent rule, so something that pushes things further out in time has a desirable effect, as well as lower the dose.

ACTING CHAIRMAN RYAN: Right.

DR. KOZAK: But yes, it's —-

ACTING CHAIRMAN RYAN: Great. Thank you.

MEMBER HORNBERGER: Ruth.

MEMBER WEINER: I have two questions that may appear to be diametrically opposed. A former member, a distinguished member of this committee, far more distinguished than I, has been known to say it isn't conservative, it's wrong. And that's Milt Levinson, and I wondered whether you had any sense of the estimation of inputs to the TSPA that were so conservative they could be called wrong? And the opposed question is, how do you in going from conservatism to what you see as realism, how do you counter the charge that, or the statement that okay, we looked at a bounding case. And if it's okay for the bounding case, it's obviously okay for cases that are less than or that are within the boundary case.

DR. KOZAK: Regarding your first question, Milt has said the same thing to me, and I actually disagree with that comment in the context of a regulatory assessment. It's absolutely correct in a scientific sense, that if you're conservative by definition you're wrong. You're not accurately predicting, which is what you're generally trying to do in a scientific sense.

In a regulatory sense, it comes back to your second point where if I could go out and have my Remy eat the waste, that's obviously wrong. No one is ever going to do that, but if it were safe in a dosimetric sense, then I wouldn't care about all these other processes. The only reason that we're delving into it in the depth that we are is because the waste is more hazardous, and so you have to have a balance between conservatism and non-conservatism in a regulatory sense because that's the most convincing argument.

If you can bound it, and do a convincing job of bounding it, that's the ideal way to do it in a regulatory sense. And it's cost-effective too, which is a good thing, because you don't have to spend years studying some particular process, which has been done with other repository programs that have studied some particular phenomenon ad nauseam, and not come to a conclusion because they were trying to get a scientific answer.

I don't think we need to do that in a regulatory sense, but I think what is a benefit of what we're doing is to say well, okay. How much conservatism is there? Now we know we meet the standard but there's always the person who says yes, you met the standard, but it's only an order of magnitude, and we know it could be much higher than that. You know, you're at 1 millirem instead of 15 millirem, and we know we could go way above that, the tails of the distributions. But in point of fact, a 1 millirem calculation from a conservative analysis should be appropriate, and since it's probably conservative by orders of magnitude, then that just gives us a better gut feeling about the safety. And I think that's the role that we're trying to play, is to help with that gut feeling of safety regardless of where the dose curves lay.

MEMBER HORNBERGER: In fairness to Milt Levinson, perhaps I should point out that I think the full text is something like "being off by six orders of magnitude is not conservative, it's wrong." So it's not just being conservative is wrong.

DR. KOZAK: True enough.

MEMBER HORNBERGER: I think he mentions the number of orders of magnitude.

DR. KOZAK: Yes.

MEMBER HORNBERGER: I know that the ACNW for many years, I think, has pushed the NRC performance assessment people to be as realistic as they can possibly be. And I can remember after one of our meetings where we recommended that to the NRC staff. John Kessler pulled me aside and said no, no. You have to be careful because after all, if you can do it with a bounding analysis, and you don't have to spend a lot of money confirming this, why worry?

DR. KOZAK: Yes.

MEMBER HORNBERGER: I do take it that your presentation doesn't really disagree with John's point of view, because all you're doing is saying that you are interested in basically giving an idea of the margin.

DR. KOZAK: Yes, that's right. No, absolutely. I think the process that you DOE and NRC are going through is appropriate, and we hope we can supplement that with the information that we can present without —- certainly without disagreeing with the general concept of how to do it.

As I said earlier, the way PA is structured, is that way for a reason. People have found that it's the most effective way of doing it, and most effective way of beginning to understand the total system behavior. So no, I don't disagree with what they're doing at all.

MEMBER HORNBERGER: Okay. Great. Well, thanks very much. I think it was a very good morning, and we're not too far past our schedule. What we're going to do is break for one hour for lunch, and even though our schedule has us starting at 2, we are going to start at 2:15.

(Whereupon, the proceedings in the above-entitled matter went off the record at 1:13 p.m. and went back on the record at 2:17 p.m.)

MEMBER HORNSBERGER: On the record. We are reconvening our meeting. If everyone would grab a seat. We're missing some.

LAS VEGAS PARTICIPANT: We can't hear you.

MEMBER HORNSBERGER: Okay. Let's see. Somebody can't hear us. Okay, now?

LAS VEGAS PARTICIPANT: Okay. We have you now.

MEMBER HORNSBERGER: Okay. Good. Thank

you. We're going to reconvene. We have reconvened. We're moving on to our afternoon session now going into the part where we have roundtable panel discussion. We're first going to have comments from our panel members and we'll go in the order here that is on the schedule. So I'll ask Jim Davis if he wants to summarize some of his thoughts for us.

DR. DAVIS: Okay. There were two main things that I wanted to bring up and one already came up in some of the other discussion which was what seems to be a lack of consistency between the field testing that seems to suggest that matrix diffusion isn't important. I believe that's referring to the work that was done at the sea wells. Is that correct? I guess as kind of an outsider coming into this process I'm surprised that there is still that level of a lack of agreement between - maybe it's an interpretation of - the field results and there seems like there should be some focus on reaching agreement about the meaning of that field test. So that was one of my main reactions.

And then the other has been mentioned several times as well which is the effect of chemistry on retardation in the alluvium I think that DOE's approach with respect to building a distribution of K_d

values has focused somewhat on the variability of the porous medium in terms of its sorptive properties, but has possibly not adequately accounted for the effect of chemical variability on retardation. I think the Center's program is closer to the mark in terms of looking at that as a variable, but I also think that that has a few key elements missing from it which are looking at complete groundwater compositions including the possible role of fulvic acids and also comparing work with actual alluvial materials in comparison to what their predictions are from their extracted model. Those are the two key areas that I felt concern about in terms of having confidence of where we are with the performance assessment.

MEMBER HORNSBERGER: Thank you, Jim. Let's see. Don, I believe, you're up. Do you have some summary comments for us? Is Don there?

PARTICIPANT: Don's not back from lunch yet.

MEMBER HORNSBERGER: Not back from lunch yet. Okay. Dick, I'll next go to you.

DR. PARIZEK: We have some new graphs of which probably need to be put up on the screen or everybody has handouts?

MEMBER HORNSBERGER: I don't know.

Michelle, do you have them?

MICHELLE: Yes, we do.

MEMBER HORNSBERGER: Okay. They'll be up in a second.

PARTICIPANT: And the handouts are in the back.

DR. PARIZEK: There are some hard copies provided. From a Board perspective, there are some points here that appear in the Board's letter to the Department and then other are observations which we can make as part of this meetings, but first we would say the field of laboratory observations analyses presented by DOE and others suggest that the natural system provides an effective area to migration of some radionuclides over time periods that may be comparable to the regulatory period. So clearly, that natural barrier can be counted upon for many of the radionuclides.

However, there are several key hydrogeological features or processes that may significantly affect fluid flow and radionuclides transport are presently not well understood that are constrained by limited or poor data or both. So this is trying to improve on the understanding and reduce uncertainty is obviously a Board concern. We always carry this baggage with us in terms of our commission.

DOE often deems with on certain features and processes by making conservative estimates of their effects on the radionuclides transports. Such conservatives tend to emphasize more rapid advective transport processes. This is sort of a similar statement as to what you heard from Frank Schwartz and presenters from EPRI are stressing. More realistic estimates could be to slower transport predictions for some radionuclides.

There is also a possibility that some other poorly understood processes or features may lead to faster radionuclide transport. So it is important that DOE develop a better fundamental understanding of the overall behavior of the natural system. We feel like the natural system to get full credit for it to continue the science and technology program and continue the research is our feeling.

There's examples on the next slide and one where we could actually improve understanding has to do with the large-scale hydraulic tests of major faults. Basically, the sea well testing complex did penetrate through faults but it's not necessarily block bounding or a large fault and it's not at all clear whether or not the major faults that are south of the footprint of the repository have an important role in regional flow.

We heard from Jim Winterle's discussion that there's obviously a steep radiant on the west side of some of those faults so there's a damming effect that the faults can play. On the other hand, there could be a duel role that the fault could also be a fast path. You could dam on one side and maybe have a fast path on the other. So this is hanging wall, footwall structural details at work. Some of you all know Marybeth Gray's work on characterizing faults and talking about the architecture and as a result, there are probability/possibilities that are not well known.

So the Board felt that there ought to be some deliberate testing of some of the block bounding faults. We get messages from the unsaturated zone in pneumatic tests. You see the faults in exposure in tunnels and so there is enhanced probability at least at that field. The question is what does it do in the saturated part of the system and we don't see a program outside of the data to deal with that story.

There's also some indication of fast paths on the Paintbrush Canyon fault and this is based on some borehole data that's been reported upon by DOE.

It gives you travel times that are in that 45 meter per year rate which is pretty fast numbers and so that draws attention to the fact that maybe faults can be important to this whole process.

But could they turn the direction of flow rates south? If it did, this would be a conceptual model failure. The idea that Bradelhoffen (PH) Conoco's(PH) discussion about can you evaluate ground water models. Well, one thing you could do is miss the mark by having the wrong conceptual model and it's really important to make sure there's no way flow can turn south on any of these major faults and that no program forecasts show that southernly component. They all kind of go southeastward and come south and the alluvium then comes southwestward, but nobody has that flow straight south. That would be conceptual failure if in fact that does happen. The only way you would know that, I think, is by the intentional drilling program. Nye County has talked about the intentions to do such a drilling.

This then leads to another discussion on the saturated alluvium and the saturated alluvium in the present modeling is two layers with, say, effective porosity that's uniformly distributed in each of those layers and then you do realizations and you let the effective porosity numbers and the conductivity numbers vary. Then you come up with your probability predictions that you get out of that.

But the likelihood of the alluvium being two layer alluvium is not too realistic. You saw from one of the presentations of just the bulk mineralogy and then the clay-mineralogy abundances that there is some sort of variation that's somewhat systematic and the deeper you went, the more different it looked than the shallower part, but the Washburn well and Jason well both had similar appearances up near the surface.

You would argue that the alluvium has a complicated history of formation and if you just take the last 10,000 years in the desert, we see alluvial fans. We see soils and peleosols of different ages and that complexity is all maybe up in the shallow FortyMile Canyon exposure level, something in that order. But there's bound to be complexity at depth and with the complexity, it could channelized flow. It could be lenses. It's not clear.

The Nye County shows cross sections with a channel in the alluvium on the one hand, but that may not have continuity going up the wash or may have. It may make a big difference in terms of really the way in which water could travel down through the alluvium. If there are clay-enriched zones whether the palesols or not, that has a blocking effect on flow and you could have sandwich flow caught underneath clay layers, for instance, for a longer distance of travel versus mixing in the vertical.

The architectural style of the alluvium is quite important and the tunnel path length discussion of one to ten kilometers versus two to ten, the one is pretty short. That's a big difference to performance. You would like to make sure that you understand where the saturated alluvium bedrock contact comes in and Nye County is still working on that. We heard that they propose to physics as well to help pin that down. That's kind of an important variable.

Then the matrix diffusion is another category. In the bed rock, there's clearly work to be done to get more credit for that. We heard this as part of the EPRI presentation in terms of this flow interval spacing, but also what does that do in treating between those flow intervals where you may have matrix effects. The whole question of blockage or coating on surfaces we don't know that much about whether they are coated and prevent that matrix diffusion in some cases or not.

When we look at the sonic core in the

FortyMile Wash area, clearly some of the class were decomposed in rock. I purposely asked that question to make sure that that stuff wouldn't survive rotary drilling. It would have broken up because you can peel that apart like an onion skin type thing. Rotary drilling would have broken that apart. So what fragments of that type problem would even have a diffusion benefit.

Then we were corrected by saying well maybe you should have gotten it out of the K_ds already. K_d data probably had time enough to respond to that and whether the matrix effects are in or are out. I guess the only way you will know that is a long term tracer test.

I raised a question about that recycling of water for the sea well testing complex. It's about six or seven months later since that slug of water was put in there and I was very pleased to hear that the program was picking on doing something with the science and technology part of it.

Maybe that water went down in the fractured, unsaturated tufts and you've lost it. On the other hand, it's worth looking for. That's one of the few places where some signal has been put in the aquifer that's quite distinct. Most of the geochemistry that we understand is coming out of boreholes that were not dedicated to geochemistry. They are boreholes open to different depths. They were boreholes that often might not have been cleaned out properly and for the geochemistry point of view, you'd like to have dedicated boreholes which is what my county was doing when it was doing the West Bay construction.

West Bay gives you this very discrete sampling of heads and chemistry, but in more recently drilling, that hasn't happened. The money hasn't been there to do the West Bay installations and so right away, you haven't quite been able to get to the discrete chemistry out of it.

But hearing what the alluvium was doing and the chemistry of the alluvium on the uranium tailings study, the first presentation yesterday, that's incredibly important work. That tells me that the likelihood of having a uniform chemistry in 40Mile Wash and valley fill sentiments is not likely.

The idea that you might sample it from all the way down Amargosa Farms all the way up the Wash and you use that in part of your modeling as Jim has pointed out is not a very smart way to do that. You rather maybe know the chemistry better in the region where you're sampling for that sort of forecast. So the chemistry story is bound to be very important.

Why? Because we have 40Mile Wash as a episodic run-off from time to time now. We surely have that during the pluvials, the chemistry signatures that you see and the water masses that have been able to be put together by this Center and by the USGS and by the Program basically chose some distinct chemistries and that's been quite important to this whole process. So we say, yeah, chemistry is kind of an important part of this whole story.

As far as colloids, the unsaturated zone colloid story, is frustrating as it's very hard to make sense of experiments that have been attempted in the unsaturated zone. Right away, there has been efforts to do something in the Calico Hills/Busted Butte experiments. Then in that area, it was difficult to capture the microspheres that we release there.

On the other hand, it's not clear that the water samples that were put in from the cross-drift and the ESF tunnel, for instance, did anybody ever sample the waters between those two for colloids? You are putting water in the one and looking down below, but gosh, I would think that you maybe had looked for colloids. Well, you have a dusty tunnel up above and maybe it's an easy experiment to conduct. But when you run those kind of experiments on Niche 1, for instance, and you put simulated rainfall up on top, you ought to be able to see whether or not colloids showed up in that water.

Colloid transport in the saturated zone is critical for the tons of colloids which the waste form is going to produce when it starts to come unraveled. Much of it may be filtered. It may be caught up in the drift shadow and elsewhere in which case you don't worry about it in the unsaturated zone. But then when you get down to the saturated zone, colloid transport becomes an issue again, both in the tufts as well as in the alluvium as it was pointed out.

The on updating the site scale model, we were pleased to hear yesterday that Bill Arnold was saying they were looking at reevaluating whether or not it makes a difference to use the old calibrated USGS three-layer model with a grate orientation that's different from the site scale model and why not go with the 15-layer model which has been on the table with USGS producing that and that's a much better model. I think it can be anywhere from 10 to maybe 50 percent of the flow of the water in a regional model passes through the site scale model and the fluxes of the borders is not too well known.

They can be in and out and in reverse directions and the quantities, but maybe that makes a big difference to how the site scale model would predict in a flow in transport. So we think that needs to be done and we are pleased to hear that it's on the schedule for the program.

Then for the use of natural analogs, obviously we feel very strongly about things like Pena Blanca experiments in terms of just how you might model and be able to deal with essentially this uranium deposit and what moved from it and where the radionuclides have gone in the unsaturated zone because it's step tufts and it's step carbonates at the desert. It is somewhat similar in many respects for what we see for Yucca Mountain, but the analog value we heard from Jim in terms of the alluvium it's in the analog report that the program intends to use the uranium information. It wasn't exactly clear how it would be used. For my way of thinking, you showed us one good way to use and make sure the chemistry is well understood and characterized if you want to get predictive value out of it. The difficulties of a few K_ds, for instance, may be systematic understanding and how the massive chemistry that water varies is kind of an important message for me to take home.

Then as far as -- That's really the next slide. It talks about this aqueous geochemical heterogeneity system that we've been talking about. This business of the flow paths, we heard from Jim Linley that by putting a high permeability feature in the 40Mile Wash area, it helped the model about whether or not there's structure down there and there's a fault or what's down there.

But from one point of view, it's good enough for government. If it works, it works. That would be one thing. But from a Board point of view, understanding why high permeability down there might be a geological feature or not is important. Then you feel better about knowing what caused it or is it really there? So it seems to me that maybe investigating some of the assumptions that went into the ruling more or less, for example, would be of some value. He indicated first he had to make an adjustments, but it's amazing the fit that he got with the model he runs and that did have a major effect on the patterns of flow.

So overall observations, it was an informative meeting for me. Very worthwhile and we hope comments from the West and the tiny differences we see there have been very helpful. We think there's a lot going on in the program. We could talk about all the progress we've seen on the Board that the DOE has made in so many different issues and areas of study.

It's a question of what's left to reduce uncertainty and heighten understanding and have a sufficient science of program going in the future so that by the time the license application is coming for review and before it comes up for all the arguments, the science gets better and better and better because that's what is going to build confidence, I think, with the technical community and the public as I see it. So we're still looking for understanding of the science, basic understanding of the processes and the features that are out there even if it's not necessary from the point of view of compliance, but it does add, I think, to everybody's confidence that we're looking for confidence building incredibility if you want to call it that.

MEMBER HORNSBERGER: Good. Thanks very much, Dick. Ines.

DR. TRIAY: I also thought that this was an extremely good meeting in terms of trying to zero in on the issues that still remain is my opinion when one looks actually at what has been presented here for the last two days. I would like to make comments on two particular topics. One of them is sorption and the other one is colloids.

In sorption, I believe that it would be a good idea to try to concentrate our efforts in trying to address this main issue of can sorption coefficients be utilized to appropriately describe retardation of radionuclides. I would like to further say that it appears to me from solubility data that was presented from sorption data that was presented from other deliberations that were made in terms of what are all the barriers that can prevent the radionuclides from mitigating that from the perspective of whether sorption coefficients can be used to appropriately describe radionuclide retardation, we're down to two elements, neptunium and uranium and whatever isotopes of uranium you want to consider, but from the chemical behavior, neptunium and uranium. I think that it would be good to look at that data that are available for uranium and neptunium and see whether we can make some kind of correlation in terms of available data for neptunium and uranium and the data that exists for site-specific conditions and try to come to some kind of resolution of that question or at least try to understand what would be the path forward to resolve that type of question.

I don't think that, in my opinion, is not necessarily for americium or plutonium based on solubility considerations and the huge sorption capacity that the minerals at Yucca Mountain do have for these elements. So it appears to me -- Oh, and when we talk about technetium or iodine, I think that essentially there we are at a K_d of zero. Right? I mean like what is delegated by Dr. Davis. There's no sense in going to ask that question because it's an irrelevant question. So I think we're down to those two elements and I think that we need to focus on how could that question be addressed based on available data both in the literature with surface complexation models versus site-specific data and try to understand (1) where are we and (2) what needs to be done in order to close that gap or answer that question.

The second point that I would like to make refers to colloids. When one talks about colloids, I think that the only recommendation or thought that I had with respect to how can we, if you will, bound this problem is I was wondering whether it is possible to use some of the information that we have with respect to water chemistry to try to bound that type of concentration of colloids that could exist in order to carry those radionuclids through a system, whether it's an natural system or they introduce radionuclides when the waste gets a disposition into the repository.

So those are my two thoughts. I think that the only other thing that I would like to point out is that I really think that this regional flow of conceptual models that were being talked about by Nye County ought to be commended. I think that it is important that we have a regional model in order to really prove to the public and scientific community that we really understand what happens from a regional perspective. I think that that is a very important effort that ought to be endorsed.

MEMBER HORNSBERGER: Jim, did you have a quick reaction? I saw your eyebrows flinch when Ines -

DR. DAVIS: No, it's not a reaction. She just reminded me. There have been a couple times when I've wondered whether some of these unknown things could combine to create something that isn't being considered. So, for example, since we don't understand colloidal transport very well in the unsaturated zone, I'm not really sure whether this would make a difference to the arrival of neptunium in the saturated zone, but if americium was being transported relatively quickly through the unsaturated zone to the water table by colloidal transport and then that gets a lot of the americium there and then it's as in the Ks to produce the neptunium whether that would make the neptunium arrive faster down gradient, I'm not sure. But it seems that you have two of these uncertain things possibly combining. Just a comment.

MEMBER HORNSBERGER: Let's see. Las Vegas. Is Don back yet? Do we have people from Las Vegas?

DR. SHETTEL: Yes.

MEMBER HORNSBERGER: Don, would you like to give us your summary comments?

DR. SHETTEL: Certainly. I think overall this has been a very good meeting. Several talks have been very useful, especially the first one by Jim Davis. Now I'm going to start some very specific comments.

First, concerning the vadose unsaturated zone, I don't believe the DOE has any substantive experiments that are appropriate for the unsaturated zone. They had used J-13 water instead of using pore water which would be more appropriate, but we still don't actually have any samples for analysis of flow in fracture water. So I don't see how we can even do any sorption experiments that are relevant to the unsaturated zone until we have some of this information. The experiments that have been done have been under saturated conditions and that just adds to the uncertainty in the unsaturated zone.

Regarding matrix diffusion, flows have been injected as has been stated at 100-1,000 or more times the natural rate. If these are into dead end fractures systems or whatever, then some of these injections may have been pressurized. I don't have access to the data so I can't say for sure, but it seems like a possibility that some of this could have been pressurized injections. This would be totally unrealistic. I think the conclusion that one could make from all this is that DOE has nothing in the unsaturated zone for sorption, nothing that is that is credible and defensible.

Regarding the saturated zone, Paul Bertetti's talk was very interesting. It shows a very systematic approach to sorption which I think the DOE could learn from. However, the state has been saying since `80s that J-13 water has been overused in experiments in Yucca Mountain so therefore it's somewhat ironic that the Center has done their sorption experiments not in J-13 water. I think this causes some test in data on their results and applications to performance assessment.

Regarding DOE's experiments in the saturated zone in sorption, at least they have the right water, J-13, although this doesn't necessarily bet on the water compositions in the saturated zone. They have not done any experiments at CO_2 pressures that are above atmospheric. They have assumed sorption without actually proving that they had sorption in those experiments.

I could make some comments on colloids here, but I think that's -- Well, they didn't qualitative models on colloids and a lot more work needs to be done on colloids to quantify those models. There are organic acids no doubt in the saturated zone. This may be at small levels, but the radionuclides that may eventually make it into the saturated zone will be at very small levels too. So there's a possibility of complexing there. These models for sorption don't always include all the possible ligands that can contribute to solubility.

So overall, I would say there is better

data for the saturated zone versus practically nothing for the unsaturated zone. But even the data that's available for the saturated zone is suspect and I think this raises a question of why are we even going to licensing at this point. You know really that is credible and defensive about radionuclide transport, sorption, retardation in the entire Yucca Mountain system. That's all I have to say right now.

MEMBER HORNSBERGER: Thanks very much, Don. Jim Clark, do you have any summary comments that you would like to offer?

MEMBER CLARKE: Well, is it okay to admit that I'm not a geologist?

MEMBER HORNSBERGER: Not everyone can be part of that.

MEMBER CLARKE: For that reason, I have a real hard time with these time horizons and for me, the elephant in the room has always been the daunting task of trying to predict the performance of something that we don't have a lot of experience with over hundreds to thousands to tens of thousands to hundreds of thousands of years.

The research that I do is focused on near-surface containment systems where you're only looking at shorter time horizons, but they greatly exceed our experience base as well. And for that reason, it strikes me that the work that's being done on risk insights and risk informing and building confidence in the areas where uncertainties are known and processes are going to be totally understood, but understood to some extent it is just very important.

I'm just using that tool to increase our understanding and build confidence. So I would greatly encourage continued use of that and really recommend that that tool be used for other system components as well to the extent that the NRC can do that.

I guess I'm still a little confused about matrix diffusion on the unsaturated zone, the vadose zone. It seems like it's important to one group and not being used at all by another group and I'm not sure. Yet everybody seemed to seem that it's quite beneficial as well so there may be looking at that a little harder. Thank you.

MEMBER HORNSBERGER: Thanks, Jim. Any second level comments from any of our panel in reaction to comments made by other panel members? Okay. Committee members? Allen.

MEMBER CROFF: My preference would be if Jim's not a geologist, I'm further out away from the center of geology than he is. But after listening to all this, I came away, I think, with two fairly strong messages. One, to paraphrase James Carville, "it's the neptunium stupid." That's really where the action is at for reasons already stated by a number of speakers. I won't reiterate them, but the importance of understanding that element and how it behaves.

Where was I going with this? I forgot my second. I got so wrapped up in neptunium I've forgotten my second point here. You fed me too much lunch.

MEMBER HORNSBERGER: We'll come back to you.

MEMBER CROFF: If you don't come back to me, I'll remember it. I'm sorry.

MEMBER HORNSBERGER: We want to take the pressure off you. Then you'll remember. Michael?

MEMBER RYAN: -

MEMBER HORNSBERGER: Oh, he has it. I knew it would happen.

MEMBER CROFF: I'm sorry. It's the importance of chemistry, the subsurface chemistry and its apparently very profound effect. I guess what surprised me is that it apparently hasn't been taken into account before. I'm a chemical engineer, but I would have thought it was fairly obvious that the chemistry of the groundwater and these complexing agents will certainly affect sorption, solubility and formation of colloids and all sorts of other things, but I think the clear need to do that and the surprise that it hasn't been done. That was my second point. Thanks.

MEMBER HORNSBERGER: Mike.

MEMBER RYAN: First, John Garrick, chairman of the ACNW, is not here and I know he would want to thank all of the participants and panel members. I know George will do this anyway, but let me offer his thanks to you for giving up your time and working hard to make this a really interesting and helpful working group meeting. I guess I came at this from the health physicist rather than geologist perspective although sitting next to George, I'll probably end up with some sort of geologist certificate at the end of the day.

But from the risk perspective, I think the work that is going on in every corner to do the risk insights work whether it's what Tim McCartin presented from the TPA or what's going on in the TSPA and also what's happening in EPRI in terms of a third independent view of risk insight, it's helpful and important and, I think, in a number of ways. One is whether you call it a margin of safety or whether you call it a conservatism or how you look at it, some prediction of reality and some prediction of a model and try and assess where's the truth in that and gain confidence in that process. I think that ought to continue.

I think it also serves a second function that it can help inform the science in terms of where do we really focus our energy and expertise and resources to answer the critical questions when we may not luxury of so much margin or so much separation from where we think compliance is first or from where confidence is. I appreciate Dr. Parizek's comments on recognizing compliance and confidence as two complimentary, but different endpoints. I think that's an important observation to make. So when we move forward with new work or additional work or complimentary work at any point in the process, I think keeping that structure in mind and those tools in mind to use the help and the thinking is useful.

A third dimension to me is in response to what the representatives from Nye County talked about. That is that the confidence building process can certainly aid some of the points that they raise as global and in terms of communicating and in terms of having a risk tool. So I think those risk tools will have hopefully as time goes on a bigger dimension other than just the review or a partial tool as a review of the license application. I think that's an important thing we heard today that's growing in importance. That's really my main comment.

CHAIRMAN HORNSBERGER: Thank you. Ruth.

MEMBER WEINER: I'm not a geologist and I'm not an engineer either. So my perspective like Dr. Croff's is pretty much that of a chemist. I am very interested to see the role that the chemistry plays, that the interaction of the compounds with the substances or surfaces they can or don't or will or won't absorb on. I think that this is an interaction I, too, am surprised that it is a little late in the game to be studying this, but the importance of this really came home to me in some of these presentations.

The second thing is that I think in structuring inputs to performance assessment we have to be very careful to make sure that the distributions reflect what we know and aren't just a convenient kind of distribution to make. Having done some performance assessment, I have an idea of how complex it is. I want to commend Tim McCartin for his presentation which put performance assessment in the context in which it can be used. I think this was a very important role.

I also hope that the people who are panelists take back to whatever their constituency is the role of risk insights because we've been struggling with what does risk insight mean. I think Tim's presentation was very clear on what it meant to performance and to performance assessment. That was very, very good.

I do agree that we need to look at the role of colloids. We need to study what happens with colloids. Finally, I was so impressed with Nye County's sonic coring. That showed some stuff that I really didn't think you could get out of a core. But that was really a very interesting presentation and very revealing. So those are my thoughts. A lot of other people said things that I agree with.

CHAIRMAN HORNSBERGER: Thank you, Ruth. Let me just make a few comments then and I certainly will open it back up for further discussion by a lot of people. First of all, I was struck in listening to Dick Parizek point out some of the perspectives from the Board on the basis of, of course, what the NWTRB's role is. What struck me was that the ACNW, of course, has quite a different role.

I probably should just point that out for the group just to make it clear. The ACNW, of course, we are to advise the Commission and if you think about what the role of the NRC is, well, Tim McCartin partially pointed it out and also Matt Kozak had the EPA regulation, the reasonable expectation argument, and the ACNW basically, our role here, is to try to advise the Commission and the NRC Staff on how they might review a license application in terms of the regulations.

The ACNW, of course, our charter, we are not driven by compliance, certainly not by blind compliance. That's not our role. Our role is certainly not to ignore science. But I think to just characterize how our role is different from other bodies is that the NRC is faced with reviewing a license application and making a decision on whether the criterion of reasonable expectation has been met.

In Tim McCartin's presentation, he pointed out that for this particular working group meeting focused on the geosphere, the way this enters is in the multiple barriers idea that the geosphere is -- It doesn't suffice to say that we meet the 15 millirem standard because we have an engineered package that will persist for tens of thousands of years. The part of the regulation requires that the geosphere provide a significant barrier.

The focus, our question, here has been "Well, all right. Where do we stand in terms of the data, the models, the analysis that can contribute to a judgment that, in fact, the geosphere does play a significant role in retarding the potential migration of radionuclides away from a repository?" I think that what we learned is -- We certainly learned a great deal. We've heard some of the presentations. Some of us had seen some of the material before from the Department of Energy in their analysis.

We've seen how the Center has treated some of the groundwater modeling and the geochemistry in support of the NRC's TPA model. Again the TPA model is not aimed at building a safety case, but in fact at gaining insights that will be valuable for the NRC Staff in their evaluation of a license application assuming that the Department does submit that license application.

I don't need to go over some of the material that people have pointed to. I think that there certainly are remaining uncertainties. There is certainly work that can be done in an attempt to reduce those uncertainties. I think that Jim Clarke pointed out or started to point out that continuing work to build confidence, to learn about how to build confidence as our horizons go well beyond human experience, will be important.

This will be important as a continuing effort even if the license application does come in and get docketed and even if it is approved. There will be a need for continued work to basically build confidence in our knowledge of the hydrogeology and the geochemistry of the site.

If it turns out that other people don't want to weigh in, I certainly do want to make sure that I offer my personal thanks to, first of all, the panelists. I definitely appreciate everybody's taking the time out of busy schedules to come here and help us out. I also appreciate the participation of all of the people who have made presentations, Nye County, the Department of Energy, NRC Staff. Again, I realize that it takes effort for people to support our working group meeting and the ACNW greatly appreciates that involvement. So --

MEMBER WEINER: One more time.

CHAIRMAN HORNSBERGER: Absolutely please. It's open.

MEMBER WEINER: I, too, forgot one point I wanted to make. I wanted to thank Matt Kozak for clarifying what we all struggle with which is the question of how conservative is too conservative. How conservative is a bounding case? What is the role of a bounding case and what is the role of realization? I think that's something that is an overarching set of principles we have to keep in mind. But I also want to thank everybody on the panel. It was great.

CHAIRMAN HORNSBERGER: Other comments? Neil, I haven't give you nearly either enough credit for organizing this nor a chance to contribute fully. I typically would have asked you more frequently for your comments and questions, but managing a time with a fairly large group on the panel, I've tended to ignore you. But here's a good opportunity for you to weigh in should you want to.

MR. COLEMAN: Yes, sir. One thing about colloids, and I'm going to say that I'm not a geochemist but as a hydrologist, I found the study of places that have had contaminant transport that it's much easier to study a site such as Hanford site where unfortunately many contaminants were released. The flow system at Hanford is known to a great detail and might even bear their aspects of the hydrology. The art is well understood as DOE might hope. But the unsaturated zone hydrology there is fairly well known.

I would just say with respect to Colloidal transport of transuranics here's where analog studies may have a very special use because is there any place in the world where plutonium or other transuranics have been seen to migrate significant distances under natural conditions without a 1.3 megaton assist. It's a fairly simple test. Under the great variety of geochemical and hydrological conditions anywhere in the world, is this known? So I think analog studies have a role there. That's all I would add.

CHAIRMAN HORNSBERGER: Dick.

DR. PARIZEK: Yes. Yesterday I asked Tim McCartin whether NRC has remained the same. You know they had certain rules and regulations that go way back in the early days and it's almost like nothing has changed since that time, but obviously a lot has changed. I think the idea of this risk-based analysis. Perhaps for the first time, I may have heard that from Tim and others would be five or six years ago, something like that, but the clarity of what's intended and how to deal with this was probably best presented today than it's ever been. So if you've heard earlier versions or early insights, it's come a long way.

There wasn't a standard, right? The EPA didn't issue its standard until somewhat recently. So if you think through this time period, a lot has happened from an NRC perspective. I think there would be other comments you might want to make about "Well, what else is new in NRC" besides the work that's being done on ability so you can make your forecast so that you can really review someone else's material when it finally comes in.

There's another aspect of this. I didn't comment on flow line sampling. Right now, there's a flow line model and there is chemistry in the flow line model. There is the model predictions in the flow line model. And of all of this, I would suggest that the flow line model is more or less like that.

Now is the time to maybe go out and drill out the flow line model in order to look for the freak mixing and for the isotopic composition of the waters and so on to find out if you in fact can get closer to so-called validity that flow line model, pieces of it, the part of that that's quite important and maybe it does go southeast and south. I just want to make sure it doesn't go straight south in which case we have a conceptual problem here. I raise that point.

But the flow line model might address the point of these 40 meter per year washes that has been talked about. There are a few places where the data seems to support that. Is that real? And then when you talk about travel of a couple hundred years if you're just plain old water going down through to the cup (PH) compliance boundary, that seems like that's a little bit too fast.

From a geological point of view, we look at the rocks. We look at the alluvium. We crawl around there and say "I don't think water goes that fast through this system. Am I off?" And I look at all of the things, maybe the conservatisms, that are not definite in the modeling and I said "I don't think it can go that fast and some the radionuclides can't go that fast either."

But you want to build some understanding that maybe 100 years, 200 years, is not realistic and although the runs show it, the combination of variables that make that run come out that way may not be realistic at all in terms of this other viewing of it. That were my feelings asking Tim on that matrix table "How do you explain all of that to someone?" And can we explain away the things that aren't realistic. The run that was 100 years, I can't too excited about that, but I think we will throw it out and say "I don't think that's credible."

It's almost like bounding calculations. It's almost as bad as that. So I'll take 100 or 1,000 years on it. I don't want to take 100 or 1,000 on the other. Can you throw some of those out on the basis that it's not a reasonable combination although the computer gave you that mix and you can't throw it out exactly.

But maybe that's the correct one. Maybe that's the flow in the system and how do we then look to see if that's the weak link the chain and that's not an easy thing to do. But when it gets to 200 years, I want to say "Can I understand that? Tim was telling us to do that. Look at that. They would do that and say this is not possible because look at all the things that have to happen to have that come out that way." Even though it might be lost in the computer, I don't know what quite combination went in there to give me that forecast, but that's the kind of analysis that I would be inclined to want to make.

And that's beyond then the TSPA thing. It's the safety assessment or the safety case, right? And the program has to make a safety case for the general well-being of the public, the people who can't understand the models necessarily. Their eyeballs roll back in their head and they look at the mathematics of it and it's very complicated stuff. You're looking for something else that you can deal with that has some reality in people's opinion.

So how do you build safety case with independent lines of evidence? The Board has said use independent lines of evidence that add credibility to the forecast. We use analogs and we like to see all you can get out of analogs, not just sprinkling them in the report, but the idea of the analogs of this uranium tailings pile story, there's some really meaty stuff there and there is the analog value that we would maybe get out of that sort of a study at that level of detail. What you should now go do in the groundwater chemistry characteristic to really get the full value out of that message that comes out of that. Even though it's a different setting, there's usual data there. That's what I get from this sort of story. I found it a very useful meeting.

CHAIRMAN HORNSBERGER: Okay. Good. Dick, actually I was going to ask you if you or others on the Board have looked at the hydrofacies, the data that go into the hydrofacies we hear about from DOE and whether or not those data are sufficient to at least give you a warm feeling about the water not going directly south.

DR. PARIZEK: You look at all of the different combinations of information, the most recent of which those fixing models, each of which takes the general chemistry and sort of sees whether the pathways make sense so that rocks with the warmest, most have been in. I keep looking for some kind of unique thing that ought to have and then I can look for pollution dispersion. That's sort of what the C-well testing might be. You dump a lot of water to given discrete points. At least, the points is where maybe that game can be played. I'm pleased to see that the program is picking maybe on doing something with that.

But to chemistry, the background work that was done over the years is kind of gurdy chemistry in a sense. A borehole was opened to different horizons that was mixing between the boreholes and if you talk to people and the survey is out, you can say that some of these holes are not the best, but they did what they could do with the data they had and carried it as far as they could. So you have these patterns of flow which is consistent with rock-work chemistry and our action stuff. They put the isotopic data on there.

All that's giving you now this flow line model which is chemistry with an isotopic point of view and from a model forecast point of view and now's the time to say "Well, can we independently go and test it?" We've already tested it and used up all the data to do that. What new data are there? Maybe that's where this speed drilling could be done to be confident that this flow line is in fact the flow line that you're dealing with.

To add the Carbon-14 in the story like that, how does this -- If you knew the young water as people have covertly pointed, then you could tell how old the water was? There's only young water there and it assumes that is if you mix it, then you come up with some kind of other anomaly. But find the young water and then you'll know how old the old water might really be.

Or if there is no young water, that would be good news. Right? But there seems to be young water in the system right underneath the footprint as well as in the FortyMile Wash and elsewhere. So where does the young water come from and that's part of the story you try to struggle with.

So the chemistry has come to a sophisticated level but there are experiments that people in the program would like to run I think to clean up their understanding and now maybe is a good time to do because you could do discrete big tests of critical areas in order to test these hypotheses. That's maybe the science and technology program funding. I'm not sure where the money would come from. There's always a possibility that no money would come from there. I would argue that in order to have this program develop the credibility it needs to go forward, survive all of the tests, the debates and all of a sudden, the community. Anything to strengthen the understanding of a complicated system is helpful because who said "When you go in the field, this is huge place." But on the little plot and you get this cluster of data when you have hundreds of acres in the area. But when you get in the field there's this big territory so there's an awful lot of stuff in between where you don't quite know what's in there.

The groundwater model has something in every box. Every box is full with some kind of rock that that may or may not exist there, some hydrologic properties which may not be correct but they are in the box. Every little box has one, but everyone can vary them. Then you start playing games with variation and you wonder "Well, I wonder how this outcome finally came out. Do I feel good about this?" And people ask me. I feel a lot better today than I would have felt some years back.

But the multi-layered models are a lot better than a three-layered model. Right? When you go back through time and realize that progress has been made, you get the full value of it. Even if you don't want to do that for the performance assessment, you need this for the Nitroni (PH) perspective in Clark County and everybody else who wants to use the models for other reasons. But the models that exist there now won't predict what level change in Devil's Hole to 1.2 feet or something which kills pupfish or something. Right?

So I mean you can't get that good on that regional model, but you could do little subset models which need the regional model for strength. That's why this ought to be a moving model system out there that you have to keep going for all the different reasons that need to be made on the land in that area and the water resources of that area.

And it's in conflict. You draw the water and the water has haddock and springs and pupfish and the other and it was a contradiction. And every drop of water in the desert comes from somewhere and you take a drop from one place and the system tells you about that. That's what's going to happen here.

CHAIRMAN HORNSBERGER: That's right. Other comments? You know you indicated, Dick, that ideally of course what we would have is some unique signature that originated in the Yucca Mountain vicinity that you could then trace and there probably isn't such a thing because even the rare earth, I think, would probably not be unique in any sense, would they?

DR. PARIZEK: Programs used to look for signatures like that. The only thing is what you put in yourself other than living 200 years. Two hundred years won't do a thing in terms of putting the repository in. You're not going to see anything in 200 years necessarily.

CHAIRMAN HORNSBERGER: We hope not.

DR. PARIZEK: So I don't where those signatures would come from, but we keep thinking about where would you find them. For a while, it would be the Amargosa River coming out of the Beatty area how to have some distinct chemistry and then it goes into the Amargosa Desert and releases a plume, but it does run around the side somewhere. So there are some patterns you get out of these, at least, this cohesiveness in some of the patterns you see there.

CHAIRMAN HORNSBERGER: Anyone else? Any other comments? Any comments from our people in Las Vegas?

LAS VEGAS PARTICIPANT: Yes, we have one.

CHAIRMAN HORNSBERGER: Okay. Please.

DR. MEIJER: My name is Aaron Meijer. I have been involved in the social program for some -- I just want to respond to the comment that chemistry hasn't been enclosed in the derivation of sorption coefficient distributions.

LAS VEGAS PARTICIPANT: Back off from the mike. Is the mike on?

DR. MEIJER: At any rate, the fact is that we have incorporated chemistry into the derivation of the absorption coefficient distribution, both the SZ and for the UZ in different ways for the two. In addition, we've also done a surface complexation modeling that Jim Davis and Paul Bertetti talked about yesterday. I wasn't here for that, but I'm pretty well familiar with what they've done. Those sorts of things are incorporated into our thing on sorption behavior, certainly in the volcanic section, but also in the alluvium. We didn't have a chance to make a presentation on all of that, but I think if you heard the presentation and presumably you'll see this stuff in the license application, you'll find that this information is in there.

I would also like to make a comment regarding the applicability of the UMTRA site, the Naturita site to the Yucca Mountain site. I've spent some time looking at various UMTRA sites as possible analogs and invariably what you find is that these are rather distinct chemical sites. Basically, you have some sort of plant that produced some toxic material, either very alkaline or very acid depending on the site and that's stuff then was dumped out in tailings piles and that went into the subsurface. So you end up with a quite unusual water chemistry, if you like, as source term. This is not likely to happen in Yucca Mountain.

So I have some question about the applicability of certainly Naturita to the Yucca Mountain site. I think the approach is useful and Jim has done a great job in applying it to Naturita and Jim has done a great job in many other ways as well in developing these models. I think in terms of the direct applicability of Yucca Mountain the link is not altogether clear or direct.

Then finally some of the comments that Don Shettel has made, I think we have responses to a lot of his comments on the assumptions regarding sorption and that at some point in the future we'll probably talk about. We do have information that bears on all of those assumptions. That's all I really have to say.

CHAIRMAN HORNSBERGER: Thanks very much. We are open by the way for comments from anyone. So I guess on the schedule this can be the Public Comments section. Steve. Do I see you have your name up?

MR. FRISHMAN: Yes. I have just a couple comments on the two places risk seems to be most evident in the conversation. First was Matt Kozak's presentation. I think he tried to draw us the distinction between "reasonable expectation" and the Commission and DOE's approach which is a much more compliance-based approach.

Before you go too far with trying to adopt this line of argument, it's important to remember that in the lawsuit that the State of Nevada filed, one of the issues in the case in the filing against the Regulatory Commission had to do with the meaning of "reasonable expectation" versus the traditional and continually used "reasonable assurance" by the Commission.

But that portion of the lawsuit is over. The Nuclear Regulatory Commission conceded in their response that reasonable expectation and reasonable assurance mean the same thing relative to Part 63. So before you go too far trying to draw any differences, remember that this issue has been litigated and it has been confirmed that reasonable expectation and reasonable assurance in its currently understood meaning is the standard for Part 63. I just wanted to let you know that in case you didn't so you wouldn't get too far afield.

The other has to do with Tim McCartin's presentation that I believe deals a misguided risk perspective. The title includes "Risk Perspective" but if you look what's going on here it doesn't do what would be expected. If your mission is to risk-inform the decision-making, then this approach really doesn't do it.

The reason it doesn't is because the real risk of the repository is not what you put in it. The real risk is what gets out and gets to the accessible environment. This presentation doesn't reflect that real risk. And I think it's in any great dispute that technetium and iodine are the big players in the dose in the first, maybe up, to 100,000 years and beyond the peak dose is primarily driven by neptunium. This presentation doesn't indicate that that would be the case.

In fact, it doesn't really tell you anything other than for long half-life radionuclides that have half-lives beyond 10,000 years you ought to pay a lot of attention to the plutonium. There may be a need to, but we don't know yet and we don't know that extent to which we have to because of the whole question plutonium transport as a colloid. It doesn't tell you in any way that the neptunium is what you really have to worry about because of its chemical characteristics in the Yucca Mountain site environment. It doesn't tell you that technetium and iodine are what you really have to worry about because of its behavior in the Yucca Mountain environment.

So if you're looking for new ways to communicate a risk perspective, it ought to at least tell you or be responsive to what the risks are rather than clouding the issue with things that either we don't know or things that are wrong. For americium, I think it's also not argued, but americium is a big player if you have volcanic disturbance early in the history of the repository. So, yes, it is a big player. It suggests that it's a really big player for one that has a half-life when in fact the primary delivery of the dose of the delivery made of the dose is going to be groundwater. So if you're going to get into new ways of communicating a risk perspective, it should at least tell you what the risks are and why you believe they are the risks rather than clouding the issue with things that, yes, they matter but we understand at least to some extent, we understand some of them, as to why they really matter, but they are not the most important issues in a risk discussion.

CHAIRMAN HORNSBERGER: Thanks, Steve. I think that perhaps I will speak in Tim's defense for a moment. Okay? In part, Tim did his presentation in response to some urgency of the ACNW after Tim had presented a full analysis of the TPA which included the full assessment of dose calculated to be delivered to the reasonably, maximally exposed individual.

Basically his presentation to really understand it I suspect, you would have had to have previous two presentations and of course, we never have time to do everything sequentially. So I don't think that Tim's presentation was meant to be a standalone presentation as characterizing a full risk assessment for Yucca Mountain. Basically, it's in response to a charge or really a desire to on his part and on the part of the NRC Staff to work to communicate some fairly specific things and that is how the geosphere, in particular, the saturated zone, impacts the understanding of how various radionuclides are impacted by the geosphere. So I take your point, but I just wanted to clarify that that your criticism, I think, isn't quite on target because you perhaps misunderstood what Tim was asked to do here.

MR. FRISHMAN: Let me comment on this because I've essentially this same presentation from Tim before not in the context of a geosphere workshop. In fact, the reason that he put in the table of those with half-lives over 10,000 years is because I discussed that very issue with him after his last presentation of this. So I'm not arguing that there is a current context for this, but this has been presented as a standalone before and my guess is that it will be again. So I understand what you're saying. Then maybe if it's going to be presented, it should be presented in the context as you say rather than giving the appearance that it is a standalone.

CHAIRMAN HORNSBERGER: Tim.

MR. McCARTIN: Yeah, Tim McCartin, NRC

Staff. I take the encouragement and criticism, Steve. I can do a better job of explaining some of it, but George is right. There's a lot of context here. I did say that when you have the dose assessment and you have the dose number, you clearly see that it's iodine and technetium. So you're going in and you have that overall "Okay."

The question is why and what else is going on. I would say that I would have a different perspective from the iodine and technetium dose if indeed iodine and technetium were an enormous part of inventory and indeed the models for release were such that I was releasing a small fraction of that inventory and it was being retarded and it was still giving this dose versus the situation we have.

It actually is a very small part of the inventory. We actually have a lot of iodine and technetium in the gap that is instantly released. In spite of that, you can see the doses are generally relatively small, but we are moving a lot of, a fairly large fraction of the percentage. It's not the release rate. So there's a lot of processes going in there that you use in helping you understand where is the bigger issue.

I will go back to release rate affects the neptunium. Solubility affects the neptunium. Transport affects the neptunium. And there are assumptions there that tend to be fairly relevant. For some of the other nuclides like plutonium, americium, there are a very large fraction of that inventory and I think you want to understand why aren't they getting out. So that's the context.

In subsequent presentations, I think it would be worthwhile to provide that total system look. But I believe this is a way to provide some information to understand how the system is behaving. I think it is important from a safety perspective. The inventory for plutonium and americium is enormous. The potential health consequences are very large and to understand why that doesn't get out is a very important part. I agree with you that indeed they don't get out, but we sure want to understand why and have confidence in why. So that's a useful context to provide.

MR. FRISHMAN: One follow-up comment on that and I won't belabor that any further. One of the reasons that I was interested in this is because I've looked at the DOE's TSPA output for releases from the repository and if you look at knowing the inventory of technetium and iodine and then look at the projected release curve and then back-calculate that to a dose, you're looking at a dose that is very close to the groundwater standard. That's one of the reasons that I'm concerned about it.

Sure. It's a small part of the inventory, but it's enough and the way it's released is big enough to actually raise the question of whether this site meets the standard or not. That's one of the reasons why I'm so concerned about a presentation like this that doesn't make it clear that the amount of inventory isn't necessarily the most important thing relative to risk and dose.

CHAIRMAN HORNSBERGER: I think we'll let it there and believe me, there is nobody that disputes that contention, Steve. The standard is the standard and people have to look very carefully and the NRC will look very carefully at a case that the Department of Energy makes for the safety of the repository and none of what Tim presented changes anything on that front. Other comments? Dick, did you have a response?

DR. PARIZEK: I was just ask Tim whether or not Table 15 includes, say, a volcanic scenario or was it not in the thought process when you were trying to erect those tables? In worst cases, it's going to be some sort of failure of the waste package in transport through the saturated zone and so on.

MR. McCARTIN: It doesn't include.

DR. PARIZEK: It almost has to be a footnote of what's not in there.

MR. McCARTIN: Yeah.

DR. PARIZEK: It's the "normal" behavior of the repository.

MR. McCARTIN: Yes. A water release.

DR. PARIZEK: Rather than explosive release or something. But that's a whole different analysis which could then drastically change which I guess is what Steve was also pointing out.

MR. McCARTIN: Sure. And one thing if I could clarify and I'm not sure I make it as clear is in that table where I have the Ds. A single D means at least 100 years, but less than 1,000. So it could be anywhere from 100 up to 999 for what it's worth. I didn't mean to imply -- I forget exactly how I explained it. It could be as low as 100, but it could be as high as 999 years.

DR. PARIZEK: And the blanks again? When you said it, I couldn't write it down fast enough. My mind lost it.

MR. McCARTIN: Well, the blanks would be

less than the lowest measure there. Now for the delays, there weren't any blanks where I had delays. So it would just be for the release rates.

CHAIRMAN HORNSBERGER: But had it been less than 100 years, there would have been a blank in the delay.

MR. McCARTIN: Correct.

CHAIRMAN HORNSBERGER: Other comments? Ines.

DR. TRIAY: I have one. I would like to take the opportunity to -- Do you want to go first? I wanted to ask a question on the follow-up comment of Dr. Aaron Meijer. Could you tell me? You were saying that you had done some of the surface complexation modeling similar to what was presented during the first day of these two days. Can you explain what has been done on the surface complexation modeling for the element neptunium and how did the sorption coefficient approach fare given your sorption modeling with more sophisticated models?

CHAIRMAN HORNSBERGER: In twenty-five words or less.

DR. MEIJER: I think we can probably spend the next two weeks talking about some of the details, but in any case, the bottomline is that surface complexation modeling we used is a freak C-based model which is basically consistent with the stuff that Jim was doing as far as I understand it. We used some parameters. That is used surface areas with rocks at the site and we also used parameters from the Center, from Paul's work and from the Center's work, in particular, on neptunium and binding constants onto silica and also a site density on silica.

Basically, I assumed that because the rocks at Yucca Mountain, certainly the volcanic rocks, are something on the order of 70 to 80 percent silica on a whole rock basis. I used silica as a substrate to do surface complexation modeling. So then with a surface area with a site density with a binding constant from the Center, you could the model, the sorption behavior of neptunium, for different water chemistries and you can vary, as I did, the pH data, the PCO_2 and vary whatever parameters you want to vary.

At any rate, we did go through the work for J-13 compositions and for p#1, we found it to be if not bounding certainly representing a good part of the range water chemistries at Yucca Mountain and we came up with these surface complexation model results which seem to be in the range of the experimental data that was available for rocks with the appropriate surface area and mineralogy. That is for instance a deepage by tuff. We have results for that.

So basically, the surface complexation modeling collaborated the experimental data and allowed us to extend that experimental data over a wider range of pH and other water chemistries. Overall, I was very satisfied with the correspondence between the modeling and the experimental data particularly for experiments that were run at longer durations. Is that the sort of thing you were after?

DR. TRIAY: That's exactly right and I would like to just point out to the members of the ACNW that the comment that I was making before was to do any exercise like the one that has been described but not presented and probably if you have an interest in exploring that, you need to take that, I think, into account before a final recommendation on this particular matter. I think that that didn't come out in the presentations that were given here in the last two days.

CHAIRMAN HORNSBERGER: That's a good point. I think that I saw Judy Treichel ask for the floor. Do you have a comment, Judy?

MS. TREICHEL: Yes. After all this time,

I think you need to get another citizen voice. I count Les Bradshaw as being a spokesperson for Nye County and there are a lot of citizens who pay attention to what goes on in these meetings, but aren't directly here.

I was glad to hear that four millirem was finally mentioned because that is what we understand as the groundwater standard and if this stuff gets to anybody, it has to meet a four millirem standard. I always kind of talk with a laundry list here because I take notes as I go along. But we're very concerned and in fact, I've written a letter. So many of these meetings sound like there's already a review going on of an LA (license application). There's sort of an assumption of what will be in there.

Of course, there can be an assumption because this has been talked about so much and DOE is continually being told "Now what you need to do when you file this LA if it's to be successful is this, this and this." But the state, the effective units of government and the citizens who are concerned and want to be involved in this and plan to be involved one way or another are never told "Now what you need to do about your concerns is this, this or this." I have written about that. I just wanted to mention it.

Every meeting, no matter if it's the ACNW or a technical exchange of the TRB meetings or whatever, generally ends with "I'm so glad to see that work happening. It's a little late in the process, but it's good that it's happening. We need more information about this, this and this." Here we are coming screaming down in this incredible race toward a license application that's really very silly to be doing it that way.

There was conversation about conservatism versus reality and I would play it out from a citizen point of view. The repository located within the Death Valley groundwater system is not realistic and you have a balanced use of water out there right now where it supports the kinds of things that the people who live out there want supported.

To introduce a repository into that is not realistic and there are so many uncertainties associated with it that there has to be high levels of conservatism. When you are using a risk-informed, performance-based kind of process here where you're talking about something where you have no track record, no performance, and you're not really all that sure what the risks are because of all of the uncertainties, the least you can do is be very, very conservative.

To talk about it being realistic, I think is disingenuous. It's sort of silly and when you ask people about analogs, they can look at a lot of analogs because what they fully expect is some time out in the future, you're going to have analogs for this thing that you could find today at Hanford, at Savannah River, at INEEL, at Oak Ridge. You can list them forever about things that were not supposed to happen with radioactive materials and they did.

What we're worried about is that the first indication that something went wrong is the Remy's kid who just doesn't seem to be very healthy. I wonder what's wrong with that kid's immune system. So from a public perspective, I think those things need to be said and need to heard. Thank you.

CHAIRMAN HORNSBERGER: Thanks, Judy. Other comments? Anyone else in Las Vegas? Anyone else here at the table? The Center? Oh, I forgot San Antonio. Anyone from San Antonio want to make a comment?

MR. PABALAN: No comments from here.

CHAIRMAN HORNSBERGER: Thank you. All right. Bobby, you're the only one there.

MR. PABALAN: I'm the only one left.

CHAIRMAN HORNSBERGER: All right. Well, I'll draw this to a close then. I'll just once again thank everyone. I think I gave my summary comments earlier and I'll just say thanks again for a productive meeting. What we are going to do is have a 15 minute break now. We will go off the record and the Committee will reconvene in 15 minutes at 4:00 p.m. Thanks again. Off the record.

(Whereupon, at 3:43 p.m., the meeting of the Advisory Committee on Nuclear Waste was concluded.)