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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

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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

(10:03 a.m.)

ACTING CHAIRMAN RYAN: The meeting will come to order, please. Thank you. This is the 151st meeting of the Advisory Committee on Nuclear Waste. The meeting will come to order. This is the first day of the 151st meeting.

My name is Michael Ryan, Vice-Chairman of the ACNW. Chairman John Garrick is unable to attend. The other members of the committee present are George Hornberger and Ruth Weiner. Also present are consultants Allen Croff and Jim Clarke.

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

John Larkins is the designated federal official for today's initial session. I believe that he is not present at the moment. So, Howard Larson, you will serve as the designated federal official for today's opening session. Thank you.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. We have received no 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 your wishes known to one of the Committee staff. It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so that they can be readily heard.

Before starting the first session, I would like to cover some brief items of current interest. First, Dr. Latif Hamdan officially rejoined the staff on June 7, 2004 as senior staff scientist. Dr. John Flack will join the ACRS-ACNW office staff as a senior technical adviser in July and should be present at this meeting.

Dr. Bruce D. Marsh has recently been added as an ACNW consultant. Dr. Marsh is professor of igneous petrology in the Department of Earth and Planetary Sciences at the Johns Hopkins University in Baltimore. His research interests also include geophysics and magma dynamics. Dr. Marsh brings with him an impressive set of academic credentials, and the committee looks forward to working with him.

I would also like to recognize our remote locations. And we are set up from San Antonio at the Center for Nuclear Waste Research. Welcome to the center. Can you see and hear effectively?

DR. SHETTEL: Yes. We can see and hear effectively.

ACTING CHAIRMAN RYAN: Wonderful. Technology is working with us. Thanks very much.

MEMBER HORNBERGER: Also Las Vegas.

ACTING CHAIRMAN RYAN: Also Las Vegas is available as well. So thank you very much.

Without further ado, I will turn the working session over to Dr. George Hornberger. Dr. Hornberger?

MEMBER HORNBERGER: Thank you, Mike.

As Mike said, today we are going to have a working group meeting on geosphere transport of radionuclides. The part 63, the regulation, requires that the NRC evaluate any license application to ensure that there are multiple barriers that are effective in the system. And the geosphere does have to function. It is supposed to function as part of the overall system to provide safety.

And so we convened this working group meeting to hear the latest information that is available. And basically for the NRC, the question will be as stated in the agenda. For Yucca Mountain, do the conceptual models, mathematical implementations, and site data, provide confidence

that the geosphere can retard the transport of radionuclides.

We have a distinguished panel. Most of the panel is here. And Neil tells me that we have one remote panelist, but he's not really a remote panelist, just a panelist who is joining us remotely from Las Vegas.

I will actually let Sharon Steele introduce the panel members for the record.

MS. STEELE: Okay. Thank you.

For the record, it is my pleasure to read the bios of the panel members. First, to my far left is Dr. James A. Davis. He's a senior research hydrologist with the U.S. Geological Survey. He has directed a long-term research program on the fate and transport of contaminant metals and radionuclides in groundwater.

He has served as Program and Division Chair of the Geochemistry Division of the American Chemical Society and as an Associate Editor of the journal *Water Resources Research*. Dr. Davis has also served on the technical direction team of the Sorption Project for the Nuclear Energy Agency, OECD, in Paris.

To Dr. Davis' right is Dr. Richard Parizek. He is a professor of geology and

geoenvironmental engineering at Penn State. He is President of Richard R. Parizek and Associates, a firm of consulting hydrogeologists and environmental geologists. He is also a registered professional geologist.

On February 11, 1997, President Bill Clinton appointed Dr. Parizek to the Nuclear Waste Technical Review Board. In 1990, he was appointed to an administrative law judgeship on the Atomic Safety and Licensing Board Panel of the U.S. Nuclear Regulatory Commission. He left that position upon appointment to the Nuclear Waste Technical Review Board.

Please note that when members of the Technical Review Board speak extemporaneously, they're speaking on behalf of themselves and not on behalf of the board. When stating a board position, it will be identified as such. And that position will generally be published and available on the NWTRB Web site.

Over to my far right is Dr. Ines Triay. She is the Deputy Chief Operating Officer for the Department of Energy's Environmental Management Program. Her work is heavily focused on coordinating the environmental management's high-level radioactive waste program and providing technical expertise to the

Office of Civilian Radioactive Waste. Previously she managed the department's Carlsbad field office in New Mexico, where her work focused on solving problems associated with radioactive waste.

Dr. Triay began her career as a postdoctoral staff member in the Isotope and Nuclear Chemistry Division at Los Alamos National Lab. She progressed through many positions to Acting Deputy Director of the Chemical Science and Technology Division and to group leader for the Environmental Science and Water Technology Group.

At Los Alamos, she researched and developed various techniques for removal of radionuclides from the environment and led the team that was responsible for the first transuranic waste to be shipped to the waste isolation pilot plant, or WIPP, which began operations in March of 1999.

Joining us from Las Vegas is Dr. Don Shettel. He is Chairman, Vice President, and Senior Geochemist with Geosciences Management Institute, Incorporated in Boulder City, Nevada.

He has been a scientific consultant on high-level nuclear waste disposal since 1986. Currently he is a consultant to the State of Nevada and has consulted with Nye County in the past. He has

also consulted with Chatham County, North Carolina on low-level radioactive waste disposal, the Saskatchewan Environmental Research Council for High-Level Waste Disposal in Canada, and with the Minnesota Department of Natural Resources and Mineral Exploration.

Before consulting, Dr. Shettel was a senior research geochemist with Exxon Production Resource Company in Houston, Texas and a senior geoscientist with Bendex Field Engineering Corp. in Grand Junction, Colorado, where he worked on the National Uranium Resource Evaluation Program.

Dr. Shettel has advanced degrees, Master's of Science and Ph.D. in geochemistry and mineralogy from Penn State, and a Bachelor's of Science in geology from the University of Michigan.

That's it.

MEMBER HORNBERGER: Thanks very much.

I'm very pleased that we have this panel joining us. I think what we are going to do now is just start right in, jump in. Jim Davis is going to do the keynote presentation on a new approach to modeling retardation by sorption at the field scale.

DR. DAVIS: Thank you, George.

I want to thank George and the other members of the working group for inviting me.

I hope to present this new approach that we have used in a modeling for potential performance assessment demonstration of a low-level waste site, but I think the approach that we used could have some scope for thinking about a new way of thinking about modeling and the geosphere transport problem at Yucca Mountain.

I do want to acknowledge my co-author, Gary Curtis, who has done the reactive transport modeling simulations I will be showing. There is also an army of other people that have helped, especially with the fieldwork. This research was supported by the Nuclear Regulatory Commission, the Office of Regulatory Research.

Next slide, please. Now, whether it's low-level waste or high-level waste, the goal here is to get to a performance assessment evaluation. At least part of that assessment is evaluating when radionuclides reach a receptive audience or population. With respect to the saturated zone of an aquifer, we would like to do this with a reactive transport model that takes into account both groundwater flow and dispersion. And in the ideal case, we would be able to incorporate as much information as we know about the chemistry and even

the microbiology that would influence the transport of whatever contaminant or radionuclide that we're interested in that causes a risk.

So the ideal situation is we build in some very detailed knowledge of our reaction processes and we would be able to put that together with a flow model that worked at the field scale. That is ideal. And we will see where we have to make simplifications in the process in order to reach an answer.

Next slide, please. I am going to speak solely about sorption during my talk. Well, there is a small part where I may be talking about reduction, but for the most part, I'm going to be talking about sorption and how chemistry affects sorption.

If we look at the classical definition of retardation with a linear distribution coefficient, we see when you have transport down a column, if you have a nonreactive tracer, this is the concentration of the nonreactive tracer that has been introduced in a pulse. And its dispersion or mixing causes its concentration to vary along the front. Then a sorbing solute is retarded. And its transport is not as fast as the nonreactive tracer.

We can describe the retardation in terms of the porosity and the bulk density and a

distribution coefficient, $K_d$, if this parameter holds constant. The $K_d$ is defined as the absorbed quantity of the absorbing radionuclide divided by the dissolve concentration of the radionuclide.

Next slide, please. Now, the problem with elements that have complex chemistry, some radionuclides have very simple aqueous chemistry. For example, cesium and strontium, when they are dissolved in water, they are dissolved as the ions. They typically do not form other types of aqueous species.

The actinide elements, however, have a very complex aqueous chemistry. And that causes $K_d$ values for the sorption of elements like uranium to be quite dependent on the chemistry of the water. So, for example, what is shown here is the log of the $K_d$ for uranium absorption onto amorphous iron oxide or ferrihydrite. And you can see over this rather large pH range it varies by a large amount, by many orders of magnitude.

But the thing I really want to point out is not so much the overall pH dependence but the very important dependence on the carbonate concentration, which is shown here where all of these solutions are equilibrated with either air, the partial pressure carbon dioxide in air, or an atmosphere that is one

percent $CO_2$.

Now, one percent $CO_2$ is a very common value for groundwater systems, which actually can go much higher than this as well under natural conditions and even higher in contaminant plumes. But the important thing is to look at how much for a system that is a one percent $CO_2$, a common value in groundwater, what that does to the pH dependence in the region between seven and eight, which is a very commonly observed pH range in natural waters. The $K_d$ for uranium varies by four orders of magnitude. So this is a problem for an approach where we would assume a constant $K_d$ value.

Next slide, please. This is a graph of the uranium aqueous speciation for a specific set of conditions; that is, ten micromolar uranium, which is about two parts per million of uranium. This is relevant to concentrations you will see later in the talk.

And if you go back one slide, the reason the absorption or the $K_d$ comes down at this high pH range, next slide, is because of the formation of aqueous carbonate complexes, which effectively pull the uranium off the surface. The uranium would rather be dissolved in solution with these carbonate ions

than absorbed on the surface.

So these red ones are the carbonate complexes. The blue ones are multi-nuclear species that complicate the uranium aqueous chemistry as you get up to higher concentrations. This is all again equilibrated with the partial pressure of carbon dioxide in air.

Next slide, please. And, as I pointed out, though, the speciation is also dependent on the partial pressure of carbon dioxide itself. Here we have a plot of constant pH of 7 or one micromolar uranium, which is about 238 parts per billion. And here we show the speciation as a function of the partial pressure carbon dioxide.

So at -2, this is the log. So this is the one percent value I was talking about before. And this is ten percent $CO_2$ out here. This is the range that we observed in the field system I am going to be talking about in a minute. So, even at constant pH, as you vary this partial pressure of carbon dioxide, the speciation is changing in solution.

Next slide, please. So in terms of conceptual models for describing sorption in solute transport, the common practice by consultants and for low-level waste situations is to assume that a

constant $K_d$ can be used because this is a simple way to move forward. This approach, however, it should be noted is valid only if you have linear absorption in constant chemistry throughout the space in time that you are modeling.

Some have tried to introduce more complexity by introducing a different type of nonlinear isotherm, such as a Freundlich isotherm, but this also only applies at constant chemistry. So this doesn't really fully take into account what we observed in contaminant plumes.

In contaminant plumes, we have variable chemistry and we have complicated aqueous speciation reactions that affect the amount of absorption in addition to the nonlinear absorption.

In this case, we have maybe two choices. We have surface complexation models, which I will show in a minute, coupling the quantification of absorption with the aqueous speciation; or we can try to describe the distribution of $K_d$ values that might occur as a result of the change in the chemistry.

Next slide, please. The surface complexation models are simply a way of describing absorption with a mass law, which is the same way we describe our equilibrium chemistry, our solubilities.

It's the way thermodynamics works, by determining stability constants and then describing the equilibria that result by a mass law.

And in surface complexation modeling, we accomplish the same way of describing absorption by writing reactions that involve specific sites on the surface particles, reacting with an aqueous species that is a master component for the equilibrium calculation, and then you have a stability constant that is equal to the concentrations of these species.

And so this is a surface species, just like this is an aqueous species. And so we can calculate the amount of this surface species that exists at a particular pH value via this constant.

The important thing is that; whereas, absorption is a function of pH, if we had done our model correctly, this constant is not as independent of pH, just like uranium aqueous concentration with acetate, that stability constant we can look up in the literature. That is independent of other values. We want this value to be independent of the chemistry; whereas, $K_d$, as I have shown, is a very sensitive function of the pH.

Next slide, please. And then we can couple these constants that we determined for

absorption. And then there is only one shown here. There may be more than one in the model.

We can couple these in an equilibrium calculation with thermodynamic data for aqueous speciation or thermodynamic data for solubilities. And so we can couple our absorption reactions together with our thermodynamic database that we have for describing equilibrium.

And so, for example, this constant could be coupled together with the constant for formation of the uranyl carbonate complex. And so you can see by that mechanism that if we add bicarbonate to the system, it starts to form this species, which competes with the formation of this. And, therefore, you can decrease absorption by forming this complex. So now ideally this constant should also be independent of pH in the carbonate concentration.

Okay. Next slide. Now, I mentioned before at the beginning that ideally we would like to incorporate all of our knowledge of reaction mechanisms into a solute transport model, but this becomes very difficult because our knowledge is constantly advancing.

For example, the species that I have just shown on the surface, while commonly thought to be the

absorbed uranium species, we are now showing in surface spectroscopy studies -- for example, in this study that I was a part of, we absorbed uranium onto the iron oxide mineral hematite in the presence of air. And at all pH values, we found using access spectroscopy that uranium when it's absorbed always has a carbonate, one or two carbonate, anions attached to it.

So it's not a bare uranium ion, uranyl cation that absorbs on the surface. In fact, it's something more complicated than that. And this was true at all pH values.

So our previous knowledge of how uranium absorbed on the surface is incorrect. And now we are working in systems with silicate. We are finding that uranyl silicate complexes also form on iron oxide surfaces.

So our knowledge about the actual chemical species that occur on the surface is advancing now as a result of advances in spectroscopy. With the Syncatron accelerator radiation, the detection limits are dropping. And we are able to determine more and more about the details of these surface reactions.

Next slide, please. So that makes things a little difficult because our knowledge, our

scientific knowledge, is advancing. And we can't constantly be to the minute or to the day adapting our codes.

So as far as applying the surface complexation model in natural systems, the approach that most people have taken has been to take the most advanced knowledge that we have, which is based on studying absorption onto pure mineral phases that are representative of what might be present in a soil or a sediment and developing the surface complexation model and then trying to extrapolate that to the natural system, either by adding up the contributions of individual mineral phases present in a soil sample or what have you.

What we have done in this study that I am going to show that is different is we have backed off of that need to know all of the details and incorporated that into the model. We are using what I would call as an engineering approach to develop the surface complexation model.

What we do is we collect data relevant to field conditions using the field materials. And then we make various simplifications to the model that allow us to move forward with a fairly precise and accurate assimilator of the absorption as a function

of aqueous conditions. I will show what I mean. Basically, this is the new part that I am going to, "new" in parentheses, be talking about.

Next slide, please. Now, the site that I am going to describe where we did our work is the Naturita UMTRA site, which is located in southwestern Colorado along the San Miguel River. There was a mill there that operated for 20 years, from '38 to '58. And shown here is an aerial photo from 1974.

Here you see the tailings. The river in this slide is flowing down this way. We have this reach. There is a two-kilometer reach here, where the aquifer is recharged by the river up along here, just above the edge of the slide. And then the groundwater flows down through the reach and discharges along this area.

This is a funny thing you see from space, a former go-cart track that was, in fact, built on tailings. So all of the dust was being kicked up and breathed by the kids riding around on this go-cart track, I guess, back in the '50s.

Next slide, please. There are a lot of houses out there. The foundations are built with tailings also. Very interesting place.

This is another aerial view of the

Naturita site in 1996. This is where the mill yard was formally located. Now, at this point, the DOE UMTRA program, the tailings had been removed in 1979, but here the surface remediation was occurring. And parts of the vadous zone were being dug up in the mid '90s and carted off to a landfill. So that's why you can see that there are pits out here at the site. This is where the tailings used to be.

I am going to talk a lot about a one-time uncontaminated sediment sample we collected here and work with. So that was up-gradient of all of the contamination.

Next slide, please. So our approach for developing and testing the surface complexation model was to characterize the groundwater flow and geochemistry at the site, measure uranium absorption on the uncontaminated sediment sample that we collected, fit a sorption model to that sorption data, and then to test that model in the field using the same uncontaminated sediment samples suspended in wells in the uranium contaminated area and also removing contaminated sediments from the aquifer and studying the uranium that was absorbed on those sediments as a test of the model. Then, finally, we did reactive solute transport modeling and a

comparison of constant $K_d$ and SCM approaches.

Next slide, please. SCM stands for surface complexation model. Sorry.

So here these red dots are all of the wells that we put in at the site. Again, the groundwater recharge occurs here at this reach. There was also a database that ran from 1986 to 1996 from the Department of Energy wells that they had there before they started their surface remediation. That data set was extremely valuable to our study.

Again, it's a two-kilometer reach. And the flow direction is this way. We have a bedrock flow boundary on this side and the river boundary on this side.

Next slide, please. Now I am going to show some of the concentration contours that existed in 1999 at our first sampling. These are the uranium concentration contours, ranging from two to ten micromolars. So that's 400. Two is 450 ppb and 10 is 2.3 parts per million. This is the original area of contamination. So you can see that the uranium has moved out of that area and is discharging to the river.

The pH throughout this region is relatively constant, around 7.1. The alkalinity

varies considerably. That has to do with the fact that there is calcite in the vadous zone material. And these tailings were either acid-extracted or base-extracted and then placed on the ground. As precipitation fell on the tailings, the water would go into the vadous zone and dissolve calcite to create these alkalinity plumes. There are alkalinity gradients that are associated with the contamination itself.

The result, if you take these pH values and these alkalinity values that were measured, the result, the range in pressure of carbon dioxide that existed in the aquifer was approximately one to ten percent. And at any point in the aquifer, the dissolved calcium concentration was controlled by the solubility of calcite.

Next slide, please. The groundwater is poised in a suboxic condition. There is very little dissolved oxygen. And this is true even up-gradient of the contamination. So there are biological reactions.

As the river water comes in, it is quickly the oxygen is removed by degradation of organic carbon. There is no nitrate in the aquifer. And there is some evidence of manganese reduction

occurring.

Then down-gradient, where it was a lower-lying area and there was a lot more -- this is mostly barren. And down here there were a lot of cottonwood trees. And so there is a lot more. Vegetation became vegetation on the ground. There is evidence of iron reduction occurring in the aquifer. No sulfide was detected in the aquifer.

Next slide, please. So this cross-section is to give you an idea of the texture of the material, very cobbly, high-gradient mountain stream. The material used in our experiments was actually dug out with a backhoe from beneath the water table. But this gives you an idea of the texture.

We used the material that was less than 3 millimeters, which was 15 percent by weight of the sediment but had 85 percent of the uranium absorption. The sediment was primarily quartz and feldspars with calcite, iron oxides, and some clay.

MEMBER HORNBERGER: Jim, give us a sense of the scale.

DR. DAVIS: Right here?

MEMBER HORNBERGER: Yes.

DR. DAVIS: That is about 10-12 feet there.

Next slide, please. But all of the material came from below that.

Next slide, please. So this is a plot of the experiments that we did with the uncontaminated material. They were batch experiments done with artificial groundwater with equilibration for four days.

What is plotted here is the log of the uranium $K_d$ versus the dissolved uranium concentration. So it's plotted as an isotherm. You might often be used to seeing these plotted as a function of pH, but we can't do that here because of the calcite in the sediments.

For each partial pressure carbon dioxide that we used in this experiments, you get one pH value at equilibrium. So each of these partial pressures of carbon dioxide, which were imposed on the system, gave us a different pH value.

You can see as you go to higher partial pressures of carbon dioxide, the absorption is dropping or the $K_d$ is dropping. Again, this is log of the $K_d$. So this is one and this is ten. There is also a dependence on the $K_d$ on the uranium concentration itself. As the uranium concentration goes up, then the $K_d$ is dropping.

Now, again, these experiments were done to mimic natural conditions. This is not like a research study to just get at these relationships. All of these conditions actually exist out in the aquifer.

Next slide, please. Now, just as a tangent now, you can see the difficulty that we might have in applying a forward model, instead of this engineering approach. Here is an example of what the material looks like.

Whether you are looking at a quartz grain or a feldspar grain, what we have on top of these grains are extremely thick coatings, several hundreds of nanometers thick of illite/smectite clay. And embedded within those are lots of iron oxide particles, some of them goethite and some of them ferrihydrite that is formatting goethite.

The scale bar here is 100 nanometers. And this is sitting on the top of a quartz particle, then down here a scale bar of nine nanometers. You can see the goethite rods. And there are many of them. If you look here backed away, there are many of these goethite rods immersed in this clay.

So if we were going to try to construct a forward prediction of uranium absorption, we would have to know how the uranium absorbed onto

illite/smectite clay, how it absorbed onto goethite, how it absorbed onto ferrihydrite, and how we have to try to enumerate the surface area of each of these types of minerals present. It is a very difficult thing to do to get an accurate prediction from these measurements made in the lab with single mineral phases.

Next slide, please. So, again, in this study, we wanted to take an approach where we fit the data with an inverse surface complexation model. And, to simplify it further, we have no electrical double layer, which is a common component of the other types of approaches, to use either a diffuse layer model or a triple layer model to take into account the effect of surface charge on absorption.

This model has no electrical double layer. And we are able to describe the absorption that we measured in the lab as a function of pH and partial pressure of carbon dioxide in the uranium concentration using these two surface reactions.

So we have these two surface reactions. Next slide. And we couple those together with the thermodynamic data for the aqueous speciation. Go back one, please. And the result is that we can now describe these data fairly accurately.

Next slide and go on. So that is how we developed the model. Next I want to talk about the tests we did of the model. We did two types of tests. One was to suspend uncontaminated sediments, the same ones that we had used in our batch experiments in the lab, suspend them in bags in the uranium contaminated part of the aquifer.

So these red dots are wells that we suspended that material. There is a range of chemical conditions in each of these wells. The samples were suspended for 3 to 15 months, but we saw no time dependence. In fact, we probably could have suspended them for a period of time from four days to a week and gotten the same results.

Next slide, please. The other type of tests we did of the model was to remove contaminated sediments from the aquifer. This was very difficult to do because of the cobbles. We never were able to obtain cores, which we wanted to obtain. We were driving the drillers crazy trying to do that. But we were able to obtain material from each of these holes as we were putting the wells in place, sometimes by collecting cuttings from the saturated zone.

And on each of those samples, we measured the amount of uranium that was absorbed by an isotopic

exchange technique in the laboratory. So from the measurement of the dissolved uranium in the water and this measurement of the absorbed uranium, we could calculate an *in situ* $K_d$ and compare that to what our model said should be there.

Next slide, please. So this slide shows in a general way a comparison of the $K_d$ values measured for the field samples with our model predicted values. The model predicted ones are the clear, and the measured are the shaded.

Where it says "NABS," that is the background sediment, Naturita aquifer background sediment. So that is the uncontaminated samples. This is the $K_d$, but notice it is plotted in a geometric scale. And then over here are values for contaminated sediments.

With the exception of these wells down here, the final two, we got within a factor of two between the model and the measurements, measurements made in the field.

These two down here, I probably won't have time to talk about it until in the discussion section if someone is interested. We believe these are down where we have measurements of ferrous iron in the aquifer. And we believe that, in addition to absorbed

Uranium-6, there is also reduced Uranium-4 present on these sediments.

Next slide, please. Now I want to make a point here. We have variability in the $K_d$ value for the uncontaminated sediments. The $K_d$ varied by a factor of 22. That is one sediment put into 17 different wells, each well having a different chemistry. And so because of the different chemistry, we got a variation in $K_d$ of 22.

If you take the contaminated sediments, which are 14 different sediments collected spatially throughout the site, and put them into one water sample, which is an artificial groundwater equilibrated with lab air, you only get a factor of 2.5.

So my point here is that the $K_d$ variation that we are observing in the aquifer is primarily due to the variation in water chemistry, not due to a large range in the variability in the absorptive properties of the sediments.

Next slide, please. So that is summarized here. I am not going to go into it, but we have observed exactly the same thing at another site, in an aquifer at Cape Cod, where we have a large variation in $K_d$ primarily due to gradients in pH values. Again,

we have made this kind of comparison, and we find that the $K_d$ variation at the kilometer scale is primarily due to aqueous chemistry and not due to changes in the properties of the sediment.

Next slide, please. So now I am going to go to some transport modeling and some simulations. Here are 1-D simulations, now where the absorption is described by this what we call the semi-empirical absorption model.

And what we have done here, the initial condition in this column is the background conditions in the aquifer, pH 7.1 and low alkalinity. And then we change the inlet at time equals zero to a different condition, where we either vary the pH, the uranium concentration, or the alkalinity and while leaving the other two variables constant at their average value.

So, for example, here at the inlet, we change to an average uranium and alkalinity. And we vary the pH over the whole range observed in 459 water chemistry measurements throughout the site. The range that we looked at was from the minimum value to the maximum value. And the average value is shown here in the black.

What you see here is the pH variation we see at the site is not affecting the transport very

much. That maybe isn't too surprising because we don't see a huige pH variation at the site. The variable uranium concentration has a much larger effect. At the higher uranium concentrations, we have less retardation.

So the maximum concentration is shown out here. So the variable uranium concentration has this much effect on transport, but we see that the largest effect is the variable alkalinity that we observe at the site. This definitely has the largest impact on how fast the uranium is being transported. Again, the variations that we are looking at here are based on the actual field observations.

Next slide, please. Now, if we look more closely at the effects of alkalinity on the model $K_d$ value, what is shown here now, for a constant pH of 7.1 is variable uranium concentration and alkalinity. And you can see that the $K_d$ is going down as we increase the uranium concentration or as we increase the alkalinity. These are model response curves.

Now and for the rest of the talk, I am going to be talking about cobble-corrected $K_d$ values. Now our laboratory measurements were made on the less than three-millimeter material. And now we have corrected up the surface area to consider the entire

porous medium. I can talk more about how we did this at the end if someone is interested.

Now I want to point out these concentrations here at an alkalinity of about 11 and 10 micromolar uranium, this is about the peak of alkalinity and uranium in the aquifer. Our model says that that should have a $K_d$ of about .32, which would result in a retardation factor of 3.9.

Next slide, please. Now, if you put those values into a column, now where we have the initial condition is the background conditions and then we put in a pulse of one pore volume, if we model it with a constant $K_d$, we predict retardation of about 3.9, as I said, but with a surface complexation model, we predict a lot more retardation. And that is because the alkalinity disperses in the column. And this causes the uranium to absorb more strongly; whereas, that is not taken into account in the constant $K_d$ approach.

Next slide, please. You can see that here. This maybe is too complicated to get into in detail. Basically, what happens, as we see in the key to figure, is that we have a plot of alkalinity, uranium, and then simulated $K_d$ values.

What happens in this one pore volume

injection is that the alkalinity is not retarded and the uranium is. So the alkalinity is separating from the uranium at the beginning, and then the alkalinity moves off. There is a uranium peak that follows at the end of the alkalinity pulse. And by two pore volumes, they have completely separated. So this is why this increases the uranium retardation in that simulation.

Next slide, please. Now, in the aquifer, we don't have a one pre volume injection. We had the tailings in the mill yard here. And they were there for decades. We had rain falling on the tailings. And that was our input. That is a continuous input, not a one pore volume input.

So now I am going to describe the 2-D reactive transport modeling that we have done. We have the source area, this brown area, this region in the aquifer. The hydraulic conductivity was estimated from age dating and from transport of chloride that was observed as a function of time in the DOE data set. There was a chloride plume from a salt roaster located at a specific place here. And we could see with time the chloride, how fast it had moved to the aquifer.

So we used those two things to estimate

the hydraulic conductivity in the aquifer. And then we had contaminated recharge bringing uranium and alkalinity into the aquifer at a rate -- the recharge was one percent of the annual precipitation. So it was a continuous input.

Next slide, please. These showed results, the comparison between the observed uranium and alkalinity values, compared with our simulations using the surface complexation model for 62 years of simulated transport.

A conservative tracer would take about 33 years to travel the whole 2 kilometers in this aquifer on average. There is not one velocity because the flow model -- there is a velocity flow field here in the flow model. And their velocities are faster in certain places, especially close to the river, and slower over here near the bedrock surface, which is why some of the highest concentrations are here. And they are less influenced by the river.

You can see we have reasonable agreement between the observed uranium contours and the simulated uranium contours using this absorption model that we developed in the lab with uncontaminated sediment.

Next slide, please. And the important

thing is to notice that $K_d$ varies not only in space. It also varies in time because of the evolving chemical conditions in the aquifer. And the model is able to handle that.

The important thing about this distribution of $K_d$'s is this is not a random distribution of $K_d$ values. This has spatial character. And the spatial character arises from the changing chemical conditions as a function of time and space.

Next slide, please. We have evaluated the model. I'm not going to go into that in any detail except to state the conclusions. We have done a sensitivity study. And the conclusion of that and what we wanted to do was to compare the sensitivity of our absorption parameters to what we understood about the hydraulic connectivity.

And here is a rough guide. The model is more sensitivity to the hydraulic connectivity than the surface complexation parameters. So that is an important thing to understand. We are less certain about this value than we are about these others or at least this has a bigger impact.

We had no electrical double layer model I mentioned. So we tested the result of matching this

species up with an anion, a major anion, in the groundwater to see if changing the charge here would have an effect on transport. It does not have an effect.

And we also used rate laws developed from kinetic studies, batch studies, in the lab to see whether our local equilibrium assumption was valid. We again got identical results, whether we used the kinetic transport model or a local equilibrium model.

Next slide, please. Where am I on time?

ACTING CHAIRMAN RYAN: You're doing pretty well. If you could wrap in, say, five more minutes, that would be good.

DR. DAVIS: Okay. So we have done simulations of future uranium migration. In particular, in this project we were working with NRC staff to do an actual performance assessment of a receptor. And so this was done by Ralph Cady in the NRC Office of Regulatory Research and is in our NUREG report.

We have also compared constant $K_d$ versus surface complexation transport simulations, starting from the observed conditions in the field, simulating transport for 500 years, and comparing peak concentrations, flux to the river, and cleanup time.

Next slide, please. This shows the range of $K_d$ values and retardation factors that we used in the constant $K_d$ approach. So we have this distribution of $K_d$ values predicted. This is predicted by the model given the aqueous chemistry in the aquifer. And then these are the distribution actually measured with contaminated sediments by the isotopic exchange. The results, the ones I'm going to show are from the isotopic exchange distribution.

Next slide, please. So this shows simulated cleanup times for an observation point that's up-gradient in the contaminant plume at the current time for this point in space right here. And the question is, how long would it take to get to the drinking water standard, which is about $10^{-1}$ micromolar or $10^{-7}$ molar?

And you can see that the red slides are for the constant $K_d$ simulations. This uses the range of $K_d$ values, again, that were found for all the contaminated sediments. This is the highest $K_d$ out here. So it takes longer to clean up at this point.

And this is the lowest $K_d$, which pumps fast. But the main thing is that the slope is quite different for the surface complexation model. Again, that gets into the fact that as uranium moves, the

alkalinity is changing. And so things don't really have the same kind of slope as you get in a constant $K_d$ simulation.

Next slide, please. This shows simulated peak concentrations at an observation point. This was the observation point used for the performance assessment analysis that was done by Ralph Cady. This again shows the initial condition, which was the current condition in 1999, and this shows the peak concentrations.

For the $K_d$ simulations, the peak concentrations are always the same. And it has to do with this peak, this highest uranium concentrations passing through this observation point.

And the $K_d$ just determines how fast it gets to the observation point. It doesn't change the peak concentration; whereas, with the surface complexation model, you actually get a smaller peak concentration as a result of a change in chemical conditions in the aquifer.

Next slide, please. This last one is to show results for concentrations in a pumped well. This was again part of the performance assessment. This well was pumped for domestic use at a rate that I don't remember.

The thing that is interesting here is that the constant $K_d$ simulations don't bracket what actually happens with the surface complexation model. You actually get a higher concentration of uranium coming in at an earlier time in these simulations than you do for any of the $K_d$ simulations. And the reason is as you pump this well, higher alkalinity water starts to come into towards the well and then exists there at the current time. So that this higher alkalinity water comes in and changes the properties relative to any of the constant $K_d$ simulations.

Next slide, please. So I will show the conclusions of this slide, and then I just have a couple of slides for discussion about how this might be interesting to think about in terms of the geosphere at Yucca Mountain.

The conclusions from our work are that current reactive transport models can accommodate the surface complexation concept. We don't think the use of the constant $K_d$ concept is really required from a technology point of view. The codes can accommodate this concept.

The real issue is how do we parameterize these models? And that is what has been I think different about our approach, the way we have

approached the problem of parameterizing.

We think that this kind of modeling can reduce uncertainty with respect to sorption because we think that we can bound this more carefully than can currently be done, at least with constant $K_d$ simulations.

I think an important conclusion for two field sites that we have looked at is that the spatial variability in groundwater chemical conditions was more important in influencing the range of $K_d$ values that we observed compared to variability in the properties of the aquifer materials themselves. And this was at a kilometer scale.

You are talking about moving from one geological formation to another. It's at the 100 kilometer scale. Of course, we haven't tested that, something like that. And our conclusion would likely not be valid or may not be valid.

Then, finally, predictions based on a range of constant $K_d$ values do not always bracket simulations result obtained using the semi-empirical surface complexation model. Random sampling of a $K_d$ distribution may overlook spatial character of the distribution.

Those are the conclusions from our study

of the Naturita UMTRA site. I have just a couple of more slides. I thought we would talk about how this might be relevant to neptunium.

This is a plot of neptunium speciation as a function of pH with a system equilibrated with air for one micromolar neptunium. You can see that analogous to the uranium, neptunium does form these aqueous carbonate complexes, although they are not as strong as the uranium carbonated complexes.

Next slide, please. And in studies of neptunium absorption, there is a similarity in that the neptunium absorption is sensitive to the partial pressure of carbon dioxide.

And in modeling that, this was work done by Kohler, *et al*. published in 1999, neptunium absorption on hematite with no carbon dioxide present with atmospheric carbon dioxide and almost two percent. And with the almost two percent, you see the absorption coming down these squares, the green squares.

And then shown here are surface complexation model simulations. I just want to point out that to simulate this data, they had to assume that neptunium formed a complex at the surface with carbonate attached to it, which is the same thing that

we observed in our spectroscopic data for uranium.

So there is again a chemical analogy here that an exact surface complexation model may require these ternary surface complexes involving carbonate.

Next slide, please. This just shows some numbers for variable integrated carbon in groundwater, a comparison of the ranges that we have at the Naturita site to what has been described for the Yucca Mountain hydrologic system.

We have in comparison to the Naturita site a much larger range of total dissolved inorganic carbon and a larger range in the partial pressure carbon dioxide, although the upper numbers here may be among the most important to look at. And they are somewhat similar.

Next slide, please. And using those data and their model for the Np, absorption of neptunium on montmorillonite, surface complexation model with a diffuse double layer model, and using this site water chemistry, Dave Turner at the center and others, including Paul Bertetti, have done a neptunium $K_d$ contour map, which they published, and showing ranges in the neptunium $K_d$ as if the aquifer were composed of the montmorillonite.

And the ranges go from 25 to -- I'm not

sure what the upper numbers are, but in this region down-gradient of the proposed repository, the $K_d$ ranged by a factor of 4, so perhaps not a huge range, but one thing to note is that there certainly is a sparsity of data in this area directly down-gradient in terms of the water chemistry.

Next slide, please. Finally, I will just make a note that another possible bad actor is fulvic acids. This is a paper published in 2000 using Chalk River fulvic acids and Chalk River subsurface material, packing a column.

This shows the transport of neptunium under the given conditions in the absence of the fulvic acids. So you had a retardation factor of about three for those conditions. And then when you put in fulvic acids that were ten times what they were in the aquifer, you were able to reduce the retardation by a huge amount, almost to the point of no retardation at all.

Now, this, of course, is very influenced by this concentration of fulvic acid they put in. But I just wanted to point out that there is another thing that could be affecting aqueous speciation and retardation of neptunium.

Next slide, please. So just as discussion

points about how what we did at Naturita might be relevant to Yucca Mountain, I put up these. There is some uncertainty, at least, in understanding neptunium retardation in the saturated zone as part of the Yucca Mountain modeling.

Because it relies on a log-normal distribution of abstracted $K_d$ values -- I guess I am referring here now to the center's approach in the modeling -- the distribution of abstracted $K_d$ values is based on montmorillonite as a model for the alluvium for the scaling of surface area.

So the difference between what we have done and what the center has done, they have done an excellent job of evaluating the effect of water chemistry on $K_d$ for this montmorillonite surface.

In our approach at Naturita, we worked with actual sediments and the aqueous chemistry distribution to arrive at the $K_d$ values. So we had less of a problem I think in this abstraction process of going from the pure montmorillonite to the real material.

There is also what you can do is measure the range in $K_d$'s and then sample this log-normal distribution when you do the performance assessment simulations. However, if you would back up a couple

of slides, please, that ignores the spatial character which is part of the range.

It's not a random set of pH and alkalinity conditions that causes this contouring of $K_d$ values. In fact, this $K_d$ contour here is 25. This is well below the median. This is on the lower end of the distribution.

So if you sample a distribution, log-normal distribution, for all of these chemistries, you may, in fact, be building too much retardation into the model for this section of the aquifer because you're treating it as a random thing when, in fact, you have actual pH and alkalinity values here that could be considered.

Next slide, please. One more. And then, finally, I don't know, actually, the extent fulvic acids have been considered as part of the Yucca Mountain problem. I just brought it up because of that one paper that seems to have some relevance.

Thank you very much.

MEMBER HORNBERGER: Thank you, Jim.

We started a few minutes late, and we're running a little late. We have a little bit of flexibility built into our schedule, but I definitely would like to break by noon. So we will take time for

some questions and discussions, but to the extent that we can keep them focused, it would be good.

Ruth?

MEMBER WEINER: First, I want to thank you for a really fascinating presentation. That was just really great. I would just limit myself to one question.

You started in Naturita with a contaminated site. And you have made some adjustments, some suggestions about moving to the Yucca Mountain site. Is there anything that could be done in the Yucca Mountain site that would be analogous to the contamination that you started with at Naturita? Is there something that you can do in the surface, take samples, whatever?

DR. DAVIS: Well, yes. Obviously the testing that we did that we were capable of doing because of the contamination that was there aided our model evaluation and maybe validation if you want to call it that in that we were able to go out and put uncontaminated sediments into contaminated groundwater. We were able to pull out contaminated sediments. So it enabled a good, better testing of our model.

The model could at an uncontaminated site,

like Yucca Mountain, be built by abstracting enough sediments and making enough groundwater measurements.

So the thing that might be limiting the construction of a model is -- I mean, there are sediments now becoming available because of the early growing program.

And the question is, is there enough water chemistry available immediately down-gradient at the site?

MEMBER HORNBERGER: Michael?

ACTING CHAIRMAN RYAN: I'll defer.

MEMBER HORNBERGER: Allen?

DR. CROFF: You have talked mostly about saturated, saturated entirely, I think. Given that Yucca Mountain, parts of it, are unsaturated or spasmodic flow, periodic flow, would this approach work or how might it work or what adjustments would have to be made to make it work?

DR. DAVIS: Well, that's a good question. We have no experience making it work in unsaturated systems, but the adjustments that would have to be made are the same kinds of adjustments that would have to be made and used in either a constant $K_d$ or a distribution $K_d$ approach except that you would have to understand the water chemistry in the water that was

flowing in the unsaturated zone. And you have to understand the surface area system. Those are the difficult things to understand to apply a model like this into the unsaturated zone.

MEMBER HORNBERGER: Ines?

DR. TRIAY: I have three questions, the first one along the lines of what has been asked for the unsaturated zone. What has been the validation that you have done with respect to these experiments that are more wet chemistry, bench chemistry, type of experiments, based on batch type of experiments versus experiments that are performed under flowing conditions, whether it is saturated or unsaturated? And that is my first question.

The second question that I have --

MEMBER HORNBERGER: Can he take them one at a time maybe?

DR. TRIAY: I'm sorry. Yes. That's fine.

MEMBER HORNBERGER: We will come back.

DR. TRIAY: I was just wondering whether he wanted to hear all of the questions.

DR. DAVIS: With respect to flowing conditions, we have done also column experiments. I didn't describe them, but we have a pretty good agreement in the prediction of transporting columns

with this model.

Then the other part of the validation I described in the top, where we put uncontaminated sediments into the groundwater system itself for periods of months, up to 15 months.

So I showed so those data, where we put uncontaminated sediments in the groundwater at the site and then pulled them up after a period of time and measured the amount of absorbed uranium on that and compared that to what we predicted with the model. And we got within a factor of 2 for 17 different wells.

DR. TRIAY: So my second question, then, is so I guess that your point is, then, that this type of surface complexation model could be applied under flowing conditions and you can get the data from batch experiments and apply it under flowing conditions and predict radionuclide migration. Is that a fair statement?

DR. DAVIS: Well, it's a fair statement as long as the local equilibrium assumption applies. It will depend on the flow rate. So you can certainly increase the flow rate. It's an equilibrium model. So as long as --

DR. TRIAY: The kinetics is not

dominating.

DR. DAVIS: If the kinetics is not dominating, then yes.

DR. TRIAY: So then that brings me to my second question. Have you done some sensitivity analysis based on the surface complexation model to try to make a definitive conclusion as to whether or not sorption coefficients can or cannot be used to describe radionuclide migration?

What I mean by that is the surface complexation model take some resources to develop, especially for actinides, for the obvious reasons. You have to get a tremendous amount of data. And you have to get a tremendous amount of data as you vary groundwater chemistry and, of course, when you start varying groundwater chemistry, the actinides sometimes behaving in a manner that is not ideal from the point of view of solubility.

So you have to really control your environment when you are trying to develop the surface complexation models for actinides, like plutonium, neptunium, americium, and the like.

So based on what you know now, is it your opinion that the sorption coefficients that are more readily obtainable for the actinides are inadequate to

predict radionuclide migration?

DR. DAVIS: Well, that's a difficult question because inadequate would depend on the criteria that one is judging inadequacy. It also depends on a performance assessment point of view how important is a $K_d$ value to the assessment.

If in the case of iodine and technetium you were able to conclude that there is no danger, the dose is small enough with a $K_d$ of zero, then obviously you would not need a surface complexation model for technetium and iodine.

So for neptunium, if there is a dependence on the assessment or the safety assessment on the $K_d$ value, then my opinion is that yes, these types of models would give more certainty and scientific credibility to the values of retardation that are simulated in reactive transport modeling in the saturated zone.

I agree with you that it costs more, but I also think that the costs are not as great as thought if you use this engineering semi-empirical approach to compare it to the more scientifically based approaches that require a complete understanding of the electrical double layer and so forth.

DR. TRIAY: What I meant by "resources,"

I didn't mean costs in terms of funding, although that is probably true, but I meant more that sometimes it is just not possible to look at all of the range that you would need in order to come up with arable surface complexation model for some of the actinides because the actinides start becoming insoluble and getting to complexation with some of the trace components in the groundwater to the point that, all of a sudden, you are studying something different.

That way you think that you are studying some resources, not from the point of view of money necessarily but from the point of view of it is difficult because we don't understand the solubility of the actinides to the point that you can actually know that all that you are varying is the age versus carbonate concentration, nitrate, so on and so forth.

So that's the concern that I have, you know, to what extent can you really do this for that very rich chemistry that the actinides exhibit at near neutral pH.

DR. DAVIS: Well, in particular, I assume you are talking about plutonium.

DR. TRIAY: Right.

DR. DAVIS: I think our studies have involved uranium. And we are beginning to work with

neptunium. We don't think we have solubility issues with those.

But yes, you do have to be able to work experimentally under conditions where you are not precipitating a base.

MEMBER HORNBERGER: Let's take that as a discussion point. Jim, do you have a question?

DR. CLARKE: I, too, thought it was an excellent presentation. Thank you for that.

This is just a fairly basic question. This approach can be extended in a straightforward way to several radionuclides. Would it shed any light on competitive absorption? Any thoughts on that?

DR. DAVIS: The competitive adsorption between radionuclides?

DR. CLARKE: Yes.

DR. DAVIS: Well, we have been working at relatively low concentrations for radionuclides. The competing that goes on for the surfaces is really from the major cations in the groundwater, the calcium and magnesium.

By using this approach, we take them into -- but we work with an artificial groundwater of the same composition and range of compositions that exists in the aquifer. So we are taking that into account.

It becomes lumped into our absorption constant, the competitive processes, with those major ions.

Now, if you're talking about competing between radionuclides, you would need pretty high concentrations approaching solubilities, I think, before those would become important.

Really, that's I think outside of the geosphere. That must be more of a near phenomenon that could be important near the waste package itself.

DR. CLARKE: Okay. Thanks.

MEMBER HORNBERGER: Dick?

DR. PARIZEK: Yes. Dick Parizek.

Again, a very, very interesting presentation. In your discussion, you didn't consider colloid transport particularly?

DR. DAVIS: No.

DR. PARIZEK: Right? That's excluded?

DR. DAVIS: That's correct, yes.

DR. PARIZEK: It's interesting. Looking at the river, it's like a conceptual model here. You say, "Well the river was a source of recharge above the tailing pile." Then it became a discharge area further down. It's sort of like the Forty Mile Wash example.

You have infiltration, which is episodic.

It would be climate-driven in terms of pluvials, monsoonals. You have differences in that. As you go maybe on the west side of the alluvium, you have more of the bedrock interface. And you also show sort of a chemistry in the slower portion of the aquifer along the valley wall versus near the river.

So the chemistries ought to be really complicated in your model, even under the present data source. For Forty Mile Wash, you expect also complicated chemistries. And so you would need a lot of data, I would think, on water chemistry as well as the hydraulic conductivity variability to make a good and reliable forecast.

Would you agree with that? It's sort of capturing, I think, the main points you were trying to bring up for us.

DR. DAVIS: Well, the richer the database on the water chemistry, the hydraulic conductivity, obviously the better your model is. The model itself is developed from the batch data. So there you don't have to collect thousands of data points. You collect hundreds of data points. And then the question is how many sediments are you going to collect to be representative of the aquifer.

In our case, because we were worried about

it, we collected a ton literally, 2,000 pounds, of aquifer material, which we screened to determine the weight percentage of different size sediments and so forth. That was more because there were no other data on the size fractionation available for the aquifer.

The complex chemistry you are talking about, the contours near the river, that is a result of delusions more than chemical reactions, just so you understand that. That is the river water coming into the aquifer and exiting back.

DR. PARIZEK: Right. What it shows is that the chemistry is quite variable, for whatever reason.

DR. DAVIS: Yes.

DR. PARIZEK: In the case of Forty Mile Wash, there is also a plume variability to the recharge along the wash versus the recharge from the bedrock portion and the tufts portion. And so there is that interface between the two along the western edge of the alluvial valley fill, where it's again sort of similar looking, kind of a complicated chemistry.

Then, again, whether or not you have channelites flow in the alluvium, how well-known is that, the samples were rotary-grilled versus sonic

core.

Now, it may be a possibility. And, again, how big a sample would you need in order to be able to say I have enough sample to say something about spatial variability, even at the core sample location?

DR. DAVIS: Well, there are two issues here. One is you go back in --

MEMBER HORNBERGER: Jim, in the interest of time, perhaps we won't go back. Just try to address the Yucca Mountain application.

DR. DAVIS: Okay. The $K_d$ contour that I showed that the government drew, that is the same kind of contour you could draw with our model. The difference is that they have used it to build up a log-normal distribution that the performance assessment code draws from randomly.

There is nothing wrong with their approach. I would just argue that if it is possible, you would not draw from it randomly. You would use the alkalinity and pH data you have and couple it to the flow, not to draw random $K_d$'s off the distribution.

MEMBER HORNBERGER: Don, are you with us in Las Vegas? Do you have a question?

DR. SHETTEL: Yes, I am. Can you hear me?

MEMBER HORNBERGER: Yes. Yes, we can hear.

DR. SHETTEL: I have a couple of questions. Phosphate is next to the complex with uranium as well. Have you looked at the phosphate concentration in all of these waters?

DR. DAVIS: In the Naturita system, we have looked at phosphate. It's very low concentrations and doesn't affect the aqueous speciation in Naturita.

DR. SHETTEL: My second question, I think Dr. Parizek touched on this to some extent, but rainfall in the West, especially the continuous loop, was rather episodic. In your case, it may have a dilution effect more than anything else. Does that factor into your model?

DR. DAVIS: The modeling that you're referring to, the one percent of precipitation that we assumed was recharge?

MEMBER HORNBERGER: Perhaps the question is you assumed a steady flow.

DR. DAVIS: Yes, we assumed a steady flow.

DR. SHETTEL: Okay. So you're averaging rainfall over the course of a year or some time period?

DR. DAVIS: Yes.

DR. SHETTEL: How much effect would that have if they weren't averaged but were episodic, if they randomly input into your model?

DR. DAVIS: Well, obviously since we haven't done that, I can't say for sure, but I don't think it would have a big effect.

DR. SHETTEL: Okay. I want to thank you for a very interesting talk. I think this raises one more question about Yucca Mountain.

MEMBER HORNBERGER: Thanks, Don.

Obviously this is a very interesting presentation. We could easily go on and have another hour of discussion, but we do have to move on in the interest of time. Thank you very much, Jim.

What we are going to do now is go to our next presentation by Keith Compton.

MR. COMPTON: Good morning. My name is Keith Compton. I am with the Performance Assessment Section in the Division of High-Level Waste Repository Safety.

I am here to talk to you this morning just to provide an introduction and some regulatory context to the NRC approach to evaluating flow and transport in the saturated zone. I will try to be brief. This

is only an introductory presentation. The technical details will be provided this afternoon and tomorrow morning.

Now, again, my objective is to provide the regulatory framework, the context for the activities that we will be talking about this afternoon. What I would like to do is to leave you with an understanding of the connection between the presentations that will be given by the center staff and by NRC staff and an understanding of how these are relevant to the regulatory requirements and to the regulatory tools that we use.

The first part of my talk will provide that context. I will be talking about two of the important regulatory tools that we have, which is the Yucca Mountain review plan and the risk insights baseline report. And that will be the majority of the talk, hopefully short talk. And the second half will just be a brief summary of the talks that will be given later so that you have an understanding of what is going to be coming.

Jumping right in, the yucca mountain review plan for those of you who may not be familiar with it provides guidance for implementing the requirements of part 63, particularly the requirements

of part 63.21, which governs the content of a license application, and 63.114, which deals with the post-closure performance assessment that's required in the safety analysis report of any potential license application.

The review plan consists of a number of topical area and model abstractions. Today and tomorrow we will be focusing on two of the relevant sections of the review plan.

The first deals with flow paths in the saturated zone. It's mainly focused on hydrology. The second is radionuclide transport in the saturated zone and is more focused on chemistry retardation.

The Yucca Mountain review plan contains detailed guidance in the form of review methods. And it tells us how to review a number of topics. These include descriptions of aspects of the abstraction and their technical basis. It deals with adequate justification of the models and the data that are used in a performance assessment, evaluation of the uncertainty in the models and data. And also it deals with how to demonstrate that the models are supported by independent evidence that's termed "model support."

Something that is important to bear in mind on the review plan is that the review methods are

given for a detailed review. It covers a lot of material. However, that level of detail is for if you needed to do a detailed review. However, in any review of a potential license application, we would tailor the depth of review to the extent to which the Department of Energy relied upon the particular abstraction to make their safety case to demonstrate compliance. So, in other words, if the DOE believes that this is an element that is important to their safety case, we would do a more detailed review.

There are two aspects to how we would determine whether they are relying on these abstractions to make their safety case. One is we would look for any explicit credit they take. And by going into the multiple barriers section of the safety analysis report and seeing what has been prevented, what they have said about the credit that they plan to take for saturated zone.

But also we would look for any implicit credit that is taken by examining the TSPA model and determining whether they have, in fact, in the model taken credit for a feature, event, or process that would affect the repository performance. So there are two parts determining the extent of the reliance: the explicit and implicit credit.

Now, turning to the details, this is a bit of a summary. There are several pages on each of these topics. I've tried to summarize them into one, rather than just reading all of them.

The review plan identifies a variety of factors that can affect flow paths. Several of the factors in the first bullet, factors such as changes in the water table or changes in potential and future climate, will be discussed by Jim Winterle in his presentation on flow paths.

I would also point out that the review plan does require an examination of how features, events, and processes have been included in the assessment and evaluation of the approach used by DOE in their abstraction, the saturated zone flow. The focus of our talks is going to be on what we have done. We are not going to be talking a lot on our evaluation of the DOE models.

For saturated zone transport, it's constructed in a fairly parallel fashion. Again, there are a number of factors that have been identified that can affect radionuclide transport.

We just heard about the importance of water chemistry to transport. That is something that is pulled out. And that is also something that Mr.

Paul Bertetti will be talking about this afternoon covering the center's approach to abstracting the two: radionuclide transport and the impact of water chemistry. Again, the focus of our talk will be mainly on the first bullet. We won't be going into DOE or evaluating what DOE might have done.

Next slide, please. The next important regulatory tool that we have is a document known as the risk insights baseline report. This report is a set of analyses that were conducted by the NRC and by the center. They're intended to identify features of the engineered and natural environment that are important to repository performance. This is also used to assist in determining the level of detail. We determined how significant different abstractions, different components are to repository performance.

There are a number of risk insights. I'm only going to talk about the ones related to saturated zone flow and transport. We have one aspect which is considered to be of high significance to waste isolation; that is, retardation and the saturated alluvium.

As many of you know, there are two components to the saturated zone. There is a fractured tuff aquifer and then a saturated alluvial

aquifer. The absorption in the saturated alluvium we believe is particularly important.

Now, because of that, it is also important to consider the distance of the flow path in the saturated alluvium. If the flow were to bypass this saturated alluvium for much of its length, that would obviously impair its ability to function as a barrier. And so that issue of the distance of flow paths in the saturated alluvium was rated as of medium significance to waste isolation.

Also, there is absorption, however, that does take place in the fractured tuff, particularly if the nuclides diffuse out of the fractural water and into the rock matrix. It's this term, "matrix diffusion." The possibility of that, the impact that that could have on performance, is determined to be of medium significance.

I would also point out that in this, the effect of colloids on transport in the saturated zone, is also rated to be of medium significance to waste isolation. It is not something that we are going to be talking about in our presentations in detail.

We have time constraints. And we wanted to go over several things in sufficient detail. And so for that reason, you are not going to see a lot on

evaluation of colloids.

So the first talk this afternoon will be by Mr. Jim Winterle. He will present the groundwater flow model that has been developed by the center. And he will talk about the sensitivity of model flow paths to different factors, flow paths and travel times, to different factors, such as changes in recharge or changes in the water table level.

And, as I have pointed out, this talk will address several of the items that have been called out in the Yucca Mountain review plan and also in the risk insights baseline report.

The next presentation will be by Mr. Paul Bertetti on development of sorption parameters. He will focus on parameters affecting transport and, in particular, how they are abstracted for the purposes of performance assessment. This will, in large part, be focused on determination of retardation factors for actinides and particularly for neptunium. Again, this talk will cover or will mention several of the factors that are identified in the review plan and in the risk insights baseline report as well.

Finally, tomorrow morning Mr. Tim McCartin of the NRC will provide a discussion on performance assessment and risk perspective. In that talk, he

will discuss how a risk-informed approach can be applied to evaluating the performance of the saturated zone.

He will be doing that by first describing the principles of the risk-informed approach. And then he will step through an example that has been developed to show how it can be used to evaluate the performance of the saturated zone as a barrier.

That presentation will illustrate the relationship between the key items that have been identified in the review plan and baseline report showing retardation, the transport distance, matrix diffusion, how these work together in working combination to affect repository performance.

And that's it. As I said, this is a very brief introductory presentation. What I had wanted to do is to just introduce you to two of the important regulatory tools, the Yucca Mountain review plan and the risk insights. Again, the review plan identifies items for review.

The risk insights assist in determining the focus and the depth of the review. And I have provided by use of the risk insights an introduction to some of the aspects that are considered to be of particular importance to repository performance and

then provided just a very brief introduction to the talks that will be given later so that you have an understanding of what is coming.

That's all that I have. If anyone has any questions, I would be happy to take them.

MEMBER HORNBERGER: Great. Thanks, Keith.

Obviously we are going to suffer a great temptation to ask Keith detailed questions about presentations yet to come. I will empower Keith to deflect all such questions. Let me start, Keith.

As you heard Ines ask Jim a question on adequacy of an approach, in general terms, can you give us some insight on the regulatory perspective as to how or how the Yucca Mountain review plan might determine what would and would not be adequate from the NRC's point of view?

MR. COMPTON: I can talk a little bit about I guess the standard to be applied, which would be a reasonable expectation standard used to determine whether something was adequate. I will mention that. And hopefully that will get to the question.

When DOE develops their performance assessment and develop their models, the standard that we would apply to determining whether their overall assessment was adequate would be reasonable

expectation.

And there is a number of things that are important to that. It includes the full record. It recognizes that you are not going to have absolute certainty and you're not going to be able to completely eliminate all uncertainties in your assessments. And it acknowledges that because this is our processes that operate over a very long time scale, that there are inherently greater uncertainties and that we will have to focus on the full range of distributions, not to pick out one particular tail of the distribution, one particular value of, for example, a retardation coefficient and focus in on that.

I don't know if that gets to your question. There are a number of items that are in the review plan. In determining whether it is adequate, we would look at the risk insights. We would look to see how important do we think this is in affecting it.

Performance is a very sensitive to changes. Then we need to know a lot more about it. We need to have a fair amount of confidence. If it's something that doesn't really affect the overall performance results, then we might not need as much information on that or as long as we understand how

important it is for performance.

MEMBER HORNBERGER: Thanks.

Ruth?

MEMBER WEINER: This may strike you as a simpleminded question, but to the public, Yucca Mountain was always promoted as being in the unsaturated zone. And that was why it was a good site. What is the relative relevance and importance of your focusing on the saturated zone?

MR. COMPTON: I'm not sure that I would be able to talk about the relative importance of the unsaturated zone and the saturated zone. I know that our work has focused largely. Again, the goal of this presentation is to present the work that we have done and the approaches that we are taking. And a lot of that work has been on the saturated zone. I don't know if anyone wants to add anything.

MEMBER WEINER: Well, we can defer it to later if somebody else wants --

MR. COMPTON: Okay. But I will not try at this point to speculate about the relative kind of importance of the two. We are focusing in this presentation on the saturated zone.

MR. CAMPBELL: I can just add -- this is Andy Campbell, Chief of the Performance Assessment

Section -- that we will keep that in mind as we go along. The relative importance of the saturated zone -- keep in mind DOE and the federal government picked the site. We're the regulator. So we're looking at and evaluating and will evaluate what DOE comes in with.

Based upon our own analyses using TPA and doing over many years work the saturated zone comes out as an important barrier. And so it isn't so much as what has gone on in the past but on the basis of all of this work, saturated zone comes out as fairly important. And that is why we are focusing on that.

That was documented in the risk insights report, which was publicly available in April.

MEMBER WEINER: Thank you. That is very helpful.

MEMBER HORNBERGER: Mike?

ACTING CHAIRMAN RYAN: Just an observation. I want to turn your attention to your first backup slide. I thought those were kind of interesting and helpful rallying points for both your consideration of the review plan and maybe the risk insights.

Let's just go through them. Maybe could we get that slide up or talk about it? There are five

bullets there: system description and model integration and so forth. This seems to kind of maybe help organize our thinking of how the other presentations might come along.

MR. COMPTON: Sure. Particularly for the different model abstractions, these are fairly standard review areas. They're broken down in this format in the review plan.

The first review area, review method is a description of the system and model integration. So in that bullet, we would look at how in a license application and the safety analysis report the system was described and how it's integrated with other sections.

For example, saturated zone flow and saturated zone transport need to be consistent with each other. And this would be a place where we would look to see that, in fact, the approaches are consistent.

The next section goes to the justification of the data and the models that would be used. So at first we have presented. We described what is there. And now we look at how well the data and the models are justified.

Next we go on to evaluating to what extent

and how well has uncertainty in the data been evaluated. The next is going more towards model uncertainty. There may be different models that could be appropriate. And that section would provide guidance on how to determine whether they have appropriately accounted for the possibility of different conceptual models.

Then, finally, model supports are a topic which deals with how well the outputs of the model compare to some kind of objective comparison. It might be a comparison with field observations. It might be a comparison with the abstracted model with a more complex process-level model, but in general we want to see that the abstracted model is supported by some kind of objective evidence. This is the section in which it would be done.

Again, the depth to which you would go in any of these elements would depend on how important it is and how much credits the DOE was taking for it.

So, for example, if retardation in the saturated alluvium was determined to be a barrier that DOE is relying on to make their safety case to show that they will meet their performance objectives, then that is something that would be reviewed to a much greater amount of detail. If, on the other hand, they

decided that was not important or they didn't want to invest the energy in it or it just didn't have any impact on performance, you wouldn't spend as much of your time reviewing something that wouldn't really have an impact or wasn't part of the argument.

So yes, these are the sections that are called out, review methods that are called out in the review plan.

ACTING CHAIRMAN RYAN: It's kind of the intersection of the two points you made earlier, that the review plan is the items for review and the risk insights. It's kind of the focus in depth of those. To me, you can't get from one to the other.

MR. COMPTON: Right.

ACTING CHAIRMAN RYAN: Thanks very much.

MEMBER HORNBERGER: Allen? Ines?

DR. TRIAY: I wanted to ask you from the perspective of the approach that you are using to review what comes in from DOE, to use the phrase that was used here before. Do you model in parallel to DOE? Do you use your own modeling capability and then compare results at the end? Do you try to use their same assumptions? Do you use your own assumptions? Could you help me a little bit in terms of how do you provide that independent validation of what comes in

from DOE in this very complicated area of radionuclide migration?

MR. COMPTON: Okay. I'll try and take that on. And then I will see whether my answer is adequate.

MEMBER HORNBERGER: And how will that be judged?

MR. COMPTON: But the first thing that is important to bear in mind is that we review what the Department of Energy submits. It is the department's responsibility to make a safety case. So it is not our job to kind of independently decide. I mean, they have to make the safety case. So that is probably the first thing to keep in mind.

The role of independent modeling, it serves a number of roles. One of the things that it does is it gives us an understanding of how to review their model.

The fact that we have done these exercises gives us our independent understanding of what is important so that we can look for those if there may be gaps. It is very hard to find what is not talked about, but that is one of the roles of independent modeling. As well, there may be some role for independent modeling and confirming the calculation if

you want to check something to see whether there is something that has been done.

Does that answer your question?

DR. TRIAY: Yes.

MEMBER HORNBERGER: Jim Clarke? Jim Davis? Dick?

DR. PARIZEK: As I sort of watched the process over the years, it seems like NRC has remained constant. You have your rules, your regulations. I think they are the same as they were when this process started a long time ago.

Meanwhile the DOE appears to shift emphasis as it has to decide what the work products have to be and marshals its efforts and produces its results. And so you could get the idea that group is moving in different directions to create the final product that you folks are going to review.

Have you evolved in this same time period? To what extent have you evolved? I see like the safety analysis or the risk-based discussions have sort of elevated through time to make that very clear.

The KTI process has always been there and the FEPs process has always been there. Are you constant? Have you been constant? You have obviously done models. You have learned a lot. You do some

models in some cases in a limited way but enough to draw attention to aspects of the problem that really need attention, perhaps by DOE, reminding them, on the one hand, or understanding the benefit you get yourself, being able to make these analyses yourself.

Where has NRC been heading in all of this while? I mean, you obviously have learned a lot, and they have learned a lot. We have all learned a lot.

MR. COMPTON: Well, I will give two answers to that. And then I may pass the rest of it off. The first is that yes, it has been evolving. The second is that I have been with the NRC since last September, not enough to discuss the evolution. I don't know if Tim or Andy --

MR. McCARTIN: Yes. Tim McCartin, NRC staff.

We started doing performance assessments around 20 years ago. And we clearly have tried to incorporate the science as it has evolved. I guess I will give a couple of examples.

I mean, one I think will be a very good one you will hear later by Paul Bertetti about the $K_d$ approach and the pH dependence, et cetera, that he will be discussing. I think that is a very good example of something that how we are evolving with

time, changing our look at the $K_d$ approach.

Another example is matrix diffusion. I will say matrix diffusion in the unsaturated zone. At one time we had in our model, I will say 10 to 15 years ago. We no longer have it there. We have the capability to do it. But we came to look on it as not a very significant process.

And so there have been changes along the way. I will talk a little bit about that in my talk a very small amount with respect to matrix diffusion.

Colloids we look to DOE, who has actually done a little more work than we have in the colloid area. We continue to keep abstract of that. We are continuing to do analyses with colloids. Another version of the TPA code will have a more explicit treatment of colloids.

So things continue to evolve with time. I like to think we haven't stood still but continue to make changes in the areas we believe are significant.

DR. PARIZEK: And clearly the role of multiple barriers has not changed, the idea that the natural system barriers have got to be there to do something, but you can't take credit if you can't more or less establish why it does something for you.

So they still take credit when they can

and are not taking full credit for other aspects of the natural barrier system. But they have to have multiple barriers, right? That hasn't changed.

MR. McCARTIN: Absolutely. And I will say maybe a prime motivation to my talk tomorrow is a process that I have been involved with the committee for the last couple of years in terms of explaining and communicating our understanding of the Yucca Mountain with respect to the multiple barriers and that that actually is something that I think has evolved very well over the last couple of years in doing a better job of communicating that understanding. So there is actually another example.

It's not just the quantitative models but the explanation and the understanding that they provide. I will say that is a very important part that I think has evolved over the last few years also.

DR. PARIZEK: Thank you.

MEMBER HORNBERGER: And I will point out that Tim McCartin did start out with NRC before last September. Mike Ryan suggested that he had red hair when he started here.

MR. McCARTIN: Sadly.

MEMBER HORNBERGER: Don Shettel in Las Vegas, do you have a question?

DR. SHETTEL: Yes. I would like to follow-up on a previous comment. I think the saturated zone is an important barrier and may turn out to be the most important one, but regarding the vadous zone or unsaturated zone, residents' time for some radionuclides in the unsaturated zone is much longer than it is in the saturated zone.

As an example, neptunium, if there is a ratio of residents' time, the UZ to the SZ, is not one, that would suggest that the vadous zone is an important barrier, at least as far as DOE is concerned. I'm wondering if the NRC is going to have a similar meeting to decide if there is absorption in the vadous zone, the unsaturated zone.

MR. McCARTIN: Yes. Tim McCartin, NRC.

Yes. As Keith explained, in getting ready for this working group, we made a choice to focus primarily on the saturated zone. And so our presentations are related to that.

However, the unsaturated zone has many attributes that need to be examined and looked at. I mean, first and foremost, just the fact that it is unsaturated and how dripping occurs into the drips, how many packages might be dripped on is an attribute. Also, the Calico Hills vitric unit is a primarily

matrix flow only unit, where you are right.

The transport time will be decidedly slow. If there is some retardation, even slower, that is in our risk baseline report. It is also something that is accounted for in our simulations.

An important aspect is how much of the footprint of the repository is underlain by the Calico Hills vitric unit. There are other aspects with respect to the potential for matrix diffusion.

In our modeling, we have seen it be fairly limited in the unsaturated zone. DOE has shown it to be a little more in their models. That will be an aspect of our review.

So there are a lot of aspects to the unsaturated zone. We did make a commitment to just do the saturated zone. That was not to diminish necessarily the contribution of the unsaturated zone. It was one of time that we thought we just made a decision.

MEMBER HORNBERGER: Thanks very much. Thank you, Keith. You got us pretty close to back on time. Thanks to the presentation. We look forward to hearing the other presentations that you have presaged.

MR. COMPTON: Thank you.

MEMBER HORNBERGER: We are now going to take a one-hour break for lunch. We will reassemble at 1:00 o'clock promptly to be on schedule.

(Whereupon, at 11:59 a.m., the foregoing matter was recessed for lunch, to reconvene at 1:06 p.m. the same day.)

MEMBER HORNBERGER: We're getting ready to start here. It's precisely 1:00 Eastern --

(Laughter.)

-- more or less.

(Laughter.)

We're going to return to our working group session, and our next presenter is Bob Andrews, who is joining us from Las Vegas. And I want to thank Bob, because I know how tough it is for the people working for DOE and the contractors to make time to do this. And I want to tell Bob that even though he probably has made similar presentations many times, we do appreciate his willingness to do one more.

Bob, are you there?

MR. ANDREWS: Okay. Thank you very much. Yes. Can you hear me?

MEMBER HORNBERGER: Yes.

MR. ANDREWS: You can hear me okay?

MEMBER HORNBERGER: Yes, you're on.

That's fine, Bob.

MR. ANDREWS: Okay. Okay, thanks. Yes, thanks. I have the pleasure of, you know, summarizing and introducing, you know, Bill Arnold, who is going to talk after I and I think after some centered discussions that focus on the saturated zone.

My particular discussion will have a summary overview of transport aspects in both the unsaturated zone and the saturated zone. I didn't want to lose sight of the fact that part of the barrier below the repository to reduce radionuclide transport is, in fact, in the unsaturated zone.

So I'll talk at least conceptually about transport in the unsaturated zone, and then Bill will discuss in greater detail transport in the saturated zone later on this afternoon.

As a point of background, virtually all of the information that's in these slides was presented in earlier presentations to the NWTRB in March by detailed individuals from Lawrence Berkeley Lab, Los Alamos Lab, Sandia Labs, and the U.S. Geological Survey. And it's very difficult for us to summarize, but I've tried to do my best and pick the most salient slides that make a discussion of the conceptual models and the key tests that support those technical models

and the parameters that are being propagated in the performance assessments.

This work is also presented in two technical basis documents and the supporting KTI agreement responses that are presented in appendices to those technical basis documents that have been sent to NRC. Saturated zone was Technical Basis Document Number 11. That was delivered to NRC last fall. And the unsaturated zone transport is presented in Technical Basis Document Number 10, which I believe was sent to NRC towards the end of May of this year, so just about a month ago.

So this is in some ways a summary of information that's in those technical basis documents, which, in turn, are summaries of information presented in the model reports and analysis reports and data descriptions that support those model and analysis reports, that support those technical basis documents, and supported the addressing of the KTI responses in appendices to that.

So what I want to do is on Slide 2 -- and I believe you're looking at me versus the slides, and at least we are here I think --

(Laughter.)

MEMBER HORNBERGER: But we all have copies

of the slides, Bob.

MR. ANDREWS: Okay. That's good, because they're more colorful than I am.

(Laughter.)

So keep your head down.

Okay. We're going to walk through the unsaturated zone flow and transport processes and some key test results and data that support the understanding of those processes and conceptual models, and then do the same thing for saturated zone. And as I said, Bill, who follows later on this afternoon, will go into much greater detail on the saturated zone part.

Slide 3 summarizes the key processes of importance to performance, both of the barriers and to the system, in the unsaturated zone. We have changes in climate that have to be considered, the infiltration at the service and ultimately the percolation of that infiltration through the unsaturated zone, contacting the repository, the things that happen in the vicinity of the repository and the couple processes that occur in the vicinity of the repository.

In particular, the thermal, hydro, chemical, and mechanical processes are beyond the

scope of this particular presentation, but there are separate technical basis documents on those particular aspects of the effects of the repository construction and heat on flow processes in the unsaturated zone.

We then are concerned with how that water moves through the mountain from a transport perspective, in particular not just the flux distribution but how that flux is distributed between the fractures and the matrix, at the faults, the effects of perched water zones, and ultimately effects of variability throughout the unsaturated zone, both in different rock types and the difference and uncertainty of particular properties within a rock type -- for example, the lower lith versus the middle non-lith, and the differences in the uncertainty of the flow characteristics in those two rock units.

When we get to transport, Slide 4 talks about the different concepts and conceptual models of importance to radionuclide transport. I think the keynote speaker hit on several of these in his introduction, which I thought was excellent. And those same processes are relevant to us with respect to the performance of the barriers below the repository horizon itself.

Those, including advection, matrix

diffusions, or the diffusion of radionuclides into the matrix, dispersion, sorption, the transport of colloids, which is a little bit different than the transport of dissolved species, they are characterized differently.

I believe Bill will talk a little bit about that, but I think we've kind of focused our presentations today on the dissolved constituents, notably things like neptunium, technetium, iodine, etcetera, rather than the colloidal leak transported radionuclides, which include things like plutonium, americium, etcetera.

Slide 5 just has some words that summarize that we have models of unsaturated zone transport. Those models are derived from in situ testing, laboratory testing, some comparisons to analog information. There is indirect confirmatory information at the site itself, with respect to things like carbon-14 and other radiotracers that have to be also evaluated with respect to the understanding of both flow and transport.

Those tests are key to that understanding. Those tests are key to the models and the confidence in the models. And the tests are also key for developing parameter distributions that are used

within the models and the uncertainty in dose parameter distributions and then propagated through the performance of the unsaturated zone portion of the barriers below the repository horizon.

So on Slides 6 and 7, just to orient people to the tests that I'm going to be focusing on, because we have done explicit tests of transport within the unsaturated zone media at Yucca Mountain, the ones that I'm going to be focusing on -- one is Busted Butte, which is just to the south end of the repository block. A picture of Busted Butte is shown in the lower left-hand corner of Slide 7, looking to the east/southeast from the crest.

And then I'm going to talk about -- Busted Butte is on a scale of about 10 meters, roughly 10 meters. Then I'm going to talk about some cross-testing conducted in the repository block or just east of the repository block itself. One is Alcove 8, Niche 3, where the opportunity of the cross drift going across the ESF main allowed the possibility of putting in water and tracers in that water and evaluating the transport of those tracers through roughly about 20 meters of unsaturated rock.

And the other test was done in Alcove 1, between the surface and Alcove 1 at the east end of

the northern ramp. And, again, that's tens of meters scale of transport, where we put the water surface and observe breakthrough of different dissolved constituents in the ESF alcove.

So I'm going to talk a little bit about each of those three test configurations, not the details, but the general understanding, conceptual understanding of transport processes that's derived from those tests.

Starting with Slide 7, it simply shows the cutaway and the actual test layout on the right-hand side for -- more or less for background. Slide 8 talks about the different tracers. Sorry about the typo on fluorescein. There's an S before the C. I think that occurs a couple of times, to be honest with you.

So this shows the individual injection holes, and we're actually looking at transport across different rock units in the Busted Butte evaluation. And then we're looking at varying ways of observing that transport through that rock mass at the scale of that particular test.

Some of the actual data are shown on Slide 9. On the left-hand side, we show breakthroughs or distributions of two tracers -- lithium and

bromide. At the right, we show a diffusive halo if you will of fluorescein as it is moving through, in this particular case, the Busted Butte test.

In both cases on Slide 9 we show the actual observed data. In the case of the left-hand side, the observed data are with the data points measured at different times for the different tracers. For example, lithium was measured at 337 days and 440 days, and you see the models in comparison to those breakthroughs.

So the models with matrix diffusion and with a very limited amount of sorption were able to reasonably reproduce the direct testing that was performed there for both the lithium and the bromide. And the right-hand side for the fluorescein, the bottom part is the model, the top part is a halo if you will observed of the fluorescein dye as it was moving through the fractured rock mass.

So the Busted Butte test wasn't so much used to develop parameters per se, but it was used to test the conceptual models of transport through smaller sections of fractured rock mass -- a little bit off of the repository block itself.

We then go to Slide 10, where we're looking at Alcove 8, Niche 3, Alcove 8 above in the

ECRB and Niche 3 down below. And you see in the upper right-hand side of Slide 10 that we've put a -- essentially an infiltration plot in the drift, and then evaluated, after a period of reaching steady-state on the infiltration side, added some tracers and evaluated the tracer migration between Alcove 8 above and Niche 3 below.

As you can see, Alcove 8 itself is in the upper lith, and Niche 3 is in the middle non, so we're kind of crossing both of the primary rock units within the repository block. Some of the data and a comparison of the data to model results are shown on Slide 11.

Again, the differences in the transport characteristics of in this case lithium bromide and pentafluorobenzoic acid are driven primarily by the different sizes of those dissolved constituents, and you see that effect with respect to the diffusive characteristics, in particular the matrix diffusion characteristics, of the fractured rock mass.

And one aspect of uncertainty that has to be evaluated and propagated is: what is the actual interface area between the migrating dissolved constituents and the rock mass? That's not something that's usually directly measurable or observable --

that fraction of the rock mass that actually is taking part in matrix diffusion.

So uncertainty in that particular parameter was first off evaluated in this test, but in the uncertainty, and that parameter has to be propagated through to the assessment of the performance of the unsaturated zone feature of the barrier below the repository.

Moving on to Slide 12, a third test in the unsaturated zone, where at the surface -- this is at the eastern end of the north ramp, just as you enter into the ESF. There was an infiltration zone put at the surface, that thing called blue cover. It's just a blue cover put on the -- above the infiltration that was artificially applied to try to minimize the amount of evaporation and control the actual amount of water that was being applied at the surface and allowing it to, if you will, recharge at the surface and then go through the unsaturated zone, such that it could be later on collected at the Alcove 1 with a series of sheets and other water collection devices.

So similar to Alcove 8, Niche 3, there's water applied. This was not an ambient system flux. It's an artificially perturbed flux, in order to get measurable concentration breakthroughs within a

reasonable period of time, where that would be in the order of years instead of tens of years or hundreds of years, whatever it might have been.

So the schematic of the test shown in Slide 12, some of the key results also indicated there as well, where, again, the process of dissolved constituents -- in this particular case I'm not sure. I didn't write down the actual tracers that were selected for that particular test. But the arrival of the tracers at the -- in this case the Alcove 1, some 30 meters below the actual surface at that point, required the incorporation of matrix diffusion type processes.

So it wasn't just an advective transport through the fractured rock mass, but it required the interaction of that dissolved constituent with the rock matrix in which it was in contact with. So that matrix diffusion process, again, was evaluated and determined to be conceptually a strong basis and valid for that scale of rock mass -- again, on the scale of 30 meters.

You see some of the tests results and model prediction results in Slide 13, where in the red we actually look at the application of the tracer, and then in green are the actual observed breakthroughs at

the point some, as I say, 30 meters below in the drift in this particular case.

So with respect to the unsaturated zone, which I know is not the focus of a number of the talks that will follow, a number of tests, in particular focused on the in situ tests here, but those have been supported by analog evaluations and laboratory tests, have kind of confirmed the conceptual basis, the conceptual models used in the unsaturated zone transport characterization and model.

Those tests are also used to provide data to constrain the parameter distributions, the reasonable parameter distributions of transport-related parameters, and for the particular sorption-related transport parameters, which I haven't presented in here. Those are primarily derived from laboratory-based testing.

But in the cases where a laboratory sorption measurement is comparable to a tracer that was used in an in situ test, the transport characteristics, the sorption characteristics, are virtually analogous. If anything -- and we'll see some examples here when we get to the saturated zone -- the laboratory sorption measurements predict a slightly lower sorption, lower equivalent Kd, than

what would be derived in situ for the cases where there is a similar dissolved constituent that's being compared. Of course, we're not testing these things with radioactive tracers in the field for very obvious reasons.

And uncertainties, then, in parameters, whether those be sorption parameters or transport parameters in general, such as matrix diffusion, effective porosities, fluxes, etcetera, have been included and are being propagated through with respect to the performance assessment, where now performance assessment -- in the most general sense of the word, that includes the total system performance assessment and the evaluation of the capability of the barrier as required in Part 63, and as will be summarized in the safety analysis report later on this year.

Switching gears to the saturated zone, we have a conceptual picture. I think we've probably used this conceptual picture several different times to show the different transport behavior of the fractured tuffs versus the alluvium, and that difference in transport behavior, transport characteristics, is directly evaluated in a couple of tests that I'm going to talk about in summary fashion here today.

I want to preface this by saying that, unfortunately, to date we haven't been able to form a full cross-fold tracer test in the alluvium for permitting reasons. I believe Nye County, who is going to talk tomorrow -- Dr. Hammermeister is going to talk tomorrow -- will talk a little bit about the current status of any plans for tracer testing, crustle tracer testing in the alluvium.

So to date the only test -- and I think I'll have one example of that -- in alluvium, a relatively large scale of transport is what can be varyingly called a huff-puff test or an injection/withdrawal type test from a single pull, where you inject a tracer, let the natural gradient take over, and then withdraw the tracer and evaluate what that tells you about the transport characteristics of the alluvium. And we'll talk a little bit about that in a second.

So we have very different, not processes, but different geologic characteristics that affect the transport behavior in both the volcanic aquifer, the tuff aquifers if you will, and the alluvial aquifers. I'm just trying to show those conceptually on Slide 15.

On Slide 16, again, an introduction. Just

as in the unsaturated zone, the Department has utilized large-scale testing to characterize the transport models, to validate those transport models, and to develop parameter distributions and their uncertainty that are used to propagate with respect to the behavior and characteristics of the capability of now the saturated zone component of the barrier to radionuclide transport below the repository horizon.

Slide 17 shows -- and this may be a little bit out of date with the most current Nye County work. It's as of about six months -- no, nine months ago, at the time we wrote the Technical Basis Document Number 11 on the saturated zone.

But it shows the individual bore holes used to -- in the saturated zone used to evaluate geochemistry, used to evaluate hydrology, in particular flow characteristics, potentials, etcetera, and a blowup of the two multi-hole locations, one in the tuff aquifer up above, the C-wells complex that's been called, and one down below closer to Highway 95, the alluvial testing complex in the -- some of the Nye County early warning drilling program polls.

I think Bill will talk a little bit about the geochemistry and the use of the geochemistry to in part constrain and evaluate the likely paths of

groundwater flow once you enter the saturated zone. I've left those out of my discussion, as well as the potential evaluations, and have kind of focused on the transport in situ testing information.

Now, these wells, as you can see, both at the C-wells complex and the alluvial testing complex, are on the order of tens of meters apart. We have used some larger scale if you will tracers that I believe Bill will talk about to help constrain general transport paths and general transport rates, although those general tracers, like carbon-14, like -- I don't think we're going to talk about uranium-234, U-238, although that is presented in the technical basis document.

There are some limitations on how far you can take those larger-scale, naturally-occurring radio tracers with respect to evaluation of transport at Yucca Mountain. So we have relied pretty heavily on these tens of meters scale tests, especially at C-wells.

Slide 18 just gives you the hydrostrategigraphy, lithostrategigraphy, at C-wells. A couple of important aspects here. Those little triangles are from flow meter logs, the actual percentage of flux in the well when it's being pumped,

and where did that flux come from.

I think as the panel is well aware, the actual distribution of transmissive features that can yield water, and, therefore, are most likely to transport any dissolved constituents is fairly limited in a fractured rock mass. Not every fracture carries water and is equally transmissive. In fact, you see, you know, for most of those holes, for those three wells, either three or four zones that are carrying most of the water. And, in fact, it's one or two zones that are carrying most of the water.

We factored that distribution. We've called that the flowing interval spacing in the technical basis document and in the model reports that support the saturated zone flow and transport to say that that's where, if there are dissolved radionuclides or colloidal radionuclides that enter the saturated zone, it would be in those features that they are principally transported within.

Going on to Slide 19, there was a long-term, year and a half-ish pumping test conducted in C-wells. That pumping test was used to evaluate larger scales, the scale now of kilometers, flow characteristics. They weren't -- didn't have transport at that scale of kilometers, but there was

at least an evaluation of the general flow characteristics in the saturated zone at the scale of kilometers that was evaluated as a result of this, you know, year and a half long pumping test in the fractured rock mass.

Slide 20, and also 21 -- but let's start with 20 -- is a representative cross-hole tracer test conducted in the C-wells for a range of different dissolved and an equivalent of a colloidal species. Those 360-nanometer spheres -- and we've looked at different size of microspheres and their transport characteristics, those different -- those spheres represent an analog if you will for colloids as colloids might be transported through the saturated zone. And any radionuclides that may be sorbed onto colloids could be transported with that colloidal mass.

Again, different tracers being used in part to evaluate different diffusive characteristics and to confirm the different diffusive characteristics, in particular the matrix diffusion, between the individual bore holes. So even though the water is predominantly moving through some of those flowing features that I presented on Slide 18, during the injection/withdrawal test, the dissolved

constituents are interacting with the rock matrix.

So, again, the matrix diffusion model and the characteristics of the diffusion being related to the size of the dissolved constituent -- bromide in this case being a larger diameter than the PFBA, the pentafluorobenzoic acid -- and, therefore, being less likely to sorb or to -- sorry, to diffuse into the rock matrix.

Again, the third bullet, the sorption values -- even though I haven't shown them on here -- or they're going to be shown in the next slide. For the in situ tracer tests confirm and, in fact, are a little higher than the laboratory sorption characteristics of these particular dissolved constituents.

So Slide 20 simply shows some of the data for a particular test. Slide 21 shows a little bit of laboratory data on top, essentially column breakthrough tests for the -- some of the different tracers used in the C-wells transport test -- in this particular case, bromide versus lithium, and then the bromide-lithium breakthrough and model results down below for the in situ test.

So, again, the laboratory sorption measurements, the column-type sorption

characteristics, indicating a Kd in this particular case of about .1 to .3 milliliters per gram, and the field Kd, to get a reasonable reproduction of the observed breakthrough of, in this case, lithium being constrained between .6 and 4 milliliters per gram.

So, again, the in situ sorption values being -- from this experiment anyway being slightly greater than the laboratory-derived sorption values. So the use of the laboratory-derived sorption values is conservative with respect to any application of them for post-closure performance.

Given that the chemistry, as we talked about earlier this morning, along those flow paths, likely flow paths remains reasonably stable and constant with time and space along that flow path.

Moving to Slide 22, this is that -- the results of that single whole injection/withdrawal. They were injected for a period of time, let sit for a period of time, and then withdrawn for a period of time. And you can't really get -- well, you could, had you used a sorbing-type tracer. You could have determined something about sorption from these tests, but we used non-sorbing tracers for this particular test.

So, essentially, what you're doing is

trying to summarize and evaluate -- and you can't distinguish between flux and velocity using these. It's one kind of lumped parameter. But using reasonable ranges of effective porosity of the alluvium, you get a range of possible alluvial fluxes, as I show there, between roughly one and nine meters per year at that particular location.

The site-scale model that Bill will talk about later gives a median value for nominal set of conditions without uncertainty of roughly two meters per year. So it's right in the same bracket as the range of possible single-hole injection/withdrawal tests.

And, as I say, there have been plans over the years to do multi-hole tracer tests in the alluvial testing complex or similar multi-bore hole locations in the alluvium. And I think Dr. Hammermeister will talk about those -- the current status of those plans tomorrow.

There is one other type of information that I -- even though I kind of focused on the in situ observations up to this point, doing in situ sorption other than with simple tracers is prohibitive, both in time and in terms of protecting the environment. So the sorption characteristics of radionuclides of

importance to performance are determined in the laboratory.

Some examples of those sorption measurements are shown in Slide 23. These are different samples showing different grain size distributions at different locations. We've looked at different chemistries and their effects on sorption measurements, different mineralogies, although limited by where we have samples, and different radionuclides. But it kind of focused on, at least for this particular slide, on neptunium and uranium sorption.

And it's these data averaged over the reasonable range of grain sizes expected that are used to develop a reasonable range of sorption characteristics in the alluvium. And similar observations are available from laboratory experiments conducted over the last, you know, 10, 15 years, some of them conducted by Dr. Triay and her co-workers in the early and mid-'90s, argues for the sorption characteristics on the tuff aquifers and, for that matter, in the unsaturated zone as well.

So, in conclusion, just as we had in the unsaturated zone, the conceptual models we have for transport behavior in the saturated zone have been developed and are based largely on the in situ testing

that we have. The in situ testing has not only been used to evaluate those models, but also to develop the parameter distributions, reasonable ranges of those parameter distributions that we apply with those models.

And I would be remiss to say that, you know, these are just a one-shot, you know, transport evaluation. There is uncertainty in the model. There is uncertainty in the parameters within the model.

I talked about some of those parameters today, things like flowing interval spacing, the effective porosity within that flowing interval spacing, the degree of matrix diffusion, and the sorption characteristics of the individual radionuclides themselves along the likely travel paths, both in the unsaturated zone and the saturated zone. And that uncertainty is propagated through both the barrier evaluation and the total system performance assessment.

So with that, I will stop and entertain any questions.

Dr. Hornberger?

MEMBER HORNBERGER: Thank you very much, Bob. Obviously, Bob has summarized a tremendous amount of information and work. It's going to be our

task to try to focus our questions on the issues that we really want to grapple with most significantly in this meeting.

But with that warning, I will proceed to questions. Ruth?

MEMBER WEINER: I have a couple of questions. Are you doing anything to the site by introducing water? I mean, you're not introducing that much water. But what's your sense of that?

MR. ANDREWS: Yes. When we -- before we do any test at the site, especially any test that's near the repository block itself, a detailed -- I think they're called design evaluation -- design impact evaluation is performed to evaluate, what's the impact, if any, on performance or safety associated with doing the test, whether we're putting water at the surface, whether we're putting water underground.

We have an advantage that before this site, assuming it's licensed, is closed, there is a lot of time that transpires. And the natural system is fairly forgiving with that amount of time. But a particular evaluation is done for that water and any other constituent that's introduced during the physical test itself, just as we do with the actual construction of the -- for example, the cross-drift or

the ESF where there are evaluations of the impact of diesel emissions and other organic emissions during the construction activities themselves.

MEMBER WEINER: My second question is: how location-dependent are your tests for validation? In other words, if you -- the unsaturated zone clearly is not homogeneous. It's clearly heterogeneous. If you did the same test in a number of different sites, how different would your results be? Do you get an uncertainty band that way or --

MR. ANDREWS: Yes. Generally, we're using the individual tests -- you know, whether it be the Alcove 1 stuff or the Alcove 8, Niche 3, or Busted Butte -- we're using those with our models to evaluate the confidence or robustness in the conceptual model and conceptual understanding itself.

Clearly, the parameters, you know, at that particular location where the test is performed are contingent on where you give the test. So you then are saying, "I have a model. I have a reasonable, you know, approximation through the observations at that particular point in space with this set of parameters." You know, whether that be matrix diffusion parameter or, you know, sorption parameter or fracture characteristics, whatever.

We then, when we extrapolate it or interpolate it to the whole mountain, have to consider, you know, uncertainty in that parameter and variability in that parameter now at the scale of a mountain and from location to location.

So in large part, uncertainty in matrix diffusion, in fracture characteristics like fracture porosity and fracture-matrix wetting area, and sorption characteristics are derived not solely from that similar test or singular test, but also from other lines of evidence, including, in some cases, literature information and other sources that we try to characterize the global uncertainty in a particular parameter that we then propagate through to the evaluation of the barrier itself. So the test --

MEMBER WEINER: So you find something -- I'm sorry. Go on.

MR. ANDREWS: I'm sorry.

MEMBER WEINER: You find something drives the uncertainty, and others don't?

MR. ANDREWS: Yes. I mean, there are some parameters within the models as they are implemented that drive, if you will, the behavior of that particular barrier. Those are generally described, the most if you will significant parameters -- and I'd

have to be careful about it -- what the measure of -- of performance that you are looking at. You know, for example, is it a median breakthrough of a particular radionuclide, or what?

But generally, it's able -- you're able to post-process if you will the model results and determine, okay, this particular parameter or this suite of parameters drove the 95th percentile on the breakthrough of this particular radionuclide. So that's kind of a -- if you will a post-processing evaluation once you've implemented the model. But that's possible, yes.

MEMBER WEINER: Finally, I assume you heard Dr. Davis' presentation on the Naturita experiments. And I'd like to have you comment on the question of you have models and you validate them against real experimental data. And presumably you benchmark them, calibrate them against that -- those data. And then you do a random sampling of your models. Isn't that correct?

MR. ANDREWS: Yes.

MEMBER WEINER: Could you comment --

MR. ANDREWS: For parameters, yes.

MEMBER WEINER: Yes. Could you comment on Dr. Davis' statement that modeling -- doing a post-

experiment model, in other words going from the back end, doing that rather than doing random sampling, how -- put another way, what is your sense of the validity of the random sampling method?

MR. ANDREWS: Oh, I think that the -- in the random sampling -- well, it's not totally random, because there's correlations, you know, of what's being sampled to the different lithologic units that you're dealing with. And if there was, you know, a variation in -- a significant variation in geochemistry that significantly affected, you know, transport behavior, there would be that correlation as well.

But I think the degree of complexity or the degree of sophistication you put in any particular representation, whether that be a fairly simplistic representation which the linear Kd-type model represents, or a more, you know, sophisticated complexation-type model, both of them have to be fundamentally compared to the observations. They are both models, and they have to be compared back to data, whether they're simple models or more complex models.

Propagating uncertainty is required in either model, either a simple model or a complex

model. You have to have the capability to evaluate the complexity and the -- not the complexity, excuse me -- the uncertainty in that characterization and propagate that uncertainty with -- generally speaking, with a more complex model you have more individual uncertainties, such as surface area or chemistry reactions, that you have to consider and propagate, whereas with a simpler, you know, sorption-type model there is generally one uncertainty, one parameter that you kind of lumped a lot of the complexity in, and the uncertainty in that also has to be propagated.

But I think in either case, whether you take a complex model or a simple model, the propagation of the uncertainty within that model as it affects performance would be I think about the same.

MEMBER WEINER: Okay. Thank you.

MEMBER HORNBERGER: Mike?

ACTING CHAIRMAN RYAN: Thanks, Bob. I agree with George. You covered a lot of ground in a real short period of time.

I've got another uncertainty question, but hopefully it's a little simpler, at least it is in my mind. If you had to pick two or three things in the unsaturated zone, and two or three things in the saturated zone, that are the drivers of uncertainty at

this point, what would they be?

MR. ANDREWS: Probably the sorption behavior of a couple of key radionuclides. Like neptunium -- I think that's the focus of some of your questions -- was on that. The actual flow characterization, you know, within the fractures, both in the saturated zone and in the unsaturated zone, ends up being a fairly significant parameter, in particular with respect to, you know, earlier breakthroughs and later breakthroughs.

So those would probably be the two key ones, but I think that probably that's for the detailed modelers who follow me to --

ACTING CHAIRMAN RYAN: If it's not a fair question to ask you, that's fine. But, you know, I guess what I'm trying to do is get in my mind some order of what things are really driving the bus in terms of uncertainty. You know, and interesting one is -- and, again, it's a question on breakthrough.

For neptunium, it's the time of arrival. But does it affect the concentration? Because concentration is what drives dose, not the time of arrival, because there's relatively little decay. So ultimately I'm thinking about these things and my list, not so much in terms of the geohydrologic model,

but do they or do they not have an impact on an ultimately calculated dose? Some of them might, and some of them might not.

MR. ANDREWS: Yes.

ACTING CHAIRMAN RYAN: Any thoughts there? Again, that may not be a fair question for you, but --

MR. ANDREWS: Yes. And I think it's a little unfair to say the time of arrival is not a significant evaluator. I think the time of arrival is in part the barrier capability that Part 63 asked for. And the time of arrival, although I'll agree with you the difference between 1,000 years and 2,000 years is not significant, the difference between 1,000 years and 20,000 years is significant to --

ACTING CHAIRMAN RYAN: That's a fair amount, and I certainly accept that.

MR. ANDREWS: -- system performance. So the time of arrival can make a significant difference to Yucca Mountain performance and barrier performance.

ACTING CHAIRMAN RYAN: And I guess that's what I'm trying to get a feel of -- in your mind, where are the ones where those differences are potentially significant or important, and where are they relatively minor in terms of, well, it's not going to have a big impact? So I'm just trying to get

your top three. And I think you've given me two -- you said neptunium and --

MR. ANDREWS: Okay. I gave you two.

ACTING CHAIRMAN RYAN: Yes.

(Laughter.)

And that's fair enough. That's close enough. I appreciate your insight.

MR. ANDREWS: Okay.

MEMBER HORNBERGER: Allen?

DR. CROFF: Can you comment on the extent to which matrix diffusion -- to which you found it to be reversible in your test?

MR. ANDREWS: I'm probably not the person -- I'll have to find someone who was actually closer to the test, to be honest with you.

DR. CROFF: Okay. Again, if it's not fair, we'll --

MR. ANDREWS: Yes. Let me try to find someone to have an answer to that, okay? Because I'm not close enough to that particular test, to be honest with you.

DR. CROFF: Okay.

MEMBER HORNBERGER: Ines?

DR. TRIAY: What have you found in terms of the effect of colloids on radionuclide migration?

Would you say that the colloids have a large impact? And to what extent have you been able to bound the effects of colloids on radionuclide migration?

MR. ANDREWS: I hate to use this answer, Ines, but it depends. It sort of depends on -- in large part on the chemistry. In particular, it depends on, you know, the pH of the solution and depends on the ionic strength of the solution.

And the pH and ionic strength, as you move away from the repository block itself, from the drifts themselves, does return to more or less ambient, but right in the vicinity of the drifts and inside the package and inside the engineered barrier system, the invert if you will. Those pH's and ionic strengths can vary significantly depending on the amount of evaporation of water that occurs.

And they -- you know, those chemical controls on the colloid stability end up being fairly significant with respect to the behavior of colloids in the drift and in the package, if they happen to get into the package. And, of course, the degradation of glass waste forms creates colloids, so, you know, smectite-type colloids.

So then the effect in the far field is somewhat more constrained, because, you know, you're

generally dealing with a more well-defined and less time-varying geochemistry. So it kind of depends on where you are and what time you're talking about -- the answer to that particular question. So sorry to give you a PA kind of answer, but --

DR. TRIAY: Everything is uncertain, right?

(Laughter.)

MR. ANDREWS: Yes, on that one.

DR. TRIAY: Let me ask you another question. Based on the previous talk on the -- on the talk of Dr. Davis, can you tell me -- you were talking about the differences in water chemistry that could be experienced at the site.

Could you give me an idea, from your perspective, given all of these sensitivity calculations, you know, that you have performed, whether those changes in water chemistry, because from the perspective of, you know, what I understood from that first talk, that water chemistry is almost all-important when it comes to applying the surface complexation models.

To what extent do you feel that that water chemistry would have a big effect on the sorption data that you're utilizing in your transport model?

MR. ANDREWS: Yes. Let me try to -- first off, the bulk of the sorption data that we're using in our transport model are derived using J-13 type water in the experiment. You know, J-13 is more or less along the flow path in the fractured tuffs between the zone underneath the repository and the 18 kilometer point of the reasonable maximally exposed individual.

So that water -- and that water is sampled, you know, periodically, and chemistry done periodically on J-13 water by the USGS. And it's been fairly stable, you know, during -- with time over the 20-plus years that J-13 and J-12 have been pumped.

Now, if you move away from J-13 -- and I think Bill won't talk so much bulk chemistry but types of chemistry in the saturated zone. Along the flow path, the likely flow paths, the chemistry and the saturated zone are, you know, fairly homogenous I'm going to say. And Bill will talk and show some plots I think of different chemical signatures, gross chemical signatures, in the saturated zone.

There are observations in the saturated zone where the chemistry is significantly different, and, in particular, where the redox state is significantly different, i.e. there are zones where it's reducing, and there are zones where it's more

oxidizing.

Our current evaluation and current model says that along the likely flow paths the geochemical system is fairly stable and likely to be oxidizing along the most likely flow paths. Those zones that are more reducing and more reducing in our system -- just for those of you not aware, more reducing in our system has a significant effect on several radionuclides transport, most notably technetium, but also the other actonides are seemingly infected by the redox state of the groundwater.

So those more reducing conditions, which are observed in the saturated zone, we believe are not really along a likely flow path. So taking performance credit for that significant change in chemistry off of the flow path we didn't feel was appropriate. And so that particular, you know, model uncertainty of the -- where the chemistry is with respect to the flow paths has been excluded from the barrier evaluation and performance assessment.

But along the likely flow paths, the chemistries and the different chemical signatures are, in fact, very similar. So using a singular sorption mechanism, not affected by time or space, we felt was appropriate.

So if you will, the complexities of geochemistry in the saturated zone are not that great along the likely flow paths.

DR. TRIAY: So that homogeneity in chemistry, does that help you to bound, then, the effect of colloids?

MR. ANDREWS: We use that chemistry in our evaluation of colloid transport. I'd hate to use the word "bound," but we use the chemistry in the saturated zone to -- in our development of colloid transport-related parameters in the saturated zone. So I think "bound" is probably not the right word to use, but that effect is factored in.

MEMBER HORNBERGER: Jim Clarke?

DR. CLARKE: Bob, the Alcove 1 test revealed that matrix diffusion was important in the areas of densely-welded tuffs. I can't tell from your conceptual model on Slide 3, is that a large area? Is this a significant retardation process overall in the vados zone? In other words, what flow will be intercepted by these densely-welded tuffs?

MR. ANDREWS: Well, I mean, the Alcove 1 is -- I mean, you're right, it's at the surface, which is -- I believe it's probably Tiva Canyon, at that particular test location. So it's probably, if you

will, more densely welded than the Topopah Springs welded tuffs. But the welded tuff characteristics and concept of in a welded tuff you can have matrix diffusion processes operative, is more or less a validation of the model, the actual parameters, than -- that we used be --

DR. CLARKE: I'm just asking how significant that is overall to transport through the vados zone. Will --

MR. ANDREWS: It's fairly significant.

DR. CLARKE: So --

MR. ANDREWS: It's not so significant when you look at something like the 50 percent arrival of mass. But it is I believe -- and there are sensitivity analyses that I didn't bring. I think it is fairly significant for the early breakthrough arrival of mass. So it kind of depends on where you are on the breakthrough curve, if you will.

DR. CLARKE: So I guess this is kind of a question for the NRC. But does that mean that translates to a risk insight? Would that have a high significance similar to retardation in the alluvium? I guess this is fairly new, these data, or is this report just provided in --

MEMBER HORNBERGER: No. I think the NRC

has seen this stuff.

MR. CAMPBELL: We've been following the C-wells and other tests for quite a few years.

MEMBER HORNBERGER: Andy, he's talking about the tuffs, the Tiva Canyon, the Alcove -- test in the alcoves and the over --

MR. CAMPBELL: I'm going to have to defer to someone who can address those funds.

MR. ARLT: Yes. Hans Arlt, NRC. No, we are aware of that. DOE does claim a lot of credit for matrix diffusion in the unsaturated zone, and we have a few agreements that do cover that.

DR. CLARKE: I just wondered on a risk insight basis, does that mean that the unsaturated zone has attributes of high significance as well? We heard about the one this morning for the saturated zone. Would matrix diffusion then have that level of significance on a risk insight basis for the unsaturated zone?

MR. ARLT: I think that was medium significance.

DR. CLARKE: Okay.

MR. ARLT: But it's been rated.

MR. CAMPBELL: Our key slides with that was with a group of three that we rated as medium as

opposed to the retardation in the alluvium. And that's primarily based upon, like I said, many years of analyses that we've done with the TPA code and other things and looking at all of the information.

MR. ARLT: That is one of the most important things that we'll be looking at from the NRC side is the matrix diffusion in the unsaturated zone, and also the Calico Hills, they are very aware of that.

MR. CAMPBELL: Keep in mind that if in DOE's model -- again, as to repeat what he said earlier today, DOE's model takes a lot of credit for that. But we're going to invest enough resources to evaluate that, the importance that they attach to that particular area. That's a very important part of their model, but we're going to invest the resources in evaluating that thoroughly.

DR. CLARKE: Okay. Thank you.

MEMBER HORNBERGER: Jim Davis?

DR. DAVIS: Yes. You mentioned that the pH and ionic strength increase near the waste repository, and that it attenuated away towards the ambient values. I was wondering what -- over what distance -- are you saying that it gets back to ambient values? And how is that determined?

MR. ANDREWS: It depends on the time. Initially, but, you know -- and I say it depends on time because of the thermal environment, and the thermal environment is changing significantly over time. And, in fact, a thermal environment, even within 10,000 years, is probably not back to, you know, ambient. It's still slightly elevated with respect to the ambient.

And it's that thermal environment that drives the chemistry evolution inside the drift. And if a package has been degraded inside the package, the degree that the thermal environment returns to, if you will, more or less ambient is in the first, you know, roughly 1,000 years if I go five, 10 meters away from the drift.

So if you just take round numbers, take 10 meters and 1,000 meters, you're close to the ambient thermal environment. The chemistry is still trying to catch up, if you will, to that to that change in temperature, and that takes another, you know -- I'm talking extemporaneously here.

I thought you'd look at the plots, to be honest with you. But another 1,000 or so years before the chemistry returns in that vicinity around the five, 10 meters around the drift -- I mean, therefore,

in the drift to close to ambient chemistry.

So it's generally in that few thousand year timeframe, driven mostly by the thermal perturbations on the chemistry, that the pH, the carbonate concentration, other dissolved constituents are significantly changing. And it's not always increasing. Sometimes it's decreasing. It depends on which constituent you are talking about due to the water-rock interactions during the dryout phase and the rewetting phase, if you will, of the thermal profile around the drift.

But to answer your question in a very, you know, general way, it's usually in a few thousand years we return close to ambient chemistry, which then would correspond to ambient, you know, sorption type and other characteristics from a geochemistry perspective.

The rock itself, although it undergoes a change, it is not a significant change in rock mineralogy during that thermal pulse. So it's mostly the aqueous chemistry that's changing.

DR. DAVIS: So after the thermal pulse has subsided, is there -- do you think that there is going to be any impact to the waste packaging or the total waste environment on the chemistry of the water, aside

from the thermal impact which will last 1,000 years or so? I mean --

MR. ANDREWS: Inside the drift, yes.

DR. DAVIS: But how far out does that --

MR. ANDREWS: Inside the drift.

DR. DAVIS: How far away from -- in the unsaturated zone is that chemical perturbation going to go?

MR. ANDREWS: That's probably on the order -- I'd have to look at the actual model report to be honest with you. But off the top of my head, I'd say on the order of meters that it extends, because the fluxes that -- the water volumes, even though the concentrations are significantly different right next to the waste, and in the invert, because of the thermal behavior in that zone, the volumes of water and the fluxes of water are significantly lower than the volumes of water and fluxes of water that are in the rock mass itself going around the drifts. So you have kind of a dilution, if you will, effect based on just volumes and masses of water that are in the rock.

MEMBER HORNBERGER: Dick?

MR. ANDREWS: There is a model that addresses that. I'd just have to get that for you.

MEMBER HORNBERGER: Dick?

DR. PARIZEK: Yes, Bob. Could you give us sort of an update on what studies are still underway in the saturated zone/unsaturated zone? Unless others later today and tomorrow are going to speak in detail. But, you know, what's still going on in the program that's part of the present work, that may be different than the science and technology initiatives?

MR. ANDREWS: I'll let, you know, Nye County and Dr. Hammermeister talk about the saturated zone, because they probably are closer -- a lot closer to that on what the current testing that's going on there, additional drilling and testing in their early warning drilling program holes is.

With respect to the unsaturated zone, there's continuing monitoring of Alcove 8, Niche 3, continued monitoring of drift scale test, the heater test, occasional chemical samples taken, water samples taken for chemical analyses from the drift scale test.

Those are probably the two if you will active testing. There's a number of tests going on with respect to the thermal mechanical behavior in the cross-drift and in the lower lith, but that's not really germane to this discussion.

That's kind of -- Drew, do you --

MR. COLEMAN: That's a pretty good list.

DR. PARIZEK: About a year ago, the program also gave a confirmation testing briefing to the ACNW, and it was quite extensive -- Debbie Barr's presentations. Is there an update on the status of the confirmation testing program that DOE is working with?

MR. ANDREWS: Yes. That plan is being revised slightly, and the actual revision will be reflected in the safety analysis report, you know, as required in the Yucca Mountain Review Plan. The actual plan I think will be available slightly before that, but maybe DOE should speak to the actual timing, you know, of that.

MR. COLEMAN: Yes, that's in preparation, and we're going to be reviewing it here in the next month or two and accepting it.

DR. PARIZEK: Then, you brought us back with this long-term test that was done in the tuffs. That's six years and seven months ago approximately when that test ended. But that year and a half pumping test delivered something on the order of about .44 million cubic meters of water, I think you indicated. And in that water that was returned back to the alluvium, somewhere down around I guess J-13. It had the tracers in it, it had microspheres in it.

Is it possible to use that as a tracer experiment?

I mean, that slug of water is in the system flowing along, and it has been there long enough that you have six and a half -- almost -- more than six and a half years, and you might find the plume and use that as a long-term tracer test or get value out of it if you could actually find the plume? Is that possible? Has any thought been given to that?

MR. ANDREWS: It's very possible. There has been thought given to it. I believe the USGS and maybe it's Los Alamos -- I'm not sure who they cooperated with -- have a -- I don't know if it's in the form of a proposal to the Science and Technology Group to do exactly what you just said. I don't know what the status of that is, though, to be honest with you.

MEMBER HORNBERGER: Thank you.

MR. ANDREWS: So it has been proposed and --

MEMBER HORNBERGER: Okay. Thank you.

Don, do you have a question?

DR. SHETTEL: Yes, I have a couple of questions. The first one involves injection rates in the UZ experiments lead to the higher matrix diffusion. First, I'm wondering if these are

accelerated in the sense that if more water is injected than might be flowing, natural episodic infiltration, and if you're injecting it into a dead-end fracture versus a free-flowing fracture, it might tend to be saturated in the matrix in that area and thereby skew your results in terms of matrix diffusion.

MR. ANDREWS: Well, they are at accelerated rates. That is true. So we are overinjecting, you know, orders of magnitude - hundreds to thousands of times the background percolation flow rate, average percolation flow rate, and the uncertainty in that percolation flow rate.

So the system is being overstressed with respect to flux in every one of the tests that I have described here -- in the unsaturated zone and in the saturated zone. When you're then comparing and evaluating diffusive characteristics, matrix diffusion characteristics, the model upon which those diffusion characteristics is being evaluated has whatever the saturation is in that rock matrix within the model.

The ambient saturation within the fractured rock mass in the unsaturated zone is on the order of 90-plus percent. So the bulk of the pores within the matrix are already saturated. There's a

remaining 10 percent roughly that are not. The issue then becomes the fracture-matrix interconnection area, which I think, you know, the second or third part of your question was getting at.

That fracture-matrix interconnection area is evaluated in a test, but it's also an uncertain parameter that is propagated through to the effect on performance. So you are right in the sense that when you've done a test the goodness of the test is contingent on the properties during the test, which include the degree of saturation between the fractures and the matrix in the test, which is then evaluated and can only be evaluated within the model that's used to evaluate that test.

So it's consistent between the model and the test, and then the uncertainty of that particular aspect, the fracture-matrix interaction term, is an uncertain parameter that's applied when we evaluate the behavior of the barrier and the performance of the system.

DR. SHETTEL: That wouldn't seem to correspond very well. If that flow is really in the form of rivulets going down fractions, that can be very tough to compare with --

MR. ANDREWS: Well, we don't think there

is episodic flow through rivulets, except for the Tiva Canyon, and it may occur within the Tiva Canyon. But once you get below the Paintbrush, and the Paintbrush being some tens of meters of essentially porous medium, that -- any potential of episodic flow that may have occurred in the first upper 10 meters is damped out, and it becomes more or less a homogenous, although spatially distributed, flux within the fracture system below that point. So it's not like we have masses of water moving through discrete zones.

DR. SHETTEL: Well, Alan Flint recently stated at the last NWTRB meeting, I believe, that there is really no reason that the boundary above the repository would be damped out.

MR. ANDREWS: I think he was talking about lateral flow. He wasn't talking about temporal damping, so -- and the degree of lateral flow, spatially lateral flow that occurs at that interface between the PTn and Tiva Canyon is a function of the flux. And because the flux -- surface flux changes with time, the degree of effect of lateral diversion, not temporal damping but lateral diversion, changes with time.

So given that we have climate changes -- I think it was on one of my slides -- we have climate

changes that are being applied to the system, and 94 percent of the time we have a climate change that has already occurred for this system. The first climate change we apply is at 600 years.

That degree of lateral flow and the fraction of flux below the PTn that's either diverted or non-diverted becomes somewhat, you know, damped out also in time. So not just the transient dumping but the spatial damping gets evaluated. So --

DR. SHETTEL: Okay. Last question. Switching to the saturated zone, Slide 23, which shows Kd's for alluvium from Nye County's drilling program, there's really only two wells on that list that are in the potential flow path from the repository. Therefore, the other data is really irrelevant.

MR. ANDREWS: I'd have to get back with you, because this is some of the data, and --

DR. SHETTEL: Yes. 19D and 2D are the only wells that are in the potential flow path.

MR. ANDREWS: Okay. I'll take your word for that. I haven't looked at it in that -- I mean, --

DOE STAFF: Yes, that's correct.

MR. ANDREWS: Okay.

DOE STAFF: There are mineralogic and, you

know, grain size -- these are consistent. You know, the alluvium was not radically different along the flow path, and it is in other locations.

MEMBER HORNBERGER: Okay. I think we need to move along. We are running a good bit behind.

Thank you very much, Bob.

What I'd like to do now is move to our next presentation. Jim Winterle is here I think, I hope. Hello, Jim. And Jim is going to give us a presentation on the center modeling of saturated zone flow.

For those of you who haven't done groundwater modeling, I'll point out to you that in advance something you can perhaps look for, it's not uncommon to have groundwater heads match within, let's say, 10 meters or so, and counted very good. And just keep that in mind as you listen to what Jim has to say.

(Laughter.)

MR. WINTERLE: Okay. I am Jim Winterle. The title of my talk is "CNWRA Modeling of Site-Scale Saturated Zone Flow at Yucca Mountain." This is a nearly identical talk to one I gave a few months ago at the NWTRB meeting. So some people in the audience may have largely heard a lot of this before. I'll try

to put it this time in a different context as to, you know, how the NRC is using this kind of information to apply risk insights.

Next slide, please.

A few disclaimers first. One is that the activities here were performed at the Center, and not necessarily any of this reflects the opinion of the NRC. And the second one is that I'm about to present several model scenarios, but these are all exploratory in nature, and they shouldn't be considered an exhaustive list of scenarios, or none of them should be considered preferred by CNWRA or NRC.

Next slide, please.

I'm going to talk about how our groundwater flow model was constructed, based on a hydrogeologic framework. And then I'm going to look at three different types of analyses we've done with the model -- one on the effects of different hydrogeologic interpretations, and one on the effects of -- what the local recharge is at Yucca Mountain, and another on the effects of increased recharge and water table rise, for instance, that might accompany a potential future climate, and what the effects of those are on groundwater flow paths.

To start, we've built this model based on

the Hydrogeologic Framework Model that was also independently developed at the Center and documented by Darrell Sims, et al. That used as input the GFM 3.1, which is a Department of Energy or USGS production, but it -- that model is just a small portion of the data that goes in this. And there were independent interpretations of geophysics and well bore interpretations.

We also lumped together the geologic units into hydrostratographic units in an independent manner differently than how DOE constructed their model. So we think that so much of the approach was done independently of the data and assumptions used by DOE that we're confident that it provides a fairly independent way of looking at things.

The hydrogeologic properties assigned to the flow model were based on correspondence to the structural features in here. And that framework model also includes several faults. I won't name them all, but here shown in black lines is fault features and a Caldera altered zone. In red, that shows where I had to extend the Caldera zone southward, and I had to extend the Highway 95 fault zone a little bit more to the east in order to get a good model calibration.

But other than that, one other change was

the fault zones in the Fortymile Wash and Paintbrush Canyon area were so close together that I constructed those as one single wide zone of -- which we would consider an intensely fractured and faulted zone. Other than that, the features included in the model are the same as what was in the underlying framework model.

And this Slide 6 shows a comparison of cost sections from the underlying hydrogeologic framework model and how that gets put into a model grid. This is just a two-dimensional slice, but it's a fully 3-D model.

The major units are alluvium. It's the uppermost lavender colored unit. Then there's the upper volcanic aquifer. A unit I'll talk a lot more about in some of the analyses is the upper volcanic confining later, which is actually a poorly confining layer. It has a little bit lower permeability than the adjacent layers.

Below that is the lower volcanic aquifer, and below that a very thick confining sequence, which is a very good confining layer, and below that is a thick sequence of what we call paleozoic carbonates. They are very deep. The depth of the model goes down to about 1,500 meters below sea level.

Next slide?

The model domain in plan view is very similar to the Department of Energy saturated zone model. It extends well north through the Yucca Mountain areas, where I'm pointing here. It extends well north into the Calico Hills, to the west of Crater Flat, and to the east of Jackass Flats, and south in Canavera, which is a farms area.

The interpretation of the water table you see in the contours here was used to assign lateral boundary conditions, and those are what drives the water -- what tells the model what the conditions are outside of the model and forces the water to go through the model.

And then, within the model, all of these blue dots you see are calibration points. I believe there were 70 in total that we used for calibrations. And they are at various depths, so there's a three-dimensional aspect to the calibration.

In the northern zone, I'll show later there's an area of recharge, and then analyses of recharge, with and without recharging the Yucca Mountain area.

Let's go to the next slide.

(Slide change.)

With the basic set-up, one of the first analyses I wanted to look at is what are the effects of hydrogeologic interpretation? With the basic underlying framework model that I described, that our flow model is based on, it has so many hydrogeologic units and fault zones and, based only on that, I'm able to get a calibrated model that's reasonably consistent with the quality of calibration the Department of Energy gets.

Dr. Hornberger mentioned that calibrations, plus or minus 10 meters of water level at a particular well, is often considered good in our model. Those big errors are sometimes due to where you place hydrogeologic units uncertainties on whether or hydrogeologic units have certain properties or not.

So what I did was I tried to add particular features to the model or maybe adjust the geometry of some features. So I don't really have data to say things are a certain way in this model, but there's nothing I've done to this that is refuted by the data. So I have conceptual basis to add particular features. Like this orange layer was an low permeability zone at the tuff galuma interface. Another change was to change the Caldera zone just a little bit further zone. And one change was to modify

the change to the Solitario Canyon fault. Little things like that -- add a third feature, a fault zone feature, to kind of limit the cross flow fault zone and the Fortymile Wash area.

(Slide change.)

And if we go to the next slide, you'll see doing that, I was able to take the base case calibration or RMS error of 27 meters, meaning the mean square of the error plus or minus zero line, averaging about 27 meters in the base case scenario and just by moving a few features around I was able to reduce that RMS error to 1.1 meter. If you look at the scales you see the error in this alternative model is down within the measurement error of water levels and wells in most cases.

So the question was what effect does that have on flow paths. Let's go to the next slide, I think it shows that.

(Slide change.)

Our original model had flow paths that start out going to the east, southeast and turn abruptly south and largely continue going south into the compliance point and then with the improved calibration, they come a little bit farther east and then again turn south and basically end up in almost

the same spot, slightly more spread to them. And so the two models you see here are basically constraints on what the effects of playing around with calibration or different ways of obtaining an improved calibration within the model and data uncertainties are on your flow paths.

(Slide change.)

And on the slide all the way to the right is the Department of Energy's base case flow model analysis. And you'll see that the flow paths coming out of their model are more or less in between the two cases that I've come up with here. So they're sort of within the range of uncertainty that we've developed with our model.

(Slide change.)

If we go to the next slide, the next analysis I wanted to look at was what are the effects of what you assume about recharge in the Yucca Mountain area because the recharge, where the flow paths are first initiating, the question we have is how important is that to how deep the flow paths go and for what units they travel in. So I looked at one case with 10 millimeters a year in the northern area, but no recharge over Yucca Mountain and then a case with the same recharge and then with five millimeters

a year at Yucca Mountain. And then we also show a recharge area in Fortymile Wash. That doesn't come into play until the third analysis I'm going to talk about later.

So we'll go to the next slide.

(Slide change.)

You see in plan view, the two flow paths with no recharge at Yucca Mountain and with 5 millimeters a year at Yucca Mountain, they're almost imperceptively different. I should mention these little blue arcs here are the approximate geometry of where the flow paths transition into alluvium and so the risk insight question we're asking is do these things affect what we've determined to be a risk-significant item in flow distance in alluvium? So I use that to evaluate. And you can see that there's almost no difference in plan view in looking at these.

(Slide change.)

But if we go to the next slide you can see in a side view that with a little bit of recharge, 5 millimeters per year at Yucca Mountain, the flow paths go substantially deeper down to almost 400 meters depth at some areas as opposed to only going about 50 to 100 meters deep in the case without recharge at Yucca Mountain.

And that, depending on what you assume about the porosity of volcanic units can have the significant effect on the travel time. And I'll tell you right here at this point that there's something I did to the porosity units. This dark blue layer is what we call the upper volcanic layer. It corresponds mainly to the Calico Hills unit and there's good evidence that that's a porous, nonwelded matrix and the flow in that unit may be largely matrix flow, but both NRC and DOE flow models conservatively assume that that's -- that all tuff is just fracture flow with relatively low effective porosity.

So one of the things I looked at in sensitivity of the travel time was what if, all tuff if it's welded, it's fractured, but these predominantly nonwelded units we assumed were matrix flow.

(Slide change.)

If we go to the next slide, you'll see that you can make a big difference between the Case 1 model with no recharge at Yucca Mountain stayed shallow and spent a lot of time flowing in that Calico Hills unit, the upper volcanic confining unit is what we call it in this model. And that some of the travel times or flow paths approached 100,000 years where the

mean was down around 10,000 to 20,000. I forget the exact number, but by adding five millimeters of recharge, those flow paths go a lot deeper and that brings the mean flow path travel time to just over a thousand years in this case.

So depending on what you assume about the porosities of the different volcanic tuff units, it can have a substantial effect on whether or not recharge is important. All of our models, the DOE models,that our model do take into account the effect of recharge on the initiation of flow paths, so there's really no discrepancy there, but I think one of the things this points out is here we've identified a risk-significant area, the porosity of nonwelded tuff units for saturated flow, but because nobody is relying on having flow in high porosity units to make their safety case, at this point it's not an area where the Staff needs to focus its concerns. So there's an application of -- identified a potential risk-significant area, but we don't need to focus resources on it, unless the Department of Energy wants to change their approach and start getting delayed travel times as a result of porous flow and volcanic tuffs.

Next slide.

(Slide change.)

So basically, this slide is summarizing a lot of what I just said. Those particle tracking simulations assume that fractured tuffs had a 0.001 porosity and then that the nonwelded tuff had a .1 value of porosity. And that's an assumption that's unique to this modeling analysis. I think there's data to support that assumption, but nobody is taking credit for that fact at this point. And if you make that assumption for a given flux, because the flux is inversely proportional to porosity or because of velocity at a given flux of inversely proportional porosity, the simulations with and without recharge show a big difference in travel time at Yucca Mountain.

I've done other analyses where I've set all the volcanic units to the same effective porosity and that big difference in flow path travel times virtually disappears. And in fact, historically, the performance assessments do not take credit for slow flow and porous tuff.

Next slide.

(Slide change.)

The next analysis I did was to look at the effects of potential water table rise on flow paths.

The evidence shows that during past climate conditions, there have been spring flows at the location of well EWDP-9S. There's evaporate deposits there and in several areas. So one of the things I use to constrain the model was to raise the heads by a constant percentage instead of a constant amount. A constant percentage means more of an increase where the heads are higher, where presumably the recharge is occurring.

And the constraint was to keep raising them up until spring flow was just initiated at this location. And an interesting self-consistency of the model was that as I raise the water table up, the first place where the water table hit the land surface was right in this general vicinity where those evaporate deposits occur.

So in addition to raising the water table, I also doubled the recharge, so there's double the recharge in the northern area and over Yucca Mountain. I also added 200 millimeters a year recharge in the Fortymile Wash. And then the results of water table rise are shown in the figure where most of the southern region it was 30 meters or less and in the Yucca Mountain area the water table rise varied from about 40 meters to about 120 meters. And that's

consistent with other analyses of how much the waters have risen in the past.

(Slide change.)

And the next question, if we can go to the next slide, is what was the effect of that on the flow paths? And in plan view it has almost no effect on the direction that things are traveling.

(Slide change.)

And then the next question shown on the next slide is what is effect of that on travel time. The increased water levels were increased more in the north than they were to the south, so that did increase the gradient and flow paths travel times were somewhat shorter, averaging just under a thousand years for the future climate scenario and averaging about a little more than a thousand years for the present day climate scenario, but they weren't what we would consider substantially different.

So even though the gradients increased a lot, most of that increased gradient is to the north where flow paths are already going pretty quickly through what we assume are low porosity tuffs and then an off-setting fact is when the water table rises, we get a couple hundred more liters of travel through alluvium. So that slows down the travel times a

little bit.

(Slide change.)

So if we go into the next slide, I think we're into the conclusions. A summary of everything we've just covered is that the model calibrations can be significantly approved by relatively minor adjustments to the geometry of the different hydrostaticgraphic and structural features, but the variability in model flow paths for those different scenarios was relatively model and we don't really consider that that affects the more significant aspect of alluvium transport distance.

Small amounts of recharge can have a big effect on the units that the flow paths travel through, but one of the other conclusions from the future climate scenario is that further increases to recharge don't add to that effect. So as long as you're considering a little bit of recharge in Yucca Mountain, you're capturing the effect that you need to capture and so it's not so important that you get the exact amount of recharge in the Yucca Mountain area as long as you got enough recharge to initially set the slow paths going in the proper direction.

Another point is that what you assume about the porosity of units like the upper volcanic

unit, the Calico Hills nonwelded tuff formation can have a big effect on your modeled particle travel times to the compliance boundary and that if one were to choose in the future probably relatively modest data collection efforts could be used to justify that assumption and improve performance predictions. Right now, nobody is taking advantage of that.

Next slide.

(Slide change.)

In the future climate scenario, it assumes five percent in the rise in the water table boundary was able to match the model -- match the observation of spring flow that had occurred in the past at weld 9S location and that the five percent increase resulted in a water table rise that varied between about 50 and 150 meters below the repository, increasing from south to north and that those potential effects of water table rise might be something to consider if that repository footprint was to be extended farther north because the slope of that water table rise seems to be pretty steep to the north.

And then the scenario of combined water table rise and increase recharge did not significantly change the flow paths for particle travel times to the

compliant boundary.

That's the end.

MEMBER HORNBERGER: Thanks very much, Jim.

Ruth?

MEMBER WEINER: You mentioned that your model was different from the DOE model. Was it different because it was independent or was it independent because it was different?

(Laughter.)

MR. WINTERLE: I'd say it was because it was independent. The results in the end aren't that different so is the model different if the results are similar? I think the ways it's different is we lumped the hydrogeologic units together differently. We defined fault zone geometries differently. Tuff alluvium interface was defined based on completely independent interpretations without any use of DOE's model for that.

MEMBER WEINER: So I think you've almost answered the rest of the question which is what impact do you think the differences between your model and DOE's model would have? How would you interpret that? Are they minor because you reached almost the same conclusion? Are they major?

MR. WINTERLE: I would say there are some

major conceptual differences in the geometry of fault zones and Caldera zones and the depths the fault zones go to, but in the end, it turns out that those don't seem to affect the flow paths. What affects the flow paths is that in the end you've got to have a model that's calibrated to reasonably match the water level observations. Once you get to that point there's only so many directions the water can go. It has to flow generally down radiant, so things like anisotropy can divert flow askew of the gradient. It's constrained how far that effect can be.

MEMBER WEINER: My other question is how much does the importance of the recharge rate depend on the results of sorption and desorption experiments? By itself, you can't really say.

MR. WINTERLE: Yes, these analyses were just particle tracking and they don't really say anything about the geochemistry or sorbing characteristics of anything. It's just you could assume whatever those particle travel times are the radionuclide travel times are going to be equal to or less than that. Whether -- independent of the sorption rates or retardation coefficients though eventually those radionuclides that aren't sorbed are going to have to where the water went, so the effects

on the flow paths should be applicable to the transport path.

MEMBER WEINER: Oh yes. Now I just wondered, all right, ultimately these things are coupled in performance assessment, of course?

MR. WINTERLE: Right.

MEMBER WEINER: One is your assessment of that.

MEMBER HORNBERGER: Mike?

ACTING CHAIRMAN RYAN: Jim, I appreciate the fact that you really focused her eon the water and getting that right and matching heads and so on. Given Dr. Hornberger's charge at the beginning and if we're within 30 feet, we're okay. Are we okay or much better than okay or great? What do you feel is the real representation of the modeling now in terms of reality, whatever that is, I don't know?

MR. WINTERLE: I think the first analysis we did suggests that being within 30 feet is okay with some caveats. You want that 30 feet not to be all biased depositive errors or negative errors. You want them evenly distributed. In most cases, the largest errors are right next to where fault zones or some kind of feature gives you a steep gradient, so your model grid kind of limits what you can define as a

fault zone, so if you were just off by 100 meters, your calibration error is off, but basically you've captured the effect that there's some barrier there that's slowing down your water, causing a gradient or directing your flow in some direction or another. You may be just off as to the exact location of that.

ACTING CHAIRMAN RYAN: To non-hydrologists, it sounds like if you do have those discontinuities and you have some physical explanation or other phenomenological explanation that says this is why that discontinuity is occurring, you're kind of bringing closure to the exercise. Is that a fair assessment of where you think you are?

MR. WINTERLE: Well, yes. As long as you can demonstrate you've captured the salient features, the major effects of the structural features, you can accept a little calibration error.

ACTING CHAIRMAN RYAN: Sure. Thank you.

MEMBER HORNBERGER: Allen? Ines?

DR. TRIAY: Yes. Along those lines, if you had design your working of the world and you could design the best way to validate the model that you have proposed what do you think remains to be done?

MR. WINTERLE: Well, if we all live to be 200, I wouldn't mind dumping a lot of some inert

tracer inside Yucca Mountain and waiting for it. I don't know how long the performance confirmation period is going to extend it to, but a large scale tracer test, I think, would really be the best way to figure out where flow paths that originate near Yucca Mountain actually do transition into the alluvium because there's a lot of uncertainty into the geometry of that interface. There's older debris flows and things down there that could complicate things. But we're limited to points of data here and there where we can put in well bores.

DR. TRIAY: So short of living to be 200, what is the next best way to validate the model? What remains to be done? I'm not suggesting that what you have done is not very good. I'm just saying if you had the ability to delineate, what are we going to do next?

MR. WINTERLE: I think maybe a couple more strategically placed bore holes would be in order. I think actually that is being done. I'm not sure where the Nye County drilling program is at now, but I know that's on-going. And they've done an enormous amount of data collection in the past few years at a reasonable budget. So that seems to be money well spent in reducing these uncertainties.

The other thing we can do is just try to keep attempting to look at different conceptual models so we understand what are the consequences of being wrong and so far haven't come up with any consequence that would be so wrong as that we really need to start over and look at things in more dept.

MEMBER HORNBERGER: Jim Clark? Jim Davis?

DR. DAVIS: Is someone at the Center looking at the effects of water chemistry from the climate change?

MR. WINTERLE: I think Paul Bertetti's talk right after me, well, I don't think we're going to look at climate change effects on water chemistry yet.

DR. DAVIS: Your conclusion is that a small amount of recharge captures all of the information that you need to know about the increased recharge, but that might not be true if you're also taking into account chemical effects that happen with increased recharge. So I think it needs to be remembered that that's a conclusion relevant to where the water flows.

MR. WINTERLE: That's a good point.

MEMBER HORNBERGER: Dick?

DR. PARIZEK: What's a five percent

increase in boundary head values related to in terms of recharge? What sort of recharge would it take to give you that increase?

MR. WINTERLE: There was no coupling of those in the model, so --

DR. PARIZEK: In other words, does that get us into a pluvial or a monsoonal? I can't quite see what amount increased recharge it would take to do that.

MR. WINTERLE: I think it's -- I'm trying to remember. I haven't really gone through and looked at the effect on specific discharge through the model boundaries and compared that to what's been analyzed or estimated for previous climates.

DR. PARIZEK: It was good enough to give you the paleo springs reoccurring where spring deposits occur, right? So that's at least --

MR. WINTERLE: I guess what happened was I got to that point, I was happy, so I haven't had the time to really dig and to compare that to other analyses and what regional groundwater fluxes had been estimated. But it's definitely something we should look at.

DR. PARIZEK: Right. It seems like changing say the water level elevation from 50 to 150

meters isn't exactly a minor shift, although it doesn't change the flow path or travel times significantly than maybe that doesn't matter too much. It's just hard to imagine you steeping the gradient that much and not have that affect travel times other than the chemistry changes that might occur, but that's what your analyses is showing us, right?

MR. WINTERLE: I guess a follow-on analysis that I should consider is to look at different segments of the flow paths and the effect on travel time so I can understand it a little better, but it looks like the flow path right near the repository where most of that water level rise is occurring, if that goes from 50 years to 5 years, it doesn't have a big impact on the full transport time.

DR. PARIZEK: The role of major faults, okay, looking for some data on fault permeability and particularly the block boundary faults and again, you seem like you've captured a lot of the details enough that it seems like what would it take to cause your conceptual model to blow up? It can't blow up because it sort of agrees with all the data you matched, right? So what could change conceptually in the model that would show that maybe the present understanding is in error?

MR. WINTERLE: I could speculate.

DR. PARIZEK: Maybe that's what we need to do now, but before everybody buys the model, everybody's flow goes south, southeast and then south, if that's a fact of reality, then we have reason to feel better about it. If it's possible that it could be straight south, say under the footprint or split where the straight south and also southeast, you may have a consequence on performance.

I want to make sure that the conceptual model that goes into everybody's simulations is correct or as correct as it can be.

MR. WINTERLE: One potential question is what if there's some structural feature that we've missed that can grab that water before it has a chance to get over into the permeable zone, beneath Fortymile Wash which that seems to be controlling things. Higher permeability out Fortymile Wash and that draws all the water toward it and then straight south from there. So what if there's some zone we've missed? If there is, I don't think -- I mean there's pretty good density of bore holes on Yucca Mountain proper so it seems like we would have at least saw the effects of that on the water table map if something existed. Maybe something could exist farther south.

DR. PARIZEK: Yes, on the footprint where the need to control this a little bit stands.

MR. WINTERLE: So those are things that I guess we could explore through modeling to look at the effect, how drastic of a feature would you need to really capture the water.

DR. PARIZEK: See, if I was in DOE, I would hear this and I would say I don't think I'm going to have much in the confirmation testing program dealing with regional flow. And I would say well, what does NRC think about that statement in terms of this counter plight between what more should be done or should the program know to get the level of confidence it's looking for or is it needed? That's essentially the question.

MR. WINTERLE: Another thing is the level of importance that's being relied on for the saturated zone. A lot of people are -- the model doesn't seem to be overly dependent on just the saturated zone flow paths or just the unsaturated zone flow paths. But we assign a high significance to the transport properties, but only a medium significance to the flow paths, but those two things go hand in hand. So I think overall we're going to have -- do a lot of thinking during our detailed review and what

constitutes adequate.

MEMBER HORNBERGER: Of course, if they had a radio tracer to the backfill in Yucca Mountain, as Jim has suggested --

(Laughter.)

DR. CLARKE: Two hundred years, I don't think is long enough.

MEMBER HORNBERGER: Performance confirmation is a long-term project.

(Laughter.)

Don, do you have questions?

DR. SHETTEL: My question has been answered already.

MEMBER HORNBERGER: Thank you. Thanks a lot, Jim. We're going to move on and Paul Bertetti is going to talk to us about sorption parameters.

MR. BERETTI: Well, thank you very much for the opportunity to talk a little bit about how we've developed sorption parameters for saturated alluvium, essentially the part of the saturated zone in Fortymile Wash.

I'm going to focus on this topic today, just in order to restrict the content area. Obviously, we've discussed and it has been brought up earlier in the day. We have some risk significance

associated with colloid-based, colloid-facilitated transport as well as matrix diffusion in the saturated zone. There's all these unsaturated zone processes that we haven't really discussed. So I'll try to focus on this to provide some detail about what we've gone and the process through which we've gone through to develop the parameters that we have.

So first what I'm going to talk about is some of our experience with experiments and modeling of those experiments, took place several years back. And our interpretation of that modeling to develop an abstraction, an initial abstraction and a range of parameters. And then the extension of that modeling to further develop those parameters in the abstraction that we can use in the performance assessment code. Then I'll kind of stop a little bit, show some examples of the range of parameters that we get now and talk about some of our work currently to help understand the uncertainties that we have remaining in our abstraction.

I think if we go to the next slide, if you back up a little bit, I just want to -- this is similar to Jim's. I just want to acknowledge that these are analysis presented by the Center and doesn't necessarily reflect the position of the NRC. Also,

the results here are exploratory in nature. Also, I will provide some results for under development portion, the TPA Version 5 and so those results should be considered developmental until NRC fully approves that.

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Just a little bit of background. Obviously, we've kind of talked a little bit about the significance of retardation, specifically of radionuclides of neptunium in the saturated alluvium. We know that the transport times of neptunium in the saturated alluvium are particularly sensitive to the range of retardation factors and $K_d$s that have been used for this point. And so it has been identified as an area of potentially highly risk significant to waste isolation.

Primary retardation mechanism, as Jim Davis explained earlier is the chemical sorption of radionuclides on mineral surfaces. And so today, I'm going to talk about, we know that there's a number of things that affect the magnitude of that sorption, but today, I'm going to talk about how we determine those particular values that we assign to that.

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So just to kind of review some of the definitions that I'll talk about today, as Jim mentioned, the $K_d$, or distribution coefficient is essentially a ratio of concentration of mass of radionuclide on solid and concentration of mass of radionuclide in solution.

I'll also mention a term of Ka and you'll also see it annotated as Ka1 and that is just a normalization of the $K_d$ to the surface area of the minerals that we've studied. Also, you'll see a term $R_d$ or the retardation factor. That's just a function of the amount of $K_d$ and the specific conditions porosity and bulk density that you have for the zone in which radionuclides are traveling.

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So the objective from NRC and Center point of view are to develop an independent data and modeling capability which to not only assess DOE activities, but also to provide input to our own performance assessment model. We'd like to develop an independent methodology that's based on parameters that we can measure. So it would be to our advantage to be able to measure parameters in the field and I'll

apply that to our model, rather than have a set of parameters that we have to essentially go by our expert judgment on. But would also like to be able to reduce overall uncertainty by being able to measure parameters directly and that ideally would help us improve realism in our model.

Then finally, we'd like to develop a set of abstracted models that are formed by what we know impact sorption and that is the chemistry and mineralogy of the system.

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So our general overall approach was to conduct a set of laboratory experiments using relevant minerals, water chemical and radionuclides for the program. And I'll just sort of mention now that in our earlier set of experiments which were started in the late 1980s and early 1990s, there was a limited amount of sort of field-based samples, so our initial approach was to try to pick minerals not only that we could understand to develop a modeling approach and interpret our modeling approach, but also to pick them in a way that we supposed were appropriate minerals for the system.

We'd also like to apply them to

appropriate mechanistic models over that broad range of conditions to see what the effects of those conditions are on sorption. We also collected and analyzed some limited field samples to inform our approach and provide a bound for our sorption parameter range.

We then applied some detailed models and experiments to build on or confirm results and that helped us develop our initial performance assessment abstraction. We'd also like to make sure that our performance assessment model has the important relevant information built in.

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Okay, so here I'll talk about some of our experimental results and some of the insights that we've developed from that.

Down on the bottom here I have a couple of graphs. One is for neptunium sorption of smectite or Montmorillonite, that's a clay phase. We also have neptunium sorption on quartz. The smectite sorption also shows data for conditions in which we have equilibrium with atmospheric $CO_2$ and also conditions under which there is no $CO_2$ present.

All of the data here on the quartz plot

are with atmospheric $CO_2$ present. I also wanted -- these are plotted in terms of $K_d$. We have a log scale and then plotted versus a range of pH that's fairly broad from about 3 to 10, depending on the type of experiments and the range of pH that was applicable.

The first thing you should notice is that the magnitude sorption for a mineral like montmorillonite is significantly different than the magnitude sorption for quartz and we'll try to talk about this a little bit further.

We studied various minerals and chemical conditions. We looked at a number of different minerals from clays, quartz, even basic minerals like alpha-alumina which helped us understand the sorption parameters required to model alumina silicates. We looked at a variety of chemical conditions. As you see here, we can vary pH and vary the amount of carbon dioxide that's present in the system as well.

We used minerals similar to what we expected at Yucca Mountain based on the known mineralogy of the tuffs and the studies, characterization studies that had gone in the 1980s and early 1990s from Yucca Mountain and vicinity.

The sorption behavior we see is similar with pH, is similar for a given actinide, even on

different mineral surfaces. For instance, we see that when $CO_2$ is present, neptunium has a sorption maximum around 8; for montmorillonite we also see the same sorption maximum around 8 for quartz. So very different mineral surface, very similar behavior and we see that for all the actinides that we have studied.

We also see the sorption behaviors effectively represented by a surface complexation modeling approach, similar to what Jim alluded to earlier today. This is a surface complexation of fit of the data with no carbon dioxide present and this is a prediction of behavior with $CO_2$ present that's independent of the experimental data. So we can reproduce the behavior appropriately with the surface complexation modeling approach.

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Here are another two graphs. One of the main features of the work that we have done is to look at the effects of surface area. When we normalize data for the effects of surface area, we see that even for different minerals in which we had different magnitudes of sorption, we get an overlap of sorption behavior both for uranium and neptunium in this case.

Here's the two plots, one in terms of $K_a1$ which is our effective surface area for all the minerals listed both for neptunium on the left and uranium on the right with $CO_2$ and without $CO_2$. When we normalize and consider these effects of surface area, we get a very similar behavior over the range of pH conditions present.

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So how do we utilize this information and take the information from the field, sort of develop an initial abstraction. What I have shown here are two plots of frequency and distribution of pH and partial pressure of $CO_2$ in saturated zone waters from the Yucca Mountain region. We know from just the slice that I showed you previously that pH and inorganic carbon or the $CO_2$ in solution are primary controlling factors in the magnitude sorption for any mineral surface, especially for the actinides.

We can use the range of chemistry then to guide our modeling approach. The variability in pH in inorganic carbon is also linked through the aqueous chemistry through the chemical equilibria that we get through carbon reactions and the amount of hydrogen solutions. So those are linked together.

We use the database of Perfect, et al. which is a USGS database, considered a large number of saturated zone chemistries in the southern Nevada region.

We paired that database down to a smaller number of data points based on chemical balancing inherent -- included in the system, a region that was more appropriate for the Yucca Mountain region to come up with the series of about 460 values that are used to develop these distributions that you see here.

Notice that we can represent a range of pH from about 6.3 to about 9.6 that's fairly normally distributed on this scale with an average value of around 7.8. Likewise, we can look at the distribution of carbon dioxide and explore its link between a pH and $CO_2$.

As a note here, there's a detailed description of the approach, not only for application of these data, but also the abstraction of this approach in the RTPA code in those two references which I have on the last slide in presentation.

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So in detail what we did was take the data

from the Perfect, et al. database and the understanding of the pH and $CO_2$ distribution that we have from there, apply a service complexation modeling approach that we know is effect at reproducing the sorption behavior. Use a particular type of surface complexation model that includes an electrical double layer that Jim mentioned earlier. We do arrive at some parameters for sets of minerals to provide us with coefficients or these sorption exchange coefficients. But we could use to model the data over a range of $CO_2$ and pH for the entire system for all the actinides.

And we use geochemical modeling software to generate a range of these surface complexation predictions and we use data on uranium and the other actinides that was available, the most recently available data from the NEA thermodynamic database.

And we combine those results to come up with some distributions for actinides.

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So down here at the bottom I have an example of the range of sorption parameter distributions that we generated and used and we call the TPA version 4.0 or 4.1.

Notice that we have americium, neptunium and uranium and all plotted on the log of this sort of $K_d$1 factor. So we can use this distribution and the range of values here to formulate the distribution an the range of values for retardation factors that we use in the TPA 4.1 code.

So we develop the sorption distributions in terms of Ka1. We use values from chemistry to constrain those distributions and then recast those in terms of $K_d$4D for use in TPA.

So these distributions are independently sampled in TPA 4.1 its independently sampled distributions,but we know that for a particular simulation it should be simulating the same water chemistry, those are correlated so that the same chemistry is applied to each one. And that's how the distribution are generated in TPA4.1.

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So maybe the next step would be to develop a set of responses over a larger range of pH and $CO_2$ and that's what I've shown here. Here are just some three dimensional plots of pH versus $PCO_2$ for all of the actinides. Notice that we can mimic the behavior.

These are just modeled produced curves, based on the same set of distributions that I showed you earlier and the same sorption modeling constants that I talked about earlier.

We can use the surface complexation modeling approach to generate these curves over the range of pH and $CO_2$ and we can use some curve fitting methods to mimic each one of these curves to a similar equation and then define a set of coefficients. And then we can use those coefficients to define and calculate a $K_a$ value for sampled ranges of pH and $CO_2$.

So now instead of inputting into TPA a range of $R_d$s that we sample and then gave to correlate after the fact, now we can input directly a range of pH and $CO_2$ that we've measured in the field and we know what the distribution is and then use the modeling to calculate the sorption factor on the fly.

And then we can use our measured values of porosity and bulk density to then calculate a retardation factor that's appropriate. So even though for each simulation run in TPA, we still use one value of that sorption coefficient. It would be based on real sample values of simple chemistry.

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So TPA co-development, this co-development for what would be TPA 5.0, they've incorporated these response services in terms of the Ka1 valleys. They used a field measure range of pH and $CO_2$. Those are stochasticlly sampled and so those are correlated together. Because they're chemically correlated as well, so we can use a correlation factor to ensure that we sample those appropriately. We can use that both for the unsaturated and the saturated zone, based on measured chemistry and the distribution of chemistry for those waters and we calculated a surface area for individual geologic layers based on measure samples and then we can calculate $K_d$ and $R_d$ within the TPA code. And since we sample chemistry directly, then we don't have to do the after-the-fact correlation. In fact, the distribution and the shapes of the distributions for the retardation factors that we produce, are relevant to the chemistry. There's no guessing as to what type of shape we should get.

Next slide, plese.

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So in the next couple of slides, I'll show you some comparisons of retardation and factor output from the two versions of the TPA code. Here I show

some density plots of neptunium and uranium retardation coefficients. These are in terms of $R_d$. And that compares the output from the range of distributions that were used in TPA Version 4 versus this proposed approach. And here we're only talking about saturated alluvium, data for the saturated alluvium and these represent 400 realizations from the code or 400 realizations from a sampled set of parameters.

For neptunium, what we see is the median value is slightly higher, the median value here is slightly higher in the version 5 code which utilizes the sampled range of pH, relative to the median value for Version 4. And the range is narrowed. So the mean value is a little bit higher and the range of sampled values is a little bit narrow. In fact, there's a range of values that go all the way out to about 3,000 in the older distributions. So it's narrowed in this approach.

ACTING CHAIRMAN RYAN: Paul, just a really quick question. Four hundred realizations. Is that enough?

MR. BERETTI: Well, what I'm trying to do is have enough realizations to sort of sample and represent the distribution. This is enough -- that's

kind of a typical range that you sample to to ensure that in the code because of the number of parameters. I guess you could do more, but I haven't done that.

ACTING CHAIRMAN RYAN: Well, I guess the question is if you did 10,000 would you really change the shape?

MR. BERETTI: I think you could get closer to more representative shape, but so my answer is I think this is okay for what I want to present here.

ACTING CHAIRMAN RYAN: You wouldn't change between 4.1 and 5 say, these two?

MR. BERETTI: No, I think you would see the same trends.

ACTING CHAIRMAN RYAN: That's all. Thanks.

MR. BERETTI: Yes. For uranium, the median value is lower in here. We have a more predominant range of values that are in the lower values, but the total range is about the same. So we have a range up here and notice that it's slightly tilted. We have a range of up to about 8,000. There's about the same number of points. So we have a very broad range and we have more values at the low end.

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This is a similar data set, except now I'm going to plot them in terms of the density function plots and this -- now I'm going to also plot them in terms of $K_d$ and one thing I have to say is the specific values of $K_d$ depending on the values of bulk density and porosity that you use, so I just use mean values in the TPA code to produce these plots. You would get slightly different numbers depending on the values that you use. And if you sample those values over the range, then you would get a different sampling. And I have not done that here.

And also note that the distribution shapes are functions of this sample pH and $CO_2$. So part of this is the fact that we have very discrete $CO_2$ values instead of the continuous range of $CO_2$ values that are noticed.

Again, here's a difference. We have mean and constrained values for neptunium $K_d$s that are slight higher than the median values produced by TPA 4.1, but the range is significantly narrowed. We don't have a large number of these very large values.

Notice all those for americium which could be an important nuclide, the values of $K_d$ are extremely large. The minimum values are on the order

of 10,000 for $K_d$. Uranium on the other hand, we have this sort of 5.0 range. It's a little bit lower. But the total range is about the same for both samples.

Okay, next slide, plese.

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Okay, so how are we approaching some of the uncertainties that we know are included in this sort of modeling approach? One of the things we like to do is update water chemistry distribution to reflect the recent sampling in the early well and drilling program. And how might those changes in sample not only water, but mineralogy affect the influence and affect these results?

We also have additional work on-going to confirm the mineral content. Recall, I said that the basis for a lot of this modeling approach was on the experiments with mineral phases that we thought were appropriate and that we'd like to make sure that they are indeed appropriate. We also want to look at specific surface areas for the alluvium. Is that range of surface area appropriate? Is it consistent with our measured values? Especially, does it reflect the recent analysis of cuttings and sonic core samples that have been collected. One of the problems with collection of cuttings is that you have a grain size

distribution that's influenced by the drilling fluid and the method of collection. Nye County's extensive work and efforts to try to collect more representative samples of alluvium resulted in the sonic core and I'm sure Dale will talk more about that tomorrow.

And we'd also like to be able to test our model outputs against experimentally measured values based on these more representative samples.

I'll show you some examples in the next couple of slides.

Next, plese.

(Slide change.)

This is just a reset. This is just another view of the saturated flow pathing system. These arrows are not calculated flow paths. They're just meant to bound the type of flow paths that Jim showed you and I think you'll see in Bill Arnold's presentation later. Give you an idea of how those are constrained in the models. The approximate compliance boundary is shown on there. And it also shows the location of a couple of wells that I'll talk about, Well 2D and we'll see some examples of data from that and also Washburn-1X. I'll also show the well location of the 19PB which is a location of the sonic coring that has taken place. Also is the location of

the alluvial testing complex that Bob spoke to earlier.

Some of the thing to note is flow direction is predominantly toward the south. You're going to have a portion in fracture volcanic rock and alluvium. We're going to talk about alluvium here in a couple of moments. We know that there's some uncertainty in the length of that flow path. As Jim alluded to, we recognize that there's a relationship between uncertainties in the flow model and uncertainties in the transport parameters that influence that.

So I'm going to show you some examples from a couple of selected wells, specifically 2D and Washburn-1X.

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This is a little complicated, but it has a lot of information. These are x-ray diffraction results of well cuttings collected from Well Washburn-1X and Well 2D. So we received cuttings as sample splits from the Department of Energy. We collected semi-quantitative, x-ray diffraction analysis at every 5 foot interval for the entire depth of those two holes. So the difference in depth here is that this

is the complete depth of Well Washburn-1X which is about 650 feet and Well 2D goes down to about 1600 feet. Don't confuse this with Well 2DB which is a newer well that penetrates all the way down to something on the order of 2000 feet, I believe.

And here we have a comparison of bulk mineralogy between the two wells and then the clay component which is shown in this green layer on the edge, then is expanded to show the types of clays that might be present in that clay fraction.

Water table is shown in blue here. I also have a couple of SEM photo micrographs at about 640 feet and 820 feet that I'll show you later. One thing that I would like you to note from this diagram is that while Washburn and 2D which are separated in space across that 40 mile wash, at least across the kind of range of expected arrivals of the flow paths have a very similar bulk and clay mineralogy at least through the saturated zone or at least through the depth of Washburn.

Also, there's not a significant change in mineralogy as you go from saturated to unsaturated conditions at the water table that exists now. The other thing is notice that we have a significant silica fraction. This is quartz and prostobilite

here. There's a lot of feldspar as you would expect from the tuffs. And we have a consistent amount of clay fraction that's dominated by smectite or that montmorillonite that I showed you previous. We also have a significant amount of zeolite that occurs throughout the depth of the hole. In fact, some have a lot of zeolite as we go deeper, so depending on the zone.

One big difference between these results for these two holes is that about 1150 feet we see a large occurrence of dolomite and calcite. We interpret this and I think it's consistent with the lithologic interpretations from DOE and also Nye County that this is the kind of a start of an older package of alluvium or maybe pre-basinal sediments that are dominated by not only calcites, but also dolomitization of tuff particles and that's where you get a lot of that dolomite.

One thing to consider though is this depth kind of encompasses the range of the depth of flow paths that Jim Winterle showed you earlier. So even though we've considered a lot of the minerals that -- I think we considered enough mineralogy to describe this zone. We might want to look at calcite and the influence of calcite as well and in fact, we have done

that and I can talk to that later, if you'd like.

Next slide, please. Well, here is an example of scanning electron micrograph of the well cuttings taken from Well 2D at about the 640 foot level. This is again below the water table in a zone where you have that small amount of zeolite and smectite. Here's a larger scale version. We see kind of the alluvial grains. Notice that they're coated with some material and there's maybe a sparse coating. These are well cuttings so they've kind of been washed off, a lot of the fine grain material has actually been removed here. And they're somewhat unconsolidated, but notice that they're sort of loosely cemented by this material. If we take a close up of this, what we see is it's composed of almost entirely of zeolites, clinoptilolite, one of the minerals that we studied in our sorption experiments and infiltrated or maybe even ingrown clay particles. So the surfaces of these grains might be well represented by clays and zeolites.

Go to the next slide, please.

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Now if we go a little bit deeper we saw a little more percentage of zeolite in the horizon. Notice we have a similar sort of coating of material.

If we look at that closely again, we'll see again it's dominated by zeolite. This is primarily that zeolite clinoptilolite and also clay. Both clays that not only are ingrown and formed in place, but also clays that probably were filtered in and kind of were filling in those pore spaces. Note again, those clays are dominated by smectites.

So I think this sort of demonstrates that our approach to not only modeling using an alumina silicate phase, but also the range of materials, quartz, montmorillinite, zeolites that predominated in our surface experiments are appropriate for the surfaces.

One of the things that Jim mentioned in his talk was the presence of iron oxides and kind of the appearance of those iron oxides. We see iron oxides here, but there's some evidence that they're contained within these amorphous silica phases on the surfaces in Jim's materials for Naturita kind of show that. There are other investigations that show that and Naturita's measurements on the tuff samples and batch studies of that also showed that, that there was sort of minimization of the iron oxide sorption capacity in those experiments.

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So to conclude or just provide a summary of what I've shown, we have geochemical sorption models that are based on our experimental results. They've been applied to produce the sorption parameters for actinides that we use in TPA.

The sorption parameters are constrained by measured water chemistries. We use recently collected analyzed water samples that are consistent with our experimental approach and consistent with the range of chemistries that we have and consistent with the range of mineralogist that we considered.

Our TPA output suggests that the saturated alluvium may be an important barrier and that has a retardation capability that could be assessed on the order of the time frame which the regulations address. And the methodology incorporated in the TPA right now are flexible, so they're not limited to a particular type of surface complexation modeling approach. We have a more generic approach that we can develop using sampled and information kind of on the order of what Jim had described earlier and we can incorporate the same sort of changes and coefficients applied directly into TPA without modifying the code substantially.

That's all I have.

MEMBER HORNBERGER: Thank you, Paul.

MR. BERETTI: You're welcome.

MEMBER HORNBERGER: Questions. Ruth?

MEMBER WEINER: I have a number of questions, Paul, I'm afraid and I'll try to condense them as much as possible. You showed us, there's a slide earlier where you showed the oxidation states of the actinides that you were looking at.

MR. BERETTI: That's correct.

MEMBER WEINER: Americium 3, neptunium 5 and so on.

MR. BERETTI: Correct.

MEMBER WEINER: Do you have spectroscopic verification of those oxidation states?

MR. BERETTI: Well, I think the oxidation states for americium and thorium, neptunium, in particular, and uranium in an oxidizing system are fairly well defined for the states that we use in the model. And I think the system and the saturated alluvium is primarily oxidizing and I would expect that the oxidation states for those four to be consistent with that.

For plutonium we used the oxidation state plus 5, sort of as a default. We don't have any evidence or a lot of other information to constrain

the oxidation state or the range of states for plutonium. We felt that at the time of the modeling that that kind of was a conservative approach, based on our understanding of how neptunium 5 complexes and sorbs and compared to thorium in the plus 4 state which sorbs much more strongly.

So the answer to that is I think there's evidence for the other four. We have direct evidence for neptunium in terms not only in our experiments, but also for uranium as well that those are appropriate.

MEMBER WEINER: What direct evidence do you have?

MR. BERETTI: Well, the direct evidence is we measured -- I don't have field evidence because I don't field waters for neptunium in those. We have done XF studies with uranium to look at the uranium complexation on montmorillonite at different pH values. We've also done XF studies with neptunium on clays to try to assess the complexation and the complexes that might occur. I have spectroscopic studies with neptunium, kind of infrared IR spectroscopy that shows neptunium plus 5 under the conditions in which we've studied.

For americium and thorium, I think those

are fairly well established, but I don't have any direct evidence for those.

MEMBER WEINER: Are you familiar with the work that has been done by Don Rye and Andy Fellmy at PNL by David Clark's laboratory at LANL, Cynthia Palmer at Livermore. I mean all of these people have done extensive work on the oxidation states of the actinides under various conditions. And I question a little bit your assumption of plutonium 5. I question somewhat your assumption of uranium 6 which is very dependent on pH.

And my suggestion is that you either get direct spectroscopic evidence and visible spectroscopy of your oxidation states or do some literature consultation, consult some of the literature in this area. This is very extensive.

MR. BERETTI: Yes, and I'm familiar with much of that. We had to make a conscious decision about plutonium because at the time we did not have a lot of information. I would agree that that's very uncertain. I still would say that I would -- my feeling is that uranium in the plus 6 state in the conditions as measured in the alluvium is appropriate. Beyond that I don't have much else. But I agree that that's something that needs to be strongly considered.

MEMBER WEINER: Because plutonium 4 forms a colloid and that may be a problem.

MR. BERETTI: Right, we were trying to model the aqueous component of plutonium sorption. I would agree that plutonium 4 on the colloid is much more important in terms of plutonium transport, but we just did not include it in this component.

MEMBER WEINER: I want to move briefly to your modeling. Does your modeling include -- when you formulate, build your model, do you minimize -- do you have some way to minimize the Gibbs-free energy for all of your solution components?

MR. BERETTI: Well, the model is built within the code the same way as all solution -- all the solution components are contained within the code the same way. So the fitting uses a code like FITEQL to sort of get the best fit estimate for the complexation constant, for the sorption parameter and then one represents the behavior, but I would say somewhat semi-empirically because we don't have direct evidence for the complexation that we are modeling to mimic sorption behavior over the range that we have used. That is incorporated then in the overall geochemical model to predict sorption over range of pH and CHH.

MEMBER WEINER: I'm asking because when we modeled the actinides for the Waste Isolation Pilot Plant, we used EQ36 and basically fed in our solution components constantly minimizing the Gibbs free energy so that we could see what remained in solution and what did not. I think Ines is very familiar with this work.

And that's the sense in which I ask. And that leads me to another question which is pH and carbonate are not independent of each other.

MR. BERETTI: That's correct.

MEMBER WEINER: And you, in your model, you recognize the buffering activity of carbonate and the fact that it's going to change as you add carbonate or $CO_2$, you're going to change the pH.

MR. BERETTI: That's correct.

MEMBER WEINER: And that is done in the model?

MR. BERETTI: That's incorporated in the model.

In the model representation for sorption over pH and $CO_2$, that's incorporated within the modeling approach because all those phases are included.

For the sampling then of those

distributions within TPA, what we do is for the version 4, we sample a retardation coefficient distribution, so there's not a chemical sampling involved.

And then we correlate that, based on our understanding of the correlation between pH, $CO_2$ and those produced distributions.

For the proposed version 5, we sample pH directly. Then that is correlated to $CO_2$, based on a correlation that we have measured, based on our sample parameter. So yes, they're directly related and yes, those are correlated. They're kind of limited by the quality of the data in the data set. And those are correlated with the factor of something on the order of minus .8. So they're very closely correlated, as you would expect chemically and that's how we choose a value of $CO_2$ that's related to --

MEMBER WEINER: I'll let you go now.

MEMBER HORNBERGER: We're getting pretty detailed into the modeling, so let's try to stay at a higher level.

MEMBER WEINER: I'm done.

MEMBER HORNBERGER: Mike?

ACTING CHAIRMAN RYAN: No, I'm all set. Thanks for a nice presentation.

MEMBER HORNBERGER: Allen?

DR. CROFF: I've got a question on one of your early slides. It was page 7 where you had some experimental results. If I read the left most graph correctly it shows that the reaction is, the sorption reaction is indeed reversible?

MR. BERETTI: That's correct.

DR. CROFF: Have you found that to be generally the case across various radionuclides and across various minerals and this kind of thing?

MR. BERETTI: For the ones that we've studied in the most detailed, yes. I would say those are primarily -- our particular experience is mostly limited to uranium and neptunium in those particular studies.

DR. CROFF: Thanks.

MR. BERETTI: But yes, that is correct.

MEMBER HORNBERGER: Ines?

DR. TRIAY: Let me ask you, I thought that this was a very good presentation as well. Let me ask you from your perspective, what is driving the sorption aspect of radionuclide migration more, the mineralogy or the water chemistry?

MR. BERETTI: Well, I think the water chemistry has the largest effect. The effect of

mineralogy in our estimation is primarily related to the effect of surface area that you see. So I would couch it in terms of if I knew an effective surface area independent of the mineral phase, then if I knew the chemistry parameters, then we should be able to model the sorption behavior.

Now what that doesn't account for are other types of reactions that occur because of the mineral phases, so it completely ignores redux sort of reactions that might occur and I acknowledge that.

DR. TRIAY: So you think that the best way to model sorption would be via the surface complexation models almost ignoring, if you will, the -- and I'm not trying to put words in your mouth, I'm trying to understand.

MR. BERETTI: Right.

DR. TRIAY: When I made a statement and say no, that's not it, it's something else. Ignoring the mineralogy and just having surface complexation parameters, bear radionuclide as a function of water chemistry?

MR. BERETTI: Well, what I would say is that -- the answer partly would depend on the chemistry of the system. For instance, if we had a chemistry and a mineralogy type where ion exchange and

a nuclide where ion exchange processes were important, then obviously that would need to be included and incorporated as well. So your model would have to encompass the range of reactions and sorption mechanisms that you propose.

I think for the alluvium the surface complexation modeling approach is appropriate or the range of chemical and mineralogical conditions for actinides. I think it's an appropriate approach.

DR. TRIAY: How do you take into account in the surface complexation modeling for sorption the difference in oxidation states for plutonium?

MR. BERETTI: We had not done that in this case, but if you had enough information to inform your water chemistry with respect to oxidation and you had previously done enough modeling to do that, the problem here is we're trying to include as much chemistry information as we can to develop a sorption parameter without explicitly incorporating all of that chemical modeling within the TPA code. And so it's very -- you would have to have a reactor transport model to appropriately do that and that's not the point where we are. I don't know if that answers your question or not.

MEMBER HORNBERGER: Jim Clark.

DR. CLARKE: Slide 14 where you show the neptunium and uranium and the differences between 4.1 and Version 5, I guess the big difference is neptunium. Would there be increased medium value for retardation factor?

MR. BERETTI: Yes, I would say that seems to be different.

DR. CLARKE: For uranium, I can't tell.

MR. BERETTI: The mean values are similar. The reason I don't mention those too much, it's kind of depending on the set of data, the realization set that you create and I mean if you happen, you have a small percentage of values that are very large and if you happen to hit one that's extremely large, then it's going to change your mean significantly. So the mean values are about the same between the two sets. The median values are different, however.

DR. CLARKE: That's what I was trying to get at. Did you really think there's a significant here and if so, do you think you know why?

MR. BERETTI: I know why is that even though that range of chemistry that we use to develop the initial distribution, we had to suppose what the shape of the distribution was, so we came up with our best estimate. The law of normal distribution is

purposely biased toward the lower end value. So we kind of have a predominance of lower end values and that has that low median.

DR. CLARKE: That's for Version 4?

MR. BERETTI: That's for Version 4. So for Version 5, we don't have to judge what the distribution shape is. We can measure a distribution of pH and $CO_2$ and then apply that and so what you see is kind of what the model produces based on that. And so as that distribution would change and as our understanding of $CO_2$ and pH would change, then that might change as well. It might also be impacted by the range of surface areas that we would measure and then also incorporate into the code.

DR. CLARKE: Thank you.

MR. BERETTI: Yes sir.

MEMBER HORNBERGER: Jim Davis.

DR. DAVIS: Very nice presentation. I really admire the work that David and you and Bobby Padwell have done.

Given that though, I do want to say something. You've compared -- in talking about what you do in 4.1 and extending to 5, you've made the statement that you might be able to work with real materials and develop a semi-empirical model and use

a similar approach. And I want to point out that there, the abstraction you're making here is somewhat -- it has other limitations than you're mentioning than just surface area. For example, I believe that these measurements that you've done as a function of pH and carbon dioxide are done in simple electrolyte solutions and so your knowledge of the effect of calcium, for example, or uranium sorption is incomplete.

MR. BERETTI: That's correct.

DR. DAVIS: And calcium is, in fact, going to be in all ground waters.

MR. BERETTI: That's correct.

DR. DAVIS: So this is another aspect of this semi-empirical approach where you begin to incorporate all of the components of ground water that you include.

MR. BERETTI: That's correct.

DR. DAVIS: So it's important to look at the composition of an artificial ground water solution. And the other thing that's being abstracted is the electrical double layer. You have included in your model a pH, the pH and CT dependence that you have in the model is, in part, includes within it electrical double layer components and so you're

saying that that pH and CT in the ground water, I will have the same electrical double layer on smectites in that ground water and that's not confirmed yet either.

MR. BERETTI: No, it is not.

DR. DAVIS: So there's some uncertainties there and then the final thing I would say is that for the neptunium, for example, you haven't worked at a partial pressure or at least the data you showed here, you haven't worked at a partial pressure of carbon dioxide about air and the values in the system are all above air. So you're extrapolating from air values up to these higher partial pressures of carbon dioxide.

MR. BERETTI: Right, it seems like I have data at higher partial pressures.

DR. DAVIS: Oh, you do.

MR. BERETTI: Of $CO_2$ that are consistent with what we predict. Also from the modeling approach.

DR. DAVIS: I thought the graph you showed was --

MR. BERETTI: I only showed is from the montmorillonite study that is in Davener's paper from a few years back. So yes, we do have data for a larger range of $CO_2$. And that's more recent. So it's not incorporated here.

DR. DAVIS: One of the reasons I mention the calcium is in the last couple of years there's been this new aqueous species that's been determined by excess spectroscopy to be possibly predominant in these kinds of waters that involves a calcium uranium carbonate ternary aqueous species. And if you take a model like this and you have to make an assumption then about what -- if that's predominant in aqueous species, it's going to affect your calculated sorption.

MR. BERETTI: Yes, I understand.

DR. DAVIS: Eventually, you're going to have to face that calcium problem.

MR. BERETTI: Yes, I would agree. Data that we can collect in terms of confirmation work would be, would consider that appropriate.

MEMBER HORNBERGER: Dick?

DR. PARIZEK: I was curious on page 18, these are not sonic log samples?

MR. BERETTI: No sir, those are well cutting samples.

DR. PARIZEK: From earlier sampling.

MR. BERETTI: Yes sir.

DR. PARIZEK: Do you expect that would differ, the sonic log?

MR. BERETTI: The sonic coring data that we have on a smaller number of samples to date that we collected last December show very similar bulk mineralogy values. The primary difference in the sonic core sample results that we have now, we have about 10 to 15 percent more by weight of the clay fraction which sort of represents the kind of fine grain material that probably is washed out of a well cutting and that's kind of consistent with what the Nye County folks expected too.

So what we see is in a sonic core sample, very simple distribution of mineralogy with a larger percentage occupied by the clay fraction.

DR. PARIZEK: And the clay abundance is --

MR. BERETTI: The clay abundance is almost exactly the same, correct.

DR. PARIZEK: So it's representative.

MR. BERETTI: It's representative just in a very similar fashion.

DR. PARIZEK: Now for performance, when I look at the smectites, they go from roughly 40 percent to 70 percent of the sample, depending upon where you are in the 2D log or for that matter --

MR. BERETTI: Forty to 70 percent of the clay fractions.

DR. PARIZEK: Of the clay fractions.

MR. BERETTI: Right.

DR. PARIZEK: So shouldn't there be some difference in just the retardation characteristics when you have that much difference in those kind of clays?

MR. BERETTI: It's likely that there could. We have a couple of factors that we've seen. When I do the -- we've done our experimental analysis and looked at that effective surface area, it seems that the clay, only about 10 percent of the measured surface area of clays is actually sorbing. Kind of consistent with the percentage of edge sites. So that's sort of modified by that factor.

And we do see a measured -- differences in measured surface areas on the sonic core and well cuttings that are consistent with the additional amount of clay. So if we scale the surface area appropriate to what we've measured in the experiments, then the effects of the fine grained materials are kind of all normalized against each other, so it's really a difference in surface area, not mineral type.

DR. PARIZEK: There seems to be some sort of a consistent pattern to the smectite abundance with depth. It's not just erratic, but rather, you have a

kind of increasing trend that values down, there's an increasing trend. That has to do something with the history of the valley fill accumulation.

MR. BERETTI: I would expect so, yes.

DR. PARIZEK: You could probably expect similar results at other holes?

MR. BERETTI: Yes, I think if you notice the sort of trend for the water table between the two holes, you find that they're very similar. I don't think that's coincidental.

DR. PARIZEK: There seems to be a whole different story when you go down below a thousand say feet.

MR. BERETTI: The interpretation of that would be sort of complex, but yes, I would agree with you that there's those trends don't seem to be random.

DR. PARIZEK: Thank you.

MEMBER HORNBERGER: Don, do you have questions?

DR. SHETTEL: That should be on now, I think. Is that better?

MEMBER HORNBERGER: Yes.

DR. SHETTEL: I believe as Jim Davis pointed out, these solutions are fairly simple and there are some other complexing ligands out there such

as fluoride which at the one ppm level in the ground water is significant if you're in more acidic solutions, especially for neptunium. As far as uranium and phosphate is also an important ligand for uranium and neptunium, but again on the more acidic side of the sorptions, so this may not have that much effect on most of the ground waters which are more alkaline, but it could add a contribution to the complexing.

And with regard to colloids, have you -- any of these experiments have colloids in them or have you looked or found any?

MR. BERETTI: No, we have not. These experiments and this sort of approach is not meant to represent colloids and in fact, it excludes colloidal sized materials as best that we could.

What we have tried to do, another sort of process level modeling approaching to look at the important factors of colloid facilitating transport. We've used DOE data and some field-derived data from a Nevada test site to try to develop that approach, but it's not incorporated in these experiments or in the modeling that I discussed.

DR. SHETTEL: And lastly, I guess, humic and fulvic acids are not incorporated as well?

MR. BERETTI: No. There's not really been a lot of work. I don't think there's been a lot of work by anyone to look at the organic acid content of the ground waters and the saturated zone. I think the total amount of organic carbon is fairly low. There has been some work by DOE to use the organic carbon to help date ground waters.

I will add that our most recent sampling of Well-19PB in which I think that was done last month, we collected a significant amount of samples specifically to characterize the organic acid composition. So hopefully, in the next couple of months we'll have an idea of what those compositions are and what the concentrations are for humic and fulvic acids for those waters. But right now, we don't have any of that. I can't really speak to the others.

DR. SHETTEL: Thank you.

MEMBER HORNBERGER: Thank you, Paul. We are now going to take a 15-minute break. We are going to start promptly at -- the clock on the far wall there, when the clock on the far wall says 5 past 4, we will start.

(Laughter.)

(Whereupon, the proceedings in the above-

entitled matter went off the record at 3:51:38 p.m. and went back on the record at 4:08:20 p.m.)

MEMBER HORNBERGER: Okay. Let me mention a couple of things. Well, I'll mention the second thing first then, and that will be the first thing. We have a change in schedule for Thursday morning. Currently on the schedule we have the DOE response to NRC independent evaluation of documents, and that has now been postponed from 8:35 until a start time of 11 a.m. It's 11 a.m. our time so that you might note on your schedules that that's a change.

LAS VEGAS PARTICIPANT: We can't hear you.

ACTING CHAIRMAN RYAN: Hang on, we're checking.

MR. LARSON: It's because the presentation is going to be from Las Vegas.

MEMBER HORNBERGER: Yes, the presentation is going to be -- can you hear me now?

MR. BROWN: Vegas?

LAS VEGAS PARTICIPANT: Yes, we're here but we can't see you and we can barely hear you.

MR. BROWN: Okay. Hold on for a second.

MEMBER HORNBERGER: Okay. Can you see us now?

LAS VEGAS PARTICIPANT: Yes, we see you.

MEMBER HORNBERGER: Okay. And can you hear us now? You can hear us?

LAS VEGAS PARTICIPANT: Much better.

MEMBER HORNBERGER: Okay. At any rate, I was saying -- what you missed was that I said the presentation originally scheduled for 8:35 on Thursday morning has been postponed and will now be at 11:00 Thursday morning.

The other thing that I wanted to mention to our panel members, you'll notice on our schedule tomorrow afternoon from 2 to 3, during that time the panel members will be invited to make summary comments on the basis of what they have heard, so please, you can give some thought to that both overnight and as the day progresses tomorrow.

All right. So I think that our next presentation since we are now hooked up is Bill Arnold in Las Vegas. Bill, you are there and you can hear us, and I think we can hear you.

MR. ARNOLD: Okay. Hello, and I'm pleased to have the opportunity to speak to you. I'm going to talk about the saturated zone flow and transport modeling and results. This builds on the presentation given by Bob Andrews earlier in which he described the conceptual models in the unsaturated zone and the

saturated zone, and the experimental basis for those conceptual models.

I'm going to describe how those conceptual models are implemented in the numerical models for performance assessment calculations. Some of the additional lines of information that give us confidence that those models are realistic and describe some of the modeling results. If we go to the second page --

MEMBER HORNBERGER: Bill, can I ask if there are several microphones on at your end if they could be turned off. We can, I think, hear some background noise. Okay. Never mind. Go ahead, Bill.

MR. ARNOLD: Okay. Well, I think it's being fixed here.

MEMBER HORNBERGER: Yes, that's much better.

MR. ARNOLD: Okay. So saturated zone flow really defines the flow paths and the flow rates of ground water through the system. And this is, of course, important to determining the releases to the maximally exposed individual. Saturated zone transport defines the advective and dispersive transport velocities of radionuclides. These can either be dissolved or attached to colloids that are

potentially released. In their transport times, we include the effects of matrix diffusion and retardation along paths of likely ground water flow. And the metric of interest here is the mass or the activity flux of radionuclides at the point of compliance, which is about 18 kilometers south of Yucca Mountain. And the basis for quantifying the above processes relies on site-specific data, hydro geology, the geo-chemistry, and the transport testing that's been conducted by many scientists over the last 20 plus years.

Now if you go to slide 3, this illustrates these two components that are important to us, namely the ground water flow pathways and the transport times. The figure on the left you've seen before shows our expected ground water flow paths from Yucca Mountain as simulated by the flow and transport modeling.

The figure on the right shows the results of some transport simulations. These are simulated breakthrough curves for Neptunium in this case at the 18 kilometer boundary. The solid black line is with no sorption, but with matrix diffusion. The dashed red line is for Neptunium with sorption, with matrix diffusion and sorption in the rock matrix of the

volcanic units, and you can see the line with transport times associated with that.

The dashed blue line shows a simulation result for Neptunium transport with sorption only in the Alluvium, and then the dashed magenta line shows simulation results with both sorption in the volcanic matrix and in the Alluvium. And this is for our expected behavior of the system without consideration of all of the uncertainties in the system, but this result is consistent with what you saw earlier in terms of NRC's conclusions about risk significance for different processes in the system. The more significant process here is sorption in the Alluvium, as opposed to matrix diffusion and sorption in the rock and the volcanic matrix.

Slide 4, we're going to discuss the regional and site-scale flow models. The regional model allows us to understand the general flow directions in the regional flow system and provides constraints on the volumetric flow rates through the aquifers, which then can be applied at the boundaries of the site-scale model. The site-scale model provides us much greater detail on the flow directions and the flow rates, much higher resolution representation of the hydrogeologic units of relevance

to repository performance at the site-scale.

And the site-scale model builds on observations of hydraulic head at the wells, permeability measures and DOE and Nye County bore holes, and some large scale aquifer tests conducted at the C-wells, and somewhat smaller scale test at the Alluvial Testing Complex.

Slide 5, this figures shows a portion of the regional scale flow system and a lot of the physiographic features in that area are labeled here. As well, it shows the rectangular boundaries of the site-scale model within the regional flow system. And the several features that are key controls on flow at the regional system, these are hydrogeologic formations, their spatial location, the individual properties. In addition, major faults play an important role in the regional scale flow system.

Also, the Death Valley Regional Groundwater Flow system is largely controlled, the water though that system is controlled by recharge and discharge. And a feature of importance at the site-scale is a local recharge along Fotymile Wash, and considerable pumping from the system along the southern boundary of the site-scale model in the Amargosa Desert region.

Let's go to slide 6. This figure shows the boundaries of the Death Valley Regional Flow Model, and within those boundaries the recharge, and recharge is primarily at higher topographic elevations of greater than 1,500 meters above sea level. You can see the darker purple colors correspond to higher values of recharge in the system. Also in this figure, you can see the outline of the Nevada test site and the approximate location of Yucca Mountain.

The highest values of recharge occur in the highest mountain ranges, such as the Spring Mountains to the south and east of Yucca Mountain, the Panamint Range on the other side of Death Valley. Recharge is a complex function of precipitation, the slope in the geology and the vegetation, and there is a considerable degree of uncertainty in the recharge estimates depending on the method that's used.

Another thing that I'd like to point out that this color scale is really not a linear scale, and there are relatively large areas that fall in this white or very light purple range in which recharge is a very small value, less than 1 millimeter per year in the white zone, so there is recharge occurring but at a very low rate in these areas.

I also wanted to point out that there are large areas where there's thick valley fill deposits in the system where there's essentially no recharge occurring in the system.

Slide 7 shows the locations of discharge regions in the Death Valley Regional Flow System, and these natural groundwater discharge locations occur in the topographic lows, in general, and significant discharge occurs from springs in the carbonate aquifer flow system, and by evapotranspiration from shallow groundwater at the playas. Taken together, these recharge and discharge estimates provide us with a basis for an overall groundwater budget through the regional scale flow system.

Okay. Slide 8, let's focus in on the site-scale flow system. The figure on the left shows an interpretation of a potentiometric surface at the site-scale. It also shows the wells that were used in this interpretation and the values, the water level values that we used in the interpretation.

I'm sure most of you are familiar with the general configuration of the ground water flow system here and the water table. Just to point out, there's a relatively high gradient to the north of Yucca Mountain. There is a moderate gradient just to the

west of Yucca Mountain that apparently corresponds to the Solitario Canyon fault. There's a relatively low gradient area to the south and east of the repository location.

Another feature I think that's significant here is there is apparent convergent groundwater flow system that corresponds and seems to be center on Fotymile Wash. This figure also shows the location of tertiary faults, and in some cases the correspondence between those fault locations and interpretations of the potentiometric surface, especially where those faults are apparent barriers to groundwater flow, such as Solitario Canyon fault.

I should also point out that this interpretation of potentiometric surface assumes isotropic permeability, and it does indicate generally a southeasterly flow from Yucca Mountain, and a southerly flow in the area of Fotymile Wash.

Slide 9 shows the hydrogeologic framework model that's incorporated into the site-scale flow model. The figure on the left shows the geology at the water table as interpreted from this model. This is a 3-D model domain. It's 30 kilometers by 35 kilometers by 2,750 meters below the water table. And the grid that's used in the flow modeling is a 500

meter horizontal spacing and variable resolution in the vertical direction, but the highest resolution to the grid is near the water table.

The interpretation of the geology or the hydrogeology at the water table has a high degree of complexity and resolution near the repository, and then as you move away from the repository and the high density of geologic information, the interpretation of the hydrogeology becomes more interpretive and somewhat coarser in resolution.

Slide 10 gives some information on the calibration of the site-scale flow model, and what's plotted in this figure are the simulated heads in the upper layer of the model close to the water table shown with the contours, and then the residuals in head are plotted with the various symbols and colors at individual wells.

The first thing that I should point out is that the general configuration of the potentiometric surface in the model matches the observations. We compare this configuration of the simulated water levels with the observed and interpreted potentiometric surface. They are very similar. And most of the water levels along the flow path southeast of the repository are accurately simulated. Those

cross symbols indicate a residual or a simulated head within one meter of the observed head, so that area with mostly -- that crosses there to the south and the east of the repository indicate that the calibration is quite close to the observed values in that area.

Values of head to the north and the west of Yucca Mountain are generally under-predicted by the model. This is probably due to simplifications in the conceptual model that exist within the model domain. And also, variations in the interpretation of the meaning of the heads, particularly directly to the north of Yucca Mountain as to whether or not those are perched, that represents perched water or is actually the water table.

Simulated head along Fotymile Wash and Amargosa Desert are generally within 5 meters of measured head, but I guess the lesson -- the point I kind of want to make here is that along the flow path, the calibrated flow model does reproduce the gradients that are observed rather accurately.

Slide 11 shows the comparison between measurements of permeability and the model calibration -- the values of permeability used in the calibrated site-scale flow model, so this is another piece of information that helps gives us confidence that we're

realistically modeling the flow system here.

The Calico Hills formation, which is a significant aquitard in the system. You can see the red dot represents the model calibrated value. The range of values for single-hole tests and for cross-hole tests there are shown for comparison. For the Prow Pass Tuff, the calibrated value of permeability which I should point out here is given in units of meters squared, is somewhat higher than the cross-hole testing would indicate, and significantly higher than the single-hole testing would indicate.

For the Bullfrog Tuff, the calibrated value is very close to the cross-hole testing results, and much higher than the single-hole test. We have reasons to think that the single-hole testing may have under-estimated permeability in this area.

At Tram Tuff, we do have some significant difference between the calibrated permeability in the model and the cross-hole testing results here, but I should point out that this cross-hole testing value that's given here is also in the single-test of the C-wells in which the Tram Tuff is intercepted by a fault in the borehole which may have biased the results there.

With regard to the larger scale flow

system, the carbonate aquifer, which is rather deep in the site-scale flow model domain, there is a good match between the calibrated value of permeability and the results of single-hole testing in the carbonate aquifer. And in particular, the Bullfrog Tuff, and to a lesser extent the Prow Pass Tuff probably most important with regard to flow paths from the repository.

Okay. The next slide, Slide 12, is an additional data set that provides confidence, confirmation of the flow paths that are simulated in the site-scale flow model. This is hydrochemical data and an interpretation of hydrochemical data in which the hydrochemical data in numerous wells within the site-scale model domain have been interpreted to fall within these different hydrochemical facies, Western Yucca Mountain facies, Eastern Yucca Mountain facies, Fotymile Wash, some of the most important ones here. But the pattern that comes out of this interpretation, as indicated by the green dots here, which is the Eastern Yucca Mountain facies, is that we do have similarities in ground water chemistry from underneath the repository to the south and east, and to the south, and further to the south giving some confirmation to the simulated flow paths that

correspond to this distribution of the Eastern Yucca Mountain hydrochemical facies that's indicated here.

There is significantly different groundwater chemistry along Fotymile Wash as indicated by the yellow dots here, probably influenced by recharge along the Fotymile Wash channel. And also some significant differences in hydrochemistry just to the west of Yucca Mountain, just to the west of Solitario Canyon fault, and a couple of wells to the east of Solitario Canyon fault too. They seem to be influenced by some underflow across Solitario Canyon fault. And then the red dots here indicating part of the flow system from Crater Flat.

I should also point out that the individual chemical species that were used in this interpretation and isotopic ratios are listed under each one of these hydrochemical facies.

Slide 13 presents a similar hydrochemical data, sort of an expanded scale here, where the dissolved constituents of importance are Chloride, Sulfate, Delta-Deuterium, et cetera. And these indicate the same trends that I pointed out before, flow system from beneath Yucca Mountain to the south and east, and then to the south, the flow system from Crater Flat generally to the south, flow system from

beneath Fotymile Wash to the south, and then to the southwest, and then a flow system from Jackass Flats that sort of downs this system on the eastern side of Amargosa Desert.

So let's go on to Slide 14, and this is the Carbon-14 data set which also provides us with some confidence in the simulated transport times to the system. Now Carbon-14 is a naturally occurring radioisotope with a half-life of 5,700 years, and there are some rather severe assumptions that need to be used in the interpretation. Direct interpretation of this Carbon-14 data is that the water acquires its initial Carbon-14 content as it percolates through the soil zone, and that in the absence of any water-rock interactions, Carbon-14 content will change only as a function of radioactive decay, thus allowing a direct measurement of groundwater age or changes in groundwater age along the flow path.

However, there are some significant uncertainties associated with these assumptions. Groundwater can acquire dead carbon; that is carbon that has essentially lost its Carbon-14 content from water-rock interactions, primarily through the dissolution of Calcite during evolution of the groundwater in the aquifer. And this would lead to

Carbon-14 ages that are anomalously old.

There's also the possibility of mixing the groundwater from different sources along the flow path. In particular, in the influx of groundwater from recharge along Fotymile Wash that has a higher Carbon-14 content, and this would also complicate our interpretation of groundwater ages through the system.

So Slide 15 shows the -- this figure shows the percent modern carbon in multiple wells through the system, and what you'll see is a pattern beneath Yucca Mountain. Most of the groundwaters have between 15 and 30 percent modern carbon in them beneath Yucca Mountain, and to the south and east of Yucca Mountain. There are a few samples, in particular, the one that's anomalous here of 5 percent, this is from the Carbonate Aquifer deeper in the system where we would expect a very low Carbon-14 concentration. The two yellow dots next to Fotymile Wash there are a significantly higher percent modern carbon, probably associated with recharge in Fotymile Wash. And there has been some modeling of the Carbon-14 evolution through the system that does take into account interaction of the groundwater with dead carbon in the system, particularly with regard to percolation beneath Fotymile Wash. And putting that information

together with these measurements of Carbon-14, the conclusion is that groundwater velocity estimates range from about 5 to 40 meters per year corresponding to advective transport times over the 18 kilometers from beneath the repository out to the accessible environment of several hundred years to several thousand years for an unretarded species.

This is not a very definitive estimate of groundwater transport times through the system, but it does -- it is consistent with our range of modeling, and does provide some confidence that there is a connection with reality there.

Slide 16 shows some more detail about the hydrogeologic interpretation in the site-scale model domain with regard to the Alluvium given its potential significance to radionuclide transport through the system, so the figures that's shown on the left there is the interpreted thickness of the Alluvium. Generally, the Alluvium is thickest under Fotymile Wash and southward towards the Amargosa Valley. And these interpretations are based on wells and on geophysical interpretation.

I should point out that this is not saturated thickness of the Alluvium, but thickness of the Alluvium from the ground surface. And this

information is used to constrain the location of the tough Alluvium contact which may be important to transport simulation.

Slide 17 shows the saturated zone site-scale flow model and transport model. And I wanted to use this to explain some of the numerical methods used here. Particle tracking method is used, and this includes radionuclide transport processes of advection, dispersion, and matrix diffusion in the fractured volcanic units, insorption in the volcanic matrix, and in the Alluvium.

The simulated flow paths from the repository occur in the upper few hundred meters of the saturated zone so they're relatively close to the water table, and the flow rates in terms of the Darcy flux or the specific discharge vary along the flow path from the repository, from about .7 meters per year under Yucca Mountain, increasing to about 2.4 meters per year at the 18 kilometer boundary of accessible environment. So as I pointed out before, this is a convergent flow system in which the specific discharge increases significantly along the flow path underneath the repository.

Slide 18 gives some information on our model of colloid-facilitated transport as it's

implemented in the model. There are two modes of colloid-facilitated transport. The first one is an equilibrium model in which radionuclides can be reversibly sorbed onto colloids. The second model is which the radionuclides are either permanently sorbed or attached to the colloids through the system, and these radionuclides then just ride on the colloids through the system with no possibility of leaving the colloids.

So for the transport of radionuclides that are reversibly attached to colloids, we assume local equilibrium, and then the colloids, the aqueous phase, and the aquifer material for the sorption of these colloids. For the radionuclides that are irreversibly attached to the colloids, there's no desorption of the colloids that occurs. The colloids with the irreversibly attached radionuclides are subject to attachment and detachment from the mineral grains, so the colloids themselves are subject to retardation through the system, but there is no permanent filtration of the colloids in the system. This is, of course, a conservative assumption with regard to radionuclide transport.

And a small fraction of the colloids with irreversibly attached radionuclides is transported

through the system with no retardation. And this is as a result of the filtration kinetics in the system. Our estimates of the rate constance for kinetic attachment and detachment of colloids, and the sort of minimum transport time through the unsaturated zone and saturated zone indicates that there will be this small fraction that would not be retarded as the colloids move through the system.

Slide 19 lists all of the parameters that are considered in the uncertainty analysis for groundwater flow and radionuclide transport. I won't go through all of these in detail but there was a question earlier about which of these parameters are probably -- are most significant to our uncertainty in radionuclide transport through the system. And I would point out that probably the most important one is still our uncertainty in groundwater-specific discharge. How fast ground water is moving through the system has a significant impact on the transport simulations.

And with regard to transport, one of the parameters that's relevant to matrix diffusion is the flowing interval spacing. This is also a parameter to which there is significant sensitivity in the modeling results. And then for Neptunium transport, the

sorption coefficient for Neptunium onto the Alluvium is a significant parameter. So that's kind of a very quick and dirty prioritization of these parameters with regard to sensitivity.

Slide 20 shows some transport simulation results. What's shown in these figures here are 200 realizations of the system in which uncertainty in all of the -- uncertain parameters is included. The upper figure shows the simulated breakthrough curves for in this case a non-sorbing species from the water table beneath the repository to the boundary of the accessible environment.

The histogram below shows a histogram of the median transport time shown in those breakthrough curves above, so the midpoint of each one of those breakthrough curves is then represented in the histogram below. The red dashed line in this case shows the median of the median transport times which is between six and seven hundred years through the system. So this suite of realization shows the variability in the transport times among realizations for species such as Technetium-99 and Iodine-129 extends from less than 100 years to greater than 10,000 years. Many of these breakthrough curves exhibit a long tail that's characteristic of mass

transfer in the rock matrix in the volcanic units, so you can discern the impact of matrix diffusion in the simulation results. And these results like the others, I want to point out that these do not include radioactive decay.

So Slide 21 shows a similar set of results in this case for Neptunium. You can see that the simulated breakthrough curves are shifted to the right reflecting the sorption of Neptunium in the system, and the variability here indicates, among these realizations, that Neptunium-237 has an uncertainty that extends for less than 1,000 years to greater than 100,000 years. And sorption and retardation for Neptunium is generally moderate in Alluvium and minor in the matrix of the fractured volcanic units. And approximately half of these realizations exhibit median transport times of greater than 20,000 years in the saturated zone. And I should point out, this is under present climatic conditions, and that holds true for all of these transport simulation results that I'm showing here.

Slide 22 shows similar transport simulation results for Plutonium that is reversibly attached to colloids. Here the variability in the transport times among the realizations extends from

less than 10,000 years to greater than 100,000 years, which is the limit that the simulations was done here. And sorption for Plutonium is strong in the Alluvium and in the matrix of the fractured volcanic units.

The reversible colloid-facilitated transport model results here, the model that's used here result in minor enhancement of Plutonium mobility. These simulation results do show the effect of colloid-facilitated transport, but it's not a dramatic effect. That's a function of the sorption coefficients onto the colloids for Plutonium, and the colloid concentrations in the groundwater. More than half of the realizations exhibit median transport times of greater than 100,000 years under present climatic conditions.

So on to Slide 23, just to summarize a few of the important points here. The saturated zone flow model developed to evaluate what the flow directions and the float rates through the system. These flow models are constrained by the regional groundwater budget, hydrochemistry, water level observations, and site-specific permeability measurements. The flow model projects flow paths in generally southeasterly direction and then southwesterly direction. The flow model predicts fluxes along the flow path from beneath

the repository in the range of .7 to 2.4 meters per year.

The fraction of the flow path in the Alluvium is a function of the flow path which is itself sensitive to the anisotropy and permeability which is an uncertain parameter, and the flow path length in the Alluvium ranges between 1 and 10 kilometers.

Slide 24, the rest of the summary and conclusions - matrix diffusion in the tuff and effective poracity in the Alluvium have been determined from tracer tests, so there is a basis for this process in experimental and field results. Effective transport velocities developed from the flow and transport model yield transport times mostly between several hundred and several thousand years for unretarded species. And these transport times are consistent with the Carbon-14 ages within that relatively broad band of uncertainty.

Processes of matrix diffusion and sorption have been confirmed in field tests, and uncertainty in groundwater flow and radionuclide transport parameters are evaluated with the model for incorporation in the performance assessment analyses. So that concludes my presentation. Thank you.

MEMBER HORNBERGER: Thank you very much, Bill. Let's go through the questions as we've been doing. Ruth.

MEMBER WEINER:: Thank you for a very good presentation.

MEMBER HORNBERGER: Mike.

ACTING CHAIRMAN RYAN: Bill, just a quick question, and it caught my eye on the case of Plutonium. Why wouldn't you account for decay? Because if you look at a period around 100,000 years, that's four half-lives or 80 percent decay.

MR. ARNOLD: I missed part of the question. Can you repeat it?

ACTING CHAIRMAN RYAN: I'm sorry. Yes, in the case of Plutonium on Slide 22, you said, as you did with all the slides, that you did not account for radioactive decay. In the time period of up to 100,000 years, that's four half-lives or so, and that's not a trivial amount of decay in your period of observation or interest, so could you help me understand why you didn't account for decay?

MR. ARNOLD: Yes. That's absolutely true. Yes, thank you for pointing that out. Just for the purposes of presentation of these results, these do not show the effects of radioactive decay. In the

performance assessment calculations, radioactive decay is included as a process. We use a numerical method for coupling these results with the performance assessment calculations, and it's a convolution integral method. And radioactive decay is incorporated in that step of the analysis, in the convolution integral.

ACTING CHAIRMAN RYAN: I guess you did that for everything even though it might not be a big effect for some longer-lived species.

MR. ARNOLD: Yes, that's correct.

ACTING CHAIRMAN RYAN: Okay. Thanks.

MEMBER HORNBERGER: Allen. Ines.

DR. TRIAY: Yes. I would like you to expand a little bit on this third bullet on your summary and conclusions when you say that this transport - excuse me, the fourth bullet - where you say that "processes of matrix diffusion and sorption have been confirmed in field tests." Could you tell me what exactly does that mean, to what extent have they been confirmed? What does that mean from the point of view of the database for diffusion, as well as sorption? Could you fill out that sentence for me a little bit?

MR. ARNOLD: Right. Let me just make sure

we're talking about the bullet that says, "Matrix diffusion in the tuff, and effective porosity in Alluvium" --

DR. TRIAY: No, no. I'm talking about in the summary and conclusions radionuclide transport, page 24. I'm talking about the fourth bullet, the one that starts with "processes of matrix diffusion and sorption have been confirmed in field tests."

MR. ARNOLD: This is referring back to the C-wells testing that Bob Andrews described, and the ability to match the cross-hole tracer testing that was done at the C-wells is taken as confirmation of the process of matrix diffusion. Also, the differences in the breakthrough curves for tracers with different diffusion coefficients. Also, the sorption process with regard to the lithium transport in the tracer tests provides confirmation that the process of sorption is occurring in the system. And in addition, it provides some evidence that the laboratory-based measurements of sorption coefficients are at least applicable at this field scale, and possibly even conservative relative to the field scale.

DR. TRIAY: Can we make a statement like this for colloid transport? Do you have the same type

of confirmatory test?

MR. ARNOLD: I don't believe that we can make a similar statement for colloid transport at the field scale. There are aspects of our colloid-facilitated transport that have been confirmed at the field scale with the cross-hole testing at the C-wells. And the aspect I'm referring to here is the retardation of colloids in the system, or the reversible chemical filtration of colloids in the system. However, not all aspects of the conceptual model for colloid-facilitated transport have been confirmed at the field scale.

MEMBER HORNBERGER: Jim Clarke.

DR. CLARKE: Bill, very nice presentation. Just one question, and this may not be something you have readily available, but in your first page of conclusions, page 23, the last bullet, "The fraction of the flow path in the Alluvium ranges between 1 and 10 kilometers." And I just wondered for a sorbing radionuclide, what's the impact on the travel time for that distance range? Is that a pretty big difference? Is that something that's being characterized a little better? Are you going to go with that, or what's the impact of that?

MR. ARNOLD: There's probably a

significant impact to that uncertainty with regard to Neptunium transport, and that's because of the contrast in the sorption in the Alluvium versus the volcanics, for Neptunium in particular. That statement is a little bit incomplete. That range of 1 to 10 kilometers is not all due to our uncertainty in flow paths, and uncertainty in the geology of the system. That's partly a function of variability in flow paths depending on the site of origination beneath the repository, so this 1 to 10 kilometers is a combination of uncertainty in the system and variability along flow paths depending on the starting point of the flow path.

MEMBER HORNBERGER: Jim Davis.

DR. DAVIS: Yes. This information in the analysis of the hydrochemical facies, is that available in one of your technical documents?

MR. ARNOLD: Yes, it is. There's an analysis model report that's devoted entirely to this subject. I can provide you with the current draft, or current version of that report if you'd like.

DR. DAVIS: Yes, I'd like that. And one other question - in looking at the Carbon-14, you have the velocity estimates range from 5 to 40 meters per year, and then in the particle tracking model you have

flow rates ranging from .7 to 2.4 meters per year. Is that the agreement in flow velocities that you're referring to in the conclusions?

MR. ARNOLD: No, these are two different quantities that are being referred to here. The .7 to 2.4 meters per year is specific discharge, and the 5 to 40 meters per year is the core velocity. What we are comparing though is the conclusion of several hundred years to several thousand years for unretarded species, that conclusion from the Carbon-14 analysis, and that result from the transport simulations.

DR. DAVIS: Thank you.

MEMBER HORNBERGER: Dick.

DR. PARIZEK: Several questions. One, in terms of the site-scale model, as you know, I guess your regional flow model was used to constrain the input to the site-scale model, and it was the old three layer Valley Regional Flow Model of the survey that was used. If you look at the updated model, the flux boundaries aren't necessarily the same any more, and the quantities you are entering in the site-scale model differ, and also in some cases even direction of flow differs. What difference might that make in the site-scale model forecast that you've summarized today, if you updated it with a multilayer model

that's now available?

MR. ARNOLD: Well, that is being analyzed. There are some preliminary results with regard to that, and there is an update to the flow model AMR in which the impacts of those -- the new Regional Scale Flow Model are assessed. It may be premature for me to state what those conclusions are at this point, because that's still in draft.

DR. PARIZEK: Okay. So it's something that's being worked on, in any event.

MR. ARNOLD: Yes.

DR. PARIZEK: Is there any permeability contrast for the bedrock that would give you flow directions that would be more southerly than what you show on -- well, the page that gives us the red flow lines, I guess it's page 3. And once again, unlike what Jim Winterle showed us earlier, but is there any way to get the flow to go south that's credible based on permeability contrast within the tuff units?

MR. ARNOLD: Well, one thing I should point out is we do consider anisotropy, horizontal anisotropy and permeability in the volcanic units. And the results that are shown here on page 3 are for isotropic conditions. And the full assessment of uncertainty as shown in those breakthrough curves

actually at the end of this report, these flow paths do vary from realization to realization. And when the horizontal anisotropy is high, as it is in some realizations, as high as a factor of 20 in the north-south direction versus the east-west direction, the flow paths are simulated to be in a more north-south direction.

And that horizontal anisotropy is kind of a lumped parameter. It sort of implicitly considers the kind of permeability contrast I think you might be referring to here, higher permeability in north-south oriented faults, or lower permeability across barriers that are oriented in the north-south direction that would lead to that anisotropy in a more north-south direction.

DR. PARIZEK: That's the one that gives you that 1 kilometer distance of travel in Alluvium, the shortest of the range from 1 to 10 kilometers, if you take that --

MR. ARNOLD: That's right. The travel, that distance of only 1 kilometer corresponds to a case in which you have a high anisotropy, and the flow path is more north-south, and the source originates from the southern end of the repository.

DR. PARIZEK: And would you expect the

effective poracity and matrix diffusion numbers that you use to vary if you went through a long-term tracer experiment? I guess most of what's happened to the Alluvium has so far been short-term push-pull-type testing. But again, if the long-term experiments are run as originally planned, do you think that would change the effective poracity and/or diffusion properties?

MR. ARNOLD: Well, my impression is that the uncertainty distribution we're using for effective poracity in Alluvium now is really a bias towards a high or a low value. I think it's probably a good estimate. It's got a fair amount of uncertainty in it. We would certainly reassess that uncertainty distribution with the results from a large scale cross-hole tracer test in the Alluvium, and I think it would reduce our uncertainty in that parameter, and give us greater confidence in what we're using in the model.

DR. PARIZEK: All right. And is there any input to the science testing, the confirmation testing program dealing with saturated zone flow and transport? Are there any studies included in there, or is that maybe a premature comment on a report that's due out later in the year.

MR. ARNOLD: I'm not personally very familiar with what's in that report, the confirmation plan anyway. There is some work in the science and technology area.

DR. PARIZEK: You have some proposals for the science and technology program. Could you kind of give us some details of what those might include, or hints at what's involved in the science and technology area?

MR. ARNOLD: I'll let Drew Coleman make some comments on that.

MR. COLEMAN: Yes. This is Drew Coleman, DOE's Saturated Zone Lead. Yes, we have in the confirmation plan to finish the Alluvial Tracer Complex testing, be kind of contingent on the ability to get permit from the state to finish that testing, but that's in the performance confirmation plan. And then they have in the science and technology program, they have a long-term pump test that we're working on the details of right now, sort of planning it with a view towards maybe doing the testing in `05. And then there's also a natural gradient test where you would put tracers in and let them travel under the natural gradient, and try to collect them in some reasonable amount of time at a downstream point. Those are some

of the saturated zone plans in the next one or two year time frame.

DR. PARIZEK: Drew, that `05 long-term test would be for Alluvium, an Alluvial Complex Test, or is that another bedrock test?

MR. COLEMAN: I believe that would be up in the volcanics this time. There's also the Nye County Tracer tests that we're going to have going in the Alluvium. It's a little north of the original tracer test and that ought to even be going a little sooner than `05, maybe late `04 here.

MEMBER HORNBERGER: Don, do you have questions?

DR. SHETTEL: Yes, I have one comment. The biggest problem I see in the saturated zone is the question of colloids. And I don't know how this is going to get resolved, but one thing from the past is the migration of Plutonium from the Benham underground test. Is any of that work being incorporated into saturated zone?

MR. ARNOLD: In a conceptual level it is, because we do have this fast fraction of colloids which move through the system that radionuclides irreversibly attach to them. And that fraction is not subject to filtration or retardation of any kind. And

that is consistent with the observation of Bob.

DR. SHETTEL: What proportion is the fast fraction of --

MR. ARNOLD: For our analysis in the saturated zone, it's a small fraction. It's less than 1 percent, but I don't have the number right here.

DR. SHETTEL: Okay. Thank you.

MEMBER HORNBERGER: Thanks very much, Bill. Abe Van Luik had an item that he wanted to present. Abe.

MR. VAN LUIK: Yes. What I did was I faxed a couple of sheets of paper. I noted listening to Dr. Davis this morning that he's a familiar figure at the Nuclear Energy Agency. He has contributed to several meetings on these types of topics, and I just wanted the group -- I know that most of you are aware of this, some of you may not be, to be aware that there are actually documents that have been created through the Nuclear Energy Agency looking at these topics that we're discussing today. And I noticed in the two examples that I give the front page and the table of contents for in my fax, that actually DOE, Sandia mostly, and in one case MTS, NRC through the Center, and USGS through Davis has participated in producing both of these products. And I think it's

interesting to look at the first page of my fax, which was taken directly from that website that you can access, anyone can access. And it shows that this first document on using thermodynamic sorption models for guiding KD investigations was published in 2001. They completed Phase 1 of the NEA sorption project, and Phase 2 is going into a lot of the stuff that was mentioned in the Q&A on the Davis talk; which is, what about Neptunium, what about some of these other questions?

I think Phase 2 is going on without U.S. participation, which is unfortunate but that's just kind of the way it happened, but it will be completed pretty soon, and a document will be available to us. So I just wanted people to be aware that there are resources internationally, especially when it comes to saturated zone transport. Every repository program in the world is looking at saturated zone transport.

And in the other document, "Radionuclide Retention in Geologic Media", it has a section on matrix diffusion. To the question does it exist, it says yes. And then it's kind of like Bob Andrews, there's a but after that. And it speaks of colloids. There's been a lot of work done by Dick Eldra, especially, in the European Union on colloids, and so

we're aware of these things. We try to participate in those, so does the NRC, so does the USGS. And I thought that those of you not familiar with these documents ought to become aware of them. Thank you.

MEMBER HORNBERGER: Thank you very much. Abe. So we're finally at the point on our program that says it's 4:15, and we now have time scheduled for public comments. And I think what we'll do is we'll start here in Rockville to see if there are any public comments, and then go to Las Vegas. Okay. Do we have comments from people in Las Vegas?

MR. ELZEFTAWY: I have a couple of comments I'd like to make. I'll introduce myself first. Can you hear me?

MEMBER HORNBERGER: Yes. Please introduce yourself first.

MR. ELZEFTAWY: I will. My name is Atef Elzeftawy, and I'm here for the second review on behalf of the Las Vegas Payute Tribe. And I have a couple of things to say to that extent, and then I have my own personal comment in general. I'd like to pass it to the committee and to keep it for the record.

I presented these two questions to the chair of our Las Vegas Payute Tribe here in Las Vegas,

and my answer to that, based on what I know so far about the Yucca Mountain program in general, is that after all those years, we have not really nailed down the so-called expected behavior of radionuclides in the tuff and the valley. In other words, we have not really got enough data for us to say is this the distribution of this behavior, is log-normal or normal distribution, or a gamma function, or whatever that is. We know that we have some data. We know the DOE has provided some information, but what does it mean to the normal person might not be really there.

The second question I think that was very good with regard to the conceptual models and the mathematicals, implementation of the site data, and the confidence of the site data with regard to the recordation of the radionuclides.

We all have our own - that's exactly what I said - we all have our own conception models, now ideas as scientists, and as people. We also have our mathematical implementations. But I think it's going to come down to the site data that would provide enough confidence beyond 50 percent range to say that the geosphere can retard, not may - remember, there's a big difference between can and may - can retard the transport of the radioactive materials in the system.

So far, my personal comment now as a public citizen is that in 1983 when I looked at the data of the saturated zone hydrogeology back with the NRC, we made the comment that it looks like surface groundwater moving toward the south, maybe a little bit southwest. After all these models, and after 21 years of work, the Department of Energy have not really made any different interpretation, or maybe to bind the groundwater system of the Amargosa Desert area.

I have a problem with all the beautiful models we have and all the money we spent. I haven't seen somebody to sort of push the envelope a little bit with regard to the models. The fellow who talks about the recharge, how about trying 5, and 10, and 15, 20 millimeters per year recharge and find out how the system is going to react using what you have done.

I have one comment to Ruth. I'm not really sure what's her last name, but if you go back to the University of California at Berkeley, there was a paper under Hilgardia published in, I think, 1973-74 related to the so-called soil water or porous media parameters that we really deal with with regard to the unsaturated zone. The soil moisture, retention curves, the hydraulic conductivities, the retardation

factors and all that. I think you need to get it and read it, and find out what Don Nielson has published in 1973-74.

I published a paper when I was working for my second Ph.D. degree in 1973 in the University of Florida, way back then, and it was dealing about the absorption of the Tritium in just the porous media. You'll find that. I think I have a copy at home, but go and find it and find out what was said about the absorption and desorption of the Tritium. And I'm not talking about radionuclide with big veins.

The gentleman by name, Jim Davis, who has a presentation that I didn't see, I think I told Al Freas and John Cherry in 1979 that that figure that you quoted from him is really misleading. Actually, it's scientifically wrong. I'm not sure if Al Freas and John Cherry has corrected that or not, but it's misleading to have this figure. It talks about "Sorbing Solutes and Non-Reactive Tracer". I think you need to switch that back and forth because if we talk about the sorbing solutes breakthrough curve, that tells me that this is only the aero function distribution, and that is not the aero function distribution.

There is a non-reactive tracer flat in the

curve, and that is not true. Usually, the non-reactive tracer acts as a "piston flow" in the porous media analysis, so I think you need to correct that draft. You quoted it wrong.

And one of the things I wanted to mention, way back then when we were working under the 10 CFR 60, we were talking about the 1,000 year groundwater travel time. During the public meeting of the Nuclear Transportation Research Board, if they changed the name, whatever the case may be - the Department of Energy made the comment - Russell himself made the comments about the transport of the radionuclide in system, in porous system unsaturated or saturated. And he said they have nothing to do with the existing regulation, 10 CFR 63. So my question to you as a public citizen, why are we sitting here in a sense wasting all that time trying to find out the nitty-gritty of the absorption, desorbtion, reversible groundwater travel time and all that, and the Department of Energy and the NRC already made the decision that they are not going to consider that, except in the performance analysis. So how can you relate all the things you do today with regard to the licensing? That's really what the bottom line is. And that probably concludes my comments.

MEMBER HORNBERGER: Thank you very much. Do we have any other comments? All right. Well, we've actually made it to our 5:15 ending point. I turn it back to you, Mr. Chairman.

ACTING CHAIRMAN RYAN: If there are no further comments or observations, we will adjourn for the day.

DR. CLARKE: And you're going to reconvene at 9 tomorrow morning.

ACTING CHAIRMAN RYAN: Yes, I'm sorry. We will reconvene and start at 9 a.m. tomorrow morning.

(Whereupon, the proceedings in the above-entitled matter went off the record at 5:16:59 p.m.)