UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON THE NATURAL SYSTEM
SATURATED ZONE FLUID FLOW AND RADIONUCLIDE TRANSPORT

Wednesday, March 10, 2004

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Dr. Priscilla P. Nelson, Chair, Morning Session
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I N D E X

P R O C E E D I N G S

(8:00 a.m.)

NELSON: Good morning and welcome back to this meeting of the Nuclear Waste Technical Review Board, Panel on the Natural System. I am Priscilla Nelson and I'm one of the panel members for this Board Panel. Today, we continue the theme of hydrogeology, fluid flow, and solute transport, but the focus now turns to how those processes operate in the saturated zone of Yucca Mountain following the unsaturated zone consideration yesterday. You may recall that yesterday we presented a list of questions that outline the central purpose of this two day meeting. And, here is the list of questions again.

Each of today's talks will address one or more aspects of these questions. At the end of the day after all of the technical presentations are concluded, we will have a roundtable discussion forum that will include Board members, Board consultants, presenters, and others as identified and I hope you'll all stay to participate in that discussion. Time permitting, we might even allow the Board Staff a few questions. I want to call your attention to the Board Staff sitting over to my left.

The first talk of the day will be given by Claudia Faunt of the USGS. That talk will present the USGS model of the entire Death Valley regional groundwater flow system of which Yucca Mountain is a part. And, after that presentation, John Bredehoeft of the Hydrodynamics Group will talk about investigating the role that faults play in controlling flow through the Funeral Mountains on the west side of the Amargosa Valley, work that his company is doing for Inyo County. After a short break, Jim Winterle of the Center for Nuclear Waste Regulatory Analyses will present his model of the groundwater flow system in the Yucca Mountain area, including investigations of the effect of climate change on the local groundwater flow system. The final talk of the morning will be given by Ken Rehfeldt from Los Alamos National Laboratory and he will present the DOE's conceptual model of saturated zone flow and transport and independent lines of evidence for evaluating DOE saturated zone model predictions. So, we have an excellent second day for the panel following a very interesting first day yesterday.

As is normal, just before lunch we have set aside a period of time for public comment and this period is intended for people who, for one reason or another, cannot wait until the comment period that's scheduled at the end of the day. Some people may simply not be able to stay for the entire program. I know it's early, but is there anyone here who already knows that they will not be able to stay until the end of the day for that comment period so that we make sure that we have time for them at the noon break?

(Pause.)

NELSON: I see a few hands. Well, that's great. Please, make sure to sign up so that we know who you are. And, by signing up, we mean to go to the desk where Linda and Alvina are raising their hands right now and sign in so that we know that you do indeed want to make a comment.

If you prefer at any point during the day, you can submit written matter for the record to Linda or Alvina. You can also pose questions through the Board themselves. As a reminder, please, silence your cell phones before we start today.

And, with these preliminaries out of the way, it's my pleasure to introduce Claudia Faunt. Claudia Faunt received her degrees from the Colorado School of Mines in geological engineering, one of my favorite fields of study. Dr. Faunt is currently a hydrologist in the San Diego Project Office of the USGS. She is a leader and member of the Death Valley regional groundwater flow system project, and in that capacity, she has studied water resources in southern Nevada and California. Utilizing geographic information systems and 3-D geologic modeling and visualization tools, she specializes in integrating geologic information into groundwater studies and that's an integration we're all interested in.

So, the floor is yours, Claudia. Thanks.

FAUNT: Good morning. I'm going to talk about kind of a large regional flow system model. It's kind of a group effort that we've put together over a large number of years. These are the different people on the team. It's split a little with people all over the western United States from Denver, Tucson, Sacramento, Boulder, California, San Diego, and a lot of people in Las Vegas, as well. It's kind of a mixture between people in water resources division of the U.S. Geological Survey, as well as geologic division.

Kind of an overview of my talk, this is kind of the topics I'm going to go through. I'm going to talk a little bit about the conceptual models and the geologic emphasis of this project, the tasks that we're going through this year, describe some of the regional groundwater flow model we've put together, go into a little bit of detail on one of the particular uses of the model which is fluxes in and out of the site-scale model at Yucca Mountain, talk a little bit about the report outline for the report that's coming out the end of the year, and then some questions.

This is a map showing the Death Valley regional flow model area. It's a large area with pretty complex geology. One of the unique features about the model is it's been put together with two major funding sources. It's been funded about 50 percent by Yucca Mountain Project and probably about 50 percent by the Department of Energy and their different funding parts with the Nevada Test Site work with the underground testing area program, the defense program, and a number of different funding sources.

The model we're putting together now is kind of an update of a number of previous modeling efforts that were done partially by the Survey and partially by consultants and DOE to try to combine all the data that exists from these models and databases that are put together to support those models and the different information learned from those models and a lot of new geologic work that's been done in the last three or four years. It's being constructed using MODFLOW-2000. It's actually a transient model that covers the time period from 1913 to 1998.

The study area, if you are familiar with it, goes from--Las Vegas is over here on the southeastern edge, Nevada Test Site is in here, Yucca Mountain is right along in here. Death Valley which is kind of the ultimate discharge area for the system is over in here. It's a pretty big area. The kind of brown box surrounding it covers about 100,000 square kilometers. The model goes from land surface down to 4,000 meters below sea level and it's covered by about 16 layers.

To kind of look at a conceptual model of the flow system, it's kind of part of the carbonate rock aquifer kind of carbonate system. But, this carbonate system has been interrupted by a number of large-scale basin, range style type faults. So, there's large mountain blocks with intervening valleys. Most of the pumping occurs in these valleys that are between these mountain ranges. The carbonate rock forms a very permeable regional system that connects a lot of the system. A lot of low-permeability rocks are also involved in this faulting and form block structures and cause a lot of the discharge in the system. A lot of springs, such as Ash Meadows, are situated along faults and structures.

So, we've spent a lot of time looking at geology and part of that is, if you look at Darcy's Law, you can split it into two pieces and half of it is basically considered the framework or kind of the geologic component and half of it is the hydrologic component. And, the studies and why there's a large team putting together this effort have been split into these, you know, different subject matters and different people are focusing on different portions and then they're being pulled together in building the groundwater flow model.

The framework is where I've concentrated on and it's kind of where a lot more of this talk will be oriented. It talks about some of the heterogeneity and the aquifers and confining units and their distribution. Hydrology is a lot of the water level data, the pumping data, some of the infiltration which Alan Flint talked about yesterday, discharge that Randy Laczniak did a lot of work on.

So, the geologic interpretations that support the model. For the last three or four years ending about a year ago, a large mapping was done in the region and synthesizing existing geologic maps. These were compiled together. There was kind of an interpretation of the tectonics. It was consistent for the entire region. Before, there was different structural styles studied and mapped in different areas and this was an attempt to integrate those into a consistent tectonic picture for the entire region. In doing that, about 32 regional geologic cross-sections were built. These are new ones that are about a 1 to 250,000 scale. A number of geophysical studies were done, gravity and magnetics, in particular, mostly looking at the area around the test site, and some of the magnetics and gravity were studied intensively around Pahrump Valley, as well.

Part of the extra geophysical data led to some stratigraphic analysis of some of the tertiary basins and this has been particularly helpful in some of the transient runs where we needed more data to look at the pumping scenarios and when water level declines where because if we had more information, then more geologic information was necessary to support the differences. And, there was some work by Don Sweetkind and others to look at the hydrologic significance of some of the structural and stratigraphic elements.

The way this geologic information was integrated into the flow model was building a three-dimensional geologic model. This is kind of a cut into the geologic model or cross-sections through the model. If you start to look at below the land surface, you start to see this aqua color which represents the paleozoic carbonate rock aquifer. And, you can start to see where it's connected and where it's not. In the north-central portion of the study area is this large volcanic field of southwestern Nevada volcanic field and a lot of the volcanics at Yucca Mountain are part of the southern end of this volcanic field. These are kind of superimposed on the carbonate rocks. The browns and purples are kind of confining. You need some basement rocks that interrupt the flow in the carbonate rock systems. And then, the yellow is the valley field and this is where a lot of the actually well development and pumping and actually a lot of our head observations are centered in because that's where the people live and it's an easy place to get some water.

You start to fill in the model and you look at it from the land surface and you get kind of a different perception of what's in the model and it looks like there's a lot more valley field than there is. If you looked down below the surface, you see there's not as much connective valley fields as it looked like on the land surface. So, this has been very important in forming the framework for the groundwater flow model.

So, you have all this complex geology. Does this demand a complex flow model? And, there's a lot of questions back and forth whether or not this is, in reality, true. A number of the regional models we've done, so far, indicate that the complexity in the Death Valley region is required. Most of this is because of the structural controls and the scale of the geologic features in the Death Valley region. Large faults with over 3,000 meters of flow on them requires some pretty detailed geology to put the blocks in the system and have the spring flow represented property. So, that was the reason we put together the detailed framework.

This is kind of switching gears a little bit and talking about the tasks that we've been going through in the last year. As a number of you realize, two main players in this project left in the last year and we still have to put together a large transient model and a report and have it published by the end of September. So, what's been going on in the last year is putting together all the datasets and having them published. All the supporting datasets are published and out. What we're working on now is the transient model report. It's been through review. It's had probably about every other month a kind of group of people get together that includes the National Park Service, representatives from DOE. Different interested parties have been looking at the model as it's been being built which has made a big difference during the review processes. We've had comments all the way along and tried to incorporate that information all the way along which has been frustrating at times, but it's also, I think, made it a better product in the long-run. The report went to review. Those people who were reviewers, as well as a lot of internal survey people, reviewed the model. We've gotten reviews back and we have to have the report with all the review comments responded to by March 31st which is in three weeks. The model is transient. It represents a lot of the things that the Park Service was interested in in terms of boundary conditions which is a big change in the model. It also represents a lot of details on the test site that weren't incorporated in terms of the geology and the volcanics in the last few models. So, in order to be published and bound and handed out on September 30th, it goes to the editor on March 31st.

Incorporated in building that model, there's a number of additions to MODFLOW that were made, most of which are related to the hydrogeologic units and incorporating like depth decay properties, decreasing the permeability with depth, and ways of incorporating geologic information into MODFLOW which has been nice for the community-at-large, as well as made a much better model in this case. We worked a lot about model consistency and trying to make some of the framework issues more consistent at both the test site models and at Yucca Mountain. Mostly, in this case, identifying areas where we need to work on that. Done some work with predictive capability and some decision analysis tools which I'll talk a little bit more about later.

The report is going to be in six parts plus two appendices. The appendices include some regional boundary water budget type information, as well as a regional potentiometric surface. There's various authors on different parts. It's an introduction, then kind of a geology chapter, kind of a conceptual model of the system. Kind of an evaluations--kind of a put-together of all the data that's supporting the model is Part D. The framework which is kind of the synthesizing of the geology and simplifying it to something we can get into the flow model and then the actual text on the flow model.

Here's some details on the model. A lot of it will look very similar to the older versions of the model. It still has a 1500 meter grid spacing which leads to 194 columns and 160 rows. This is a satellite image of the area. Spring Mountains are in here, Yucca Mountain is right in here. You can see Red Cone and Black Cone from Crater Flat, the Amargosa Desert, and Death Valley.

This grid across the model is the one and a half kilometer spacings of the model grid. It's 16 layers. The top of the model now goes to land surface, but this upper layer is convertible now. So, it can dry out and receive water. It's wet-dry and it represents an unconfined condition which was one of DOE's concerns at one point with the model before being all confined. The layers have changed. Most of them follow the water table except those upper layers in order to represent that drying system.

The discharge is represented by drains which I'll talk a little bit more about in the middle. The recharge is based on infiltration model that Alan talked somewhat about. It's a transient simulation, as I mentioned before. The first stress period is a steady state and it replaces the 2002 steady state published model that was kind of a hard merge between the two existing models.

This just shows some of the representation of the discharge by drains. There's quite a bit of data constraining the model that's from spring flow and evapotranspiration studies in the area which is kind of unique for a groundwater flow model. Most of the models are constrained mostly by head observations. So, we have head observations, as well as discharge, constraining the model.

And, the way this dataset was put together, it was a lot of satellite imagery was interpreted to get vegetation types. We had actual ET stations out in the field to measure the rate of evapotranspiration at the sites and then these areas were--the numbers collected from the field and the satellite imagery were put together to determine the rate of evapotranspiration in each model cell. Then, that was used as a calibration target and then the area of each vegetation type within each model. So, it was used to come up with that rate.

Recharge is based on Joe Hevesi and work Alan Flint have done on infiltration model. This is the work that was published in 2003 with Joe is the senior author. This is the infiltration model that was put into the model. Higher infiltration which we're assuming basically simulates as equivalent to recharge. High in the mountain ranges that are very permeable like the Spring Mountains and the Sheep Range. One thing that's different about this model than other infiltration and recharge we've used is there's focused infiltration in some of the channels. You can see Fortymile Wash here in kind of a blue. And, that's a big difference in matching some of the head levels, especially like UE-29, A-1 and A-2, up Fortymile Wash. People are familiar with it.

One of the things we did to help calibrate it was to split the infiltration areas into zones based on high-permeability rocks and infiltration rates. So, if you had high-permeability and high infiltration rates, we made one zone which is kind of this yellow color. If you had volcanic rock and a moderate infiltration rate, you've got another color. So, we had these nine different zones and we used those to form a multiplier times the infiltration rate to help calibrate the model and that worked pretty well. The range in change between the actual infiltration rate that was given in Joe's model and what we're using in the model ranges from 85 percent of what the infiltration rate is to about 115, 120 percent. So, it works pretty well, just some little tweaking and moving things around.

One of the other constraining factors on the model is the hydraulic head observations. We've classified the data into kind of three categories. Kind of steady state which represents no kind of change in water levels based on human changes in the last, you know, 100 years, and those are shown in yellow on this map. And, that was what was used basically to constrain models in the past. There's also some transient data where the head observations only represent changes--represent having some pumping effects in their observations and those were hard to use in the past because we were trying to represent steady state conditions. Then, there's wells that have both transient and steady state information and those are shown in red.

So, there's quite a few points that just have transient information, and by going to a transient model, we were able to incorporate that information and it's helped constrain the model quite a bit which if you look at the distribution of these wells, it's kind of telling of where drilling is. It's just the Pahrump Valley, Amargosa Desert, and then you have a lot of data out on the Nevada Test Site area, Yucca Mountain in particular right in here.

Here's some of the transient information and the distribution of wells in the model. It's the same satellite imagery kind of zoomed in. The pumping was combined into one well per cell. The highest pumping rates down in Pahrump and the Amargosa Desert, there is some pumping represented on the test site. If you look at the change in pumping over time, early-on wells were pretty deep in Pahrump Valley pumped from basically Layer 4. As you go on with time with more and more pumping, it is increased. There were some changes in the later time periods where we just had some spikes and some decreases and changes.

One of the ways we looked at the calibration over time is to look at the head observations and the residuals to see how well the model was matching. Green dots represent pretty good matches, and as you get more towards the warmer or redder colors, the observations don't match the calibration as well. We also looked at the changes in the water levels with depth. In blue here is shown the contours from the upper layer of the model and red is the contours of, I think, Layer 16 in the model. So, we used the GIS and going back and forth in and out of the model helped calibrate it. It made a great deal of difference. We also looked at a lot of the drain data. In brown is where it is simulated, too low discharge and yellow is too high. So, we have a mixture of both of those. So, we're matching both the evapotranspiration data and the spring flow rates.

We're also looking at hydrochemical data more as kind of a qualitative basis instead of looking at actually doing hydrochemical modeling. There's some chemistry data that indicates flow needs to be coming from this direction or that direction and Gary Patterson talks some about that. The model tends to do okay with some of that. We've looked at some of the stuff with Ash Meadows and where flow paths are coming from Ash Meadows.

If you start to compare this model to the 2002 steady state model that was published, there's a much better match to the flows which are the evapotranspiration and spring flow. There's many more head observations which I pointed out and the bias and the match is a lot less. We have some that are high and some are low and it's not skewed as much as the previous model. The boundary conditions have been a huge change in this model and we've quantified the amount of flux coming in and out of the boundary based on water budget studies. Jim Herrill and Doug Bedinger looked a lot at that information and how much flow needed to come in from like Saline in Eureka Valley and Panament Valley (phonetic) to support water budgets in those adjacent basins. One of the big contentions in that study is looking at the Sheep Range and flow going in and out of there. Probably the biggest difference and the most painful during calibration was converting the upper model layer to a convertible layer and converting it to a transient model.

So, it's a regional model. It's good for answering regional type questions. It's not meant to look at super site-specific type questions and that's what site models are more geared toward doing. People are going to talk more about site models. Some examples of things that it's planned to be used for and is being used for are boundary conditions for site-scale type models, both on the test site and at Yucca Mountain. Looking at pumping scenarios by managers with decrease in spring discharge based on pumping in the region. Looking at changes in water levels based on pumping over time. There's been some talk about using it for climate change and having it transient and going to land surface will help a lot in trying to do that as a possibility. But, we need the more site-specific models to address the more detailed concerns.

I was asked to try and talk a little bit about how the fluxes into the site-scale model at Yucca Mountain have changed over time. This is kind of the three models I'm most familiar with. So, I've put the flow rates in and out of the sides of the site-scale model on here. This black box represents the sides of this Yucca Mountain site-scale model. In blue is the first three layer regional model that Frank D'Agnese and others put together about seven or eight years ago. In green is the 2002 steady state model. And, in red is a model run from a couple of weeks ago from the transient model. The arrows are scaled by the values that are in the table for the flow rates coming in and out of the side of the model. It's pretty similar on the north. It hasn't changed a lot. It's probably the area we know the least about. We don't really know how much flux is coming down from the north. There's a pretty good change on the west and south side representing how much flux is coming in and out of the western boundary along the Amargosa River channel. In the 2002 model, the flux for that entire west side was out. And, in this model, there's more flux coming in representing that flow down the Amargosa River area.

One of the changes that affects the south and this east is the structures down here in the Specter Range and all the thrust faulting and broken and shattered carbonate rock and how much is coming through that. Depending on how you calibrate the regional model is where that flux actually comes through. I think we had a lot more flux than we needed to coming through that seven years ago and I think we've scaled it back. The amount coming out itself now is in between the two models.

One of the things about this model is it's supposed to be more than just a model. The Deliverable is a report on the model, but it needs to be a model that others can use and put together. Part of the way we built this model was to have a geographic information system in a system internally kind of called GeoPro that stores the model and makes it more useful to other people. All the data is stored in geographic information system format, basically ArcInfo and World Coordinates. We have some analysis and visualization tools that should make it easier for people to go use the data in and out of the model.

Part of the task for this year when we transfer the model over to DOE is to have the supporting databases and this geographic information data as part of the package. So, it's relying heavily on GIS and access to this information. There's some custom tools that are GIS based and there's also some 3-D model data like Earth Vision and Strata Model that will not be perfectly usable by everybody in the world, but if you have those tools, at least, you can use the files. So, a lot of the data is loaded in kind of commercial software, especially the framework data, which it was built in Strata Model, but we're funding this year to convert it into Earth Vision which make it easier for the community-at-large.

What we're looking at doing right now in some of the out-year funding and in some of the science and technology funding is local grid refinement to facilitate coordinating between the more site-scale models and the CAU unit models. Mary Hill has been doing a lot of research in this area about more integration of the head and flow data between the models so there is not as much of a hard just taking flux data back and forth.

We're also looking at new methods to rank the importance of new observations and monitoring both for geologic and head observations and seeing if--like if you drill a well here and get this water level, do you need to recalibrate the model? Does it have enough information to it that we need to recalibrate the model or is it just kind of supporting what we have now? Methods to kind of look at the framework model and the importance of the geologic information and how much is that actually going to change the flow model as opposed to putting all that information in and having to recalibrate it just to find out.

So, that's kind of the directions we're going and kind of a summary of where the model is now.

NELSON: Thank you very much.

Let me just ask you straight out. Nelson, Board.

In the model, will you include the characterization of uncertainty, at all, linked to your grid system? Will there be some evaluation of that or is it just going to be the overall model that is made available?

FAUNT: It's built with MODFLOW-2000 and with parameter estimation and some like composite scale sensitivity information built into it that way. The way it's set up now, there's not like an uncertainty analysis in the middle of the report. There's some of that information that's built into the statistics that MODFLOW-2000 generates. We talked about maybe next year doing some more details on actual uncertainties.

NELSON: Thank you. Nelson, Board. Just one other thing. Do you include an upward flux out of the paleozoic limestones in this model in some areas?

FAUNT: The base of the model is like 4,000 meters below sea level and includes most of the carbonate aquifer system and down to a depth where we don't think there's going to be upward flux. But, if you look at like the flux in the carbonates to the volcanics in some areas there's an upward flux and then you get up to the north of the test site and there's a downward flux. At Yucca Mountain right now, we simulate an upward flux from the carbonates into the volcanics.

NELSON: Thanks.

Okay. Thure?

CERLING: Cerling, Board. If you could go to Slide 19, I was just wondering on some of your changes in directions of arrows and the size of the magnitudes of the arrows, what information was the most critical in causing both directions in values to change?

FAUNT: A lot of new data gathered by Nye County along that southern end of Yucca Mountain, I think helped define the extent of the volcanics and that's a big change between--the blue, green arrows both were pre-Nye County data basically and the red was just a transient model that incorporates that data. I think that made a big difference on the extent of the volcanics and where we had them in the model and the new interpretations for the structures down in the carbonate rocks and how we had the carbonate rocks. And then, also, I think matching the--they had some problems matching the discharge or we had some problems matching the discharge in the 2002 model. I think with the better matches in the discharge in the Amargosa area, that's what caused this reversal in the 2002 model and actually the '97 model matched some of these discharge areas better than the 2002 model. I think that's why you've got the correct flow in this valley in here.

CERLING: Well, and I guess then as a followon to firm up your final numbers, what areas do you think you're missing key information?

FAUNT: Let's see, some new information helps, some new information doesn't do a lot of good, and a lot of times you don't know until you've drilled a well whether it's going to help you or not. It's kind of a hard question to answer. We don't know a lot with depth. If you start to look at these wells, they're mostly shallow. They mostly go like 100 meters below the water table and that's it and then we're trying to represent a deep system here which we're guessing based on geophysics and a geologist's interpretation what the distribution of the carbonate rock aquifer looks like below the land surface and how deep these basins are. The depth of basins are based on gravity data. Where we have wells that go through the entire sedimentary package down to basement, sometimes they match and sometimes they don't. What they've done is adjusted the gravity to make a better model to make it match better. And so, the more points we have that go deeper and help constrain some of the basement information will constrain this model better mainly because the main feature represented in this is the carbonate rock aquifer with the volcanics in the alluvium as a smaller system that this carbonate aquifer kind of constrains to a certain extent.

NELSON: Okay. We have plenty of time. So, I want everyone to think up questions. Claudia talked too fast.

So, next, Dan Bullen?

BULLEN: Bullen, Board. Actually, as a non-hydrogeologist, first, my compliments on explaining a very complex model in simple enough terms for me to understand. But then, that also raises the issue I can ask questions about stuff that I don't quite understand.

In fact, I'm glad we're at Figure 19 because I kind of want to get an understanding of the respect for the magnitude of the arrows and maybe an understanding of is there a conservation of mass within the system, specifically with respect to water? The reason I'm asking that is because if you did incorporate a climate change and you had more infiltration or more flux coming in from each of the surfaces and you wanted to represent the groundwater table rise below Yucca Mountain, is this the type of capability or the type of inputs that you would need to do that? And then, the followon question to that is that you measured from the north. There seemed to be maybe a dearth of data and would that be an area where you'd like to gain more information with respect to this conservation of mass?

FAUNT: Okay. Let's see if I can remember all of these. The conservation of mass, the MODFLOW does a conservation of mass and the regional model has a conservation of mass and it has an error balance, I think, of .2 percent right now.

BULLEN: Okay.

FAUNT: In terms of climate change, you'd have to get new recharge and new discharge information and a sense of like paleo lakes and paleo discharge deposits and some kind of "estimate" of how much water was coming out those different parts of the system. You can do some of that based on paleo information. I think Jim Paces will talk about it a little bit or did talk about it a little bit. He talked yesterday, right? Some of those numbers are hard to quantify. There are like Lake Manley (phonetic), there's lake level stands in Death Valley, and you can use some of that.

Grady O'Brien and Frank D'Agnese and I put together a model in 1999 that was a very simple model trying to represent climate change just by increasing recharge and trying to match lake levels and paleo discharge deposits. That model had some problems because it was a steady state model and we ended up with lakes on tops of mountains and things like that. The convertible layers on this would help solve some of that problem. The more data you have, sometimes the better it is to build a model; sometimes, it's harder.

I think the way this is set up, it yields itself to climate change better because it's already set up for transient and also because it has the convertible layers on top. It's just the data issue and it would take time to put together all those datasets and where does recharge change and by how much and is it going to be focused more in the streams or do we need to incorporate a river package now? The Amargosa which is ephemeral now, is that actually going to be a flowing river at all times and be a perennial stream? The same thing with Salt Creek in northern Death Valley. So, there's a lot of things to look at and it's not going to be like a five minute turn-the-crank and it would be representing climate change. There's a lot of details.

BULLEN: Bullen, Board. Actually, I appreciate that and I also appreciate the candor of your answer because I think it is difficult to do the kinds of calculations that are necessary.

You did make one comment with respect to infiltration and recharge. And, in this model, did the recharge essentially equal the infiltration or were there other losses in the infiltration that didn't necessarily make it all the way down to recharge?

FAUNT: We didn't--what we took was the infiltration model and then we made these multipliers that were either like--they ranged from like .84 to 1.18, I think, in the current version right now. That may not be the final figure in the final model, the fraction of that infiltration, more or less, that went into the model. In some areas, we had to increase the infiltration rate that the infiltration model--

which increased the recharge which we're using the infiltration model. We didn't do anything to redistribute it. At one point, I did some averaging and spatially distributing it out and it actually matches better if we keep the recharge rate where the infiltration model is.

One thing we have done in a couple places where we have some really tight rocks near the land surface in the framework and we're getting ponding of recharge, we basically zeroed out the infiltration rate or made it very small in those areas and increased the rate around the edges of those cells. So, that helped redistribute it a little bit. So, it was a little bit of redistributing only where we had really tight rocks and we were getting this mounding effect.

BULLEN: Thank you.

NELSON: Parizek?

PARIZEK: Parizek, Board. On this particular figure, would you go so far as to say that you could do more work on just the fluxes? As one of the things, if you had to have new data, obviously not one drill hole, but if you were trying to confirm the red arrow to the south or any of the arrows to kind of build confidence, would that be a worthwhile cause?

FAUNT: Sure, I think you'd also spend more time looking at hydrochemical data and seeing like if the hydrochemical data--I know Gary's talked about the compartmentalization nature of the flow system and different chemical signatures in different areas. If you start to look at that to make sure where the fluxes are going are matching some of that hydrochemical data, that might add to some more certainty in this. This is a composite of the entire side of the model. You could start looking at the variations with depth and maybe like the variations of the flux coming in through the carbonate versus the volcanics in the alluvium in the system. I haven't done that, at all. I, quick and dirty, did this one afternoon after it was requested that I kind of put this together summarizing it.

I mean, yeah, there's a lot of work you could do with that and I think you probably could get a better understanding of the system and maybe a better understanding of some of the limitations to how things are represented because you have to remember it's one and a half kilometer spacing so it's--I think, it's 20 by 30 cells.

PARIZEK: Right. Parizek, Board. And then, also, what happens in the Funeral Mountains, the next speaker will give us more insight about that and that may help constrain it further. So, these are the kind of experiments that can give you additional value.

FAUNT: Yeah.

PARIZEK: Figure 16 shows the pumping distribution. Now, if you look at the Pahrump area, there's an awful lot of water coming out of the system. Do you put any of that water back in or is it all consumed by your model? You assume you consume it. Where is--is a lot of that sewage affluent going back into septic tanks?

FAUNT: We didn't look a lot at like return flow. The pumping rates that Randy put together assumed a little bit of return flow in them so that the amount of pumping was decreased by a fraction to kind of quasi include return flow, but we didn't do a lot in detail with it. Partly timing and partly what we were able to simulate with the system, the data isn't detailed enough to--

PARIZEK: But, it is partially captured by--

FAUNT: It's partially captured.

PARIZEK: What about springs? Like the Ash Meadows area and elsewhere, again there's a high evapotranspiration loss, but I'm sure some of that water probably reenters the groundwater system. Have you been able to put any limits on that or estimations or does the model consider that?

FAUNT: Randy Laczniak would be a much better person to answer this, but what he did when he calculated the evapotranspiration in the spring flow rate, he looked at the fact that you've got these springs flowing out and a lot of evaporation is actually from spring flow. And, I can't remember if he decreased the spring flow rate or decreased the evapotranspiration rate to take that into consideration, but it was thought about in the process of determining those discharge areas. The way those drains are set up is most of them are in Layer 1 representing evapotranspiration, but where there was significant flux from a spring and it was warm temperature so it was thought to represent the regional system, the actual drain location was put at the top of the carbonate system to represent flux out of the carbonate aquifer.

PARIZEK: Parizek, Board. How faults may have been handled--say, principal faults or something that you might really think have hydrologic significance more so than others, can you give us some idea how that was done?

FAUNT: Uh-huh. Some of the faults act as conduits and some act as barriers and some act as both. The way they were explicitly put into the flow system and into the model was as barriers using the hydrological flow barrier package, HFB package. This model didn't need as many of those barriers as other models because I think the juxtaposition of the units was represented more accurately so you could have the low K rocks juxtaposed against the higher K rocks and that juxtaposition causes a lot of the discharge. A few of the barriers were needed and those seem to represent faults that have like a core of impermeable material from like the basically Las Vegas Valley sheer zone is a good example of that where you have like probably some low-permeability material fault gouge that's actually blocking the flow in the actual structure.

The carbon aquifer--actually, almost all the aquifers were zoned based on their structural province they were located in and how fractured or shattered they are and like whether they're coarse-grained or fine-grained. So, they're kind of highly faulted, highly shattered. Carbonate rocks tend to be very permeable rocks and those were represented as zones in the carbonate aquifer. So, in a way, structures are represented that way by like kind of a shattered zone in the carbonate. So, those would be kind of more conduits. Those were very important in the carbonate system especially--and in some of the volcanics--to kind of be kind of conduits to flow. The location of the Eolian and the clastic confining units kind of controls the flow system and those barriers and where those are in the framework model so structures kind of that way are important.

So, they're kind of represented partially by the framework model which has the juxtaposition of the units, partially by flow barriers which actually act as a linear barrier between model cells and represent a fault, and then partially by the zonation of the different aquifers and confining units in the model.

PARIZEK: Parizek, Board. So, when you say some faults have both roles, it might depend upon the depth of that particular fault where it's serving one role in another part along the line or another depth position would have another role, but not both roles at the same location. Do you have any field evidence where you could have damming effects, say, on a foot wall and maybe shattering on the hanging wall and, as a result, have both a drain and a damming effect on the same fault in the same horizon or same hydro structure unit?

FAUNT: There's some data like lower carbonate aquifer when it's in the upper thrust plate, it's been shattered more and it would have a higher permeability. And, that was put in as like a zone. And then, that zone happened to abut against a flow barrier which it probably does down in the Las Vegas Valley sheer zone. You kind of have that situation, but it's not represented explicitly like that.

PARIZEK: Parizek, Board. One more question about now having gone to the transient model, you've obviously had to do a lot of things to calibrate it using transient data in different parts of the model domain. If you then go back to a steady state model, say, back for program use, that's a better model as a result of having gone through this transient model?

FAUNT: I think so.

PARIZEK: Can you give some sense of improvement, how much better improved it is?

FAUNT: There are a lot more different ways you could represent the alluvium and basin fill deposits, basically the fine-grained and coarse-grained gravels and sands and clays. With just the steady state data, you could get very different hydraulic conductivity values in those units, in particular. And then, when we put the transient data and you put in the pumping, that really constrains and limits those values and it made a much better separation between the conductivity values between those aquifers and confining units. So, I think, in that way, it helps improve the data even in the steady state model. I think that's partially what helped improve matching the spring flows and the drains because most of those are located in the valley field deposits, as well, as well as along structures.

PARIZEK: Thank you very much.

NELSON: Frank?

SCHWARTZ: Yes, hi, Schwartz. Claudia, one of the things I wanted to ask you was the balance between recharge and discharge in the steady state model. Obviously, they probably balance. Recharge seems to be something that you define fairly rigorously and so probably, although you tuned it a little bit, it sounds like the numbers you started with were sort of fixed. Discharge, I understand, you determined sort of independently. I mean, you have a model estimate, but you also have a sort of a field estimate of discharge. The amount of discharge you actually get out of the model, when you first came up with your independent discharge estimates, field estimates, I mean, how close were they? Did they match or was there a discrepancy and you kind of said, well, we've got to find some more discharge here and went back out there in the field and looked? I mean, how well did the discharge actually match your best estimate of recharge?

FAUNT: It matched pretty well. A lot of care was taken to make sure that the discharge rates weren't affected by pumping that we were trying to establish. We actually went back and took old reports where there was photos and distribution of freataphytes (phonetic) in the past and used those to distribute the amount of evapotranspiration areas in Pahrump, in specific. We did have problems matching discharge in Death Valley and Pahrump. Actually, during the last year and a half, the amount of discharge estimated from Death Valley doubled from what their initial estimate was. And, the amount in Pahrump changed by a factor of a third and I can't remember if it went up or down. Most of the pumping --and I thought the pumping would be a relatively small feature in this system and not a large amount of volume, but the amount of pumping actually ends up being about a little over a third of the amount of discharge coming out of the system. It's a lot of water coming out of this system from pumping. Most of that is coming out of storage, but there's a lot of water that's been taken out of this system by pumping.

SCHWARTZ: Because where I guess I was going with the question was looking at the whole problem of uniqueness of a model of this kind. And, clearly, if recharge and discharge are not too constrained, I mean, you can make them anything and just adjust Ks and, you know, the model will give you the same head distributions. So, do you feel confident that that's your major proof that--or tendency toward uniqueness is this balance between recharge and discharge or are there other things, as well, that you think that would let you sort of believe that this is the unique model?

FAUNT: I think, the fact that the boundary conditions change so significantly, they didn't affect the internal part of the system very much. It helps constrain that it's somewhat of a separate area in the internal parts. A lot of work went into those evapotranspiration and spring flow studies and putting together that information. In general, models don't tend to have that much discharge information to calibrate to. And, the fact that like, especially in Pahrump, we have changes in discharge over time where the springs actually dry up and then start flowing again and we tend to match that with the pumping data, I think that helps constrain it a lot. I think the fact that we have head observations in multiple layers where you have gradients upwards and downwards, I think it makes it a lot more unique, especially adding the transient data and the discharge changing over time. Pahrump is the only place we actually have data where the discharge from the ET areas is changing over time, where we have an estimate of what we thought it was before development and we have a development estimate and then we have where they decreased the pumping and springs started flowing again and trying to simulate that change. We don't match the actual magnitudes exactly, but we match the kind of general trends. And, I think that's encouraging.

SCHWARTZ: Good, thank you.

NELSON: And, van Genuchten?

VAN GENUCHTEN: Yeah, van Genuchten. I was intrigued by your Slide 20. You know, I don't know if we can get that, but I'm sure in this impressive study, you guys put a lot of time and effort and sweat and tears in this and you want to get some credit out of this also. So, I can understand you want to protect all the stuff you developed. But, at the same time, you hint here, you call it knowledge exchange, that some of the data may be available in commercial software. Personally, I think it's great because we have this available in the wider scientific community and all the people kind of scrutinize and use or may misuse whatever you developed. Could you comment on that? Is there a certain company or how do you do this and is it expensive? It is going to break the bank for us to get this?

FAUNT: We worked hard at trying to find ways to transfer the data in less expensive systems. Most of the data is stored in ArcInfo types of grids. All the model information that goes in and out of the model actually is stored in either ASCII tables or ASCII arrays for MODFLOW input. And, also, those representations are stored in ArcInfo grids, arrays, Vectra coverages or Point coverages, and also like Access database tables. So, Microsoft Access is relatively cheap. Probably, we'll release the GIS data, shape files, as well as grids. A lot of software can read that. So, depending on what software you have, you can look at that.

The kind of sticking point with releasing of the data is kind of the 3-D geology data and how you represent that and how you give that to somebody. It can have arrays with thicknesses and tops of units and it can be used or misused or represented correctly or not. Even going from Strata Model to Earth Vision which are two 3-D geological modeling packages, they're very high end, they're very expensive. They're built originally by--one is (inaudible) Industry and one is kind of environmental. They are tens of thousands of dollars. They even don't communicate exactly the same and you can't just take the arrays and plot them out of Strata Model and plot them into Earth Vision and have the same looking model. We have the arrays represented in

MODFLOW and putting those into Rock Ware which is a relatively inexpensive visualization package. And so, we're releasing the framework model that way. The graphics aren't as pretty as Earth Vision, but the data is there and you can look at it to a certain extent. So, that's the one that probably has the biggest sticking point of how accurately you accurately want to represent the 3-D geology data.

Actually, the geological arrays are in ArcInfo. You can use ArcScene which is kind of a pseudo 3-D thing and look at the geology in it. And, I actually spent a lot of time re-representing the geology in ArcInfo so other people could look at it easier and you can put basically the equivalent to a well in each cell and then you can see the geology in each cell by clicking on it and stretching it and making the unit stretch to the thickness. So, there's tricks and ways of getting it out and looking at it in relatively inexpensive software. A lot of it is hinged around ArcInfo and ArcInfo isn't cheap, but it seems to be a pretty big standard and a lot of people have access to it.

NELSON: One last thing. Nelson, Board. Well, just following up, I think you may find in the future open source capabilities here that happen very fast, I think, within the next year. So, it would be really great to try to get this available through open source. To what extent did you use any information, thermal information, in this model?

FAUNT: We haven't used temperature data hardly, at all. I mean, there was talk originally about trying to do it and we haven't done it. There was a lot more of starting to look at it with the site model and I think someone is going to talk about the site model later. I'm not sure how much--I haven't been involved with that enough to know how much it was incorporated. I mean, qualitatively, we looked at it in terms of the spring discharge and figuring out which were regional springs, but we're not representing it as part of the flow system.

NELSON: Nelson, Board. That would be one area that there is information that's not yet been captured.

FAUNT: That's true.

NELSON: Okay. And, just finally, does your model tell you anything about the style of faults' behavior hydrologically in the tertiary volcanics? Are they typically permeable, impermeable, or is there anything typical about them?

FAUNT: Nothing is typical in those volcanics. The welded rocks where they're shattered tended to behave more as aquifers. We did a pretty detailed kind of like gridding of like where things were altered or non-altered by zeolitization, where rocks were welded versus nonwelded, and made categories and zonations based on that. In general, the welded, shattered areas tended to be more aquifers and higher permeability and the altered, nonwelded rocks tended to be more of the confining units and lower permeability. And, that actually was more true that those properties controlled the flow more so than the unit. Like the Calico Hills formation is one formation, but it was definitely an aquifer in some areas and a confining unit in other areas. That was based more on the properties. I know Dave Bush is going to be on the field trip and probably talk some about that and I've talked with him in the past about this may not be the unit so much, but the properties of the units that are the actual factors that control the permeability.

NELSON: Thank you very much.

FAUNT: Sure.

NELSON: Thank you, Claudia.

We're on schedule, at least, by my clock. So, our next speaker is Dr. John Bredehoeft and he is an extremely well-known researcher and scientist. He accumulated 32 years of service in the USGS where he held both research and management positions and his expertise is in water resources, especially regarding groundwater. He's testified before Congress on issues from national policy to the use of numerical models and management decisions. He's served on many National Academy and National Research Council committees and panels. He, himself, is a member of the National Academy of Engineers and he has received numerous prestigious awards. In 1995, having retired from USGS, Dr. Bredehoeft established the Hydrodynamics Group. One of the projects of this group with Inyo County is what he will be talking about today.

Welcome, Dr. Bredehoeft.

BREDEHOEFT: Thank you. Thank you very much.

As you said, my partner and I have been engaged for Inyo County as their oversight consultant almost for eight or nine years at this point in time. And, I want to talk about Inyo County's concerns, but before I do that, I want to make a few philosophical remarks about the whole issue of modeling and what we're doing.

If I have some claim to fame, part of it is due to the fact that George Pinder and I developed the first widely used flow models and the first widely used contaminant transport models for the saturated zone. And, we did that in approximately 1970. So, we've been engaged in the modeling business--I've been engaged in it for more than 30 years. So, it's been one of my principal activities. And, I've been concerned about the whole idea of how we model and the idea of the philosophy of modeling and I want to spend a few minutes just talking about that. As I do that, I want to disassociate that from Inyo County. These are my ideas. They're not in any way associated with the--I mean, these are really my ideas; Inyo County didn't pay for these activities and so, as I say, they're my ideas.

Okay. Next slide, please? All right. So, about a year ago, I published a paper in Ground Water which you can see the title there, "From Models to Performance Assessment; the Conceptualization Problem", and that's what I focused on, the conceptualization problem. Now, you know, the whole basis for any modeling we do is the conceptual model. That conceptual model is an a priori decision by the analyst. Now, the analyst decides what the conceptual model is going to look like. And, certainly, we say we have some ideas in science on what the prevailing conceptual models are, and among those, you select what the conceptual model for the particular problem is.

Now, as a result of that, certain things, it seems to me, happen and the next slide is the result of the consequence of selecting the conceptual model by the analyst. Now, these are the points that I made in that earlier paper and I'll just read them off. One of the things is that usually you select a conceptual model and you stick with it. So, once you've selected that conceptual model, that's generally the conceptual model unless something else happens and I'll talk about that in a minute. But, usually, the idea is that I've got a conceptual model and I'm going to stick with it. Now, when you start to look at how well did we do predicting with these models--and I'll talk a little more about that in a minute--you find out that in many cases the errors associated with the predictions are associated with, in fact, the selection of the conceptual model itself. So, the errors result from the conceptual model.

The next thing is that, more often than not, you can take your dataset and fit it to your conceptual model. Whether the conceptual model is right or not, you don't know, but more often than not, you find that you can take the data that you have and fit it to the conceptual model and now we have a lot of automated procedures to do that--tests, MODFLOW-P, various things--which will reduce the error between the observations and the model predictions and you get what you think is a reasonably calibrated model. That doesn't mean just because I calibrated it that I got the right conceptual model. All it means is I got a good calibration.

All right. Now, the last thing is that these other things that we do in PA which is probabilistic sampling of the parameter set does not assure that you have tested the appropriate conceptual model. All you're really doing is looking at the possibility that there are errors in the parameter set itself. So, it doesn't mean that you have, in fact, tested the conceptual model itself.

All right. Next slide, please? Now, as I said I published those ideas about over a year ago in Ground Water and I got into a discussion with Shlomo Neuman. Shlomo didn't disagree with my conclusions, but the question is what do we do about it? How can we solve this conceptual problem? So, Shlomo's idea is that what the analysts should do is, in fact, set up a set of conceptual models, then look at the data, and try to select among that set of conceptual models what is the appropriate conceptual models or what is the appropriate set of conceptual models for the particular problem? Now, if you follow along with Shlomo's argument, it means that the analyst then has to set up this set of conceptual models and the question that I asked is, you know, how good are we at selecting this set of conceptual models, either the individual conceptual model or a set of conceptual models? So, I tried to look at the data that we have and the data is extremely sparse at how well we do at setting up the conceptual model.

Now, this led me to another sort of philosophical discussion and that is the whole idea of surprise. And, the idea is relatively simple. You have a conceptual model. All of a sudden, you collect some more data and that data says the conceptual model that we have is invalid. So, then you've got to go back and readjust your conceptual model and create a new conceptual model. And, we all know that that happens. I mean, you start with some idea. Alan Flint talked about Yucca Mountain yesterday and you can see that this happened along the way at Yucca Mountain, but it happens all the time, I think.

Now, probably looking at sort of what are these big surprises, you know, the one in geology in the last century, in the 20th century, was plate tectonics. You know, Alfred Wegner came along in 1912 and said the continents are moving and the geologists said, no, no, no, that can't be right. The continents can't move. And then, we came along in 1960 and we measured remnant magnetism on the ocean floor and we found these stripes on the ocean floor. All of a sudden, we realized that there was sea floor spreading at the oceanic ridges, and as a consequence of that, the plates have got to move. So, you know, really what happened was you had a conceptual model--the conceptual model in this case was that the continents can't move--all of a sudden, you find this new data, and it says, hey, wait a minute, the conceptual model is all wrong.

Now, as I said, we all are aware of those kinds of things which I'm calling a surprise in this case. And, you know, we think about these things and we say to ourselves, well, they happened--and big problems. You know, they're big problems. We have big scientific problems, and all of a sudden, people come along and get some new data and it throws out the original hypothesis. The question is how often does that happen sort of in the routine investigations that we're engaged in? Does it happen and how frequently does it happen?

Now, my two examples sort of in the nuclear waste business is, first of all, at WIPP, the idea--let's back up. Why did we go to salt as sort of the primary storage media? One of the reasons, at least, was there was a National Academy committee in the 1950s in which there were very prominent hydrologists. C.V. Theis and King Hubbert were both on the panel. And, they said, you know, salt, that's the media of choice and it's going to be dry. So, we'll put the radioactive waste in salt. And, that was the conceptual model when we started on the WIPP site. You know, the conception was that the salt at WIPP was dry. And, it wasn't totally dry. It was known that there were vesicles in the salt and those vesicles contained brine and there was about a half of one percent brine in the vesicles. Everybody admitted that. However, we went underground and, all of a sudden, we find that the mine is wet. Not wet, but it's, at least, damp and, you know, you saw indications of this when you went underground. The first real data we had was they ran a heater experiment. Where they turned the heaters on, they circulated dry nitrogen in the holes and, of course, the nitrogen came out wet. So, it was immediately realized--or, not immediately, it took some period of time, that this conceptual model of dry salt was, in fact, not right; that there was one to three percent brine in the interstices and that brine would move to the repository and the repository would be damp, at least; so, a new conceptual model.

And, I think, you see the same thing at Yucca Mountain. You go underground and you find chlorine-36 and then, all of a sudden, you have to say to yourself, well, there are fast paths and the fast paths suggest that our conceptual model of what's going on in the unsaturated zone is, in fact--we've got to throw it out--it's invalid. We've got to create a new conceptual model of what goes on. So, these things happen.

Next slide, please? So, I said to myself, all right, how frequently has this happened in my own experience? So, I've been consulting for roughly nine years. I've been involved with 21 model studies, more or less. And, out of these model studies, I find that four to six times, something of that order, we had to change the conceptual model. We started with a given conceptual model and that conceptual model had to be radically changed. And, it's not just that we have a new parameter set, we've got to change the conceptual model itself, not just change the parameters.

That led me to say to myself, well, that's my experience. What is the experience associated with post-audits? You know, we have been modeling now for 30 years or so and the question is how well did the models do? So, there have been, as you know, a series of post-audits to look at what are the model predictions and how good were those model predictions? And, the numbers of these are not very large, seven or eight, something like that. It turns out the model predictions weren't very good.

And, I went back to look at that information to say, okay, not just where the parameters were changed, but where was the conceptual model itself bad? Where did we have to really look and say to ourselves that the errors in the predictions were associated with the conceptual model? And, it turned out that--you know, it turns out that 20 to 30 percent of the time, the conceptual model itself was bad. So, my total dataset then turns out to be 29 studies including my own. And, out of those, the conceptual model was changed seven times significantly and then there were another two or three that were questionable.

So, what it's suggesting is that we have trouble selecting an appropriate conceptual model, at least, the first time around and that many times the conceptual model--oh, many times--20 to 30 percent of the time, the conceptual model we select is not the conceptual model we end up with. Now, of course, we've got this other three-quarters of the studies where we accepted the conceptual model and we went ahead with it. We don't know how many of those are wrong.

So, my point is that selecting the appropriate conceptual model is not so easy and we make mistakes rather commonly. And, those mistakes, I think, are often--I mean, I go back again to my initial slide saying that, you know, even with a bad conceptual model, you can calibrate this thing. That doesn't mean that it's--just because you calibrate it doesn't mean you've got the right conceptual model. So, this is a difficult problem and I think it's a very sticky problem in modeling.

Next slide, please? So, I think I've made those points that, you know, my experience suggests that 25 to 30 percent of the time we have problems. All right. So, it seems to me that this leads to uncertainty and this kind of uncertainty is not covered in the sort of performance assessment that we normally do. We look at the problems associated with the parameters, but we don't look at the problems associated with the conceptual model itself. All right. So, so much for philosophy.

Now, next slide, please? Some more comments about the idea of surprise, and even though Shlomo was saying--you know, even he admits that it's not uncommon to find new data that says the conceptual model is wrong.

So, anyway, the next slide, please? All right. Now, getting on to Inyo County and what are the concerns of Inyo County? As all of you know, the lower carbonate aquifer is thought to discharge in Death Valley, into the big springs in Death Valley. You know, if we get contaminants to the lower carbonate aquifer, that's where the stuff is going, so Inyo County is concerned about the discharge from the lower carbonate aquifer. That's how it gets into Yucca Mountain really. So, what we have been trying to do then is to look at the discharge area in more detail in California, basically. And, basically, since most of it happens in Death Valley, we're looking at the springs in Death Valley and what happens to those springs.

All right. The next slide, please? Now, you're all familiar with this. There's the test site. This facility is the closest and we end up with these major springs in Death Valley.

Okay, next slide, please? Now, we have been supporting some work at the USGS to map the carbonate aquifer in the Furnace Creek Mountains. And, Chris Freidrich of the USGS has been doing that. And, so this is Chris' geologic map of the Funeral Mountains. And, you can see the carbonate aquifer exists. The carbonate blocks are these pinkish blocks which exist right in here. We don't see them too well from here. Okay. And, there is--these blocks are all faulted, and right in the center here, the carbonate aquifer is actually faulted out. So, there's a big block of carbonate sitting in here and then another block of carbonate which exists in this area of the Funeral Mountains. So, Chris has mapped that in fairly detailed--as detailed as he can from the observation. What happens is that since those blocks are faulted, you can only see certain exposures of the fault. So, you've got to project the fault into the subsurface.

So, the next slide, please? So, there are different interpretations of what the bottom of this fault zone would look like. So, as you can see here, these are the carbonate blocks and they're faulted in here. So, what Chris has done is prepared a contour map on the base of the carbonate aquifer and this is that contour map on the base of the carbonate aquifer. This is the Funeral Mountain Fault here along the front of the Funeral Mountains. And, the major springs discharging are the springs in this area here. The three big ones are Nevares, Texas, and Travertine. Then, there are some smaller ones, two smaller ones up here, and another one down here. And, you know, the reason we think those springs are discharging from the carbonate aquifer is based on their geochemistry. You know, it looks like carbonate water chemistry.

All right. So, this is Chris' bottom of the aquifer and right in here is this area that's cut out that's faulted out. So, we have an area in here where there is no carbonate aquifer. This area right here, there's no carbonate aquifer. And, when you look at this map, the elevation of the bottom of the carbonate here is about 1600 feet above sea level, if I'm reading that correctly. I think that's a 1600 foot contour. So, we have some information. We know that the springs here exist in this area. We have a pretty good estimate of their discharge. They discharge approximately 5 second/feet. Our best estimate is that it's 5 second/feet. And, we have some areas over in here where the Amargosa River looks like it's very wet. There's vegetation along the Amargosa River and it appears that there's discharge from the carbonate aquifer to the Amargosa River. The elevations here are about 2200 feet. The Devil's Hole which is back up in here has an elevation that's somewhere between 2100 and 2200 feet. So, it looks like the head in the carbonate aquifer is around 2200 feet in this area on the east side of the Funeral Mountains. Okay. So, this is Chris' bottom of the aquifer with the sort of shallowest fault zone. In other words, you can only see a portion of this fault zone. So, you project it into the subsurface and you picture this plane as relatively low dipping. You get this bottom of the aquifer.

Next one? And, this is another realization where you say to yourself, well, these faults are a little steeper and when you make the faults steeper, of course, you get a different bottom and the bottom here is considerably deeper and we get sea level somewhere right around in here. So, this area that we're concerned about right through here where most of the water has to come through is considerably lower. So, instead of the bottom being around 1600 feet, we're now about 500 to 1,000 feet lower.

So, the next slide? So, what I did then is to say to ourselves, okay, let's see if we can model the flow through the carbonate aquifer and we're going to assume that, you know, it behaves at a continuum. You know, we're not doing anything exotic. We're simply saying to ourselves we've got to use the general flow models and I used MODFLOW actually to model the system. This is a model representation of flow through the carbonate aquifer. It has a constant head boundary up here along the Amargosa River and then we've got these discharges of the springs here and we know what the elevation of those springs is, as well. So, we have some constraints on what the elevation of the springs are and what their discharge is. From that information, we can put together a model of the aquifer and compute a head distribution in the aquifer.

Now, what's interesting about this is that right in here, the model suggests that the elevation of the water table in the carbonate aquifer would be about 1600 feet. So, what it's saying is that that shallow realization that Chris put together is probably not, at all, feasible because basically we're saying that there is no aquifer thickness in here. The model is fairly interesting because you see all of the flow in the carbonate aquifer has to come through this area and then come down here to discharge in the big springs in this area. And, there is some discharge right here, but it looks like there is discontinuity from the carbonate aquifer. This spring is considerably higher. It's somewhere around 2,000 feet. So, it appears to be pretty much disconnected. The flow for that spring has to come sort of this pathway through here.

So, basically, what we've done is put together this model of the aquifer system. It suggests to us that, you know, this shallow realization of the fault system is not really feasible and so it looks like the faults are much steeper than Chris would have predicted with his shallow fault model. And, we can fit this thing pretty well to the discharge. I mean, we can make the model reproduce the discharge in the major springs here, particularly, as I said, Travertine and Texas and Nevares. So, the model does reasonably well.

Now, one of the interesting things is that this fault doesn't seem to have very much effect. So, I played with the idea of, you know, suppose the fault is more permeable, suppose it's less permeable. I didn't get any better results with less or more permeable. So, it appears that the fault is in this case playing no particular role in the flow system. The flow is going basically through the fault zone.

All right. So, this leads then to what are we hoping to do? Well, we're hoping to drill some holes to the carbonate aquifer over in this area here. So, on the east side of the Funeral Mountains, we would like to drill to the carbonate aquifer, get head information, geochemical information, and establish the fact that the head that we get over here is consistent with some kind of flow system that looks like this and that the geochemistry of that water resembles the water that we're seeing discharged at the springs. So, basically, establish--well, get some confirmation for our conceptual model of what the flow through the carbonate aquifer looks like. We have money to drill. We are hung up somewhat logistically by the contracting procedures in Inyo County. This is a kind of new ball game for Inyo County and we are having some logistical problems in getting the county to move, basically. We've also drilled some holes over here in the discharge area and we've got one monitoring well also in the discharge area. But, the more meaningful observations, I think, are those of the carbonate and over in here and we've done geophysics to try to locate spots where we think we--where we're pretty sure we can get the carbonate aquifer, get saturation within reasonable drilling depths. So, that's where we are.

The rest of the slides simply summarize that. You know, and I might make one more comment. You know, because we have head distribution, we have discharge from the springs, we can calculate a transmissivity of the aquifer. Now, you're not quite sure what that transmissivity means because you're not quite sure what the thickness of the aquifer material is that's really transmitting the water. So, out of the full modeling, we can get a transmissivity, but you know, converting that to a permeability depends on how thick you think the aquifer material is that is conducting the water. And, as I said, it's fairly insensitive to the permeability of the Furnace Creek Fault.

I think I'll stop here. There's one or two more slides, but I think I've covered everything.

NELSON: Thank you very much.

Questions from the Board? Dan Bullen?

BULLEN: Bullen, Board. Could we got to Slide 13 first? I guess, the first question that I have based on the initial part of your talk was that this is your estimate of the movement of the groundwater. So, I guess, I have to ask if you think it's correct, and if it's not correct, where might it not be correct?

BREDEHOEFT: Well, let's back up a second. There is every indication from the geochemistry of these springs that the water we're getting from the springs is coming out of the carbonate aquifer. So then, you say to yourself, okay, we've mapped that carbonate and the distribution of the carbonate is pretty well-known. So, you say to yourself, well, there's carbonate water coming out of the springs and the water has got to come through the Funeral Mountains. Then, this is a reasonable picture of what that's got to look like and it's pretty hard to change that thing dramatically.

Let me back up. One of the things I did in here was to put in the uniform permeability for the carbonate--or uniform transmissivity for the carbonates through the entire Funeral Mountains. So, you could come back and play games with making different permeability distributions or different transmissivity distributions, but it seems to me you don't have much data to do that with. So, you know, you come back and say to yourself, well, what's the simplest model? Well, the simplest model is to use a uniform transmissivity. The pictures kind of look something like this, I think.

BULLEN: Bullen, Board. Basically, as a followup, on Figure 11, you show the proposed wells that would go in that region and I guess I just wondered why there weren't wells near where the spillway is? Would that not give you the information that you need or is that too hard to get or--

BREDEHOEFT: It's pretty hard to get to, first of all.

BULLEN: Okay.

BREDEHOEFT: Then, there are logistical problems because you're right in the center of the National Park. National Park doesn't want us drilling. You know, they're not anxious for that, although we have drilled some wells over in the discharge area. So, you come back and you say to yourself, well, where can you get that carbonate, you know, within reasonable drilling distance on the other side of the Funeral Mountains on the northeast side. And, these are the sites. It turns out that when you start looking at the logistics of where the park is, you know, where is it possible to drill, it comes down to a fairly limited set of places.

BULLEN: Bullen, Board. Thank you. Actually, I had one other question. Based on the previous talk and trying to understand sort of the regional nature of the groundwater motion, how does your information feed back into the process or is there a mechanism whereby this information can be utilized by either the GS or the Yucca Mountain Project and what kind of information feedback do you have?

BREDEHOEFT: Well, particularly in the models that we did here, we were looking only at the carbonate and we're looking at the carbonate, you know, sort of this is the water table in the carbonate in the Funeral Mountains. That's basically what you're looking at. How much saturation is there of this carbonate material in the Funeral Mountains, itself. So, you're looking at only one unit and we're looking at it over a fairly limited area. And, we're saying to ourselves that the discharge from that system are these major springs in Death Valley. So, we've got a very--a much smaller picture, a much smaller piece of this sort of regional model that Claudia was talking about.

BULLEN: Thank you.

NELSON: Ron?

LATANISION: Latanision, Board. I enjoyed very much your comments, philosophical comments on modeling. And, certainly, my experience in modeling bonding interactions in solids resonate with the comments you made. But, I'd like to turn to Slide 3 and ask you one question. It has to do with the use of the word "calibration". In my experience--and I'm just curious of your reaction to this, but in my experience in bonding interactions, we often attempt to--after developing a conceptual model to use it to calculate something that is known; for example, an elastic constant.

BREDEHOEFT: Right.

LATANISION: And then, to treat the model in order--I hate to--maybe tweak or force, I'm not sure which is the right language. But, to make the model fit and then to use it to calculate something that is unknown with hopefully some degree of confidence based on what I would describe as not calibration, but verification or validation. Are we using the same language?

BREDEHOEFT: I don't like those words, but that's okay.

LATANISION: Okay. Are we using the same language or is it just semantics here?

BREDEHOEFT: Yes, I think we're using pretty much the same words.

LATANISION: Okay.

BREDEHOEFT: Let me back up. You have a conceptual model. You have some observations. And, you adjust the parameters within your conceptual model to fit the observations.

LATANISION: Right.

BREDEHOEFT: Okay.

LATANISION: Well, we use the model to calculate something that's known. I--

BREDEHOEFT: Okay. But, that's what we do, too. We say to ourselves, okay, we've got a bunch of water levels. Claudia talked about it. We've got a bunch of water levels out here. We're going to use the model to calculate those water levels and see how well we do. We accept the fact that we've got to come back and adjust the parameters to make a better fit to those calculated water levels. Okay?

LATANISION: Right.

BREDEHOEFT: But, what I'm talking about here is in some of these cases you come back and you say to yourself, hey, wait a minute, I've got data here which says that I can't fit this conceptual model. My conceptual model doesn't work. So that, to me, is a very different situation than where you say to yourself, we've got observations, we're going to change the parameter set to fit the observations, but we didn't tinker with the conceptual model.

LATANISION: Yeah, yeah. Well, I wonder about the implications of this short conversation on very complex engineering problems in which we adopt, for example, the TSPA approach to dealing with all the many variables that are involved. I mean, I'm not quite sure how you could do it otherwise. But, I do wonder about the fact that there is such an overwhelming independence on modeling. How good is a calibration when you're dealing with a very, very complex system as opposed to something which I consider on the scale of things to be very simple and I'm talking about bonding interactions in solids? So, what's the implication for TSPA based approaches to large engineering systems? Am I putting you on the spot? I'm sorry.

BREDEHOEFT: No, I've been on this spot before. Let's look at the PA for a moment. What you do in the PA is you say to yourself we're going to accept the conceptual model over here. What we're going to look at is suppose we made errors in our parameterization? So, we will run a range of parameters through and look at what the model predicts with this variation of parameters. But, we have not tested the conceptual model. What was the conceptual model? I mean, I'm saying the conceptual model is wrong 25 percent of the time. You haven't tested that. And, you haven't tested that with the calibration. You can calibrate to a bad conceptual model. So, what you're saying to yourself is--you know, the implication of this is that you didn't test the conceptual model and that conceptual model may easily have errors of some significant amount associated with it.

LATANISION: Latanision, Board. The operative word from my perspective is confidence. I mean, how much confidence can we attach to the calculations that follow the evolution of a model and calibration and so on? Once again, I don't know how to answer that, you know, on the scale of things we're talking about, but it seems to me to be a very, very important issue.

BREDEHOEFT: Let me try to answer it another way. It seems to be one of the things you want to do is when you get through, you want to be sure that what you're doing for society is robust and as robust as possible. I think that's where you've got to look to yourself. You know, you do all the calculations. You say, okay, but then you say to yourself, well, you know, is this system sufficiently robust to accept the fact that we may have made some errors?

LATANISION: All right. Oh, I'll buy that. Thank you.

NELSON: Richard?

PARIZEK: Parizek, Board. John, you could have added to that answer what you actually published and that is in terms of Yucca Mountain application, one way to enhance your comfort level would be to perhaps leave the repository open longer. Would you want to kind of add a little bit to that?

BREDEHOEFT: You can do all these calculations and do all the PA and so forth. I think there's still a reasonable chance that we've made errors. You know, that we didn't get everything right. So, one of the things, it seems to me, is leave the thing open and watch it as long as you can before you close it. I mean, you know, what's the rush to closure? Particularly, when you get into a system like this where you're presumably going to put hot waste in there and you--I mean, I don't know. You ask yourself how much confidence do you have in these calculations when you now start getting things at 130 degrees, 140 degrees C. I don't have much confidence. But, that's just my bias.

PARIZEK: Thank you. That's a published statement so we can track that one down. The other thing--

BREDEHOEFT: I've said 1,000 years, Dick. George Hornberger was arguing with me that the 1,000 years is too long because we don't know what society is going to look like in 1,000 years. But, assuming we had a reasonable society, what is the rush to closure?

PARIZEK: One other question about the National Park Service, as an example, as a constraint to try to understand something that could be quite vital to the Park Service, as well as to kind of a national understanding. So, here's a case where there may be times when perhaps you have to violate pristine lands for the purposes of addressing nationally critical issues. And so, again, you don't have to respond to that, but it seems to me we really as an agent, at times, need to know some answers to some critical things. And, if the spillway is really kind of important to the backup of water in Claudia's model, then the site-scale model and a lot of things cascade from it, I, for one, would think that there's ways to gain access that would be not particularly damaging perhaps. Again, other people have to weigh in on this, but I feel strongly that there are times you've been kept out of certain terrain where maybe you ought to be allowed in that terrain in a very controlled way in order to get this job done.

BREDEHOEFT: Dick, I would come back and say to you that the Park Service has been most supportive. We have all kinds of very good cooperation with the Park Service. And, you know, their concerns are that they don't want some drilling rig sitting out there for two or three months where the public is going to be--you know, it's going to be obnoxious for the public. But, they have been very supportive. So, I think we can work those problems out.

PARIZEK: Thank you. That's a political answer, I think, but thank you.

BREDEHOEFT: One thing I would say, you know, one of the things at the moment is the one hole we have at Yucca Mountain shows this upward gradient from the carbonate aquifer into the overlying tertiary material. Now, that's a protection for the carbonate aquifer. You know, as long as that flow is upwards, you're going to have a hard time getting waste to move into the carbonated aquifer. However, if you think about sort of water supply and then we're going to go to that carbonate aquifer as a big source of water, particularly for Pahrump where we have water problems, they begin to lose that hydraulic head and you're going to lose some protection from the aquifer as you reduce that hydraulic head. And, the other implication of what we talked about is that those springs are also going to be very sensitive to losing hydraulic head in the Amargosa Desert. So, if we have development in the Amargosa Desert, you can see where the springs are going to be impacted. You can see where this upward gradient at Yucca Mountain would also be disturbed. So, to the extent that Nevada develops that water, you can see very big changes with respect particularly to carbonate aquifer.

NELSON: Nelson, Board. Let me ask you one question related maybe to bring the two parts of your talk together that has to do with the idea of designing experiments to test conceptual models so that specific sets of observations get made that actually are addressing the conceptual model uncertainty separate from the calibration issue of an existing model. Do you think that that strategy has been used appropriately on this project? Is it a strategy that should be used?

BREDEHOEFT: Oh, yeah, of course, it should be used. I mean, you know, if you go back to the philosophy of science and you say to yourself all we can do is invalidate which was Pauper's view of science, you know, then you say to yourself, well--set up these experiments to try to test that conceptual model. You know, is it right or not? I'm not sure I want to say whether we did that well in this case or not. I'm not sure.

NELSON: Nelson, Board. My experience has been that this is--with the reductionist framework that we've moved into in many areas of science, we tend to get a calibration or a testing of one model at a time with one set of data so we don't have this possibility and it's a shortcoming across the board.

But, let me ask you about maybe the vulnerability of this water system if there's a climate change or a significant water table change in the region. What are the potential impacts that this conceptual model would predict?

BREDEHOEFT: You know, from the point of view of Inyo

County and Death Valley and so forth, you know, if you get more water in the system, that's helpful. We're going to get bigger spring flows, probably increase the head in the carbonate aquifer, those kinds of things. That would be helpful. And, I think, most of the climate change--I sat there yesterday, I don't know. It seems to me that we're in a more dry period of the climate at the moment. So, probably what you're looking at is winter conditions. And, as far as the carbonate aquifer, that's probably beneficial.

NELSON: Nelson, Board. Does your model indicate any vulnerability for loss of that upward gradient to flow that protects that resource?

BREDEHOEFT: Well, it seems to me if you come to the northeast side of the Funeral Mountains and you say the hydraulic head at that point is controlling the flow through the Funeral Mountains, if you reduce that hydraulic head with development, that's going to be detrimental as far as the springs are concerned.

NELSON: Okay. Any other questions?

(No response.)

NELSON: Staff?

(No response.)

NELSON: No. Well, we thank you very much.

BREDEHOEFT: Uh-huh.

NELSON: We are three minutes ahead of schedule. You do get that three minutes on your break. So, we will reassemble here to the tune of some music at 10:10.

(Whereupon, a brief recess was taken.)

NELSON: Regardless of whether Richard Parizek is back, we're going to start. So, grab your coffee and have a seat.

I want to just make one clarifying statement and where I was coming from in my question regarding conceptual model testing and I think it permeates many, many fields of science and engineering.

There's a major project at the National Science Foundation right now called the Network for Earthquake Engineering Simulation. And, in the field of earthquake engineering, perhaps reinforced by National Science Foundation's grant policy, has been a long string of small awards given to single investigators to investigate their model in their context and gather their data through their experimental setup. With the Network for Earthquake Engineering Simulation, what's going to be set up is a complete collaborative environment where data, visualization, tools, and analytical codes are all available to the entire community where each project that comes forward will be placed on the Web and anybody in the community can propose parts to the experiment, piggyback opportunities on the experiment, that will actually allow many models to be tested with one experiment. It's going to be a culture change and it's probably going to be somewhat painful, but I think that it's where the future of many aspects of engineering is. So, that's where I was coming from in that specific example.

But, it's my pleasure to reconvene this session and to get a little bit more up close and personal towards the site. And, it's my pleasure to introduce Jim Winterle. Jim received his bachelors and masters degrees in hydrology from the University of Arizona at Tucson and he comes to us as Senior Research Scientist with the Geohydrology Group at the Center for Nuclear Waste Regulatory Analyses in San Antonio, Texas. For the past six years, he has been a principal investigator on saturated and unsaturated flow issues related to the proposed high-level waste repository at Yucca Mountain under contract to the U.S. Nuclear Regulatory Commission. His work at the Center allows him to apply his broad variety of hydrology interests which include interpretation of aquifer pumping tests, aquifer responses to earth-tide and barometric effects, contaminant transport in porous and fractured media, groundwater flow monitoring, multi-phase mass and energy transport modeling, recharge estimation, and the interpretation of groundwater temperature patterns which are of particular interest to me.

So, I invite Jim to the podium to make his presentation. Thanks.

WINTERLE: Thank you. Thanks to the Board for inviting me and to the Staff and technical people for putting on such a great set of presentations.

I'm really glad to follow John Bredehoeft's presentation not only because he's such a distinguished scientist, but because his philosophical comments on conceptual model testing lead nicely into what I'm about to present.

I thought before I start that, I'd offer a few philosophical comments that I'm borrowing from a recent article in the latest issue of the Ground Water Journal by Amat Hussan of Desert Research Institute. He argues that the term "model validation", which inspires a lot of ire in some hydrologists, is acceptable, but as long as it's understood that we're referring to confidence building. And, some of the statements borrowed from him is that model validation is a process, not an end result. That is that the process of model validation cannot insure acceptable prediction or quality of the model. Rather, it provides an important safeguard against faulty models or inadequately developed and tested models. If the model results become the basis for decision making, then the validation process provides evidence that the model is valid for the decisions, not necessarily a true representation of reality.

And, I think we see that a lot in the Yucca Mountain Project. It's when we have a set of competing conceptual models, we often pick the more conservative one if there's no data to support any one over the other. In cases like that, we're pretty sure we're not reflecting reality, but we're pretty sure we're building a sound basis for decision making.

The purpose of this model that I developed of the Yucca Mountain site is to test conceptual models and I'll start off with the usual notes that this work was funded by the Nuclear Regulatory Commission, but nothing I present here necessarily represents their regulatory position. And, that the model scenarios and results I'm about to present are exploratory in nature and intended to gain a better understanding of what affects the flow system and nothing should be considered as a preferred model.

I'll get into the outline. I'm going to talk about how the model is based on the hydrogeologic framework, the effects of hydrogeologic interpretation on the model calibration and how that affects groundwater flow paths. Then, I'll go into a second set of analyses on effects of local recharge in the repository area and how that drives flow paths into different portions of the aquifer. And then, a third set of analyses that I've heard inklings of interest in is the effects of increased recharge and water table rise possibly due to a future climate on the model flow paths and travel times of groundwater.

The hydrogeologic framework model that we start off with is developed independently at the Center. So, we're starting off with a completely independent interpretation of the geology as the basis for this model, although one of the data sets did go into this interpretation was the USGS GFM Model 3.1 which was also an input to the DOE's model. It's one of our inputs and we interpreted that model somewhat differently by lumping hydrostratigraphic layers with similar properties that are adjacent to each other into single units. So, that interpretation is also different from the DOE approach. Then, we extended the model region based on the Center's interpretations of geology and geophysics. And then, I took that as the basis, extracted a region from that model and assigned hydrologic properties based on the correspondence to units.

There's also several faults and structural features in the model. You can see pretty much everything on this graph is in the model, all these different faults. There's a caldera zone in the hydrogeologic framework model. This red line shows where I had to actually extend that region of the caldera southward in order to obtain a better calibration. That would correspond to something in the DOE model that they also had to do that they called a northern region or northern zone. I forget the exact name of it. And then, the other modification is I had to extend the Highway 95 fault zone just a little bit from where it was in the original model and then pretty much everything else is similar.

I think I missed a couple points here. Yeah, some of the other changes were the Bow Ridge, Midway Valley Fault zone, Paintbrush Canyon Fault zones here in the middle of the mountain. These faults are all so close together that I just discretized that into a single fault zone and then I made a separate fault zone for the entire area between the Paintbrush and Fortymile Wash Fault as part of the model construction. Then, that was all discretized into a 300 meter horizontal grid size. Vertically, the smallest grid sizes near the water table is a 15 meter vertical grid discretization. That's that.

And, this shows you a comparison of the underlying hydrogeologic framework model and then how that ends up in the flow model. This is a cross-section through the middle of the model domain that goes through about the repository area, east-west. And, you can see the different material colors that I used to incorporate faults. I didn't extend the faults all the way down to the bottom of the model domain. I just keyed them into the underlying permeable unit. This brown unit here is a low-permeability volcanic confining unit that extends over a good portion of the domain. The paleozoic carbonate units, the lowermost unit. The lower volcanic aquifer. This blue layer is an upper, what we call, a confining unit, but it's actually semi-confining. It's a little bit permeable compared to this brown volcanic confining unit. Then, the upper volcanic aquifer in gray and then the alluvium is in the lavender color.

The model domain, you can see in the square box over the satellite map and then I used the interpretation of water table based on water levels and heads to get a starting point for the top model boundary. The top boundary is something I used in MODFLOW. It's called a confined/unconfined flow boundary. It allows--if there's not enough flow into a cell to keep that cell saturated, it allows it to go dry and become inactive. And then, if you should increase recharge during a run, theoretically, it lets it re-wet and become active again during a simulation. So, I set the top seven model layers in that mode because those were within the range where the water table might become active or inactive in a cell. No-flow bottom boundary. The size of the domain is 28.5 by 41.4 kilometers. It extends from 1500 feet below sea level to 1200 feet above sea level. And, 70 wells used for calibration points. Recharge points were in the north area and also considered in the Yucca Mountain area and Fortymile Wash area for certain scenarios.

There's an oblique view of the model that you can see how the top of the model tapers down in active cells where the water table drops down to the south. You can also see certain faults in the model and the different configurations of the material types.

The first analysis I'm going to present is how we--so the question we asked is--the DOE has got their model and it's got a certain amount of calibration error. Somewhere about the means^2 error in their model for 80 or so wells is about 30 meters which, you know, 30 meters a head off in a well could be a lot. So, our question was is that something that's drastically going to affect flow paths where travel time is commenced? So, we set out to take a standard model and calibrate it as best we could and then to take an alternative model and shift things around a little bit to get the calibration even better.

In this model, we didn't necessarily constrain ourselves to having features supported by data. For example, you know, along these fault zones, you have big head drops in some areas and that seems to be where you get a lot of your errors in the model. So, one of the ways to fix that is to take a model cell and shift it over to the right one or make your fault zone two model cells wide instead of just one model cell wide and you can reduce a lot of error that way. Another thing we did was to sculpt the shape of this caldera zone. Another adjustment we made was in some areas between the alluvium and tuff interface, we put transitions on that had a lower permeability. And, there's conceptual bases for all of these, but they're not necessary supported by data and that's why we're calling this an alternative scenario. The contours on here show the different calculated water table elevations and they're not that different between the models to look at.

But, when you look at the calibration, the original model had an RMS error of 27 meters, very similar to what the DOE. That was the best we could get using a trial and error approach. The biggest errors were up in the north area where heads are high. So, they're not necessarily problematic because they're off at the flow paths. Then, the second zone of high errors is what we found mostly along fault zones where there's a steep change in the water table gradient. So, that's where adjusting the fault zones left or right came in handy. And, if you look on the right, we were able to get our RMS error down to 1.1 meters on average with 70 wells just by moving things around a little bit. And, that was all trial and error and adjustments. It took one person full time about a month to do this which if you had to set a PESS simulation up, there's just so many parameters, it probably would have taken it a month to run anyway. So, we were happy with it. The error is down in the range of what the water table measurement error is. So, we decided that's a good stopping point. We're really not going to get any better than that.

So, let's compare the results of the two models. On the left, you see the original model has flow paths going pretty much to the south. What happens is they start flowing east and then they hit a zone where Bow Ridge, Paintbrush Canyon Fault zone, it's more permeable than where they started out and that's like a stream going into a river and it just makes the turn when it hits the river and then it just flows straight south.

In the alternative model, the distribution of permeability is going east to west. It didn't really change drastically until we hit the Fortymile Wash Fault zone. So, they go a little bit farther to the east before they also make a quick southward turn and essentially end up in the same spot. I should say about this alternative model, along the way we did several analyses of flow paths when we were adjusting different features and there's a lot of calibrations that are nearly as good or almost as good that didn't go as far east as this and pretty much looked the same as the original model. So, you could view this alternative as the farthest east we could get those flow paths to flow and it really happens to be a very good calibration at the same time.

And, comparing that to the latest DOE model predictions that we have access to, you can see they're generally in the range of between what these two models are predicting and also end up in essentially the same point at the end. So, you know, starting with a completely different model, completely different approaches, and completely different levels of matching your calibration data, there's not a big variety in flow paths. So, that's the end of this interpretation before I move on to the next one.

I'm not going to show you the travel times for these two flow paths because there were some things I didn't like about that that will come up in the next analysis. But, just to say that the travel times didn't differ too much between these scenarios. So, that gives us confidence that we don't really need to go in and collect enough data so that we can get our calibration down to one meter and still accept the results for the purpose of the model.

So, the next thing I wanted to look at was the effects of recharge in the local Yucca Mountain area because when these flow paths from the unsaturated zone hit the saturated zone, the amount of recharge affects the downward gradient and it's going to drive them into the system and hence the flow paths. So, we wanted to know how important it is to get that recharge rate on the saturated zone flow paths. So, Case 1 only has recharge in the area to the north which they assume to be 10 mm/yr for this analysis and nowhere else. Case 2, I added the yellows on of 5 mm/yr which is pretty close to the average infiltration rate base case that the DOE is using. For a later simulation, I'll talk about recharging Fortymile Wash, but that's not in the ones I'm about to show you.

For the case with no recharge in Yucca Mountain, the flow paths are pretty much the original case I showed you from the last analysis. And then, with 5 mm/yr recharge in Yucca Mountain, they don't look very different, at all, until you look at it in the vertical view. And, you can see that with no recharge, they stay very shallow near the top of the aquifer. And, with a little bit of recharge, they go down quite a bit deeper down to about--most of them are no deeper than about 300 meters, although there's a couple of stragglers that go deeper. That lengthens the flow path some and then also spreads out where they arrive at the 18 kilometer point.

In terms of travel time, there's a big difference. I think I should show you that for the original case, it's sort of a bimodal distribution. The earliest times came from the south end of the repository and then the longest times stretch out beyond 10,000 years for quite a few of the flow paths for almost the whole northern half of the repository; whereas the case with only 5 mm/yr recharge in Yucca Mountain significantly shortens that. So, even though the travel distance is a little longer for these deeper flow paths with the recharge, the mean travel time in this scenario--remember, we're not talking about reality here; this is a model--is 1,000 years going up to a couple thousand at the most; whereas this has travel times going up into the 40,000 year range.

So, what's the deal with groundwater travel time between these two simulations? Why are they so different? That gets into what I had to make assumptions about for the porosity. In my simulations, I assigned a value of .001 for welded tuff units, and then for nonwelded tuff units, such as that upper volcanic unit which corresponds to the Calico Hills, I gave it a value of .1. My basis for that was that there's quite a bit of well data that shows that's a relatively unfractured porous type of formation and so we might expect a different flow regime in that unit than in the tuff units. So, as most of the hydrologists know, for a given flux, the average groundwater velocity is going to be inversely proportional to that porosity.

In the simulations where I had no recharge at Yucca Mountain, the shallow flow paths traveled a much greater distance through this upper volcanic confining unit and that is the main reason why you had a distribution of flow paths that went beyond 10,000 years to the range to several thousand. And, it's solely because I'm assuming a value of .1 for that upper volcanic unit. If I did as the DOE assumes in their performance assessment and set all volcanic units to the same value of .001, in that case it makes very little difference whether I have recharge or not in the Yucca Mountain area to the flow paths. So, take away from this is that although nobody is currently doing it in performance assessments, there's a possibility that this UVC, upper volcanic confining unit, the Calico Hills unit, could have a porous flow regime that would add a lot of time to the groundwater flow path. And, I already mentioned that historically most of the performance assessments have been conservative in their assumptions about that.

The next analysis I'm getting into is the potential effects of climate change and I guess I should say a little bit about my thinking process that went into what's going to happen to the water table when climate changes. You know, this all went on inside my head, and one afternoon in my office, I decided that here might be a good way to approach it, and by the end of the afternoon, I had model results that I'm presenting to you today.

So, what I thought about was would it be a uniform water table rise? In that case, there was really no point in running the model because it's not going to change the gradient if everything rises the same amount. The hydraulic gradients are all going to be the same. And then, I considered that, well, what's really probably going to happen is you're going to get more recharge in some areas and that's going to be the area where the water table rises the most is in the areas of higher recharge. So, in the original model, the highest recharge areas had the highest starting heads on the boundary. So, I decided rather than raise the heads by a fixed amount, to raise them by a fixed proportion. So, that way, the groundwater table rise was higher in areas where there was higher recharge and higher boundary heads.

So, I arbitrarily picked 5 percent as my first amount and that happened to work. What I used as a constraint was this location of approximately around Nye County Well 9S. In that location, there were some evaporate deposits where historically you can infer that water table has intersected the groundwater surface in that area and that I should constrain the model by rising the water table enough so I just initiate some groundwater flow in that area. I used MODFLOW as a drain package where you put a cell in there. And so, I raised it by 5 percent and it just happened at the elevation of those evaporate deposits, it was the first portion in the entire model domain where the groundwater table intersected the surface. And, at that amount of increase, there was just a trickle coming out of that drain cell, about a meter cubed per year, which is consistent with the formation of the evaporate deposits, a slow seep coming out that can evaporate and leave deposits behind. So, that was my constraint and I got lucky and nailed it on the first try of water coming out of there.

The other thing I changed was I doubled the recharge in the north region, doubled the recharge in the Yucca Mountain region, and added 200 mm/hr recharge in the Fortymile Wash region. That is arbitrary and if anybody would like to suggest to me different values of recharge increase, I'd be happy to test them. But, in the meantime, I'll show you the results for this particular scenario.

And, that is that I got a water table rise constraint here at 9S that was equal to the ground surface of about 30 meters and that increased to the north. In the repository area, the water table rise was between about--I didn't write it on here--I think, it was between about 70 and 150 meters--70 to 120 meters was the water table rise. And, that just happens to be very close to what the Department of Energy models are assuming for water table rise in their model. And, you can see it increases from north to south which might--if any future modifications should take that repository horizon farther north, you can see the water table rise could be in this model scenario much higher than that 50 to 100 meters. So, that might be a factor to consider if there were any changes in that footprint area.

The other thing we considered was how does that change flow paths? The Department of Energy, the last model version that I had access to, treated climate change by just increasing the fluxes through the model by, I think--Bill Arnold can correct me--I think, a factor of 3.9. And so, what we wanted to know was how much do the fluxes change in an alternative scenario where we actually raise the water table elevations. And, you can see that the flow paths don't change much, at all, for the before and after scenario. If you study it closely, there are some minor differences, but nothing that we would consider significant.

The case with travel times is also not that much. There's a few particles get there a little bit earlier with the higher water table. Again, in this scenario, I'm considering the porosity distribution that I presented before. There's that thin tuff layer with higher porosities than the rest of the tuff. And, you get one flow path that's a little bit longer than the longest flow path for the present day case, but on average, they're not that different in terms of travel time.

So, those are the three analyses I wanted to present today. And, the conclusions that I come away with are that the model calibrations can be significantly improved by relatively minor adjustments to interpreted geometries and hydrostratigraphic layers and structural features, but the variability of the flow paths and travel times for the two scenarios was only modest.

Considerations of small amounts of recharge to the potential repository has a significant effect on the depth of the flow paths and volcanic units through which they travel. However, the further increases in recharge above that 5 mm/yr did not appear to add to that effect. What I'm saying is in that climate scenario when I had further doubling of the recharge in the repository area, those travel times didn't change much from the present day or the flow paths.

The comment I made on the porosity of the upper volcanic confining unit can have a dramatic effect on the groundwater travel times to the compliance boundary. If data collection efforts or perhaps mining of existing data were to focus on evaluating that porosity, it might improve the understanding of the effectiveness of the saturated zone barrier.

An assumed 5 percent increase in the boundary head values to account for a potential water table rise results in initiation of model groundwater rain flow near the Nye County Well 9S which is consistent with the location of the spring deposits. That 5 percent boundary head scenario resulted in a water table rise beneath the repository of--oh, here's where I had it--50 to 150 meters, increasing from north to south, and those potential effects should be considered if repository footprint is extended to the north.

The scenario of combined water table rise and increased recharge including additional recharge at Fortymile Wash did not significantly change model groundwater flow paths or travel times to the compliance boundary.

And, that's the end.

NELSON: Thank you, Jim.

Could we look at Slide 14 just for a minute? I guess, I was really struck by the apparent importance or impact of having recharge right on the Yucca Mountain area on the flow paths. But, I'm not sure I understand the difference between these two figures in terms of what the subsurface stratigraphy is showing because they are a bit different.

WINTERLE: Okay. Without the recharge, you see this blue layer here? That's that high porosity unit. And, there's nothing to really drive them down through that unit. So, they tend to stay up there and travel very slowing, especially the ones initiated in the north end of the repository. That's why I had that bimodal distribution on the travel times where some of them were getting there quickly, but you know, the north end of the repository was taking an excess of 10,000 years travel times. And, it was all due to this significant difference here. And, in the lower scenario--these are actually the same model, though you see different stratigraphy. I made a mistake and took a slice from the next cell over in this model. But, you can see that that recharge drives it down through that zone and then at that point it's into the permeable unit with low porosity where for a given flux it just flies along at a higher velocity.

NELSON: Okay. Nelson, Board. What's the total thickness of these sections?

WINTERLE: In the upper zone here where I'm pointing, the top 10 layers or so, each of those grid cells is 50 meters. There's a 7 to 1 vertical exaggeration here. So, it looks exaggerated as to how far those are coming down, but it adds maybe 200 meters to the flow path which isn't much in terms of an 18 kilometer transport distance.

NELSON: Okay. Bullen?

BULLEN: Bullen, Board. Could we go to Slide 13, the previous slide? I just had a quick question for you because one of your conclusions stated that if the repository is shifted a little farther north that you may be getting close to the water table rise. That would be a problem if, for example, you did the 5 percent increase in recharge rate. But, what repository footprint did you use for the calculation? Was it the SR footprint or is it the more recent--

WINTERLE: It was, I believe, from the SSPA which looked slightly farther north.

BULLEN: Bullen, Board. Actually, the most recent footprint we see has the north ramp and, you know, four different panels and the like. Have you seen that latest layout and does that actually overlap with some of the areas that have like the 250 meter rise?

WINTERLE: That's a good question. I can't answer it. I have seen drawings of that. They haven't filtered down to our database to the point where they can be incorporated in the models. But, I would say if they're getting up into that north area around where Well G-2 is or even just a little bit south of that, then it might be something to think about how close that water table could get.

BULLEN: Bullen, thank you.

NELSON: Parizek?

PARIZEK: Parizek, Board. Thank you for your presentation. It was very interesting. On Page 21, it's sort of surprising that when you turn on the pluvial again, you end up with a rise about 30 meters at the paleo spring deposit and from 50 to 150 meters under the footprint. That gives you a steep gradient and yet that didn't seem to change the travel time. You've got a much steeper gradient. So, you'd think that you ought to get a greater velocity out of this. Can you explain why that--

WINTERLE: The real steep gradients are mostly just north of the repository area and in low-permeability units. And, also, they're steep because the recharge was increased in that area and it's a low-permeability zone. So, there actually may be some conceptual problems with my interpretation there where the zone where I'm showing several hundred, like 300 meters, of rise in the far north portion, the rock there might not be able to accept that much water. So, it might actually be more runoff and less of a water table rise in that area.

PARIZEK: Parizek, Board. With regard to the precipitation amount to get the springs, I guess at the Horsetooth formation, the paleo spring to just begin to seep, there are several other paleo spring deposits in that area. How much more precipitation would you need to maybe kick those in or would that be another trivial amount or not? I mean, it's good that you got them to come out, first of all.

WINTERLE: Yeah, it's actually the whole zone around there. The 9S area was the first one, but there was just north of there on the other side of those hills, that little corner of Crater Flat that tucks down behind the Highway 95 area, there was--very close to the ground surface, hydraulic heads there. And, I believe, down in the southeast corner of the model, they were getting pretty close to the ground surface which is--Ash Meadows isn't in the model domain, but you're getting down towards that area at that point in the model.

PARIZEK: Parizek, Board. They're going in the right direction. So, that's encouraging.

Figure 14, you had some black dots in the upper diagram. I don't know what those are.

WINTERLE: Those were the calibration points.

PARIZEK: Okay, I'm sorry.

WINTERLE: Yeah, a point I should have made was that the calibration points cover a variety of depths, not just horizontal distributions. So, we're matching upward gradients in our calibrations, as well.

PARIZEK: Does the model require anisotropic properties in the role of faults built into here in order to get--

WINTERLE: I assume--

PARIZEK: I mean, your calibration is so fantastically exciting, you say, maybe I shouldn't believe it. But, I mean, what did you do with your faults and anisotropy?

WINTERLE: Every model cell was assumed homogenous and isotropic here and it's just I played around with the position of them and it's sort of like sculpting a statue. It's like, well, that doesn't quite look like I want it to. Let's move that cell this way or that way. You know, to me, that's a little more defendable than getting an inverse code like PESS and say, well, you decide where the permeabilities need to be highest. At least, you know, I'm in control of the conceptual basis for where I move a cell. And, I'm sure a PESS simulation could have given me a heterogeneity plot that looked like a shotgun blast and maybe come up with as good a calibration, but I wouldn't be able to defend where to assign the permeabilities.

PARIZEK: Parizek. One other question. That's on the Fortymile Wash. You had a green line that you were going to put into another run. Which run actually had the Wash recharged?

WINTERLE: That was the water table rise scenario. I doubled all the recharges in the north and Yucca Mountain area and added 200 to Fortymile Wash.

PARIZEK: Yeah, and again you were asking for challenges to what's better than 200 or--

WINTERLE: Yeah, that is arbitrary.

PARIZEK: You're not sure where the chemistry or isotropic data might help put some limits on that?

WINTERLE: Yeah, I thought to maybe include a river package in MODFLOW for that. I don't know if the Fortymile Wash would be a perennial stream in a future climate. There's different ways to look at it, but I thought 200 mm/yr is a pretty good slug of water compared to what's going through there now. And, if that's not going to move it--

PARIZEK: And, unlike Claudia, you didn't have any lakes on top of any mountains, did you, in your runs?

WINTERLE: No.

PARIZEK: Okay, thank you.

NELSON: Thure and then Ron and then one from Staff.

CERLING: Cerling, Board. I guess this gets into the issue of calibration versus validation. So, in one of your models, your wetter model, you increased in the distal regions groundwater by 30 or so meters to have it come out the same as a paleo spring deposit, and then further up, you proportionally increased the groundwater table.

WINTERLE: Right.

CERLING: And so, what I'm wondering is there any evidence that you can use to corroborate that higher level in drill cores that has to do with petrographic evidence, stable isotope evidence, etcetera? Have you looked for that to see?

WINTERLE: That would be good to have some data. It would be nice to have some access to walk around in those hills up there and look for old spring deposits perhaps as a constraint on how high water table has gotten in the north. You know, based on what I have, all I could do was make some arbitrary assumptions.

NELSON: Ron?

LATANISION: Latanision, Board. Of the long list of KTIs that are of interest in discussion between the project and the NRC, are any of those encompassed by the work you are doing, and if so, can you give us some indication of what the state-of-the-art is?

WINTERLE: Oh, the KTI, this work was all done on the unsaturated/saturated flow under isothermal conditions, KTI. The status is we've--what we're focusing on now is the agreements that were made back in the 2000-2001 technical exchanges where based on our review of the site recommendation and our preliminary review of the SSPA document, we had given DOE an indication of what we had thought were the extra things they needed to provide to defend their model and to move toward LA. Since then, a lot of models have changed and, you know, we've got a whole round of these technical basis documents to review now. I think, in a lot of cases, you know, the changes have been more defensible. In some cases, the changes, as far as we can tell, so far, are not defensible. I don't know what's going to happen when an LA comes in, if there's going to be another round of technical exchanges. You know, I can't speak for the NRC. I would imagine there's going to be some. I find it impossible to think that there won't be any comments on an LA that haven't already been raised, but where we're at now is we're probably--part KTI is about halfway through resolving the agreements that we have made. There's maybe 15 or 20 left, I think.

LATANISION: Thank you.

NELSON: Dave Diodato?

DIODATO: Diodato, Staff. Thanks for a very nice presentation, Jim. I wanted to look at Slide 15 first just for clarification. The Case 2 on the bottom, that would be for a conserved species, right, with no--

WINTERLE: Yeah, that's a Mod Path particle tracking simulation which is designed to go where the groundwater goes. So, think of it as a water molecule.

DIODATO: All right, thanks. So then, there are none of the phenomena of--

WINTERLE: Yes, no matrix diffusion, no dispersion. The only dispersive effect would be the macro dispersion of the various flow paths coming through.

DIODATO: Thanks. So then, if we back up to Slide 14 and look at the path links, you talked about the importance of porosity in the volcanics in terms of determining the velocity of that water molecule. And then, also, the path that it follows, you have this in cross-sectional view and you had the other simulation aerial view. Could it make a difference then in terms of how much matrix diffusion would occur during sorption depending on if you're in the volcanics or if you're in the alluvium and that sort of thing, adding those things in? Do you think that then that might potentially be something that would make you look at your conclusion that it doesn't make a lot of difference in travel time? I mean, if you add that other layer of complexity into it, could it potentially then make a difference, the path of groundwater flow?

WINTERLE: I'm not sure I understand the question. Could--

DIODATO: The question is like--yeah, I worded it poorly. So, let me rephrase it. So, if the path of groundwater flow--your conclusion initially was that travel times aren't all that much different. You know, they're all about 1,000 years for the water molecules, the particle tracking simulations. But then, if you consider the effects of matrix diffusion and sorption, would that potentially cause you to alter your conclusions about one model versus another model?

WINTERLE: Yeah, I would think that if I had to pick a model to believe in, I'd take the one where they're going a little bit deeper because we can be pretty sure there is some recharge. So, even with those deeper flow paths, they do go through a portion of this upper volcanic unit and I would think that, you know, the porous flow regime would make for a lot more sorption capacity than just matrix diffusion occurring in the fracture flow parts of the domain. So, I would say not so much the case of whether or not there's recharge would make a difference in that, but the case of whether or not you consider porous flow in that particular layer could make a big difference and even a bigger difference when you take into account radionuclide transport.

DIODATO: Thank you.

NELSON: One question reserved for me. Can you tell me what your studies tell you about what the permeability of the faults must be?

WINTERLE: What the permeability of the faults must be?

NELSON: Is it high? Are they high-permeability, low-permeability--

WINTERLE: No, well, just to get a calibrated model, pretty much--I can't see how you can get around it. You have to assume that the Solitario Canyon Fault is low-permeability. I haven't looked at anisotropy if it's directional permeability matter. But, you also have to assume that in the Bow Ridge, Heepress Canyon (phonetic), and Fortymile Wash regions that those faults are high-permeability. And, you also have to assume that whatever that structure is in the Highway 95 zone, whether it's a fault or just some altered rock region, you have to assume that's low-permeability.

NELSON: Nelson, Board. So, most of the flow is actually running along--much of the flow is captured by the fault zone?

WINTERLE: It's not really captured in that in the case of Highway 95 and--

NELSON: Oh, I'm sorry, I was thinking about east of the Yucca Mountain.

WINTERLE: Oh, east? Yeah, because it's higher permeability, the gradient runs toward it. Once it hits there, it turns abruptly south. Now, the difference between reality and my model is that that transition is probably not quite so abrupt as, you know, one minute it's in low-permeability and the next second it's in high-permeability. So, those turns to the south could be a little more gradual is maybe one difference.

NELSON: And, the exit point from the fault zone or from the rock into the alluvium is similar for your model than to DOE's model?

WINTERLE: It's similar. The DOE model is--we don't really have a lot of cross-sections out of their hydrogeologic framework model to understand so much the geometry that's incorporated into their model. That's one of the agreements we have is that we're trying to get cross-sections of the alluvial basin and comparisons to their model. We don't have that information yet. That's a key uncertainty we want to explore also is the nature of the tuff-alluvium transition and that's probably something we'll use this model for in the future is to look at different versions of that.

NELSON: Okay. Closeout question to Richard?

PARIZEK: Parizek, Board. On Page 7, you show some contours that are kind of wiggly up near the footprint and I'm just wondering what the basis for that contouring is?

The diagram shows a pretty smooth contouring interpretation in that same general area.

WINTERLE: The interpretation is that's--you've got to wiggle them like that to match the--if you assume that those hydraulic head ops or water table elevations are within 10 centimeters of the true values, you have to wiggle them like that. Potential explanations for that could be that there's a couple of areas where some flow leaks through the Solitario Canyon Fault causing those two bulges that come out through the side there.

NELSON: This really is the last one.

PARIZEK: I might alert Ken Rehfeldt later on that when we get to his Page 19, we compare the two and see whether he--his is smooth, yours is not. So, we just want to understand the basis. Thank you.

NELSON: Thank you very much, Jim. It's very hard to corral the senator from Pennsylvania sometimes.

Okay. We move into our final talk of this morning and I've been asked by Russ Dyer to have an opportunity to make a preemptive statement before we ask Ken Rehfeldt to come up.

DYER: Thank you. Before we move into the next four presentations, I've been asked to repeat a disclaimer associated with a topic on here, the expected or median travel time of a water molecule. The talk I'm going to give is exactly the same talk that I gave yesterday except for the last slide. I just want to highlight a couple of things.

Again, I want to repeat that the presentations that you're going to hear do not address the expected travel time of a water molecule either in the unsaturated zone or this afternoon, you--or subsequently you'll hear about the saturated zone.

Secondly, we don't routinely do such a calculation. We have done such a calculation in the past which I'll show you on the next slide, the same slide that was shown yesterday. We don't think that any of the information that's been garnered in the resulting several years would change the results much. We showed this yesterday and talked about it a bit. What you'll be talking about today primarily would be the saturated zone part of this.

The final thing I'd like to point out is that for the four remaining presenters--that's Ken Rehfeldt of Los Alamos, Gary Patterson of the U.S. Geological Survey, Stephanie Kuzio and Bill Arnold of Sandia National Labs--in the information that they present, they'll be using radionuclide breakthrough curves to list a predicted transport behavior of--just a minute--of calibrated saturated zone models and abstractions. Those breakthrough curves don't represent the expected travel time of water molecules. They portray a full probabilistic sampling of input parameters. And, as you probably picked up from George's presentation yesterday, they're often developed with very conservative inputs to fully assess the impacts of uncertainty.

Thank you very much.

NELSON: All right. We'll move on and invite Ken Rehfeldt to come up to the podium. Dr. Rehfeldt received his PhD from MIT, that place where some people know about, in civil engineering. So that he's one of the good guys; no bias there. Ken works on saturated zone groundwater flow and transport studies for the Yucca Mountain Project. He has more than 20 years experience in the field of groundwater hydrology including more than 10 years in the assessment and modeling of groundwater flow and radionuclide transport from the underground nuclear tests at the Nevada Test Site.

And, we are very interested in hearing you.

REHFELDT: All right. Thank you very much. It's a pleasure to be here.

What I want to talk about just before lunch here so we can let you go and have some lunch is to briefly talk about the conceptual model of the saturated zone flow and transport at the site-scale, but primarily to concentrate on some of the independent lines of evidence that we use to give us more confidence in those model calculations.

I want to point out that this was a collaborative work with a great many of the researchers both at Sandia and at Los Alamos. In fact, this is their work. The modeling that I'm going to present is not the work that I've done myself, but it is the work of these folks here.

So, the outline of the presentation is I'm going to very briefly talk about the conceptual models of groundwater flow and radionuclide transport and you'll hear more about that a little bit later this afternoon from both Bill Arnold and Stephanie Kuzio. And, I'll show you some of the site-scale flow and transport model calculations, some of which you've already seen this morning and then work primarily to present the independent lines of evidence to support those calculations.

Here is the outline of the site-scale model that's sitting out here. And, as Claudia pointed out earlier this morning, the site-scale models sits inside of a much larger flow system, the Death Valley regional flow system. And so, what goes on in the site-scale is not independent of the regional. In fact, the Death Valley flow system provides a fair amount of control over the site-scale model. It defines general flow directions, recharge, discharge. For example, there's much more recharge to the north of the site. Discharge is primarily to the south in Death Valley and also in the Amargosa.

Other features that control flow in the site model are the local geology, as Claudia pointed out, at the regional scale. You have the spatial location of the different geologic units, their material properties, and then there's also the role of faults which can change that.

And, finally, on top of all this, then you've got the local conditions such as local recharge and what may be going on at Fortymile. So, these were all features that were considered in the flow model.

In transport, several key features here to keep in mind. First of all, we've got potential radionuclides migrating from the repository, down along the water table, out into the alluvium, and the Amargosa here. Within the volcanics which is primarily the longest part of this flow path, we've got several different properties that we need to consider. Those would be advection, of course, and matrix diffusion. Within the matrix, we allow sorption to occur. In the alluvium, then it's considered a porous medium flow system. And then, you have on top of this, sorption can occur to the alluvium. In addition, there are processes that we consider--let's see, over here, down here, some other processes would be radioactive decay, of course, because these are radionuclides and then the role of colloids and how the transport of colloids may influence the transport.

Now, what I want to show you is very briefly some of the results from the site-scale model and then we'll get into some of the other lines of evidence. You've seen the left hand side of this picture before. These are the flow paths that were calculated based on the calibrated isothermal site-scale flow model. And, again, they start at the repository, migrate to the southeast, and eventually are drawn into the Fortymile flow system, and then head on to the south.

The other thing to point out here on the left side is a cross-sectional view of those flow paths. So, you're looking from the side and we can see here that again some of the flow paths because there is some recharge a lot at the site, some of these flow paths do go down. This is about 300 meters or so of displacement in the vertical. At some point in the flow system, you see that the flow lines converge in the vertical as they're going through a very narrow aquifer and they again spread out as you get further to the south.

This is an example of some of the flow lines--or these aren't flow lines. These are transport calculations along the flow paths that you saw in the last slide. These are breakthrough curves of radionuclides at the accessible environment after release at the water table beneath the repository. These breakthrough curves include all of the processes that I spoke of earlier which would include the advection, diffusion, and in the case of Neptunium, sorption. And, you're going to see more of this information later in

Bill Arnold's presentation, but I just wanted to give you a flavor of the kinds of results that are coming out.

The left figure here represents the transport of radionuclides that are considered conservative or nonreactive. So, it would something like carbon, technetium, or iodine. And, these calculations were made allowing uncertainty in model parameters including the specific discharge as a result of different recharge scenarios and other model parameters including the transport parameters.

Then, if you look over on the right side here with neptunium, it's the same uncertainty parameters in terms of flow, but we've added in a moderately sorbing radionuclide, neptunium, and you can see the significant difference that sorption makes.

Now, what I want to go over is the different lines of evidence that we use to give us some confidence in these model predictions. And, I'm going to step to four different sources of evidence and I'll start with the first two on this slide.

The first part of this is correspondence to measure data and that's both through the calibration process and through something that we're calling validation. But, during calibration, of course, the model is matched to observe potentiometric data and we use also hydraulic conductivity data. So, as we're adjusting hydraulic conductivities in the calibration process, we're comparing back to our measured hydraulic conductivities.

If you look down at the bottom here, when I say correspondence to regional observations, this represents the boundary fluxes that Claudia presented earlier. And, what we were using in this calibration of the flux is from the 1997 regional model. We did not calibrate to the later versions.

If you look at the calibration data for heads and conductivity and the fluxes--and those aren't going to be presented just here really as a matter of time, but the calibration was actually pretty close. In other words, as you would expect, the modelers kept working with their model until they got the calibration parameters to match closely to what was measured. And, there's really no surprise there. And, I don't really consider the calibration data truly independent lines of evidence because, in fact, you use that in the modeling process.

However, we do have some validation data. This is information, for example, water levels in the Nye County wells. During the calibration, we had some water levels in some of the wells, but during that process and afterward, other wells were drilled and other water levels were collected that were not used during the calibration. So, we can compare our model predictions to water levels collected after the fact as a way of checking to see how well we did in our simulations. The other thing we did is we have some cross-hole hydraulic conductivity data from the Alluvial Testing Complex which again was down near Fortymile Wash near the compliance boundary or the accessible environment. That information was collected after the calibration, as well.

The other two types of evidence that I will present, the first is corroboration with hydrochemistry. We looked at the water chemistry in actually the whole Yucca Mountain region and used that to assess potential flow paths looking at either the chemical evolution or, depending on the particular isotope, the lack of evolution as an indicator of flow paths. Used that information to address mixing of different water types and, in fact, depending on if you look at something like carbon-14, you may be able to get some sense of how long the water has taken to get from the land surface to its present location. You're going to see more of this type of information later this afternoon from Gary Patterson.

Finally, we also looked at groundwater temperature. We did some what we call validation simulations where we tried to simulate the temperature of the groundwater and to see how well we did again because temperature is somewhat independent of the flow process. You can look at that information, as well, to get some credence to your calibration.

So, let's start off by looking at the new water levels that were collected at the Nye County wells. This is Highway 95 to the south. The values that I've listed here in the blue represent the water level residuals, the difference between the calibrated value and the measured water level. These were obtained during the calibration. So, this is data that we had at the time of calibration; here, over here at the Washburn Well, Well #2-D, and several of the wells up here to the west. The data in red represent water levels that were either measured later in different completion intervals than we had earlier or in wells that were drilled after the calibration was completed.

And, several things I want to point out in here. If you look at this region down at the bottom of Fortymile Wash, there are certainly differences between what was measured and what was observed, but the magnitude of the differences here based on this new water level data is in the same range as what we saw during the calibration. So, in other words, we weren't getting any surprises from that. As we moved further to the west, you can see that the difference between measured and observed values is increasing. We're not doing as good a job of matching observed water levels, but again they're in the same range as the calibration. So, in other words, the new water level information isn't showing us anything we didn't know already. So, that gives us some confidence in what we had earlier.

Another piece of data is the hydraulic conductivity values from the Alluvial Testing Complex. There was a cross-hole test conducted at the Testing Complex after the calibration in the alluvium and we obtained a value of the permeability, intrinsic permeability, of 2.7×10^{-12} m^2. That value is 19 percent lower than what we used in the model. So, the model was using a slightly larger value. But, again, we didn't have any measured conductivities in the alluvium down here at the time. So, this was actually a pretty good fit.

If you take the water levels that were measured at the ATC, the Alluvial Testing Complex, and the new hydraulic conductivity and calculate what the Darcy flux would be, you'll find that the model calculated Darcy flux at that location at about 27 percent larger than what we get from the data after the fact.

The third line of evidence that we can use to give us some confidence in our model predictions is the hydrochemistry. And, again, Gary Patterson will be presenting much more detail on this later, but what I want to show here are the red lines or the red arrows which represent flow paths that were obtained based on looking at the water chemistry data. Then, over here, you'll see the black lines represent the model predictions. And, in general, we see that there's a pretty good correspondence that the chemistry does support the calculations that were made based on the hydraulics.

Another aspect that we looked at with the water chemistry, we did a chloride mass balance approach to estimate what the recharge rate might be, particularly in some of the wells in the northern part of the model area. And, based on that mass balance approach, estimated the recharge somewhere between 7 and 14 mm/yr. Now, the calibrated model uses a recharge rate of about 4 to 5 mm/yr. If you recall from the presentations yesterday, the unsaturated zone studies, they estimate that infiltration rates somewhere in the range of 1 to 11 mm/yr. So, again, this chloride mass balance approach gives us recharge values that are in the same, really pretty close, or maybe slightly above the value that was used in the calibration.

And, I did want to point out that, at least, in the version of the model that we were doing, the entire site-scale model recharges less than 5 percent of the total flow through the whole system. We have much more boundary fluxes than we do local recharge, at least, in the version that we were using.

The last set of data I want to talk about is this what we called validation-thermal modeling. And, what we were doing here, we were going to model the distribution of temperature in the saturated zone assuming both conduction via the natural geothermal gradient and convection caused by groundwater movement. And, this was done in two steps.

The first step was to look just at thermal convection. So, we didn't allow groundwater flow. It was simply a thermal model. So, you allowed heat to rise up through the different geologic units. We had different heat properties, different thermal conductivities for the different units. We took into account the topography, etcetera. Then, calibrated that convective model and what we found is that we had 94 observations of temperature in 35 wells, temperature ranged from 22 to 62 degrees Celsius and we were able to account for 80 percent of the variability in temperature with this conduction model alone.

On the right is a figure that shows the residual difference between the calculated and observed temperature from this conduction model. And, you can see that, in general, most of the observations fell within a range of roughly -6 to +6 degrees C.

Then, we did a second step where we took that calibrated conduction only heat model and combined it with the calibrated isothermal flow model which I presented earlier and we used those two pieces of information to define specified pressure and temperatures at the boundary of the model area. Then, we did a coupled heat and flow simulation and ran that to steady state. Now, what I want to point out is we did not make an effort to dual calibrate that model. It was simply one run, did an independent heat conduction model and essentially an isothermal flow model. Even without calibration, we were able to obtain 85 of 94 observations within 10 degrees Celsius; not a great fit, but it isn't terrible, as well.

This figure on the left represents the temperature residuals based on the conduction model alone without groundwater flow. What we can see in here is to the north and out here primarily to the west, even a little bit to the south, the model is under-predicting temperature and this is temperature at the water table. So, what this is showing is that the conduction model is predicting temperatures a little lower than it should be in this region and then it tends to over-predict temperature a little bit right here at the Yucca Mountain site itself. When we bring in the effect of groundwater flow, in some areas it doesn't change much. For example, in the north, there isn't much of a difference, and over here all the way to the south, there's a little bit of a difference. But, we do see a difference here in the region of the site where now in some cases, especially to the south where we may have been under-predicting before, we're over-predicting a little bit now. What this shows us is that there clearly is an impact of the groundwater flow. The groundwater does and can change the temperature distribution that you would have gotten from the normal geothermal gradient. But, even without calibration, we're still getting, I think, a match to observe temperatures that are reasonably good.

So, in summary, what I want to talk about is that we had multiple, what we believe, are independent lines of evidence to corroborate the calculations of the site-scale model and increase our confidence in these models. We looked at calibrated data; water levels, conductivity, and boundary fluxes. But, again, those aren't really independent because you would expect those to be matched pretty well. We have some validation data; new water levels and new hydraulic conductivities that weren't available at the time of the modeling. The hydrochemistry which was an independent way of looking at the water information. So, we didn't use the hydraulics; we were using the chemistry. And then, finally, the thermal information which is going to tell us something about vertical flow probably more so than horizontal flow.

So, I'm done.

NELSON: Thank you, Ken.

Can you tell me--this is Nelson, Board. Is your water moving in the fault predominately, the fault zones, or is it moving through tertiary volcanics as fracture flow or by whatever, predominately?

REHFELDT: Yeah, it's primarily through the tertiary volcanics through this dual porosity medium. And, we have some faults in there, but they're primarily barriers to flow; Solitario Canyon, the Highway 95 Fault. There is a structural zone which could be a fault that's in the model under Fortymile Wash which, of course, is more of a conduit.

NELSON: Okay. Questions? Dan?

VAN GENUCHTEN: In the sense--van Genuchten.

BULLEN: Oh, I'm sorry.

NELSON: I think the problem is when I say Dan, it's also van, van Genuchten. Can you wait, Dan?

BULLEN: Sure.

NELSON: Okay, thanks. Go?

VAN GENUCHTEN: On Slide 7--van Genuchten. So, on the lines of the earlier question, the one breakthrough there after 10 years, what--you know which pathway that went?

REHFELDT: No, I don't--I couldn't tell you exactly what pathway that was. I don't know if it's a fair question to pass off to Bill Arnold, but Bill will be presenting these results, among others, later this afternoon.

VAN GENUCHTEN: Okay.

NELSON: Bullen?

BULLEN: Bullen, Board. There's always a risk with giving backup slides because, you know, some of us actually go and look at them. So, if you'd go to Slide 22, I just have a quick question. Based on the results that we saw this morning from the new regional scale model, could you explain the results that you see here and, in fact, I like the fact that you actually gave us some real flux numbers here. So, I was just wondering if you could summarize the results here and say how do these relate to what we've heard already in previous presentations?

REHFELDT: Okay. To start out just to give everybody an explanation of what's shown here, this again is the site-scale model boundary. When we were doing the calibration to the regional fluxes, we actually broke that boundary into a series of segments. So, these different red and black regions represent the different segments. So, we calibrated to those segments individually rather than just to the entire model boundary. But then, over here on the right, what I've done is I've summarized the comparison between what came out of the 1997 regional model and the values that we obtained from the calibration summed over the whole boundary. So, I think what you're asking is knowing that these particular boundary fluxes, say, summarized over the whole boundary have changed substantially between the '97 version and 2002, and of course, the current transient version.

A couple of things I want to point out in here. We are currently doing an alternative model calibration with the site-scale model using the 2002 boundary fluxes. So, we're using the updated geology at the regional scale and the updated boundary fluxes. So, we will by the end of this fiscal year have a comparison for that, as well. So, we'll be able to see what the differences are.

Another thing to point out here along this eastern boundary. What you see is that the majority of the flux in the '97 model was down here in the southeast corner. That comes from a little triangle of carbonate rock that was in that version of the regional model. And, what's happening here is you've got water coming into the carbonate from the east, bypassing that corner, and going out the south. So, you see larger fluxes through the south and from the east, but it's really just flow in the carbonate down here in this corner and it doesn't impact much, at all, the flow that we see coming off Yucca Mountain and into the alluvium above it.

So, I don't know what the results are going to be later this year, although I would expect that we probably won't see tremendous differences between this version and the newer version because, you know, a lot of this boundary flux difference is beneath the flow system of interest.

BULLEN: Thank you.

NELSON: Okay. Ron?

LATANISION: Latanision, Board. I should just preface this comment by pointing out the symmetry in what I'm about to say and that is that since we're talking about the saturated zone and I'm personally beginning to feel a little bit saturated on the information, this question may be a little odd, but I want to make sure I have the perspective clear. If we could turn to Slide 7, if we were to look at the equivalent breakthrough time for water as opposed to radionuclides, am I correct in my impression that we would find that water is likely to rise before the radionuclides and the breakthrough time for individual radionuclides may be a function of such things as their own transport characteristics and ion interactions or colloid interactions or maybe magnetic fields or whatever? Do I have the correct perspective or am I missing a point here?

REHFELDT: I guess I'm not quite sure how to answer that question. I know and I think you got--the second part of that is, yes, for each individual radionuclide, you're going to have differences as a result of the size of the molecule and its diffusion characteristics, in addition to how it might sorb or not sorb to either colloids or the aquifer material.

LATANISION: Yes.

REHFELDT: As far as the transport time of water, I think that's a more complicated problem than it first appears. You still have this whole issue though if you think of tritium as representing water because it can be part of the water molecule. I mean, the transport of tritium itself has some complexities because of diffusion in and out of the matrix. So, I'm not sure really how to answer your question in terms of really what is the velocity of the water itself.

LATANISION: Latanision, Board. There's a temptation to think and maybe it's a zero order expectation that if water is transporting these radionuclides that when water appears, radionuclides appear. All I'm thinking is that that's not necessarily--in fact, it's obviously not the case because the transport of the radionuclides is going to be a function of a lot of variables that are different than--

REHFELDT: Right. Yes, it wouldn't necessarily--this is not necessarily representing the velocity of the water.

LATANISION: Right.

REHFELDT: Yes, that's correct.

LATANISION: Thank you.

NELSON: Thure?

CERLING: Cerling, Board. If we could go to Slide 16? So, you sort of have two scenarios here, one of which you include the convection and one which you don't include the convection model for heat. And, some of the circles, a lot of the circles, are still between 3 and 10 degrees Celsius off and some of them actually switch signs and are now in the opposite direction.

REHFELDT: Yes.

CERLING: How do you anticipate using this information to improve what you have or can you use this information?

REHFELDT: No, I think we can use this information. One of the primary effects we'd expect to see from flowing groundwater in a situation like this, if you have essentially horizontal flow of water with sort of a natural geothermal gradient, you really don't expect to see--at least, I wouldn't expect to see--much of a disruption in that geothermal gradient. And, where I see large differences between the water temperature indicates, at least, to me it's more of an indicator of vertical flow influences or potential vertical flow influences. So, either downward flow of recharge or upward flow of warmer water will disrupt that geothermal gradient. So, I think that using the temperature information gives us more confidence in how we've characterized the vertical flow than it will in how we characterize necessarily the horizontal flow.

CERLING: Okay. Well, I mean, just to follow on that, do you anticipate using this to significantly improve your understanding of the flow paths itself or is this kind of just going to be used as a calibration or a validation of the model?

REHFELDT: At this point, what we've done is to use it to give us a little more confidence. If you look at the flow paths that I showed earlier, they're primarily horizontal. There's a little bit of vertical movement and then these things tend to skirt along near the water table. What we were using, the temperature information, was to help us and give us confidence that, in fact, we weren't missing something that might indicate a significant vertical movement either upward or downward. You know, the overall temperature range in this system is around 40 degrees Celsius. So, if we had significant and rapid downward movement or upward movement somewhere, I'd expect to see much larger temperature differences than what we're seeing here. That's what it was used for.

NELSON: Okay. All right. We have three more; Richard, Frank, and then Leon.

PARIZEK: Parizek, Board. I like the approach of trying to pull all the pieces together with all sources of data. That sort of helps on one hand with the calibration confidence building, but also where you can, you add to the validation process and you've shown us a whole range of approaches that were used. I was just rummaging through my pile for the technical basis document that talks about Figure 19 or, at least, gives another alternative. So, if you could go to your 19 here? There's an interpretation that the model result is on the right. The observed contouring is on the left. But, in the technical basis document, there's another interpretation of contours of head that look more like the model simulation. The comment here is that you have the finger-like projection at that 775 contour. You know, it's off in the--it's a long nose that comes down--

REHFELDT: Are you--

PARIZEK: To the left, to the left, to the left.

REHFELDT: Oh, down here.

PARIZEK: We have a ridge in there.

REHFELDT: Okay.

PARIZEK: And, conceptually, that ridge requires some interpretation. One, it might be the result of contouring, say, perched water in which case the ridge doesn't exist here. It would look, more or less, like the contours on the right. And then, also, another interpretation that was drawn based on head data, not the model runs, but the actual interpretation. Then, that would ask for higher permeability patterns that might be dammed to the south, east, and west with flow concentrated and then kind of locked in, sort of like leaking under a dam. Another possibility would be higher recharge rates in that area relative to the areas on either side, same permeability or lower permeability east and west of that ridge. But, in any event, there's different ways to look at this. And, you say, well, how can I get a ridge like that in the conceptual model? I mean, it requires some permeability effects in the system. And, there are two fault zones that are either side of that nose. And, damming on the nose could be the Highway 95 Fault on one hand;

there's also volcanics in that area, the basaltic units, that could also be lower permeability. So, I just need--either the nose is there or it's not there. If it's not there, well, then your model simulations come out looking better. If it is a nose, then the model simulation has missed it and conceptually why does it miss it?

REHFELDT: Well--

PARIZEK: What do we do about that?

REHFELDT: Well, I guess, what I want to point out here is that this version of the observed water levels was a contouring that we did just based on some of the observed values and was machine contoured. So, I think, some of what you're seeing in here is an artifact of concentration of a couple of data points right down in here and then very little data above it. And, what I really should have used here was the contour map that was developed by the U.S. Geological Survey to represent all of this information. So, I think--you know, I may be misleading people because I don't think this represents what would be the general consensus for the water table. This was done primarily just to give us a general sense, are we capturing the same general directions of flow from the model, but wasn't intended to look at specific contours. So, I don't think--or, at least, I don't believe that we consider this to be a definite feature that we had to calibrate to. I think it's an artifact of the contouring.

PARIZEK: Parizek, Board. It does, nevertheless, represent one alternative interpretation. And, given the fact you have two and then John Bredehoeft reminds us how easy it is to sometimes get caught up in one and say, well, maybe they're equally likely. Unless the program can really throw out this one on the left for legitimate reason--it's just like the chloride-36 thing. You can't just throw it away now. You're got to sort of convince yourself that there's error or there's some difficulty with the dataset. Right?

So, here, if it's perched water and it's based on that, that's one thing. If, on the other hand, it may be a coequally useful interpretation, it gives you an alternative model. It's a groundwater ridge and it has a divide in it and then you can create--I have about five different ways I can create it conceptually. Then, I say, well, I think I have to track it down and eliminate each of the alternatives before I can feel good about it because it does affect flow or could affect flow if, in fact, that's the correct interpretation.

So, with all of the other contributions that were made through the review showing all the different lines of evidence you use, this is one that sort of jumps out begging for some interpretation. If you throw it out, you've got to justify doing so. Okay?

REHFELDT: Okay.

PARIZEK: Or if the program throws it out.

NELSON: Okay, thank you. Frank and then Leon.

SCHWARTZ: Schwartz. Could you go to Slide 7, please? Ken, one question I had about Slide 7 there on the left panel where it looks at, say, carbon, technetium, and iodine, there's one breakthrough curve that's really short there, you know, 10 years. What conditions in the model actually have to come together to produce that fast a breakthrough?

REHFELDT: Let's see, I'd actually have to defer to Bill Arnold to give you the exact answer. I mean, in the general sense, what you're probably looking at is the tails of a whole series of distributions; you know, the most rapid velocity, no diffusion, or very limited diffusion, things like that, maybe the smallest porosity that was in the distribution range, etcetera. And, you start combining all of those things together and you can get what really turn out to be physically impossible results.

SCHWARTZ: Okay. The second question I had was it looks like the--you know, just looking at how black it is in the density of curves there on the left hand side, it looks like most of the breakthrough curves are between 100 and 1,000. Is matrix diffusion turned on for the transport through the fractured rock part? It seems like--

REHFELDT: It is.
SCHWARTZ: It is?
REHFELDT: Yes.
SCHWARTZ: And, it seems relatively ineffective, one would think--I mean, in performance assessment, for example, one of the ways they get similar breakthrough curves is this idea of saying that the rock blocks are very big and that flow is through a very select group of fractures and matrix diffusion and that setting doesn't have an opportunity to work very well. I would think your model is constructed differently and yet you've got similar results. And, I wondered have you tried to capture this idea of flowing intervals somehow in your model or--
REHFELDT: Yes.
SCHWARTZ: I mean, it's not obvious it would be here, but I wondered how you've captured that and that's where I'm going here.
REHFELDT: Yes. This afternoon, Stephanie Kuzio is going to be presenting some of the transport parameters and how those were conceptualized and the distributions we used and she will--one of the parameters that she'll talk about is the flowing interval spacing which I think is exactly what you're getting at. It's how far apart are the actual flowing paths, not just the fracture itself.
SCHWARTZ: Thank you.

NELSON: Leon?

REITER: Leon Reiter, Staff. This question may in some part be due to my large problem of misunderstanding. Could you put on Slide 13, please? The bottom bullet sort of gives the impression that local recharge is not that important. It's a very small amount. I'm thinking back to what Jim Winterle said where there are a very small amount of local recharge coupled with, I think it was, the lower porosity that resulted in some rather large changes in travel time, shortened travel time. So, is there a difference of opinion here? Did you take into account--I see you took into account local recharge. Did you take into account changes in porosity in a volcanic aquifer?

REHFELDT: Well, first of all, I'm not sure on terms of the porosity. You know, I apologize for that, but I don't--I think it was just a constant porosity for the--no, it wouldn't have been because the porosity would have been variable, as well. There will be more information on that this afternoon. Bill Arnold will be presenting those same breakthrough curves that I showed earlier, among others. So, I think he will address that question in more detail.

To get to maybe another point, the bottom bullet there represents the particular calibration or set of model runs that we did for the flow model. You recall that we used the 1997 regional fluxes and they were quite a bit larger than what are coming out of the more recent regional simulations. So, I think that this comment which is correct for the version of the regional model we used may not be correct when we start looking at the more recent fluxes because the proportion of boundary flux to recharge will change.

REITER: So, Bill Arnold is the appropriate person for this?

REHFELDT: I think, if you want to get questions related to the parameters, the porosity, etcetera, yes.

NELSON: Okay. Thank you, Ken.

Okay. We're to that period available for public comment. I've received a sheet of paper that has four people listed and I just want to make sure that these four people are here and that they need to talk now as opposed to at the end of the day.

The four people are Atef Elzeftawy, is he here? Yes, of course, he is. Sally Devlin, Matt Kozak, and Judy Treichel. So, is that true you all need to talk now and not the end of the day?

TREICHEL: No--

NELSON: Judy can wait. Okay. That just gives us a little bit of flexibility.

We're on schedule. We have about 20 minutes. What I'd like to do is ask you to keep your comments to five minutes, and at five minutes, I will stand up and walk towards you and stand quietly behind you. That's a subtle indication that five minutes is up.

So, perhaps, we could go through in sequence as I read them. So, first up is Atef Elzeftawy, I'm trying, from the Las Vegas Paiute Tribe.

ELZEFTAWY: Well, you can call me Osama bin Laden. That's--

NELSON: I don't want to call you that.

ELZEFTAWY: I'm not going to take the five minutes, but you know, it's funny that Osama bin Laden highjacked the Moslem religion, George W. Bush highjacked the surplus, and the Department of Energy with the modelers highjacked the science of this site.

If you haven't read the book that was published by James Watson about a couple of months ago entitled DNA, I think you need to buy it and read it. It's about $65 a book and he, in it, lists the things when he got involved 50 years ago about the DNA analysis and the DNA as it went through in 1953 until last year, 50 years anniversary. In it, I think, there's one thing to remind me when I left the University of Illinois and the bad Marty Mifflin brought me here to introduce me to the arid climate is that science and politics mix quite well. Before that, I thought science and politics, like oil and water, they don't mix.

And, I think, these are just food for thoughts for you guys. I know that you want me to get out of here. You're hungry and you're tired and you need to go. But, as I look back, I think, I see a lot of similarities between all these activities. I wish we had the money available to the science that now we know that we have unsaturated flows more than 1 mm/yr. And, I remember Marty and myself back in 1982 when we decided that it should be more than 1 mm/yr just by the back of the envelope analysis and me, as a physicist, and he, as a hydrogeologist in terms of the Yucca Mountain thing. And, the DOE at the time insisted at the time that it's 1 and it's matrix flow. They submitted the EA, environmental assessment, to the NRC and they insisted it's 1 and matrix flow. It took the Department of Energy, what, 15 or 16 years to be able to say--I don't know why they took that long to learn the language--it took them that long to say that it's more than 1 and there's a fracture flow. And, that's awfully sad. I think the benefit we got out of that is that list of those people who got employed for the last 10 years to provide us with that little comment.

I want to go back to this <u>DNA</u> book because in it we had two players. One of them, Linus Pauling with his (inaudible) prize and his arrogance--sorry about that--or ego and then this James Watson, the guy who was just 24 or 25 years old opened things. It's the attitude again. Linus

Pauling was working on the DNA and Bragg and his people was working on the DNA in Cambridge. And, you cannot divide three chains by two and you come up with one and one in a sense. Linus Pauling had this conceptual model--again, remember a conceptual model of three strands of the DNA--and this James Watson and (inaudible) over there, oh, it should be only two based on all the data. Remember the data information they had. What Pauling did is what we do today; we just get the data we have and mix it together and finish the model and we make these little crooked lines and so on. Well, he didn't get it. And, one day, Watson sat down and he just was worried about his story, worried about all these things that he's doing and the model, and finally he got the cardboard and put it together and he got it.

The amazing part of that is that you cannot model the DNA. We know today that if you flip from one base to another, either you get sickle cell problems in your hemoglobin or you don't get it. It's 1 out of 3 billion base. Now, how can you model that? That's my critique to the performance assessment of the DOE. What may go wrong in terms of finding out what is happening in the basic part of the science? So, just think about that. Think about we put all our eggs into this kind of a basket. I think, that's hard to do.

She's going to kill me now. So, on behalf of the

Las Vegas Paiute Tribe, thank you for coming. Come back again to Las Vegas. It's good to have everybody here and I think that's the comment of the Chair for you guys to come. So, it's better than going to Washington, D.C. It gives more people visibility and gives you more visibility and that's on the site. So, come back again.

Thanks for your time.

NELSON: Thank you, Atef.

ELZEFTAWY: Thank you for not hitting me.

NELSON: Next up is Sally Devlin, public.

DEVLIN: Good morning, everybody, and again, as always, thank you so much for coming to Nevada. I hope next time it's in Pahrump or close to Pahrump.

Anyway, what I wanted to talk about today is a comment that really echoes what was just said. And, I was very disturbed yesterday by the report on the Mojave Desert. And, I asked the man why did you do it on the Mojave Desert and he said that's where I had the money from. Now, I don't think that's a good sign. That's when our area needs the science. And, of course, we're 2,000 feet higher than the Mojave Desert and we're a very different desert. So, I don't think that really affects Yucca Mountain.

But, the reason I asked these wonderful men here to put up the slide is I'm going to talk about Nevada. When I had the last meeting here, I said Nevada--and I apologize to everybody--was the bottom of the barrel. Well, we're really only 49. But, the reason we're #49--and I'm going to give you another reason for it--is in the middle of this thing, everybody can see the boundary between Nevada and California. That line is called the Von Schmitt line. That line was created based on the 1823 Mormon marches from Utah to Los Angeles. This line was done in 1872 and it's called the Von Schmitt line.

Now, the next line I'm going to tell you about is Clark County. Clark County where you're sitting right now is using an 1881 boundary survey map. The State of Nevada never had a boundary map until 1979 when two convicts said we don't want to be in an unbounded state, and therefore, they had to go through two sessions of the legislature and remember we only meet every other year. And so, in 1982, they got a boundary map. But, they didn't bother to check the boundaries. And so, you're looking at a line that was done in 1872.

Now, what's interesting about it, back in the '70s when I lived in Reno, they had a conflict because the State of California said we own Lake Tahoe and State Line. So, they had to go and measure the Von Schmitt line from Lake Tahoe all the way up to Oregon. This line goes all the way down south to the Colorado River.

Now, the reason I'm telling you all this nonsense about boundary lines and so on is on this particular map which really was wonderful because you know I love my friends from Inyo is you're seeing the size of the boundary which I doubt has ever been bounded or surveyed even by global, what do they call it, positioning. And, it really kind of bothers me because for the last nine years when I found out Pahrump does not have a boundary map that has been surveyed, we have had problems.

Now, the only reason I'm bringing it up and I think it's a question that's never come up is you see how out of step Nevada is and well-deserving of our #49 status. Of course, one mapping is the main thing and that's why I'm here for this hydrology learning session and that is supposing the water crosses that make believe line of Von Schmitt's from 1872? Who is going to sue who? Now, supposing the water from California from Inyo goes to Amargosa and kills 15,000 cows? Supposing all this stuff works together and who is going to sue who and on the length of time and so on? So that unless you realize we do have a social problem and I really think it is a social problem that something has got to be thought about it and done about it.

I do not believe that this 10,000 years that you're talking about monitoring Yucca Mountain--and, of course, Abe and I will be sitting on top of both Yucca Mountains playing gin rummy--I don't think 10,000 years--and you're seeing it with the neptunium and so on--well, you're talking 100,000 years. I think that Yucca Mountain should be monitored for, at least, 200,000 years. I think more should be noted on the millirems coming out and the exposure to the people and so on.

Of course, if the volcano blows or my Ingrid blows and you know that Amargosa--that the ash can't go to Pahrump or Beatty or Death Valley, and of course, my concern on all this stuff is it does go to Death Valley. And, Death Valley is our national monument and you've heard me say for years you can't kill (inaudible). Well, what you're teaching me is that that line, that imaginary 1872 Von Schmitt line, can be penetrated from both sides maybe.

And so, I think, it's rather important that you do look into this and realize about the mapping and the boundary lines and the State of Nevada is in big trouble because of this and I don't want my friends here to get in trouble either.

Thank you.

NELSON: Thank you. Third up, Matt Kozak from Monitor Scientific?

KOZAK: I appreciate this chance to talk to you. I am actually here representing the EPRI TSPA team. And, the reason I came up here, I actually hadn't intended to talk to you, but I started hearing some things creeping into the conversation that I thought ought to have some discussion and it was really crystallized in some of John Bredehoeft's comments on philosophy.

Really, the point that I wanted to make sure that we don't lose track of is that there is a fundamental difference between the scientific uncertainties and the regulatory uncertainties and they are addressed in quite different ways. John talked about validation and the importance of validation and history matching and so forth. There was a raging debate about 15 years ago in the waste management community on this whole issue on the people that follow Carl Pauper's theory and those that follow Thomas Kunz' theory. We're at screaming matches at these meetings. That's pretty much been laid to rest about 10 years ago and this is not just DOE/NRC kind of coming to an agreement. This is internationally people have come to a very consistent philosophy on how these things are done in highly developed confidence. So, I don't think we ought to lose that in this discussion. We don't want to go back 10 years and start talking about validation again. There's a reason why validation does not show up in some of the DOE and NRC documents. They talk about confidence building and things like that. There's a very good technical reason, a number of technical reasons, for doing that.

The second point that I picked up was on the treatment of alternative conceptual models. Again, this is not something that is particularly new. This has been an intrinsic part of the waste management community for, at least, 10 years. I know I was publishing stuff on it 10 years ago and I was citing prior work. So, there's a good body of work, good body of literature on that whole topic and on how to resolve conceptual models or alternative conceptual models. It's an intrinsic part of--if you look at the DOE TSPA documents, they're required to consider alternative conceptual models. It's part of the license review program for NRC. It's a basic part of the NRC's review process. So, again, I don't think we ought to get off on the wrong track of thinking that this is something new or something that's outside of normal experience. It is not typical of scientific approaches to modeling. It is typical of regulatory approaches to modeling.

In addition, as part of the resolution of those alternative conceptual models, we also need to keep in mind that there is a forward program on confirmatory evaluation and additional data collection and so forth that allows us to address any residual uncertainties that may be important to safety.

So, finally, I'd just like to say that one of the other comments was on modelers reviewing their conceptual models as immutable. If you think back over the last few days, we haven't seen a single immutable model. They've all been evolving, they've all been changing, and very open to change and considering alternative conceptual models and trying to come up with the best safety-based case. The one idea that I did like was that all this modeling has to be directed toward robustness and robustness needs to considered in terms of overall system safety, not simply in terms of the influence of some residual scientific uncertainty. So, if we can keep the idea of how the overall system responds to these uncertainties in terms of safety, I think if you keep that clearly in mind as you go through your deliberations, I think that would be of great help to you.

Thank you.

NELSON: I ask Judy Treichel if she wants to comment now or to hold? She's going to hold, okay.

In which case, we are two minutes before our chartered time for the end of this session. I thank all of our speakers and all of you for participating. We will begin the next session at 1:15 on schedule.

(Whereupon, a luncheon recess was taken.)

AFTERNOON SESSION

BULLEN: I'd like to welcome everybody back to the afternoon session. Please ask you to grab your cup of coffee, grab a seat. I'd specifically like to have my Board members back, if I can find them. Okay, the entire board back.

Good afternoon. I have prepared remarks, which I'll read, and then I'm going to do a little extemporaneous speaking. Welcome back to this afternoon's meeting of the Board Panel on the Natural Systems. I'm Dan Bullen. And, contrary to what it says in the agenda, I am not a member of this Panel. I could be ex officio. This might be one of the few panels that I have never been a member of, but I still enjoy participating.

Having said that, I will be chairing this afternoon's session, and this afternoon, we're going to continue with the theme of saturated zone fluid flow and radionuclide transport. If you can recall from this morning, that we were presented with a list of questions and outlines of the central purpose of the entire two day meeting. Each of these talks this afternoon will address one or more of the aspects of those questions.

The first talk this afternoon will be presented by

Gary Patterson of the U.S. Geological Survey's Yucca Mountain Project. He will discuss geochemical mapping of the groundwater system, and what the interpretation of that geochemistry can tell us about the waterflow in the saturated zone.

The next presentation will be by Stephanie Kuzio, of Sandia National Laboratory, who will discuss the transport processes of sorption, matrix diffusion, and colloidal facilitated transport, and how those are represented in DOE models.

At that point, we're actually going to move the break. We're going to put the break at 2:20 this afternoon, and that's up from 3:10, and we'll have the break just prior to the last talk presented by Bill Arnold from Sandia, who will discuss the modeling predictions of the transport of radionuclides through the unsaturated zone, and how those predictions are abstracted for the total system performance assessment.

Now, following the presentation by Bill Arnold, we had originally schedule a roundtable discussion, but at the present time, DOE elected not to participate in our roundtable discussion, so we still value the opportunity for technical discussion, and input into the process, so what we have decided to do is change the format of this afternoon's session. What we're going to end up doing is have essentially an open forum. That open forum will allow for the discussion of the materials that have been presented during the course of this meeting, both yesterday's presentation and today, and we'll actually provide input from our consultants, members of the Board who are here, the Panel who are here, and members of the audience who may wish to participate.

I'm going to facilitate that discussion or that open forum, and we have identified a 90 minute time frame, at which we're going to continue this work.

Now, I understand Dr. Parizek is going to give a few opening remarks, and, so, I'll put the pressure on him now to realize what he's going to say at that time, and we'll have our consultants also make a few opening statements. But, then, we would invite any member of the technical community, or the public in the audience, who would like to make some comments with respect to the issues that were raised, either in the five questions, or other issues that are associated with unsaturated zone or saturated zone transport to come forward to the microphone, and to make their comments known.

If there any questions about that, we can discuss it at the break. That will happen before the meeting--or before the open forum.

with that, I'd actually like to mention one more thing. We've been very good at it so far in the meeting, but I'd like you to silence your cell phones so that we don't have any interruptions for our speakers this afternoon. But, I'd like to call Gary Patterson forward for the first presentation of the afternoon.

PATTERSON: I think I should start off by apologizing in advance for not having the results of the numerical model to present. But, I do mention modeling a couple of times, so I hope you'll let me into the club anyway.

Primary objectives of the saturated zone hydrochemistry and isotope program at the USGS use major and minor dissolved ions, stabile isotopes, radiogenic isotopes, and both inorganic and organic carbon 14 ages to determine flow domains, flow paths, identify discharge sites, and estimate flow rates.

We intend to independently validate flow paths generated by saturated zone flow and transport models, and independently constrain flow rates generated by saturated zone flow and transport models. That's my reference to modeling.

When we began this effort a few years ago, the first thing we did is we constructed a series of isopleth maps or concentration plots of the major ions in the system, just to get a first cut at what the system looked like. And, when we did that, several things became apparent. The first thing that becomes apparent, if you'll look at the sodium concentration plot on the left-hand side, can everybody see that, the first thing that became evident was this long plume of low sodium concentration that's propagating beyond the Fortymile Wash system, and then slightly elevated sodium concentrations in the Yucca crest, elevated concentrations in the Oasis Valley, and another low plume of sodium concentrations in the Crate Flat area.

If you'll look at the calcium plot on the other side, it shows similar relationship with elevated calcium on Fortymile Wash, and lower calcium on Crest, and also higher calcium at Oasis Valley.

So, this immediately began to suggest the compartmentalized nature of flow in the Yucca Mountain area, and it wasn't just one sheet of water soaring from one area to the other. It was consistent hydrochemistry.

This is further supported by carbonated plot on the left-hand side, and even some of the minor constituents shows similar relationships, which is the fluoride plot on the other side. You can see we had elevated fluoride on the northern end of Yucca Mountain, and then slightly decreasing fluoride as you get into the Fortymile Wash system.

So, this suggests to us this compartmentalized nature of the hydrochemistry, and prompted us to divide the wells into a series of hydrochemical facies.

And, rather than draw boundaries around these hydrochemical facies, and many of you have seen these before, I've chosen to just plot the wells that we include within each facies in different colors so you can see the western Yucca Mountain facies is represented by wells in this light blue. Eastern Yucca Mountain facies is green, Fortymile Wash is yellow. Bare Mountain, red. Amargosa River in blue, and the Eastern Amargosa facies in black.

The constituents that are listed underneath of the facies title are really the most significant constituents that we use to differentiate these hydrochemical facies. The open circles down here below Highway 95 just indicate that the distinctions between the facies get a little more fuzzy when you get down that far, due to mixing and the amount of alluvium that it's passed through. So, we can't really be too confident in some of the distinctions down there.

Binary plots are probably the simplest way to indicate some of the distinctive nature of these facies. You can see the bicarbonate and sodium binary plot separates walls from Fortymile Wash, the Fortymile Wash facies, the eastern Yucca Mountain facies, the western Yucca Mountain facies, Amargosa River facies and the Bare Mountain facies. It doesn't do a very good job on the eastern Amargosa facies.

Sulfate and sodium does a little better job of separating the eastern Amargosa facies. You have the same separation here, based on sodium. The three facies that I'm particularly going to talk about today, which are the eastern, western, Yucca Mountain facies, and the Fortymile Wash facies. And, these are just examples, binary plots of different constituents, chloride and other ions will show similar relations, depending on which facies you're looking at.

One distinguishing feature of the eastern Yucca Mountain facies is elevated uranium 234 and 238 ratios. These elevated ratios have been interpreted by Jim Paces as indicating limited local recharge through the thick sequence of unsaturated rocks underlying Yucca Mountain. The elevated uranium isotope ratios decrease to the south and within the Fortymile Wash facies.

I'd like to refer back to the infiltration map that was presented several times in yesterday's presentations that Alan Flint made, that indicated that the southeastern trending washes on the north end of Yucca Mountain are estimated to provide 50 to 100 millimeters per year of recharge under current climate conditions, and 100 to 500 millimeters per year under the glacial transition period.

So, the hydrochemistry data here sort of indicates that most of the water that enters the groundwater system in the Yucca Mountain area may derive from those fault controlled washes on the north end of Yucca Mountain. You can see from this borehole here, with the elevated uranium 234/238 ratio, that's Borehole G-2, it may be right in the area where most of the recharge occurs, and then the water from the eastern Yucca Mountain facies travels south, southeast towards the Fortymile Wash system. It also seems to be somewhat isolated from water under the Yucca Crest.

The relationship between these from the Yucca Mountain facies and Fortymile Wash facies is demonstrated by these spider diagrams. Spider diagrams are constructed by referencing the chemical constituents of water well normalized to that of another well. So, if the wells contain the same chemistry, then the plot would plot along this one to one ratio line.

So, for this plot, I've taken these wells in the central and southern part of the Fortymile Wash closest, and normalized those to Boreholes UE-25 A-1 and 2, which are also in the Fortymile Wash system, but are north of Yucca Mountain. Those are the first two plots.

And, then, the third one is the same group of wells normalized to Borehole C-3, which I've taken as representative chemistry of the eastern Yucca Mountain facies.

So, the first plot is referenced against Borehole A-1, which again is north of Yucca Mountain in the Fortymile Wash facies. And, there's a very shallow well, and it's influenced primarily by the alluvium. So, you can see as the water flows down gradient from that borehole, reference to that borehole, the primary changes that you see are elevated fluoride concentrations. There is a little bit of unknown differences, potassium in sulfate in one borehole, but overall, those are plotting pretty much along the one to one ratio line, until you get to the fluoride concentrations.

The second plot that is normalized to Borehole A-2, which is immediately adjacent to A-1, but is influenced primarily by water from the volcanic aquifer, shows the same increase in fluoride as you move down the flow path, but it also shows increases in magnesium and potassium. This is representative of the changes caused by moving from the volcanic aquifer into a more alluvial aquifer, alluvial dominated aquifer to the south.

Then, the third plot normalized to Borehole C-3 from the eastern Yucca Mountain facies shows a similar increase in magnesium and potassium, since its water falling from the volcanic aquifer into a more alluvial dominated aquifer in Fortymile Wash. But, the fluoride concentrations, though, are similar between eastern Yucca Mountain and the lower part of the Fortymile Wash facies.

So, to summarize, the mixing between waters in the two facies results in only the fluoride concentrations in the Fortymile Wash facies before the confluence, and elevated magnesium potassium result from the influence of alluvium in the Fortymile Wash facies.

So, after looking at countless isopleth maps, binary plots, spider diagrams, we finally came up with a map of major flow paths identified from hydrochemical and isotopic parameters. These are not meant to represent a particle track. These are supposed to represent large masses of water, you know, myriad particle tracks together, flowing in a similar direction.

So, we're not, you know, we're not really concerned about a particular part of the track in this instance. But, the three primary flow paths that we're concerned with now are the Fortymile Wash flow path, eastern Yucca Mountain towards Fortymile Wash, and the southerly flow path from Yucca Crest all the way down to Highway 95.

I dashed this line because we have a group of wells up here on the crest, and we have another group of wells down here on Highway 95 with similar chemical characteristics. But, there's quite a long distance in between those two groups, and I think it might be a little bit of a stretch to pretend that we're too confident that that's the same water flowing down there.

So, those were attempts to put constraints or to verify some of the flow paths generated by the modeling efforts. In an effort to constrain some of the flow rates and travel times that are being generated by some of the modeling efforts, we attempted to use carbon 14 dating as a tool.

Carbon 14 is a radioactive isotope, has a half-life of 5,730 years, and it can be used to date groundwaters back to around 50,000 years.

There are some assumptions involved in dating with carbon 14. One is that water acquires its initial carbon 14 content as it percolates through the soil zone, enters the groundwater system.

In the absence of water/rock interaction, carbon 14 content would change only as a function of radioactive decay, thus, allowing the direct measurement of groundwater age.

Theoretically, carbon 14 measurements from wells situated along known flow paths would allow calculations of travel time between each pair.

But, we live in the real world, and dating with carbon 14 in the groundwater system has several problems. Particularly, carbon 14 measurements may give erroneous estimates of residence time if assumptions are not strictly met. The acquisition of dead carbon from aquifer rocks will result in determined carbon 14 ages that are anomalously old.

Mixing of water from different sources, such as merging flow domains, or recharge along flow paths, results in carbon 14 ages that are not the true age of the water up gradient.

So, I've put three plots of carbon 14 ages along flow paths, or generally along flow paths anyway. They're represented by these sort of trace lines that are on this base map over here. So, the first one is from the same Boreholes A-1 and A-2 over here, down along the Fortymile Wash system.

The second trace includes the eastern Yucca Mountain facies as it flows towards Fortymile Wash, and then the third one are these wells that represent the western Yucca Mountain facies.

You can see from the plot at the top, the carbon 14 age of Boreholes A-1 and A-2 are some of the youngest waters we've measured in the whole area. They're very shallow.

There's a lot of recharge up there. And, the ages are 3,000 to 4,000 years.

And, if you were to assume that all the carbon 14 assumptions were met, you could calculate the distance between Borehole A-1 and A-2 and J-13, and assume 10,000, or roughly 8,000 year travel time between those points. The problem with that is that Borehole J-13 is located there, is below the compliments of where we think that the waters from the Yucca Mountain system merge with the waters from the Fortymile Wash system.

So, if you look at the ages of the waters from the eastern Yucca Mountain system, they're approximately, what, 12 to 15,000 years. If you mix those with the age of the water from Boreholes A-1 and A-2, you come up with an estimated age of about 10,000 years, which clearly is not true age of the water at that location.

If you go further down the system where the mixing is expected to have already occurred, it's conceivable that we could use the age estimates from these boreholes to make at least an estimate of travel time all along the segment of the flow path. One of the problems with that, though, is that Fortymile Wash is, most investigators think that there's a certain amount of recharge along Fortymile Wash, so the addition of recharge along Fortymile Wash would cause these estimated ages to be younger than what they really are.

But, if you look at it in terms of those ages perhaps being minimums, with your eyes open and realizing that there may be recharge added to the system, you could take this distance and estimate a travel time between these wells on the order of thousands of years, and just leave it at that.

The eastern Yucca Mountain facies from what I'll call the headwaters up on the northern end where these fault controlled washes exist, you can see that the estimated ages are about 12,000, 13,000 years. There seems to be a fairly steady progression away from that point to 15,000 years at Borehole C-3. But, if you go further down the system, and I apologize, we plotted this based on UTM Northern on the X axis, so these wells are actually a little juxtaposed. WT-3 is actually west of J-13, and it is the next well along this flow path.

If you go to WT-3, you'll see that the groundwater age is about 2,000 years younger than it is in the upper gradient well. WT-3 sits at a fairly unique location. It is right near the intersection of where the Bow Ridge Fault intersects with the water table, and it's expected that there's a certain amount of recharge in that borehole. So, the cause of this younger age is probably mixing of recharge.

But, if you wanted to, you could probably take the difference in age between this short segment of the flow path and, again, make an estimate that travel time is on the order of thousands of years.

In western Yucca Mountain, I'll go through this one very quickly, but I gather you have a fairly good progression of the ages in these wells on Yucca Crest, which you could estimate travel time of, again, thousands of years between those points. But, if you tried to use the same technique and include these wells down along Highway 95, first of all, we're not sure of the flow path down there, and, secondly, the ages are again younger, probably as a result of modern recharge.

To help us improve our estimates of travel time and groundwater age, we have begun using dissolved organic carbon 14 ages to use in conjunction with the inorganic carbon 14 to try and look for evidence of water/rock interaction, and other things. While dissolved inorganic carbon 14 activities are altered by water/rock interaction, the dissolved organic carbon 14 should remain unaffected.

Dissolved organic carbon 14 measurements are still affected by mixing, by the introduction of recharge, and the presence of organic contaminants.

These are very preliminary results, and I think some of the members of the Board have seen this before. Unfortunately, we haven't been able to increase the size of this data base, but this is a plot of percent modern carbon from the inorganic carbon 14 on the Y axis, and dissolved organic carbon 14 on the X axis.

If these were plotting and giving us the same age, they were all plotted along this concordant line. And, you can see it for the wells in Yucca Mountain, they are mostly from the volcanic aquifer. It looks like there's very little correction that's going to be required to convert the inorganic carbon 14s into a true age.

But, when we get into the wells from the Nye County Drilling Program, which are these wells down here in green, and a group of wells in the Amargosa Farms area, you can see that they're quite variable, and there will be some considerable correction having to be made. Most of the organic carbon 14 ages are considerably younger than those predicted from the inorganic carbon 14.

So, conclusions are that the saturated zone waters near Yucca Mountain can be divided into six distinct hydrochemical facies that maintain their chemical and isotopic character over long distances.

The hydrochemical facies can be used to identify general flow domains and mixing relations between facies.

Water from the eastern Yucca Mountain facies may obtain its unique isotopic signature due to recharge through the fault controlled washes on the northern end of Yucca Mountain. This water then flows south/southeast until it eventually merges with the Fortymile Wash system.

Water from western Yucca Mountain facies flows south, at least as far as the southern tip of the mountain, and perhaps as far as Highway 95.

Although estimates of travel time over long distances based on carbon 14 ages are difficult, travel times within part of Fortymile Wash and part of the western Yucca Mountain facies appear to be on the order of thousands of years rather than tens of years.

Thank you. Well, I guess I do have one more slide. I'm sorry.

One of the questions the Board asked was what can we do in the next several years to increase our understanding of the system. We have several programs that are ongoing now that I think will help. Continued measurements of unsaturated zone pore-water chemistry in the deepest part of the unsaturated zone will help assess the nature of the UZ/SZ interface and refine our interpretations of flow paths away from the repository.

Continued refinement of the methods used to sample and analyze dissolved organic carbon 14, along with dissolved inorganic carbon 14, measurements and reaction path modeling will provide better estimates of transport along certain flow paths, which is work that we're doing under the Science and Technology Program.

And, then, continued investigation of three dimensional flow and the interface between the volcanic aquifer and the alluvium south of Yucca Mountain will help refine the interpretation of flow paths, and we're doing this as part of the Nye County program.

That's it.

BULLEN: Thank you, Gary.

In my introduction, I was remiss in not giving a little bit of background on you, so I've got to make up for that now, particularly since you graduated from the University of Illinois, where I now live.

After that graduation, you spent some time on the Sheffield side in Wisconsin for the USGS, and have been, for a very short time with the Yucca Mountain Project, since 1989.

PATTERSON: Yes.

BULLEN: And has been in planning the design of the C-wells complex, multi-well pumping test, and been a principal investigator on the pneumatic pathway and gas phase circulation in the ESF. So, I wanted to get that on the record before we asked if there were questions from the Board with respect to your presentation. We'll go to Thure Cerling, Priscilla, and Richard.

CERLING: Cerling, Board.

I was just wondering how your carbon 14 data in these water measurements can be interpreted in the light of how long things take to travel through the unsaturated zone. Is it compatible? And, part of that question, a follow-up question would be have you analyzed carbon 14 content of sort of deep soil gas in the unsaturated zone?

PATTERSON: We have analyzed carbon 14 in the deep gas phase. I'm trying to remember, it was a number of years ago, in the deep UZ, I think the carbon 14 ages that we came up with were on the order of, oh, 7,000 to 15,000 years, and I guess if we assume that the water picks up its carbon 14 as it passes through the soil zone, then we have to assume that the travel time in the unsaturated zone is sort of rolled up into part of this. But, there is a lot of gas phase exchange between matrix in the unsaturated zone, and water that's in the unsaturated zone, and in general, so that the actual carbon 14 age that reaches the saturated zone is possibly significantly altered by the time it reaches the saturated zone. Good enough? You look doubtful.

CERLING: No, no, I just understand it, I just wondered if you'd sort of tried to model the carbon 14 information with respect to transit in the unsaturated zone, and then once you're in the saturated zone, you're starting at some initial conditions. And, it kind of cuts to the question of well, when hits the unsaturated zone, what's the sort of zero age?

PATTERSON: I think Al Yang did a lot of calculations on gas phase carbon 14, and actually I did quite a few of them myself, but I was primarily involved in the ESF, and, so, they never really got linked from the surface down. And, the ESF, of course, is well above the saturated zone, so I'm not sure I could really give you a number. But, there have been efforts to do that.

CERLING: And, then sort of a related follow-on question, just for all of the data that you've presented, you didn't say in the very beginning over what sort of hydrologic interval that might represent. I mean, I presume it's all pretty much the same, but I wasn't sure.

PATTERSON: Three dimensions?

CERLING: Sure.

PATTERSON: Okay. Most of our data base is the upper few hundred meters of the saturated zone. The bottom investigation that you see there is to look at more detail in the three dimensional flow. We're embarking on an effort to use a model, it's called M-3, it was developed in Sweden, and it's a combination principal component analysis and kreeging model that will interpolate data between the various points that we have. We don't have a lot of control at depth in some of these areas. Nye County effort in the alluvium is adding to that quickly, and we now feel like there's enough data that we can pursue that effort.

CERLING: Well, then, in one of your earlier diagrams where you showed the sodium and the calcium and other plumes, I was just wondering how different the aquifer matrix was between different areas in that plume. Is it essentially the same?

PATTERSON: Well, there are differences. The Fortymile Wash facies flows primarily in an alluvial dominated system. The Yucca Mountain facies are both in volcanic rocks. As you move south of Highway 95, you're back into alluvium. Bare Mountain is greatly affected by Paleozoic carbonates. The Amargosa River facies is affected by Pre Cambrian quartzites. So, there are a variety of different aquifer materials in the area.

CERLING: Thank you.

BULLEN: Priscilla Nelson?

NELSON: Nelson, Board.

I've got two questions. One is over this time framework extending to 20,000 years, is anticipated past climate changes enough to affect interpretations of mixing or flow paths? Is that a possibility?

PATTERSON: Well, it's a possibility, but I think most of the models that we've seen over the last day and a half don't seem to indicate that flow paths have changed very much in the climate conditions. You can get more infiltration. We are probably looking at waters now that came from the events 10,000 years ago, or so. I don't think that, first of all, I don't think we have enough information to really assess that. Yes, I think it's possible that it could affect them, but the evidence that we've been given in the last couple of days, if you believe the models, the flow paths at least have not changed a lot.

One effect, if I may, is that as the water table rises, we may get changes in flow across faults that are now being considered as barriers.

NELSON: Right, it is complicated. Just following up on that, I was struck by the Center's models that showed, because it's a relatively low gradient for the flow underneath the repository, so the influence of change in precipitation right over the mountain can be significant in terms of causing flow. So, it seemed that that might be another factor that further complicates this.

PATTERSON: Yes, I think it would be. And, you're right, it's a very flat gradient.

NELSON: You want to go after the interpretation, but it's hard in this case, isn't it? Let me ask you one other thing. Have you done similar kinds of work that encompass the perched water zones?

PATTERSON: We have chemical analysis from the perched waters.

NELSON: Any carbon 14 analyses?

PATTERSON: Carbon 14 in the perched waters is somewhere in the 10,000 to 12,000 age range, fairly similar to that of Yucca Mountain, where we have good samples. Some of the problems in some of the perched water bodies, we haven't really been able to get what we consider a high quality sample, and, so, some of the ages look a little weird from that, and we don't like to really talk about them.

NELSON: Do you think that they would be informative? Could you get a good sample in trying to help figure out this question that Thure was raising about UZ?

PATTERSON: Some of the identified perched water bodies have started at the bottom in some of the UZ boreholes, and things like that, where we were able to use a bailer to pull up water, which was primarily mud, and probably fairly affected by the drilling. Those water bodies that are large enough that we could pump and collect a good sample from, we have already.

NELSON: Those would be interesting. Thank you.

BULLEN: Richard Parizek?

PARIZEK: Parizek, Board.

Thank you again for a good integration of your data sets. One question on the last bullet on what you plan to do, or ongoing activities. Will that allow some additional drilling dedicated drilling for this purpose? Because obviously all Nye holes or any new holes help, but the idea of multiple completions, the old West Bay idea, hasn't been carried on recently, I guess for funding reasons. And, so, the quality of information you would like to have isn't always available. You've had to kind of make do with often pretty mixed up samples with holes at variable depths, and different units, and sort of a (inaudible) data base. Do you include dedicated drilling for these refinements?

PATTERSON: The only drilling that's included in any of these studies is that that's conducted by Nye County.

PARIZEK: So, you piggy back onto that process.

PATTERSON: Right.

PARIZEK: Do you have a chance to weigh in, I hope, in terms of what you'd like to see on these deliberate pathways to finally, because you have a validation opportunity which can be value added to the program, providing you could feel comfortable with the location of wells, and where they're completed, and the flow channels as identified; right?

PATTERSON: Yes. I think we can at least discuss things with Nye County, and their new sonic drilling program may allow us to extract pore water from the consolidated cores and get depth discretized samples. And, if that program continues, and if it gets funded, I know they're interested in doing a lot more of that. And, if they do, then that's how we expect we might be able to expand that 3-D data base.

PARIZEK: Parizek, Board again.

Since you've been out in the program so long, you were a member of the TSPA/VA days, when the plume was wide and deep and quite dispersed, and then it got narrow, and it's sort of what's captured in presentations this morning. So, it's narrow, and I can't help but look at your figures on Page 3 and 4, and your chemistry smears along the Fortymile Wash area. Now, again, you could argue maybe there's infiltration occurring through time at different places, and it sort of broadens that whole thing out. But, what do you think? Do you think the narrow, not quite pencil line thin, present model runs are supported by your chemistry, whether you use the sodium, the calcium, or others? Can you help us with that?

PATTERSON: Do you know which page that is?

PARIZEK: What you've got is 3 and 4 of yours.

PATTERSON: I think the flow maps are probably around 6 or 7.

PARIZEK: The arrows are 10.

PATTERSON: The arrows are on 10? Well, the narrow flow paths along Fortymile Wash, I think are, in one way, they're a function of--they're model generated. And, if you assume each recharge along Fortymile Wash, then you have to assume that there's a hydraulic mound, no matter how small it is, there has to be a mound under Fortymile Wash. So, the model will now allow flow to go past that mound, or to go up gradient along that mound. So, I think it tends to take the flow from the west and just sort of slam it up against that hydraulic mound and then immediately drive it to the south.

I think what we envision, again, is these myriad flow paths coming across. And, I've got this drawn so that it sort of bumps into Fortymile Wash, and I think the spider diagrams indicated that there's mixing between those two, and I also think that there are wells, if we have the facies map up there, you'll see wells of eastern Yucca Mountain facies waters that are all the way down here. So, it doesn't all slam in a mix with Fortymile Wash. So, I think some of it does come in here and mix, and I think some of it does flow in this more southerly direction, and persist all the way down into this area here.

So, you know, whether that's a narrow flow path or not, I don't know. Again, it's a function of flow paths, and there are a bunch of particle tracks anyway, and, so, the relationship from the mixing at the margins of these facies is something that we don't have real good control of. And, so, I guess I'd have to say that I can't really argue with their representation of flow as this being probably the most important flow path that we're talking about right now in terms of a repository. And, I think if you're going to model it, I think that's probably the way you have to do it.

PARIZEK: Parizek, Board, again.

It's a idea of whether or not that mound would exist maybe during periods of intense runoff, episodic recharge, that might be there for a while, and then dissipate and reappear. So, that again, could create a lateral smearing basically of a plume, which is beneficial to diluting the plume. So, the details of that might be worth investigating if you really think it's a pen line right up against those two bodies of water. But, it may not be that. It may be spread out more.

PATTERSON: There's no reason to believe that that has been in the same place over long periods of time either. But, again, how we would go about investigating something like that, I don't really know.

PARIZEK: There was another opportunity, we've asked about, some years back, about whether you have any unique waters that, say, come out of like Crater Flat, that comes along with distinct signature, and then it mixes, and, so, which you can then begin to get some sort of dispersivity numbers from, long-term experiments. And, this was raised before. I guess there was no clear-cut places you could do that very well; right?

PATTERSON: I think the most unique feature that we found so far is Jim Paces finding of 234, 238 ratios, that seems to be a tracer of sorts. As a matter of fact, I think if we had a lot of three dimensional control and different, you know, additional boreholes, that could be used almost as a tracer.

PARIZEK: --dispersivity number, which the program really doesn't have right now. So, would there be value added if you could do that?

PATTERSON: Well, sure.

PARIZEK: It's just a question of whether you could do it?

PATTERSON: It would take additional boreholes, and it would take, you know, a fair effort. You know, we live in the real world, drilling a borehole out there is pretty expensive. So, I don't think it is really my place to stand here and say we need a hundred boreholes.

PARIZEK: But, if we take John Bredehoeft's recommendation, leaving it open for a thousand years, think of the manpower that it takes to guard that place. So, in a way, if a regional dispersivity number is useful, it's possible maybe to squeeze one out of this, out of years of work, and now you're at a point where if you could drill, you could probably optimize--

PATTERSON: I think that's true.

BULLEN: Bullen, Board. We're going to have to move along, Richard. And, Frank, we're going to ask you to delay your question until we have the roundtable--or the open forum discussion a little bit later, and maybe we can get a comment or two. Oh, he's not going to be available. That's right.

Okay, so Frank's is the last question. Or question? Two questions, but they have to be less than two minutes long.

SCHWARTZ: The questions will be short. It's the answers that will be long.

Gary, I had one question here with regard to the slugginess of the flow paths from the geochemical indication seem to be much slower than sort of the model calculations would suggest. I mean, is that your feeling as well?

PATTERSON: Yes. I think they are. I mean, I think because of the way the models have to use extremely conservative values for all of their parameters, they're going to be realizations that make the water just fly. But, I don't think we see, in the places where we can actually feel reasonably confident in our estimates, the travel time seems to be longer.

SCHWARTZ: The last question I have is with respect to where might there be young water? I mean, I guess I'm impressed by all the dating and even the corrected dating seems to rarely find young water. I mean, Number 1 and 2 in the north there seem to be young. But, even under Yucca Mountain, if you assume fast flow paths exist, for example, you would expect to find some young water there, and I guess it's my impression that there is no young water to be found from a geochemical perspective. Am I wrong there?

PATTERSON: Well, if you consider the amounts of recharge that we're talking about here, with infiltration rates of, you know, 50 millimeters a year, or whatever, and this huge reservoir of water that's already in the saturated zone that's a mixture of waters from older and younger, I don't think you would see real young water. I think A-1 and

A-2 are the closest thing that we've got to young water. If you, you know, dropped an ounce of water in a bathtub, an ounce of water with one characteristic in a bathtub, you probably wouldn't see it when you made the measurement.

SCHWARTZ: But, I guess to me, it kind of implies that this idea of these fast flow paths perhaps is not delivering very much water.

PATTERSON: No, and I don't think anyone--

SCHWARTZ: I guess that's where I was kind of going.

PATTERSON: I think the talks that you heard yesterday were the same, fast flow paths exist, don't exist, you know, there's a small amount of water and they don't really matter.

SCHWARTZ: Thank you.

BULLEN: Thank you, Gary. Moving right along, we have our next presentation by Stephanie Kuzio, who is the manager for saturated zone department for BSC, from Sandia National Laboratories. She has her civil engineering degrees, undergraduate and graduate from the University of Maryland, and she basically is a saturated zone manager for Yucca Mountain Project. She manage and coordinates the saturated zone technical activities related to the products for license application. And, she will be speaking about sorption, matrix diffusion, and colloid-facilitated transport in saturated zone radionuclide transport models. Stephanie?

KUZIO: I'm figuring out the technical difficulties here. My presentation today, will cover the three key transport processes that are included in the saturated zone transport modeling, and that's sorption, matrix diffusion, and colloid-facilitated transport.

I'd like to recognize the PIs on this work, and they are Sharad Kelkar, Paul Reimus, Arend Meijer, Hari Viswanathan, Rajesh Pawar and Mei Ding from Los Alamos National Laboratory, and from Sandia National Labs, Bill Arnold.

A brief outline of what I'll be discussing today. For each key transport process, sorption, matrix diffusion and colloid-facilitated transport, I'll discuss the conceptual model or approach, and then we'll look at the barrier capability of each one of these transport processes, and then I'll conclude.

So, at a very high level, matrix diffusion of dissolved radionuclides is implemented in saturated zone transport modeling in fractured volcanic units.

Particle tracking with the FEHM code is used to simulate the radionuclide mass migration in the saturated zone. The valley fill alluvium is simulated as a porous medium, using the effective porosity approach.

A linear sorption approach is used in the matrix of the volcanic and alluvium units. And, colloid-facilitated transport of radionuclides is simulated to occur by two modes, an irreversible mode and a reversible mode. And, I'll talk about those in further detail as we go through the talk.

This is a satellite image of Yucca Mountain. The yellow outline represents the repository. The model boundary is shown in this red border around, which is 30 by 45 kilometers. That's the site scale flow and transport model domain boundary. The blue line is Highway 95. The red crosses are well locations. Fortymile Wash comes down, shows up very nicely here on this plot. Crater Flats over in this area.

This is our hydrologic framework model, which the flow and transport model is based upon, Claudia discussed this morning. We have an orthogonal grid, and we have 500 meter spacing. There's variable resolution in the Y direction, or Z direction, and that is more finely discretized around the repository area, which is about up in here. The units that are most important to transport are the Crater Flat group, which consists of the Prow Pass, the Bullfrog and the Tram hydrogeologic units. Because the flow is within the first few meters, as some other speakers have pointed out, those are the units that are impacted the most. And, this cross-section down in the corner also reflects that. This is a cross-section up near the repository area. You can see a lot of these colors that are associated with these units.

You've seen this figure before. This points out the various different transport processes that we have in the saturated zone transport model. This is a north/south cross-section through the Yucca Mountain area. The proposed repository is shown here in black. The radionuclides will migrate down through the UZ, with the breaching of the waste packages. They will reach the water table, and enter into the volcanic aquifer, which we, our conceptual model is that this is a fractured medium.

The radionuclides then will continue to travel through this volcanic aquifer until it reaches the alluvium aquifer. This is an uncertain area where this contact actually is, and it's treated that way in our transport modeling.

Within our fractured medium, we have advection within the fractures, flow is within the fractures, and we have matrix diffusion occurring into the matrix block. Within the matrix block, we have sorption, sorption occurs within the matrix block.

This figure shows our schematic diagram of our matrix diffusion submodel geometry and assumptions. Our approach utilizes a dual porosity model with equally spaced, parallel fractures, as shown in this figure. The effectiveness of matrix diffusion is dependent upon various different things. The properties of the matric itself, and also the spacing of these flowing intervals, the zones that actually transmit groundwater flow. The closer those are together, the more matrix diffusion that can occur. The farther those are apart, the less amount, the less diffusion that will be able to occur.

This approach is fairly well based on results from the C-wells reactive tracer test, and that leads to my next slide.

These are results from the C-well reactive tracer test. The C-well reactive tracer tests occur, were done in the fractured volcanics, and pumping from well to well at a distance of approximately 30 meters.

The first thing to notice in looking at these figures is that we have two peaks, or two humps that occur in all of these results, and that's what's interpreted as that there was two advective pathways that resulted in those two humps.

The second important thing, looking at the PFBA and the bromide results, well, first looking at the scale, and this is a normalized concentration on a log scale on the Y axis, and it's logged as well on the X axis, and there's slight differences visually here, but this is a log scale. There's a difference between the PFBA and the bromide reactive tracer results, and the primary reason for that is the difference in the diffusion coefficients between the bromide and the PFBA.

The lithium, the peak is at the same time, but it's at a lower concentration, indicating that sorption is present as well as matrix diffusion. At later times, the lithium second peak actually occurs at a later time, which is what one might expect with absorption and matrix diffusion occurring at the same time.

The very last curve is microsphere results from the reactive C-well test, and they show a very similar fit. Microspheres are delayed. They break through a little quicker. Their peak concentration here is a little bit quicker than the others, but there is some attenuation that does occur there.

So, the important thing to note about this is these were modeled successfully including a dual porosity matrix diffusion approach. What is not on this figure is the last bullet, the preliminary, we have some single well tracer test results, which confirms a porous medium behavior.

This is to look at the saturated zone barrier capability for matrix diffusion. How much does matrix diffusion contribute to the delay of radionuclides through the system.

The solid black curve, which is right next to the dashed red one here, is our base case simulation, which includes a diffusion coefficient, but it includes no sorption. So, in comparing this diffusion case, the is a median value for a diffusion coefficient, and comparing that to a no diffusion case, which is the light blue dashed line, you can see that there is some benefit, some delay in spreading of the curve as a result of including matrix diffusion.

Now, to talk about sorption within the saturated zone and how that's implemented. We expect certain radionuclides to have sorption capabilities. There are some radionuclides that we've shown through testing that don't sorb, for example, technetium and iodine, and they have transported through the system without any sorption.

For radionuclides that do sorb, examples of that would be neptunium, uranium, we have a linear sorption approach that's used in the matrix of the volcanic and the alluvium hydrogeologic units.

We assume no sorption on fracture surfaces. The sorption that occurs, occurs within the matrix. We also assume geochemical conditions along the entire flow path length. The geochemical conditions assumed are oxidizing, as opposed to a reducing condition. If we had a reducing condition that we assumed, our Kd's would be higher, which would result in greater transport times.

There's many factors that influence sorption coefficients, so in order to capture the uncertainty for sorption coefficients, we've done that through probability distributions. We use a single value for a sorption coefficient for any one particular realization. We do, though, distinguish between the volcanics and the alluvium units.

The probability distributions for sorption coefficients include variations in water chemistry, in rock surface properties, and mineralogical compositions. The probability distributions are based on experimental data that we have available, and, then, professional judgment has been used regarding the impact of variables not considered in experimental program. There are some recent alluvium experiments that were conducted for neptunium, uranium, and the results from that, the Kd's, are corroborating the distributions that we currently have for neptunium and uranium.

There was also a study done to compare variability of sorption coefficients at the scale of the model grid blocks to variability at the lab scale. And, what we found was that the variability at the lab scale is less than the variability at the--the variability at the grid block is less than the variability at the lab scale. So, that was incorporated when we put together these probability distributions.

So, to look then at the barrier capability for sorption, the first curve, this is our same curve that we saw previously, this solid black line, the non-sorbing medium diffusing base case curve, and we're comparing that to three other curves that include sorption. The first curve is this little dashed red line, and that includes just matrix sorption. So, you can see we do--there is some increase in transport times there as a result of that.

When we look at just alluvium, there's a significant increase when we look at alluvium sorption added onto the models, this light blue dashed line. And, then, when we look at the combination of matrix and alluvium sorption, we're looking at on the order of I think it's approximately two orders of magnitude that we're able to accomplish. And, the Kd's that I used for that are listed here. Neptunium is a fairly moderately sorbing radionuclide. So, even for a moderately sorbing radionuclide, we show some significance, and times and spreading of the distribution.

Now, to talk about saturated zone colloidal transport, in the saturated zone at Yucca Mountain, we were aware that there are naturally occurring colloids. And, with the breaching of the waste packages, the degradation of the waste packages, we'll be adding, the system will be adding, the waste packages will be adding additional colloids to the system.

One of the first types of colloids which is shown, this figure is a conceptualization of these two modes of transport that are implemented in our model to represent colloidal transport, and the first type is this theory on this very bottom figure which is shown, this is to represent fractures, and this is our matrix here, and these are the irreversible type which they're created from the high level waste glass products as they degrade. And, here, the radionuclide is part of the structure. It will not, these types of colloids, the radionuclide is embedded in the structure and will not come off. And, I'll talk about the details of how that's implemented in our model in the next couple of slides.

Then, the other kind of colloid is this large brown ball here that you can see in the fractures, and that's our reversible type colloid. And, that type of colloid, we can have attachment and detachment of radionuclides onto it as it travels through the system.

Something to notice in this figure is that these colloids stay within the fractures. They don't diffuse into the matrix at all.

So, we have two types of colloidal transport, a reversible attachment to colloids and an irreversible attachment.

For irreversibly attached colloids, they are in equilibrium with the aqueous phase and the aquifer material. In this mode of transport, the effective retardation of these radionuclides during transport is dependent on three primary things. How strongly the radionuclide will sorb onto the colloid, the concentration of groundwater colloids available for those radionuclides to sorb onto, and then the sorption coefficient of the radionuclide onto the aquifer material as it moves through the system.

For irreversible colloids, radionuclides that are attached irreversibly are transported at the same rate as the colloid. Colloids with irreversibly attached radionuclides are themselves delayed by interaction with the aquifer material.

The implementation of that is we have a retardation factor that's applied in the volcanic units for irreversible colloids, as well as a separate retardation factor that's applied in the alluvium.

For these types of colloids, irreversible type, there is a very small fraction that is transported through the system unretarded, very quickly, as was mentioned yesterday in the UZ talks, and this phenomenon has been observed at the NTS site, a couple different field observations has confirmed this. So, we've included this in the modeling.

This slide describes the implementation of the different radionuclides that are transported colloidally through the system. Plutonium and americium can be transported in two ways, either irreversibly or as a reversible colloid. That's why you see them in both sections there. So, plutonium and americium are two radionuclides that are transported irreversibly, and then plutonium, americium, thorium, protactinium and cesium are treated reversibly, and they're done in this manner down here in these three bullets.

Cesium and plutonium are transported separately, and then americium, thorium, and protactinium are transported in one group, the Kd's on, so the colloid are very similar, so we've grouped them together.

Again, this is our same, this is the barrier capability for colloids, and this is our same base case curve again, which we're comparing to a median value for irreversible retardation in the volcanics and the alluvium. And, here, the barrier capability is approximately an order of magnitude, with inclusion of irreversible colloids.

So, to summarize, the key transport processes included in the saturated zone transport model are matrix diffusion, sorption and colloid-facilitated transport.

Matrix diffusion delays transport times and spreads the arrival times of radionuclides. Sorption in the alluvium can increase the transport times by orders of magnitude for even weakly sorbing radionuclides such as neptunium, the example we looked at. And colloids irreversibly and reversibly bound to radionuclides may be delayed by several thousand years.

That's it.

BULLEN: Thank you, Stephanie. I'll start with questions from the Board. Dr. Nelson first?

NELSON: I'm always the first one. Have you noticed that.

I want to ask just a general question. At some point in the past, this is Nelson, Board, we had a lot of discussion about whether the water, the saturated zone water, was reducing or oxidizing. So, I'm wondering what the current thinking is of that, and whether that has any impact on transport.

KUZIO: Right. I meant to talk about that a little bit as I went through. We've assumed the oxidizing conditions. We do have some results from testing at different wells that does indicating reducing conditions, but it's not consistently shown everywhere, and we have a limited data set.

NELSON: Okay. So, you're assuming oxidizing because it's conservative?

KUZIO: Correct.

NELSON: But, it might be reducing?

KUZIO: In some areas, it's shown that it is reducing. The full story isn't in yet.

NELSON: Okay. Let me ask about one other thing. The accent on matrix and flow through rock mass, fractured rock mass, what if the flow is really in a fault zone through the rock mass, what part of your story would be different, how well do you have the fault material characteristics characterized? Has that been the subject of thinking?

KUZIO: Well, I would say that's more in the flow area, and that the faults have been represented in the flow model, either as people have pointed out here previously, some of those faults are barriers to flow, and some of those faults are conduits for flow.

NELSON: Nelson, Board.

So, if one is a conduit, and it's taking quite a bit of flow, the character of the transport might be quite different than if it were through the rock mass; is that true?

KUZIO: Right. And, you've seen the results that Ken Rehfeldt presented earlier that show--I mean, we've got a tremendous uncertainty in our parameters. We've got some very early breakthrough times that may be representative of that sort of thing. Bill Arnold will probably talk about this to some degree.

NELSON: Oh, we're going to have to talk to Bill for a long time.

KUZIO: Yes, I know, long awaited discussion with Bill.

NELSON: We've been deferring a lot of things to Bill.

KUZIO: Bill is our point man.

NELSON: Okay, then I yield to Bill.

KUZIO: Okay.

BULLEN: Rien van Genuchten.

VAN GENUCHTEN: Yeah, I have two different things I want to raise here. One is on your Page 8, or Slide 8, when you talk about matrix diffusion. We all agree about the conceptional picture. I'm curious how this was implemented in your model, if you use actually diffusion integrations, or you use a first order exchange, or how did you do this?

KUZIO: Well, Bill will talk about this probably in a little more detail. There is a particle tracking that is implemented that utilizes the Sevougian equation. Does that help at all? That's how the diffusion moves along into the matrix and diffuses back.

VAN GENUCHTEN: So, how does the matrix eat away from the concentration and the fractures? How is that implemented?

KUZIO: Eat away from the fractures?

VAN GENUCHTEN: Well, the diffusion loss.

KUZIO: I'm not sure I understand. The motion, how that's included? I'm sorry.

VAN GENUCHTEN: The mechanics of how does this being modeled, I'm curious about. You say this is done with particle tracking. So, you have actually particles moving into the matrix?

KUZIO: Well, simulated--I'm probably not the best person to answer that. But, yeah, the particles move along through each grid cell, and depending on, there's a library of breakthrough curves that are used based on the Sevougian equation that moves the particles along, also the concentration, it randomly moves them into the matrix and out again. The dual porosity model, I'm not giving it what you're looking for.

VAN GENUCHTEN: Okay. Then I have a few questions about the colloids. I was a bit confused. First of all, you have, you say, reversible colloids, attachment and detachment. Is that considered to be filtration, or absorption?

KUZIO: You're referring to reversible?

VAN GENUCHTEN: The reversible.

KUZIO: They attach to--they attach and detach from the colloid themselves. What was the second part?

VAN GENUCHTEN: Yes, is this done in a kinetic way so there will be a forward and a backward rate degradation?

KUZIO: That isn't how it's implemented in the model, no, we don't include kinetics in that model, no. It's an effective retardation, basically, is how it's implemented.

VAN GENUCHTEN: So, that's on your Slide 15, you stated it, so you use an equilibrium process for that.

KUZIO: Correct.

VAN GENUCHTEN: That's right. And, then, for the irreversible ones, are you using a sink drum, like a first order rate degradation for that?

KUZIO: Again, there's retardation factors that they looked at, those retardation factors were based on looking at attachment and detachment rates, but they came up with retardation factors that we could then apply essentially as an effective Kd through the system. So, it's not explicitly included. And, we don't filter colloids. We don't lose any physically. I didn't make a point of that either.

VAN GENUCHTEN: Okay, thanks.

BULLEN: Frank Schwartz, and then Richard Parizek. Frank gets to talk before you, Richard.

SCHWARTZ: Thank you, Schwartz.

The question I had for you, maybe if you go back to Slide Number 8, the direction there, 2B, the distance between the fractures, is that a large number to reflect this idea of flowing intervals, such that that number would be 10 or 20 meters, say?

KUZIO: The distance between the zones that we consider flowing is 20 meters. And, that's our--

SCHWARTZ: Yes, that's okay. And, that's the way the model is set up?

KUZIO: Yes.

SCHWARTZ: The question I had for you was that I wonder if there's correlation among the variables that you're using. For example, when you create a head field and you need a velocity out of that head field, you need an effective porosity number, I'm wondering do you select the effective porosity randomly, and then select block size randomly as well, or is there correlation among that pair of variables?

KUZIO: Effective porosity in the alluvium?

SCHWARTZ: Well, effective porosity in the fractured rock.

KUZIO: In the fractures. Those are separate samples independently.

SCHWARTZ: Okay.

KUZIO: They're not correlated.

SCHWARTZ: Because you could run into a situation where if you chose a big effective porosity, you know, a fairly small effective porosity could imply big 2B, and yet I guess, you know, if the effective porosity was, say, .01, that would imply 2B was a lot smaller, because there would be a lot more fractures.

KUZIO: Right.

SCHWARTZ: So, that was always an issue that was one of my sort of pet peeves along the way, was the sort of lack of correlation among the obvious verticles. I just wondered whether you've been handling it.

KUZIO: We're still sampling those independently.

SCHWARTZ: Okay. I think that's it for me. Thanks.

BULLEN: Richard Parizek, and then David Diodato gets the last question.

PARIZEK: Parizek, Board.

Sonic drilling methods were used in perched factor core recently by Nye County, and some of the classes that came up were highly decomposed, chemically altered, would not have been preserved in rotary methods of drilling. But, obviously, they have a matrix diffusion possibility. The program I guess did not take any credit for matrix diffusion in the alluvium, is that correct?

KUZIO: That's correct.

PARIZEK: Even though it looks like you could get credit for it based on what we see from the samples that came out of that core.

KUZIO: Well, there was one bullet where I did talk about some testing that was done at the ATC. These are preliminary and fairly new, where they did not see matrix diffusion in the alluvium. That was my goal, continuum most appropriately.

PARIZEK: Okay. Parizek, Board.

For some of the variables that were not included in testing, you did use external peer review process in the past, technical basis document refers to this. I was wondering whether or not the program intends to do that again with regard to the newer data that may have come out of the alluvium testing, you know, some of the single wells, some

cross-well testing.
KUZIO: In terms of our expert panel?
PARIZEK: Right, trying to get anymore data out of what exists here to help constrain your models. Do you know if that's planned?
KUZIO: Planned to have an expert elicitation?
PARIZEK: No, like reviewing the data base, basically, with external opinion.
KUZIO: At this time, that isn't planned.
BULLEN: David Diodato.
DIODATO: Diodato, Staff. Thanks for this presentation.
You know, in your talk, and in other talks, and then sneaking ahead and looking forward at Bill Arnold's talk, it seems clear that the program has an idea that matrix diffusion might in fact be fairly significant and an important process in terms of radionuclide transport. And, you have this flow and interval spacing, and I guess there's some field measurements that go with that? There's some field tests and observations you've made to determine this flow and interval spacing; is that correct?
KUZIO: Yes. Flow meter surveys were primarily used. We have a limited data set on that, but we looked at USGS borehole reports, flow meter surveys, and basically did a statistical analysis looking at the spacing between the zones

that were flowing.

DIODATO: Thanks. Diodato, Staff.

Just to follow up on that then, how many measurements do you have of flow and interval spacing?

KUZIO: If my memory serves me, it was a while ago, it was about 27 data points that were used to determine the distribution for the flow and intervals. But, we've made some very conservative assumptions in how we did that. I mean, we couldn't distinguish from the results that we got from the GS flow and meter surveys, which in non-fractured zone, which fractures are flowing in that zone. It could have been one, it could have been many. So, we said, okay, we're going to say it's in the dead center, there's one zone that's flowing in the center of that interval, and that's really a pretty conservative assumption.

DIODATO: Thank you.

BULLEN: Thank you, Stephanie. I feel like I'm the person who calls for the commercial just before the announcement of the winner of the best picture at the Academy Awards. So, what I'm going to do is I'm going to make this a twelve minute break, which means everybody is back here at 2:45, because we all want to hear what Bill Arnold has to say.

(Whereupon, a brief recess was taken.)

BULLEN: Bill Arnold has worked in the area of performance assessment at Yucca Mountain project for nine years. He's been involved in numerical modeling of groundwater flow, contaminant transport, and probabilistic risk assessment for several programs at Sandia.

Prior to this, he worked in hydrogeologic research at the Kansas Geologic Survey, and in the mineral exploration industry. Bill?

ARNOLD: I'd like to thank Dr. Bullen for working up the suspense here on this talk. First of all, I'd like to say that this work is sort of the synthesis of a lot of work that's gone sort of upstream from this modeling, the saturated site scale flow modeling development and calibration. Just to mention a few people, George Zyvoloski at Los Alamos National Laboratories. The site scale transport modeling, Shared Kelkar at Los Alamos. Perimeter uncertainty analyses that went into this also is Stephanie Kuzio and Kathy Economy at Sandia. And, then, the abstraction and interface with the total systems performance assessment work by Elena Kalinina, Greg Roselle and Dave Sevougian, and others out here in Las Vegas.

So, if we could have this next slide, this is an outline of the talk. I'll start out with an overview of the approach taken to the total systems performance assessment and the abstraction for saturated zone flow and transport. We'll go over the assumptions in that modeling approach and the implications of those assumptions. We'll talk about uncertainty, saturated zone flow and transport for these TSPA analyses. I'll show some of the modeling results, and then we'll talk about a sensitivity analysis that was performed on saturated zone flow and transport modeling, this subsystem of the TSPA.

This slide is a diagrammatic representation of the saturated zone component of the TSPA, and I show it principally to point out the connections between the saturated zone and other components of the analysis. Radionuclides escaping from the repository would be transported principally vertically downward in the unsaturated zone to the water table, then primarily laterally in the saturated zone, where they would be available for discharge to the accessible environment and biosphere at some point downstream.

As has been discussed earlier, flow is primarily through fractured volcanic rocks beneath the repository and upstream in the system. At some point downstream in the system, that transitions into flow through porous medium of the alluvium. And, with regard to some of the specifics in how these components are linked together, radionuclides arrive at the water table, are put into the saturated zone model. At a point source in four regions beneath the repository, there was some discussion of this earlier, there's uncertainty in where that point source would be located. And, that uncertainty is incorporated into the analysis, and that point is moved around from realization to realization.

Also, the radionuclides are placed into the fractures of the saturated zone. This is a conservative approach. Radionuclides in the unsaturated zone model are transported both in the fractures and in the matrix. The way the modeling is performed in the saturated zone, there's not a way to numerically distinguish between the fractures and the matrix, and it is conservative to place that radionuclide mass flux at the water table into the fractures.

However, it's probably not as conservative as it might sound at first glance, because early in the repository history, the first arrivals from the unsaturated zone will be principally in the fractures of the unsaturated zone, which would link up presumably with fractures in the saturated zone.

In the interface with the biosphere at the downstream end of the saturated zone, all of the radionuclides that cross the boundary of the accessible environment are assumed to be dissolved in a representative volume of groundwater at that location, concentration calculated from those two inputs, and that is the concentration of radionuclides in the groundwater that would be used by the reasonably maximally exposed individual in the biosphere.

The general approach that is used here is that for the transport abstraction in the TSPA, is we use the three dimensional saturated zone site-scale flow and transport model to simulate radionuclide mass transport to the accessible environment from a point mass source, as I described earlier.

We use the convolution integral method to couple radionuclide mass source term from the unsaturated zone and the saturated zone in the TSPA calculations. This convolution integral, you can think of as a numerical shortcut, and there are, I'll explain this in more detail in a minute, and I'll also explain the assumptions that go into this method of coupling the two models. But the motivation behind this is that it allows us to run this fairly detailed three dimensional site scale flow and transport model ahead of time for multiple realizations of flow and transport, save the results from those rather complex model runs, and couple them to the TSPA analysis through this numerical shortcut of the convolution integral.

Radionuclide concentration in groundwater source to the biosphere is calculated by dividing this radionuclide mass crossing the boundary of the accessible environment by the 3000 acre feet per year, as I also described.

Climate change is incorporated by scaling the radionuclide mass breakthrough curves in proportion to the flux changes in the saturated zone with climate change. So, the model simulations are done for present climatic conditions. Then, in the TSPA modeling, at the time of climate change, those breakthrough curves that were derived for the present climate, are scaled by that factor of the increase in the groundwater flux in the saturated zone.

To give you the kinds of numbers that are involved here, our estimate is that for monsoonal climate conditions, that multiplication factor is a factor of 2.7 times higher. For glacial transition climate conditions, it's 3.9 times higher groundwater flux in the saturated zone.

We also have a separate model which is an abstracted one dimensional transport model, and this is used for radioactive decay chains. The three dimensional site scale model does not include the process of in-growth of radionuclides, only of decay, and for several radioactive decay chains, we use this 1-D model to calculate the concentrations of the daughter products at the downstream end of the saturated zone.

This is a figure that represents the model results. The figure here shows a satellite image draped over the topography, and it is shown above the water table surface below it. The repository is located about here. Stephanie Kuzio pointed out some of the features in the site scale model domain earlier.

The lower part of the figure has projected onto the water table surface the tracks of, the particle tracks from the numerical model.

One thing I should point out here that I don't think has come through in the previous talks is from beneath the repository out, down gradient through the system, there's a significant convergence of groundwater flow in the system. We have a gradient that comes in from the west across the faults to the west and to the south of the repository site, and a gradient that comes in from the east. So, it is a convergent flow system. And, there's a significant increase in the average groundwater flux or specific discharge along this flow path from beneath the repository to the boundary of the accessible environment, increasing by a factor of approximately five over that distance.

The particle tracking method that's used includes all of the transport processes that Stephanie discussed earlier, advection, of course, dispersion, matrix diffusion in the fractured volcanic units, and sorption. And, let me take this opportunity to try to answer one of Dr. van Genuchten's questions about the algorithm that's used to implement the matrix diffusion with the particle tracking.

I haven't really prepared figures to describe this in detail, but this is a particle tracking method that uses a continuum representation of the fracture network. Linked to that is the analytical solution for matrix diffusion out of multiple uniformly spaced parallel fractures, using the Sudicky and Friend analytical solution.

And, the way this algorithm works is for each time step--not time step--for each step of the particle through the system, it travels some small distance through the cell. We know what the groundwater velocity in the fracture is. We know what the spacing of the fractures is. We know what the diffusion coefficient is. All of the parameters that go into this analytical solution for matrix diffusion.

So, for that step of the particle, we can derive a distribution of possible transport times between Point A and Point B for that particle in the system. Then, we draw a random number, uniformly distributed between zero and one, and we go to that distribution of possible transport times, taking into account matrix diffusion, and we advance the particle in time for that spatial step by that amount of time. And, we do this over and over again for multiple particles, using small spatial steps through the system, and it reproduces the analytical solution.

VAN GENUCHTEN: Thanks.

ARNOLD: A couple of other notes. The simulated flow paths from the repository occur in the upper few hundred meters of the saturated zone, and Ken showed those results earlier.

The simulated flow paths cross the boundary of the accessible environment about five kilometers west, northwest of the highway intersection of the Amargosa Valley. So, approximately in this location right here, it's very close to Nye County Well 19, is where the model simulations indicate that this simulated plume would cross over into the accessible environment.

This is a diagram that illustrates the convolution integral method. The three dimensional site scale model is given an assumed step input for mass into the system, and then the model is run to derive a breakthrough curve at the boundary of the accessible environment. And, as I mentioned earlier, this is conducted for many different realizations of the system, for realizations of uncertain parameters. It's conducted for the four different source regions, and it's conducted for the multiple classes of radionuclides that are simulated, for which transport is simulated.

All of these breakthrough curves are stored as a library of breakthrough curves then. So, this is done outside of the TSPA calculation itself. The dashed line shows what occurs within the TSPA calculation. Within that calculation, the unsaturated zone transport model is run, and generates the output of radionuclides at the water table below the repository as a function of time. So, this signal of output for radionuclide mass from the unsaturated zone is convolved with the breakthrough curve from the saturated zone using the convolution integral method, and this time varying output of radionuclide mass at the accessible environment is the output from the convolution integral that then goes to the biosphere model.

Let me describe at kind of a high level the one dimensional radionuclide transport model. This is used to simulate the transport of four simplified decay chains. This is a 1-D representation of the system, and it's implemented directly in the TSPA model using the GoldSim software with the pipe module. This is a module which can track radionuclide decay and in-growth, as well, this 1-D model includes all of the relevant transport processes that we have in the one dimensional model, matrix diffusion, sorption, colloid facilitated transport. These are all done in a manner that's consistent with the 3-D model.

However, it's still an abstraction, because it's a dimensional simplification. It's not able to capture all of the complexity of the 3-D model.

This figure on the left shows which radionuclides are simulated to be transported within the different models. In the 3-D model, we simulate the transport of all the fission products, and we simulate the transport of these parents in the decay chains, the americium, plutonium, and uranium.

Within the 1-D model, we of course have to start with the ultimate parent of each decay chain, and we simulate the entire decay chain in the 1-D model. However, the output of the 1-D model in the TSPA only uses this portion of the decay chains, only outputs this portion of the decay chains. The upper end of the decay chains are simulated in the 3-D model.

I should also note that a couple of end members here, I think there's radium 226 and actinium, are calculated to be in secular equilibrium with their parents in the TSPA calculation.

This is a comparison between the one dimensional model and the three dimensional model, just to give us confidence that the one dimensional model is an adequate representation, and we do see that the one dimensional model gives an accurate depiction of transport through the system for a wide parameter range.

This figure shows the results of the 3-D model as the symbols, compared to the 1-D model, which are the lines here, the dashed lines and the solid line here, for three different cases, a fast case, a median case, and a slow case for simulated neptunium transport. So, we do get very good agreement between the 1-D model and the 3-D model.

I should note here, though, that we would not expect this good an agreement for all realizations of the system. This comparison was constructed for a source location in the center of each one of those four source regions beneath the repository, and we're not able to capture all of the variability in the 1-D model, and one type of variability that we don't capture is the variation in the flow paths and the flow path lengths when that source region at the upper end of the saturated zone model varies from realization to realization.

Let's talk about some of the key assumptions for the TSPA with regard to saturated zone flow and transport. We're assuming steady-state groundwater flow in the saturated zone, and this has been discussed to a certain extent earlier. This is an assumption that is probably adequate for the system. Has not been observed to be a large degree of transients in the water levels, at least along the--near Yucca Mountain or along the flow paths down gradient.

We also assume in instantaneous change in the saturated zone groundwater flux with climate change. This may be a significantly conservative assumption. As the TSPA goes through times, we go to wetter climates, so the groundwater flux in the saturated zone increases with these climate changes.

In reality, there would be some kind of a transient response in the saturated zone system. It would take some time for increased recharge to reach the saturated zone for fluxes to increase in the saturated zone, but we're conservatively assuming that these instantaneously increase in the saturated zone.

We also assume that there's no change in the flow paths. This has been substantiated to a certain extent by some modeling with the USGS regional scale model where they did simulate glacial climatic conditions, and there was no real significant change in the flow paths from beneath the repository.

The matrix diffusion model is assumed to occur from uniformly spaced parallel fractures in the fractured volcanic units, as implemented in the Sudicky and Frind analytical solution. This is an obvious idealization of the fracture network system. However, we have significant uncertainty in the input parameters to this that cover a range of behavior with regard to matrix diffusion.

There's also a potentially significant conservatism associated with this approach, too. We're implicitly assuming that flow only occurs in the fractures of the system. If there were to be significant flow through the matrix of some of the volcanic units, this would violate this assumption, but it would also lead to longer transport times and greater sorption in the matrix through which this advective flow could possibly occur.

The next bullet has to do with the boundary condition with the biosphere, which is assuming that all of the radionuclide mass is contained in this representative groundwater volume usage of 3000 acre feet per year. This is probably a reasonable assumption. This is a large volume of groundwater for pumpage on an annual basis, and could easily capture the entire contaminant plume from beneath the mountain. And, then, that the average concentration in this volume is released by pumping to the reasonably maximally exposed individual in the biosphere. And, this is an assumption that's really based on the regulations.

We're assuming equilibrium linear sorption occurs in the tuff matrix and the alluvium. Stephanie discussed this to a certain extent.

We're assuming that for the transport of radionuclides that are reversibly attached to colloids, there's local equilibrium among the colloids, the aqueous phase and the aquifer material. So, this is assuming a rapid sorption and desorption of radionuclides onto the colloids themselves, and onto the aquifer material.

For radionuclides that are irreversibly attached to colloids, it's assumed that there's no desorption of those colloids during transport in the saturated zone. This is a conservative assumption. Laboratory measurements suggest that there will not be a breakdown of these colloids or a desorption of the radionuclides that are embedded within the colloids. But, for the very long time periods for transport through the natural system, it's not entirely clear that that's a valid assumption, but it is conservative.

Colloids are subject to attachment and detachment from the mineral grains, but no permanent filtration of the colloids occurs. So, these colloids with the radionuclides that are embedded within them, once they enter the saturated zone, they're not permanently filtered out of the system.

They will eventually come out at the downstream end. So, this is obviously a conservative assumption also.

This is a slide that summarizes the uncertainty in the saturated zone flow and transport modeling. I have broken this down into uncertainty in groundwater flow and geological uncertainty. And, the parameters, the individual parameters of interest here are the groundwater specific discharge. We have uncertainty in how fast groundwater is moving through the system. Horizontal anisotrophy in the permeability within the fractured tuffs is an uncertain parameter. This is significant because we do have an anisotrophy in the permeability in the volcanic units. This could steer the path of the plume through different flow paths towards the accessible environment. We have geological uncertainty in the alluvium tuff contact in the subsurface. This uncertainty has been reduced to a large degree thanks to the Nye County Drilling Program, but we still do have a certain amount of geological uncertainty included with regard to this.

Now, for radionuclide transport, we have uncertainty with regard to matrix diffusion in the fractured tuffs, and there's several underlying parameters that are uncertain. The flowing interval spacing that we discussed earlier, the effective diffusion coefficient in the tuff matrix, and the flow porosity in the tuff.

We also, obviously, have uncertainty in the sorption coefficients for the different types of elements in the tuff matrix and in the alluvium. Dispersivity, both longitudinal and transverse dispersivity, effective porosity in the alluvium, the source location, the colloid retardation factor. Stephanie mentioned this is a different distribution in the tuffs and in the alluvium. We have uncertainty in the sorption coefficients onto colloids, and uncertainty in groundwater colloid concentration.

Just to give you an example of how we assess uncertainty in a particular parameter, and this turns out to be a relatively important uncertain parameter in the analysis. This is a CDF of our uncertainty in specific discharge, where this is cumulative probability on the Y axis, and on the X axis is the log of the groundwater specific discharge multiplier. So, in log space, a value of zero is our median value. This would correspond to our calibrated flow model, our expected base case for flow through the system.

However, we do have uncertainty that goes as high as one order of magnitude higher, so ten times higher than expected, and to some value of something significantly less than one order of magnitude lower. And, the shape of this cumulative distribution function for our uncertainty is based on results of the saturated zone expert elicitation, and on more recent well testing at the alluvial tracer complex. And, we have combined information from both of these sources.

The saturated zone expert elicitation panel had a relatively broad distribution of uncertainty in specific discharge beneath the repository. And, that basically defines the bounds of this distribution.

There were several factors that went into their uncertainty in what the specific discharge would be to the system. There's a certain amount of uncertainty in the hydraulic gradient through the system, but that's relatively minor. Most of their uncertainty was attributed to uncertainty in permeability, fracture permeability or bulk permeability, in the volcanic units.

Now, since the saturated zone expert elicitation, there has been this well testing at the alluvial tracer complex, and Ken mentioned earlier that we got fairly good agreement between the interpretation of the well testing at the alluvial tracer complex, and what our flow model was predicting before those tests were conducted.

So, we viewed this as not only a confidence building, a certain extent, a validation for the model, we also took this as an indication of reason to decrease our uncertainty in specific discharge through the system. And, the bulk of this uncertainty distribution, 80 per cent of our uncertainty, falls between these two points that are 1/3 times the expected value of specific discharge, and 3 times our expected value of specific discharge. But, of course, we left these tails on the distribution that are taken from the saturated zone expert elicitation.

So, this is the kind of thinking that goes into development of uncertainty distributions for some of these parameters.

Now, the uncertainty analysis itself, and I'm sure many of you are already familiar with how this kind of a probabilistic analysis is conducted, but it's a Monte Carlo analysis in which we sample the uncertain parameters using a Latin Hypercube sampling method, which is the method implemented in GoldSim.

We produce multiple simulations, in this case, 200 equally likely realizations, of groundwater flow and radionuclide transport in the saturated zone, using these uncertain parameter vectors in the 3-D SZ site-scale model.

These radionuclide transport simulation results consist as radionuclide mass breakthrough curves. And, this resulting library of breakthrough curves is used in the TSPA model via the convolutional integral method.

Okay, these are some of those results for the 200 realizations of our uncertainty in groundwater flow and transport in the saturated zone. This is for non-sorbing species. So, this would be for carbon or technetium or iodine. And, you can see that the results vary over several orders of magnitude. Many of these breakthrough curves exhibit a long tail that's characteristic of diffusive mass transfer in the rock matrix of the volcanic units.

Plotted below is a histogram of the .5 breakthrough point, or the median transport time among these realizations. So, we get this kind of a distribution for median transport time. And, this red dashed line is the median of the medians. It's on the order of 650, 700 years.

Now, let me take this opportunity to answer another question that came up earlier, and that was about these few realizations that exhibit very low, or very short transport times through the saturated zone. And, I should emphasize again that these are transport times through the saturated zone, so this is release of the mass at the water table beneath the repository in the saturated zone.

First of all, to look at this in the context of a probabilistic assessment, these are all equally likely realizations of the system. So, these realizations that exhibit very rapid transport are unlikely. We only have a probability of a few percent, less than 5 per cent that these transport times would be less than 100 years. So, you have to look at these results in the context of an uncertainty analysis.

But, to explain what goes into these results, we haven't examined these on a realization by realization basis, but it's easy for me to see what goes into it. These would be cases for which we have a relatively high specific discharge. We have a relative high value of anisotrophy in permeability in the volcanic units, which would tend to steer the flow paths in a more north/south direction, which results in less--or shorter flow path length through the alluvium. They probably also correspond to very minimal matrix diffusion, which is some combination of the parameters that influence matrix diffusion, low diffusion coefficient, large spacing between the flowing intervals, so highly channelized flow in the volcanic. And, then, finally, a potentially low effective porosity in the alluvium itself.

So, you have combination of fairly unlikely values for individual parameters that taken together, result in these realizations with the short transport times simulated through the saturated zone.

Now, these are the results for the transport of neptunium through the saturated zone. Again, under present climatic conditions, I should note that all of these results that I'm going to show you are for present climatic conditions. And, so, neptunium is moderately sorbing in both the volcanic matrix and in the alluvium, somewhat higher sorption coefficient values in the alluvium than in the volcanic matrix. And, the variability among these transport times extends from less than 1000 years to greater than 100,000 years. And approximately half of these realizations exhibit median transport times of greater than 20,000 years in the saturated zone for present climatic conditions.

And, again, you see the histogram of the median transport times among all these realizations shown at the bottom.

And, just kind of to round out, the range of behavior among these radioelements, these are simulated breakthrough curves for Cesium, and if you recall, Cesium is transported via colloid facilitated transport, the reversible colloid facilitated transport. So, Cesium is subject to sorption onto colloids. It's also subject to sorption onto the aquifer material. However, Cesium is very strongly sorbing onto the matrix of the volcanic units, and in the alluvium.

It has a relatively high sorption onto the colloids themselves also, but still, taken in aggregate, we have, most of these breakthrough times are out beyond 100,000 years. And, of course, given, for Cesium 137, given the relatively short half-life of Cesium 137, there's essentially a zero probability of breakthrough of Cesium 137 in the saturated zone, as predicted by these model results.

I wanted to talk about sensitivity analysis of these simulation results. You've seen some sensitivity analyses that were presented by the previous presenters which looked at single model realizations that try to illustrate the sensitivity of the model to particular parameters or processes. This is going to be a little more complex sensitivity analysis. It looks at all of the results in aggregate from this probabilistic assessment, and in that sense, gives us more of the sensitivity analysis information all at once. But, it's a little more complex to understand.

But, this sensitivity analysis does provide us with information on the relationships between our uncertainty in individual input parameters, and our uncertainty in model predictions. And, in this case, we're using the median transport time from the simulated mass breakthrough curves as the dependent variable. So, the mid points of those simulated breakthrough curves are what we're taking as our model predictions that we're going to conduct the sensitivity analysis on.

And, this kind of analysis can provide us with an enhanced understanding of the model behavior, and also valuable information for strategies to reduce uncertainty in the model predictions.

The method that's used is a stepwise linear regression. And, just to explain this in a very summary fashion, this method constructs a series of multiple linear regression models that relate the uncertain parameters to the model predictions.

The stepwise process adds the most important uncertain parameter first to the regression model. So, the first regression model only includes one independent parameter. The second step includes the top two uncertain parameters, and the third step includes the top three. So, you build up this series of multiple regression models.

What comes out of this is delta R squared. This is the change in the coefficient of determination with the addition of each new independent variable to the regression model.

So, what's plotted here is the delta R squared for a number of uncertain parameters for a number of the radioelement classes here. So, these different radioelement classes are shown by the different colors or along this axis.

For example, for technetium, or this could be carbon or iodine as well, this is this first row of results here, and I should point out that on the next slide, there's a key that's given that describes what each one of these input parameters is, but what you can see is that this parameter, GWSPD, which is our uncertainty in the groundwater specific discharge, is the most significant parameter for uncertainty in model predictions with regard to technetium transport through the system. And, it has a value of about .65, and one way you can interpret this is that about 65 per cent of our uncertainty in the model predictions is accounted for by our uncertainty in the groundwater specific discharge input parameter.

So, it's a very important parameter, and if you look across these radionuclide classes, for many of them, this is the dominant uncertainty in the system, our uncertainty in just how fast groundwater is moving through the system is a dominant uncertainty. And, this kind of reflects back on some results that George Moridis showed yesterday with regard to transport through the unsaturated zone. I think that a similar result there also, they have this high infiltration case, expected infiltration case, and low infiltration case, and he showed the transport predictions varied over, you know, a couple orders of magnitude for those different infiltration cases. And, that was the greatest sensitivity that he showed. So, I think we might have kind of a similar result in the unsaturated zone and saturated zone here with that regard.

The second most important parameter for technetium transport is FISVO, which is the flowing interval spacing in the volcanics. This is that spacing between features that conduct significant amount of groundwater in the volcanic units. So, the degree to which groundwater flow is channelized in the saturated zone. And, about 10 per cent of our uncertainty in model predictions is associated with that parameter for technetium.

HAVO is the horizontal anisotrophy. So, this is that steering of the radionuclide flow paths, or the particle flow paths. That does have a small but significant impact on our uncertainty in the predictions.

And, then, NVF19 is the uncertainty in effective porosity in the alluvium.

Now, if we move to something where sorption becomes more of a factor, for example, neptunium, we still see a predominance of our uncertainty in the groundwater specific discharge, but now we see that the sorption coefficient for neptunium in the alluvium has a significant impact on our uncertainty in model predictions out here. We actually see a reduction in the importance of the flowing interval spacing, because we only have a moderate amount of sorption in the volcanic matrix for neptunium.

I could go through several others of these. The really anomalous one that I should explain here, this is for plutonium, or it could be americium, it's irreversibly attached to colloids, and by far, the most important parameter here is the retardation factor for colloids in the alluvium, for the colloids that are irreversibly carrying these radionuclides through the system. I think that's enough on that, so next slide, please.

And, this is just the key to what those uncertain parameters are in the previous slide.

So, in summary, the three dimensional SZ site-scale flow and transport model is used for the radionuclide transport simulations in TSPA. The matrix diffusion is explicitly simulated by the particle tracking method. I should note here as implemented in the FEHM software code, in the SZ site-scale model, these results are abstracted for the TSPA calculations using the convolution integral method. The 1-D transport model is used to simulate the transport for decay chains, and uncertainty in key groundwater flow and radionuclide transport parameters is incorporated into the multiple realizations of the system.

And, finally, this sensitivity analysis indicates that our uncertainties in specific discharge and in flowing interval spacing probably have the greatest impact on our uncertainty in the transport predictions for most of the radionuclides.

Thank you.

BULLEN: Thank you very much, Bill. We'll entertain some questions from the Board. Do you still want to be first, Dr. Nelson?

NELSON: Sure.

BULLEN: Dr. Nelson.

NELSON: Nelson, Board.

I was struck by Slide 14, and your discussion about the early arrivals. In many of the characteristics that you cited there in discussion of those early breakthroughs, and also that you talked about regarding groundwater specific discharge and flowing interval spacing in volcanic units and horizonal anisotrophy, all of those things seem to be characteristics that you would expect if in fact there were fault directed flow. So, I'm just working the case, and I don't know whether this is a case of a model uncertainty investigation or a geologic uncertainty. Maybe in a model like this, changing the geology is kind of a model uncertainty.

But, I'm wondering since we've heard from the project that there isn't any clear evaluation of the permeability, the character of the faults, particularly to the east, and when we see some modeling that deduces the presence of such high conductivity along faults, as one way to describe the data, it seems to me that there is a reason to think about that as a not unlikely occurrence. What do you think about that?

ARNOLD: Yes, I agree that there would be potential importance to the role of faults in the system. And, there are ways in which we are implicitly considering the potential role of faults or relatively high permeability fracture zones in the saturated zone in these analyses. And, I think the most clear-cut example of that is the horizontal anisotrophy in the volcanic units. This indicates that there's a relatively high probability that the permeability in the north/south direction through the volcanic units is higher than the permeability in the east/west direction, in a rough sense. And, this is substantiated by pump test results that have been analyzed for anisotrophy.

But, this behavior is probably the result of through-going structural features of some kind that have a more likely north/south orientation that gives us a higher permeability in the direction of those major faults. So, you could say that we are implicitly including the effect of higher permeability faults through that horizontal anisotrophy factor.

Another way in which we are implicitly considering the possibility of high permeability faults in fracture zones is this flowing interval spacing parameter, which has a mean value of 20 meters, as Stephanie mentioned earlier. But, it has significant uncertainty about that value, too, and it goes up to values of, you know, tens or even over a hundred meters, and this would correspond to highly channelized flow in widely separated zones in the saturated zone that could correspond to this conceptual model of flow through high permeability faults.

NELSON: Nelson, Board.

What occurs to me is your comments there when you said them related to these early ones, whether they were unlikely. So, maybe some of the comment is that I suggest because of model uncertainty, they may not be so unlikely. There may actually be a reason to consider at this point on the basis of model uncertainty, the viable presence of these, and their importance may drive to go find out more about them. But, it doesn't seem that it's necessarily something that can be captured by stochastic distribution, that it's really, I mean, it's a yes or a no, that may actually be there. And, so, those early times could possibly, could be viable reflections of the system.

ARNOLD: They are certainly included in the analysis when it goes to TSPA.

NELSON: Okay.

BULLEN: Dr. van Genuchten?

VAN GENUCHTEN: This actually is really not a question. I want to go back to some of the comments I had earlier, and it really was in the framework, so when you go up, it's something like too many things, too little time, you know, too many questions, or too many things, too little time to explain, but also for us sometimes too little time to observe these things. There's a lot of material being presented here.

I appreciated your explanation of the matrix diffusion things. It makes a lot of sense now to me. The other thing here is I understand very well now that this should be viewed in a probabilistic framework, so it starts making sense.

The other thing that I was agonizing for about a day and a half about was the lack of tailing in your curves here on this particular graph. And, I talked with quite a few people and actually was complaining to my distinguished colleague here, Dr. Schwartz, but now I understand that that's plotted as a function of log of time. So, it's not really visible, so I finally discovered that. So, a lot of things I struggled with, finally became clear.

Thanks for hanging in there with me.

BULLEN: Dr. Latanision?

LATANISION: Latanision, Board.

Could we turn to Number 13, Slide Number 13? The last two bullets, could you just walk me through how all that plays out? I mean, I understand the concept that you're using breakthrough curves as a simulation of transport. But, how then do you take this library and walk through your deconvolution--or convolution integral method? What's the process?

ARNOLD: Okay. Yes, what I was trying to explain was some of the mechanics of actually transferring this information to the TSPA analysis. So, by a library, I mean here we have a series of files that contain all of the breakthrough curves from these 200 realizations for the 3-D model. Those are handed over to the TSPA.

Then, in the TSPA analysis itself, the convolution integral is implemented by a software code that's addressed by the GoldSim software within the TSPA analysis.

So, for a particular realization in the TSPA, it goes to these files that contain the appropriate breakthrough curves for that realization, say for realization Number 1.

It reads them into memory, and then when the convolution integral is conducted by this convolution integral software code, it takes the appropriate breakthrough curve to perform the convolution.

LATANISION: Latanision, Board.

What does it actually take from the curve, though? Is it the breakthrough time, is it the half-rise time, where on the curve are we interested?

ARNOLD: It's the entire curve. It's convolution of the entire curve with the transient input signal from the--of the mass from the unsaturated zone. It's essentially a numerical integration.

LATANISION: It's a numerical integration? I see. Okay. All right, thank you.

BULLEN: Dr. Leon Reiter, and then Dr. Parizek.

REITER: Leon Reiter, Staff.

Bill, I wonder if you could explain something to me that I may be misunderstanding. Stephanie showed a plot of saturated zone varied capability colloidal transport, in which the reversible colloids had travel times, or breakthrough times, by an order of magnitude longer than the base case. Yesterday, Bruce Robinson, summarizing the results of the unsaturated zone, said colloid-facilitated radionuclides had travel times of 20 years, it's like several orders of magnitude less, shorter, than the other radionuclide.

What's causing this tremendous difference between unsaturated zone colloids?

ARNOLD: Well, I might be sticking my neck out a little bit here without having Bruce to confirm this. We have, this is for the colloids with irreversibly attached radionuclides, in the saturated zone, we have a simple retardation factor for those colloids, and it's a significant retardation factor, and that's why the plot that Stephanie showed, showed the significant delay in the breakthrough for those colloids.

Now, to use that retardation factor, we're assuming equilibrium between the forward rate and the reverse rate for what we know or think we know is a kinetic process of the attachment of colloids onto the aquifer material and the detachment of the colloids onto the aquifer material. And, this is dependent on, you know, transport time scales through the saturated zone.

In the unsaturated zone, I believe that the time scale for transport through just the fracture, the fracture continuum, is short enough that that assumption of equilibrium and the retardation may not be as valid. Again, I'm kind of speculating on this one. So, we might really want to get that answer with some more information from the

UZ people.

REITER: So, does this relate at all to the discovery of very high, very colloidal transport in NTS, it's one of the--being rather rapid, how does that observation jive with what we're seeing here, your conclusions, Stephanie's slide?

ARNOLD: Well, there's a lot of uncertainty about why those radionuclides transported associated with colloids occurred so rapidly, you know, at NTS. My understanding is that one conceptual model is that the radionuclides are sorbed onto the colloids by kinetic process that, given the relatively short time frame over which this transport has occurred, has not been able to reach equilibrium, and so we have a non-equilibrium transport of the radionuclides that are sorbed onto those colloids. That's one interpretation. Alternative interpretations that I've heard, but I'm not sure if this is still valid or not, is that those colloids may have some special character associated with the source of the radionuclides in the underground testing at NTS, that perhaps the plutonium is embedded in the colloids in some way that is associated with the source.

BULLEN: More questions, Dr. Reiter? Okay, Dr. Parizek?

PARIZEK: Parizek, Board.

I look at Slide 18, and it has to do with specific discharge and flow interval spacing. There was not an awful lot of flow interval spacing data available at one stage in the program. Has there been new tests from, say, the Nye wells that have added to that data base? And, particularly with regard to the rocks of choice, that is, below the footprint within the upward, what, 200 to 300 meters below the water table, that seems to be where all the action is. So, given those sensitivities, are there new data on flow interval spacing from the Nye well, or any other wells beyond the data set we've seen sometime in the past?

ARNOLD: Not to my knowledge. The data sets that were used were fairly old. The most recent data that were used in that flow and interval spacing analysis were from the C-wells. Those are also probably the best quality data with regard to, you know, vertical resolution. And, of course, none of the Nye County wells have been drilled or logged really very near the repository.

I can say that there is one of these S&T initiatives funded by DOE that is associated with well testing in the saturated zone, and one component of that is an assessment of the channelization of flow within well bores and getting at this flow and interval spacing parameter.

PARIZEK: I wasn't aware that that was in the S&T program, but that would definitely be a good starting point to see if you can't narrow it down.

What about the specific discharge? Is there any need to do anything more with narrowing that down, or do you think you've captured it all in terms of the way in which you've handled it in the TSP runs you've done?

ARNOLD: Well, I think we've done a fairly objective job of capturing uncertainty in that parameter, given the data that are available to us. It's possible that that uncertainty could be reduced by testing associated with the S&T initiative, or even the full scale testing that was originally planned at the alluvial tracer complex that had to be cancelled because of inability to get a discharge permit for that testing.

PARIZEK: And, Parizek, Board. One other question.

In order to add some comfort, I guess, all the runs always have the pathways south and eastward, then the alluvium and down, and it would be kind of helpful, I guess, to list all the other lines of observations and evidence that support that. I mean, again, the models show that, but there's some chemical data, there's a combination of data that sort of justifies that interpretation, and Priscilla has been bugging you about many faults creating some surprises, and maybe there are no surprises, but it would be helpful to list in some clear place where all the lines of evidence are that sort of says that's why the flow ought to go that way.

I guess it's in there somewhere, but it's useful maybe to draw attention to that just to make it clear.

ARNOLD: Yes, I think we're, you know, these are very valuable data, the hydrochemistry data that Gary Patterson presented earlier. There is also a similar, and in some ways, more extensive analysis of the hydrochemistry that was conducted by Ed Kwicklis at Los Alamos National Laboratory, with the specific intent of providing additional confidence in the flow model. And, so, there's a tie between Ed Kwicklis's analysis and his report on the hydrochemistry and the flow model report that was written by George Zyvoloski with regard to confidence in the flow paths and the use of the hydrochemistry data.

PARIZEK: That was all C-mixing models, the freak data, that sort of thing?

ARNOLD: That's right.

PARIZEK: Thank you.

BULLEN: Other questions from the Board or Staff? Seeing none, Dr. Arnold, I guess your little gold statue will be arriving shortly. We appreciate your hour of presentation to us.

We have to change gears now, and I'll start it off by calling on Dr. Parizek, but just a second.

As we move into the open forum section of our meeting, what I'd like to do is ask Richard Parizek to say a few opening remarks, and then we'll call upon our consultants. I want to reiterate the fact that the public comment period still remains at 5 o'clock. So, this forum is not for public comment. This forum is for the technical interchange, and perhaps to address maybe the five questions, would you like me to put those back up again, Richard, when you start your remarks. We'll have them up there for that purpose, or any other comments that technical people would like to make with respect to unsaturated zone or saturated zone transport.

With that, I'd like to call on Richard Parizek to say a few opening words.

PARIZEK: As I chair this Panel, I'd like to thank each of the presenters and the organizations that they represent for thoughtful remarks, and very clear presentations that have been made through the last two days. It has been very, I think, deals with the questions we've posed in the advertisement for this meeting, and they have been responsive in addressing those points.

I know the program probably considers meetings like this an annoyance, like flies that are bugging you. On the other hand, I think that time was spent doing this, and discussing and sharing ideas in an open forum helps clarify points. You have to talk about them, you have to present the findings. You get different views. It's a helpful thing. It shares understanding with a broad audience. So, I think there's value added from this.

The clarity of presentations were outstanding. The content of these presentations was outstanding, if you compare this with where we might have been some years back. So, clearly, we appreciate that richness in the presentations.

There's clearly a transparency also in the way in which the presentations were organized, and the speakers addressed detailed points, and proof of that is the fact that the true non-geological natural systems people, could as such intelligent questions, as you could see.

We wish to thank Dave Diodato for organizing the meeting, to help pull together the speakers and other Board members and staff for their effort, including Linda and Alvina for their work always in making these meetings work. I think when they go home, their friends think that when they go to Vegas, this is sort of a junket, but I think they could tell you otherwise.

There's a correction point in terms of the chloride data, the chlorine 36/chloride ratio data. Some of the times, we've seen illustrations (inaudible) and facts, still current interpretation of the fact. As we understand it, there's still some discussion and some independent work being done by Gene Kline and others to sort of see is it real or isn't it real. And, so, if it's maybe not real, that's a final outcome. Maybe the use of those slides has to be softened, or some other discussions have to be done there. But, just drawing attention to the fact we've heard presentations using those illustrations, and if in fact it's been established.

Model validation remains really imperative throughout this whole process, and we've seen places where the program has tried to get at model validation, bits and pieces of the field observations, in some cases after certain forecasts were made, to help lend some credibility to the whole study. And, I think the program has been quite transparent in trying to show us how they've done that.

On the other hand, I think there's opportunity for further testing that was not yet done, and the Nye program always talked about a long-term test in fractured tuffs at some other location other than the C-well complex, to sort of see whether or not you actually have anything new that could come out of that, which might include the role of faults. Surely, there's C-well testing in the alluvial testing complex, the long-term tests that have not been conducted, should be conducted, because there's value to come out of those tests. And, we would hope that the program can see its way somewhere along the line of getting that work done.

The sonic drilling has turned up, as some people who have not see the core, or had a chance to look at it, will be the kind of key to try to unravel the stratigraphy and the sequence of sediments that make up the valley fill of alluvial materials critical to performance. So, we endorse that. We hope there will be more sonic drilling done to provide the kind of quality information that's needed.

The use of multiple lines of evidence, the Board has always asked for that independent, sort of multiple lines of evidence, the strength in the TSP analyses we feel everybody can sort of see where this is going and feel good about it. We still hope that the program works at the independent lines of evidence listing.

And, we look forward to the confirmation testing program. We haven't heard much about that lately. But, confirmation testing can be tied into a number of things that deal with the natural system's behavior, and we hope that that program does not ignore some of the natural system elements that we've heard in the last two days.

We endorse Margaret Chu's program on science and engineering, and we hope that funding is there, and that the program continues. I'm glad to hear the discussion that the spacing is included in that, and some other issues like that. And, we hope that throughout the LA process, and beyond, there will be a strong science and engineering program.

And, it seems to me the assumptions in the program, at all times remember when anybody has given us this list of assumptions, it's very helpful to sort of see those up front, because that gives us a basis of understanding what's in and what's not in the analysis that's being claimed.

In some regard, the base case, every time you see that solid black line, you sort of forget, well, what the hell is in the base case. Don't you? So, it might be useful to have the base case listed again, put on the side so we could always immediately say no, we're going beyond that including these variables in the analysis.

And, my opinion over the years that I've been affiliated with this program, I've seen immense progress, immense progress in terms of the natural system elements and pulling together what's really a complicated subject matter, with experiments, state of the art kind of efforts that have been required, particularly for the unsaturated zone. Colloids remain a mystery in a sense of how to quantify and capture them, find out if they really do exist and do move in the unsaturated zone in particular, because there will be tons of colloids produced from the repository environment, and it's kind of critical to be able to gain confidence when you understand the colloid story as fully as possible.

So, these are sort of some points from my perspective, and we can then go back to Dan and see how he handles this next phase. Thanks.

BULLEN: Thank you, Dr. Parizek.

Would you gentlemen like to make your presentations from the seated position, or do you want to stand at the podium? Okay. Then, we'll go with Rien, and then Frank.

VAN GENUCHTEN: Thanks, Mr. Chairman.

Actually, this has been a very interesting two days for me. I want to go back in '91, I was part of what they at the time called the Yucca Mountain Peer Review Team, and I still remember convening here in Vegas with a number of people. Al Frieze was there, and Jim Mercier and Gresack and Popoudophilis, and a few others, and we were discussing various mechanisms of how slow fractures could be generated within Yucca Mountain. They had a lot of fun and drank a lot of beer at that time. It's 13 years ago. That's 13 years ago. It's now 2004, and it's clear an awful lot of research has been done, and actually, I was just looking at this special issue of Contaminant Hydrology that was edited by Bo Bufresson and Clifford Ho and Bruce Robertson. It's extremely impression, and I think, Richard, you mentioned that. It's awful, how much has been done over the last so many years.

But, it also shows how complicated the system is. This is not something that's going to be resolved in an easy manner, or in a great manner, and it's very important to keep understanding that these scientific issues will be at the table for quite a while to come. And, actually, in a way, it was fun yesterday especially to see us starting to discuss again, like we did in 1991, the various mechanisms of how flow in fractured media are generated and sustained. And, so, we had a lot of discussion, you may remember from yesterday, about different conceptualizations of matrix fractures, fracture interactions in terms of unsaturated flow in the mountain.

And, I think it's important to realize that, and this goes a bit back also to some of the comments of Bredehoeft about uncertainty. These are really conceptional uncertainties we're still struggling with.

I completely agree with the approach that was taken in terms of the active fracture model as an attempt to include in the models the idea that there is limited contact between unsaturated flow in the fractures and the matrix, and that's a conceptional picture, a conceptional model. It's still going to be a question how that's been translated into a mathematical model, and I can see several different formulations arising from that, not just the power function of relative saturation, of effective saturation that was used in the models of Liu and all.

So, there are still a number of uncertainties in terms of conceptional formulation, and how that translates in models. The similar way is we're going to even go back a step before that, and say is this really a contact problem, or maybe I tried to push that yesterday, a problem of maybe having coatings in there and limited interaction between the fracture matrix, not because of necessarily limited contact area, but also limited, lower values of the effect of conductivity of coatings, not necessarily coatings that you can see, but there may be some stuff below the surface literally that inhibit this effective interaction.

And, similarly, it has an effect on matrix diffusion. These are things that need to be pursued. I think it's important that not just one formulation be pursued, but that there is room for different conceptionalizations, different conceptional models, not everything should be, you know, all the eggs in one basket.

There are a large number of people here with enormous backgrounds, impressive conceptional reasoning.

when we do a big experiment like that that costs us several hundreds of thousands of dollars, I think it would be very cheap to put a few good--on the computer and let them get the most out of it.

And, this actually is similar comments, I think, are important also for other issues, that lateral flow there, we talked about in the PTn unit. Some people claim it's very little. There's still a little bit of a legacy of earlier investigations that that might be quite important. I think getting some people together and looking at these issues is important, and let's resolve it as best as we can.

I probably have a few other things. I'll pass those up. I want to thank, in closing here, I want to thank the Board for inviting me. This was just extremely educational for me, having not really been involved with Yucca Mountain for about 12, 13 years to see the excellent signs being done. I want to thank all the people that gave presentations. It was just great, and thank you so much.

BULLEN: Thank you, Rien. Dr. Schwartz, did you want to sit at the table or stand at the podium?

SCHWARTZ: I'll sit.

BULLEN: Okay.

SCHWARTZ: Well, like Rien, I'd like to express my thanks to the Board for the kind invitation. In my real life, I'm a professor at Ohio State. My work at Yucca Mountain has been carried out mostly with EPRI and John Kessler's group at EPRI. I've worked in various DOE panels dealing with thermal testing and the Nevada Test Site in general.

There's some real benefits of being a Panel member here. As I've looked out into the crowd, one of the things is that with my age and vision here, I'm actually close enough to the screen to see the presentations, and as I've been wandering around the room, we up here get these nice color copies, as opposed to the black and white ones there.

There's some disadvantages, too. As I've kind of looked out in the crowd periodically, you're able to doze off there periodically, so one of the down sides of being a Panel member here is that it's hard to doze off up here.

I'd like to follow up on some of John Bredehoeft's comments. He kind of opened Pandora's box for discussing issues of philosophy related to modeling. That's an area that has been of some interest to me, not only so much for modeling, but looking at sort of the philosophy of science and related to progress in the hydrologic science in general. I actually have a Ph.D. student at Ohio State that kind of works on this issue.

John Bredehoeft, you know, talked about this issue of surprise in science, and one of the points that maybe didn't come out in his talk is that surprise in science is really a normal process. And, it's a normal process in the sense that that's the way most progress is made in science.

Matt Kozak in his brief remarks talked about Thomas Koon and the idea that he had, and there are important ideas he had about the idea of revolutionary science versus normal science. And, I think John Bredehoeft gave a very good example there that, you know, this idea of plate tectonics, when it comes time for some prevailing idea to be overturned, that often times, there's a revolution or a culmination or a collection of ideas that come together to overturn that idea. And, so, a step forward in science occurs through a revolutionary step forward.

But, after that revolutionary step, there is this process that Koon calls normal science, and that's the day to day plugging along, taking care of all the details, doing the fundamental work, and so on. So, I think, you know, the work at Yucca Mountain is no exception, that we see the progress in the science measured by some revolutionary steps forward, which turn out to be surprises, you know, from the DOE perspective probably not happy to see them, but in the overall perspective, it shows that the progress of science is surely marching on.

Now, one of the things we see in hydrology, is we've studied this idea of evolution in science idea, is that we usually don't see so many major surprises. They're more like paradigm shifts, or some kind of disruption of the status quo. They're not quite as severe as all of a sudden waking up one day and discovering plate tectonics. But, these, nevertheless, these revolutionary steps are important, and they are the normal way in which new knowledge is developed.

Now, I think there's been a history of surprises in the science at Yucca Mountain, revolutionary changes. I think Alan Flint's discovery of high recharge rates back in the early to middle 1990s would qualify. Fabryka-Martin's chlorine 36 information, whether it kind of stays or goes, still ranks as an important generator of revolutionary idea. I think there have been other important areas where there have been some important revolutionary steps.

On the modeling side, I think the work at LBL in identifying the unique characteristics of the Paintbrush non-welded unit and the basal vitropheres, some of the unique properties that they have and some of the important things that these units do for transport of gases, transport of contaminants, and so on.

So, my view, like Rein said, is that Yucca Mountain is a very complex applied science problem, and as I sort of began my work with EPRI in the late 1980s in this project, clearly, if you look back over those 12 or 15 years, you'll see the tremendous advance in theory. You know, in retrospect, probably the theories we had for flow in fractured media was immature and not sufficiently robust to describe the kinds of systems we talked about.

But, I think as I've sat here over the last two days, I guess I've been really impressed by the maturity of the science ideas that have been developed, in really looking at the major advances in developing the really the intellectual tool kits necessary to support the calculations to support the basic theory. So, I think there is some important progress in that respect.

Now, the question kind of, John's talk implied that, and some of the others, is really can one analyze complex systems subject to uncertainty, and that uncertainty, as we've learned, comes in the way of processes and parameters and future states, and we're not sure of any of those things particularly well. And, clearly, the answer has to be yes. I mean, if you think about what geologists, engineers, hydrologists, and metallurgists do, I mean, that is what the kind of engineering analysis is all about, to make decisions with relatively limited data sets, insights, experience, and so on.

And, the evaluations here that we've seen have involved simple tools, simple calculational tools, some pretty sophisticated model analysis, and all of that is checked by sort of seat of the pants engineering concepts, and trying to provide for what I consider to be coherency of results, so that if you make one model conclusion, do other processes and other observations fit well with that conclusion, and that's sort of a coherency that one model can explain several things.

And, I think the work that Bo Bodvarsson and his group at LBL, and colleagues with the USGS and the UZ, I think they've shown that there's pretty good progress in actually providing the coherency of results that there started to be needed to create confidence that the understanding is in reasonably good shape. And, I cite for that some of the material we've seen here would be the geochemistry, the occurrence of perched water. Some of the things that we didn't see here would be sort of air flow calculations, and these things as well.

So, I think in the unsaturated zone, there is good progress, and you have a feeling, at least I do, that the modeling is moving in a good direction, that the surprises would be minor, and hopefully will be in the normal science phase.

I think in the unsaturated zone, my feeling is that the results are conservative, that I think there are performance benefits yet to be wrung out of the saturated zone.

However, as we look at the saturated zone, I think what performance there is, even under the conservative assumptions, are very helpful to the safety case. I think that the sorbed species are extremely retarded within the saturated zone. I think the unsorbed species have meaningful retardation. So, I think even at this stage, the saturated results are pointing towards some certainly advantage as far as the safety case is concerned.

My own impression of the interpretation of geochemical data is in line with the question I asked Gary, that I've written similar kinds of things, I think the geochemistry and the isotopic data support a much more sluggish kind of flow system, and I think there is still some inconsistencies. I think in the saturated zone, there may be room for surprises, but I expect they will be pleasant surprises as far as the safety case is concerned. I think there's, you know, opportunities for improving things certainly, but given that they're relatively conservative now, I think the possibilities of degradation in that safety case is probably minimal.

And, so, again, thanks. I appreciate the audience staying.

BULLEN: I'd like to thank Richard and our consultants for their opening comments, and now I'd like to ask anyone who'd like to either come to the podium, or to the open mike, to step forward and make comments on anything that they've heard in the past two days, or the questions that we've listed and were posted. Go ahead, feel free to step forward, whoever wants to be first, and identify yourself.

MIFFLIN: I'm Marty Mifflin, and like others here, I find this a very interesting and improved type of review of both the vadose zone and the saturated zone hydrology. My background is as follows. I first became acquainted with the Yucca Mountain repository proposal in 1981 as a consultant for NRC, and over the years, went from NRC to technical oversight with the State of Nevada, and with a contractor for Inyo County and Nye County as well for a period of time.

So, I saw the early days, and I remember the smoke filled room back in Silver Spring, Maryland in 1981, sitting around with the various gurus that had been called in to try to decide what NRC's position should be with the vadose zone, and what should be looked for. And, one suggestion was that, well, let's just treat it as a black box, and worry about what comes out beyond the disturbed zone, the so-called disturbed zone at that time, which would be some type of definition of the edge of the thermally disturbed area.

And, surprisingly enough, there was quite a few people that, when I say quite a few, of the ten or fifteen in the room, a fair number thought that was a good idea. I thought it was a very poor idea, primarily because you didn't quite know what was going to come out if you didn't know what was going on in the inside of that black box.

One of the things that I wanted to say is that right now, nobody is supporting my work on this, and, so, I can say exactly what I believe. I think the site is a very poor site, because of its complexity. We've heard two days worth of very complex analysis, yet for the most part, they have to be heavily dependent upon poorly constrained conceptual models.

Now, a lot of the presenters felt that their analyses were conservative. But, conservative in one man's view may not be very conservative, whereas, the other person may think it's a very conservative analysis. It's a very subjective type of evaluation.

I've had a whole series of conceptual models in my own mind's eye over the years, and they've quite often differed with the popular conceptual models over those various periods of time. I might add that most of those models that I thought were more realistic have come closer to what has been determined over the years.

The other thing I'd like to remind the Board in particular is that the focus is on the right questions at this point in time, far better than it has been at any time in the past. However, the data bases and the funds that were expended in developing those data bases are not very well designed in many areas because of the nature of the field data base programs.

For example, we heard some type of description of how many wells were available at such and such a time, something like 40 mentioned, and most of those at that point in time, which was in the Eighties, were drilled with water based fluids. Here, we have a repository that was supposed to be a dry repository, and the data base very critical, determines just how dry is dry. And, so, a lot of these data bases, which the current experts are trying to utilize on some fairly sophisticated type of questions, are not really designed for those types of analyses.

For example, the hydrogeochemistry, we've got boreholes that are being used that are several hundred meters of open borehole in the saturated zone, and we've got some that have less than, say, 50 meters. And, we have a very complex way or type of volcanic sequence, and if you go back and look at the early testing in the saturated zone, H-1 and the G hole, you'll see that there's very, very highly transmissive zones, but very few of them. And, the head data is based on some type of average head. So, you don't really know, in other words, all the testing, and so forth, went on after the hole was completed and cleaned out, and so forth.

So, there's a lot of uncertainty in what, say, water chemistry means, if you have any type of stratification. And, one of the points made was that maybe a three dimensional, an attempt at three dimensional hydrogeochemistry was appropriate. But, for 15 years, nobody worried about that. And, that goes for most of the saturated zone and the vadose zone.

Another point I'd like to raise is that I watched an evolution, this is more philosophical, but I think it should be said, I watched an evolution of not only the site selection and licensing criteria, but also the effect it had on the scientists addressing the site characterization and analysis. It started out with an agreed upon site selection guidelines between NRC and the Department of Energy, and these were pretty reasonable. It also had, you know, the key licensing criteria, which was the groundwater travel time. And, those were--there was site selection criteria, and then there was the fundamental bottom line licensing criteria, which was related to groundwater travel time.

That approach has changed, but one of those site selection guidelines was a very important one, and everybody has forgotten what it was. And, that is site complexity, had to be confident in your characterization and analysis of performance. Everybody has forgotten that.

Well, right from the start, the selection of the unsaturated zone, the vadose zone, as I like to call it because it has water, was taking an unknown environment in terms of either what process is going on, as well as how do you determine those processes and get the data bases, and that was, in a way, a fundamental mistake, because we still don't have great confidence in the details of the processes.

And, you know, fractured volcanic terrain where you have welded, embedded and altered tuffs is also a fairly complex saturated zone environment.

I'll make a specific comment of what I heard on the saturated zone. The hydrogeochemistry was recognized as some variations way back in the maybe Sixties--or mid Eighties, I'm sorry. And, what has been noted is maybe you have a stagnant type of situation, less active flow in some areas. The other thing that I'd like to point out is that just because you have gradients off to the east doesn't really mean in these fractured terrain areas that you have flow. These very steep gradients raise and issue, and one of the big surprises in the early characterization studies was drilling a hole up on top of the mountain and finding out that you were still, on the east side of Solitario Canyon, and finding out that you were still on the high part of the fluid potential. So, in other words, it was in the wrong spot for the fault to be causing the marked difference in fluid potential from east to west.

In my, in one of these alternative conceptual models, in my opinion, is the flow is right down all of the faults, more or less north/south. If you go back and look at the hydrogeochemistry data base, that's pretty well supported. Now, the NRC modeling that we saw just assumed that the gradient is down, or the flow is down the gradient, and if I heard him correctly, he said it was isotopic and homogeneous type model.

Well, in fractured rock terrain, that type of modeling is not a very good characterization, as far as I'm concerned. It just is too large of an assumption. And, one of the things that NRC worried about in the early days was how to characterize the saturated zone. Many of the consultants to NRC at that time thought that there had to be multiple well testing, not just the C-wells, but pump testing to find out whether or not these faults were barriers, or not barriers. And, that never happened, but over the years, the people--this has been going on so long, that there's so many different people involved, the human element comes in and there's no real institutional memory involved from early to intermediate to later to later thinking on how to characterize the site.

There's another key point here that I picked up that I'd like to comment and alert the Board to. That is that we heard quite a bit about climate change. We've heard some numbers on what the monsoonal and transitional climate impact is on flux. If I recall correctly, one was the monsoonal was 2.7 times estimated current flux, and the transitional was 3.9.

Very early on yesterday, we heard the terminology effective moisture. Effective moisture is an important concept, not very well defined, but what it really means is what's left over after all the evapotranspiration occurs--well, what it originally meant was the following, because I defined it. In looking at these Paleoclimate, Paleohydrology that resulted in the hydrographically closed basins in the Great Basin, there was a whole series of pluvial lakes that occurred. And, these pluvial lakes were in the bolsons (phonetic), somewhere up along the sides, most extended only onto the bahadas, and if you measured the amount of moisture that came into the lakes to maintain a stable lake level, the high shoreline, what you were basically doing, if you assumed any rate of evaporation off the lake, you had a hydrologic budget from the catchment basin to the discharge, which was direct evaporation from the lake. And, it was independent of precipitation, and it's independent of, as far as a direct measure, of either precipitation or of temperature, or of a whole series of other factors that might influence the size of that lake. But, it was clear that you had effective moisture that maintained the lake.

Now, what is important in the flux through either the vadose zone or the saturated zone is effective moisture, not what the details of the climate are. It's just how the flux changes. Well, one of the really interesting things in the Great Basin and why I pick up on these relative factors is that all through the northern part of the Great Basin where you have the lakes, and in a few areas, you have modern lakes, where you have modern estimates of groundwater discharge where there is no lakes, you can go back and you can compare the full pluvial climates, lake size, compared to the catchment basin, with the modern pluvial--the modern climate discharge. And, you find out that these differences in effective moisture ranges from about ten times to about 15 times. Okay? That's the whole pluvial climate.

Now, if you look in these basins carefully, by the way, these pluvial lakes stop just north of the latitude of Yucca Mountain, the southernmost one is Gold Flat, a little bit to the north, if you look very carefully at these basins, and a lot more work has been done in recent years, you have both the last full glacial type shorelines, and then you have the younger driest type shorelines, which is more or less assumed to be associated with more of a monsoonal type warmer, wetter type of pluvial period, almost 2,000 years. And, somewhat earlier, you had, based on packrat midden evidence, you had a much drier, but colder type of full glacial climate, last full glacial climate.

The lake levels, however, based on those shorelines, are very, very similar. The younger, driest lake level, or high shoreline, is usually, for all practical purposes, if you're measuring an area in the basin, the same number, but it's a little lower.

So, I think that the Board, and actually the Project, should look a little more carefully at how they're coming up with their climatic flux, both through the vadose zone and through the saturated zone. I also noted in this last talk that the flux is rather important with respect to what type of actual numbers you get in terms of the sensitivity analysis.

So, if it's not--if it's three times, it's one thing. If the current flux is 3,000 acre feet a year, and it was 30,000 acre feet a year, that's another thing during the full climate, pluvial climate.

BULLEN: Marty, are you pretty close to wrapping up? Because we've got a couple more people I think that probably want to say a few things.

MIFFLIN: Okay.

BULLEN: You're like the professor. We give you any time, and it turns into 50 minutes.

MIFFLIN: I want to say one more thing.

BULLEN: Okay, that would be great.

MIFFLIN: Because we're changing the regulatory--this is in response to the gentleman from EPRI, I forgot who it was, because we changed the regulatory rules on this thing, at that point in time, I decided, well, let's go back and base everything on, from the scientific perspective of what the objective was with deep geologic disposal of high level waste. And, I think it's worthwhile for the Board to keep that in mind. There are the regulatory issues, but there also are the true objectives of the program.

And, the reason that the National Academy of Science, if you go back and read that document, or that recommendation, the reason that they recommended deep geologic disposal was to isolate the waste from the biosphere, because of the long-lived nature of the waste.

I think the site is a bad site from that perspective, because it's emplaced in the biosphere. As soon as the first canister fails, you have gas phase release into the biosphere within months, based on the air circulation evidence, and within, certainly within the inventory life of the majority of the radionuclides, if you have an engineered barrier is the only barrier, and relatively short travel times, then you have discharge back to the land surface. Anything that isn't absorbed is going to come right back to land surface.

And, these pluvial climates, and the intervening climates, which are the transitional and the monsoonal climates, make up both of the future based on the Milankovich idea. It's something like up to 70 per cent of the future.

So, we have the radionuclides that are long-lived coming back to land surface, and then they spread around. And, I don't think that's what the intent was of the National Academy of Science.

So, that's my comment.

BULLEN: Thank you, Marty. Anyone else like to make a few comments? I see George approaching the microphone.

Would you like this one, or do you want the front one? I'm comfortable sitting here, so I'm just going to stay. So, why don't you go right ahead. Identify yourself, please, and your affiliation.

HORNBERGER: I'm George Hornberger, I'm a professor of environmental sciences at the University of Virginia, and even I'm a professor, I won't take my full 50 minutes.

BULLEN: Thank you.

HORNBERGER: First, I wanted to say that I first got into notions about the disposal of radioactive waste in 1980 when I was asked to serve on a National Academy Panel that had been asked by the Swedish Program to review the KBS3 plan. The KBS3 plan was important, because the Swedes had actually, in law, determined that unless they could show with absolute certainty the safety of geological disposal, they were going to have to shut down their electricity generating plants. And, of course, the scientists on the NRC Panel, National Research Counsel Panel, were a guest at this. We said, well, we may as well give up now, because we know there is no such thing as absolute certainty.

Fortunately, of course, the Swedes took the legal system, takes a pragmatic view of what absolute certainty means, and moved forward.

Now, fast forward. One of the things that I do is

I serve for the Nuclear Regulatory Commission, the other NRC, on their Advisory Committee for Nuclear Waste, the ACNW. The regulation for the NRC, as everyone knows, is not absolute certainty. Thank goodness. I don't have much confidence that our legal judicial system would know exactly how to handle that. It is reasonable expectation, and I think that it's worth keeping that in mind.

Again, as a professor, I can't avoid now that John Bredehoeft opened the door and Frank Schwartz walked right through it in terms of philosophy, because professors just love to pontificate on something they know very little about.

But, in the 1970s, John Phillip, who is a famous soil physicist from Australia, wrote a paper on prediction in catchment hydrology, and that's a problem basically you might picture it as trying to predict or forecast the fate and transport of agricultural chemicals from a catchment. And, John Phillip pointed out that this was a problem that could certainly be formulated as a rigorous scientific question, and actually, you could address it in the standard hypothetical deductive procedure.

Phillip went on to say that if one did that, the benefits derived from actually doing the necessary measurements would be tremendously small relative to the expense that one would have to undertake to do this as a scientific project. Some of my more cynical friends would say that Yucca Mountain actually illustrates Phillip's point very well.

Phillip didn't, of course, throw up his hands and say that this meant that science didn't have a place in such projects. It obviously has a place, and rigorous science has a place. But, one can't approach these things in the same way as one would design a research program.

So, what we've been, I think, discussing here, all of the discussions and all of the questions that the Board posed, are basically oriented toward how one goes about looking at reasonable expectation. I think, like what people have expressed here, I'm quite impressed with the progress that has been made.

I did have my--the biggest question I had actually Leon asked for me right at the end, and that was that if I go down the list of questions, the last two there, what is the technical basis for estimates, and how much could the technical basis be improved, I was struck by the colloid transport materials we heard. There was precious little that I saw in the way of the technical basis. That is, I didn't see very much data. I didn't understand what the technical basis was for a 20 year travel time in the unsaturated zone and thousands of years retardation in the saturated zone. I don't anticipate, because Leon didn't get an answer to his question, I don't anticipate that I'll get an answer to that question, but I think it is something that really does deserve the Board's attention.

Thanks.

BULLEN: Thank you, Professor Hornberger. Any other people who would like to make comments, please come forward and identify yourself. And, you can have either podium or microphone.

FISK: Good afternoon. I'm Terry Fisk. I'm the hydrologist for Death Valley National Park, and I didn't know I'd have the opportunity to have this weapon of microphone in front of me today, but I'll try and behave myself.

And, I also want to go back to John Bredehoeft's talk this morning, and Dr. Parizek's comment. And, first of all, I'd like to say that the Park Service is getting immense value out of the work that Inyo County, through the hydrodynamics group is doing, and also we support a very small degree financially, the USGS flow system model, and the research that's gone on at Yucca Mountain. So, I'd like to get that on the record.

Despite the support for that, and looking at drilling within the park, we can definitely be a pain in the ass, even to those projects that we would like to see go forward. I would certainly like to see some work done to further flush out the mapping that Chris Friedrich has done, and the modeling that Dr. Bredehoeft has done.

To make that happen, we go back to the Organic Act of 1916, and the purposes for which the park was designated in the enabling legislation, and so on, and that means following the National Environmental Policy Act, and so, logistically, it's difficult, and policy-wise, it's difficult, but it's not an insurmountable issue, and I would like to try and work with Inyo County on making that happen. And, if the Board could see its way at some point, it might be a letter might be advisable on the record in support of that. So, I just wanted to make that point and get that on the record, that we do support that work.

The other issue that I wanted to get at a little bit is the one that John also mentioned, and this I put in a memo about two and a half years ago to the Water Resources Department of the National Park Service in Fort Collins. I don't think it had wide distribution from that group. But, the idea that there is a head difference right now, an upward gradient from the lower carbonate aquifer into the volcanics, and one of my concerns, and that of Death Valley as a whole, irrespective of Yucca Mountain and radionuclide transport, is potentially use of that carbonate aquifer as a water supply for the municipalities in the region who are actively looking at that as a water supply source, and, so, a danger that I see and others have seen is reversal of that gradient, which is a factor both for protection from contaminate transport and also for protecting flow to the springs in Death Valley.

And, I think that's--I'll stop there. Thank you very much.

BULLEN: Thank you. Other members who would like to make technical comments? Linda, please come up and identify yourself.

LEHMAN: Thank you, Dr. Bullen.

I'm Linda Lehman, and I think most of you know that I work for the State of Nevada. I haven't given a presentation to the Board in a couple of years, but I think there's still a critical mass of Board members who remember my models that I presented to you on behalf of the State.

The thing that struck me today was that we see what we want to see, and we hear what we want to hear. I heard Bill Arnold say yes, he feels the lines of evidence support his flow path. Well, I also heard the same thing. I feel that a lot of things that were presented today actually support the more southerly flow path that I have presented to you several years ago.

I also wanted to thank Jim Winterle for his excellent presentation, and I would like to, I don't know if we can use any of those slides from that. I'm thinking about his representation of the alternative conceptual models, and it's this one. Page 11. Anyway, I think his talk illustrates a number of concepts that I was trying to present in my models some years ago. He shows that the flow from the repository moves to the east until it intersects the first fracture zone. And, his first fracture zone is the Bow Ridge that he's combined, Midway Valley. That's basically the same concept that I was trying to illustrate.

The only difference between what I see he is presenting and what I have presented to you in the past is that I included the Ghost Dance Fault, and the Ghost Dance Fault lies just to the west of the Bow Ridge. So, in my model, you would have movement from the west to east until it hits the Ghost Dance, and then comes down more in the middle of the mountain block.

If the Ghost Dance is being transmissive and carrying things to the south, I'd like to remind you that the largest part of the repository mass is on the west side of that. You may have, I haven't seen the very latest plan that you mentioned earlier, but the one that I saw, the biggest part of the fuel is stored on the west side of the Ghost Dance, and there is some being stored on the east. Those radionuclides that are on the east side would probably move as Jim has shown in his presentation, to the east. So, that I thought was quite similar to what I had been trying to get across in terms of concepts.

The other thing that I would like to bring up is the coupled heat flow calculations that were presented by Dr. Rehfeldt. And, the particular graphic, I have it marked here, I'm just trying to find the page number. Number 16. And, I can't see too well from this graphic that we had, but it looked to me like he was making the case that conduction and convection is important for the lower part of the model, but he didn't feel--he felt he had a good enough match, basically, in the top part of the flow field. And, I guess I want to disagree with that, if I'm reading this correctly, it looks like, to me, the dots in the northern part of Yucca Mountain are anywhere from 3 to 10 degrees C. off from the actual measured values.

And, across the region, there's only about a 4 degree spread in temperature if we believe Sass's temperature model. And, unfortunately, there wasn't one presented today. But, the reason I chose to use the Ghost Dance fault, and some of you remember, is that there appears to be a cold water finger that moves down the Ghost Dance Fault, at least it's centered on the Ghost Dance Fault on the maps, a cold one again through Crate Flat, and a cold one through Fortymile Wash.

So, I feel that his temperature representation does not support his model of flow path, and I would think that they would have some additional work perhaps, doing calibration, or whatever, to try to correct that. And, if I read this correctly, it looks like the temperatures are about 3 to 10 degrees too hot, which would indicate that they maybe don't have enough cold water coming in to cool the upper units. And, they could also use as calibration data Sass's heat flux distribution as well.

That's it.

BULLEN: Bullen, Board. A quick question before you go, Linda, because I'm not a hydrogeologist and I've asked all these questions anyway, so that never stops me, but as I saw some of the lines of constant head that we've seen, or for the flow field development, it looked to me like there were some of the embayments that you presented to us maybe two, three years ago kind of creeping into that. Is that--do I misinterpret?

LEHMAN: No, I saw that in Dr. Winterle's presentation.

BULLEN: Okay.

PARIZEK: It's Page 7 of Winterle's graph.

BULLEN: Oh, okay. Thank you, Dr. Parizek.

PARIZEK: The other interpretation smooths that out. But, he sort of followed the data, so I'm not sure what data is different here than what maybe you saw sometime ago. Apparently, you complained about him having left out the wiggles. This is conceptual if you argue the faults and the washes are enhanced permeability, it's how you might be inclined to make the wiggles, unless the data says you should put the wiggles in. We almost have to see the data again to see why this interpretation exists.

LEHMAN: Okay. What I used to use as my justification was the USGS water table maps. When they recalibrated the water table surface and releveled all those wells, they did not use all of the data points that came out of that analysis. Some of them were left out on the basis that there was no physical reason that they saw to use those lower head values. Granted, they're very small, hundredths of a meter, but still, if you're going to believe the rest of the data, it's nice to believe all of the data. So, that's where I got them. I contoured all of the data, not selective.

BULLEN: Thank you, Linda. Anyone else from the audience want to comment on the forum portion of our presentation? Or all you all thinking if we get done 12 minutes early, I can beat traffic home? Don Shettel raises his hand. I couldn't get out 12 minutes early with Don in the room. So, come on up. Identify yourself, please.

SHETTEL: Don Shettel for the State of Nevada.

It's hard to follow the first two philosophical talks here, because I'm going to get into the nitty gritty I guess, but somebody has to, I suppose.

The three points here, the first one involves the drift shadow effect, and I must say that even the conceptual diagram of this is a little hard to believe, because if the matrix gets saturated in places there, that would seem to lead to fracture flow. And, at least my main comment on this, which is a comment that Dr. Bodvarsson made about a year ago at this meeting in the Long Street Inn, where he said there were billions and billions of fractures in the vadose zone, and we can't put which ones are going to flow. So, how does he know there's going to be a drift shadow? Those two concepts seem to be incompatible.

Now, the second point involves sorption coefficients. In the first set of AMRs that came out three or four years ago, there were ten--there were a number of assumptions, but there were ten assumptions that involved sorption coefficients, and essentially DOE said all these assumptions needed to be confirmed, and one of these assumptions was that the sorption coefficients in the saturated zone will be the same as for those in the vadose zone. But, all the experiments were essentially saturated experiments. So, my question is essentially to the DOE, have they confirmed this assumption, and where is it located?

And, the last point would be diffusion coefficients and flow in the unsaturated zone, and specifically I'm referring to George Moridis's talk yesterday where he supposedly had some very conservative assumptions regarding essentially one on performance assessment, I believe it was, looking at just flow in the fractures in the vadose zone, and essentially just the diffusion coefficient was I think the only thing, and maybe some sorption of it was retarding. But, he seemed to have some incredibly long travel times under those extremely conservative assumptions.

Now, when you look at the fact that we've found chlorine 36, which is essentially where it's been found essentially a 50 year travel time, and he's showing travel time for technetium, a non-sorbing species like chlorine 36 that are in the thousands of years, in fact, I think on the order of 10,000 years for less than half of the technetium to reach the water table seems to be a disconnect there.

I think the question there is how is DOE modeling the flow in fractures? Are they modeling it as thin films on both sides of the fracture, or is it rivulet flow? Now, obviously one of those assumptions is extremely conservative, and the other one isn't, so the question is which model is DOE using, and the justification for that.

Thank you.

BULLEN: Thank you, Don. Anyone else who would like to address the--Dr. Van Luik? Please identify yourself, and your affiliation, or you could just be a member of the public, if you want.

VAN LUIK: I'm Abe Van Luik, I work with the Department of Energy, and I have a part-time job, which is to be a chairman of an international expert group for the Nuclear Energy Agency. And, from the perspective of that job, I'd like to say something.

We sponsored a workshop at which I was not in attendance, so it's not colored by my ideas, in Terku, Finland, to which the TRB sent two representatives, on the role of the engineered barrier system in total system performance assessment, and something that Marty Mifflin said reminded me of this. I would recommend that the Board read the report from that meeting.

In that report, there's a reflection of the maturing of the international view of geologic disposal, and a recommendation that one should pursue engineered containment for as long as practicable, and that after it fails, and it will fail, it's inevitable, that then the releases should not be harmful to human beings or the environment. And, I think if you look at our EIS and other long-term looks, applying ICRP-72, updating the model, we pretty much follow that ballpark, and so I feel pretty good about the fact that we do, in the mature view of geologic disposal meet what the international community thinks it's all about.

That's all I wanted to say.

BULLEN: Thank you, Abe. Dr. Parizek is raising his hand, and I know that I only have four minutes left, because I want to be on time like my colleague this morning.

PARIZEK: I'm the Chairman. I have one quick point.

BULLEN: Dr. Parizek.

PARIZEK: Terry Fisk brought up a point about, you know, the possibility of drilling in some critical location. I only raise that appeal in the event it's necessary. Right away, there are drillhole possibilities in the Echo Canyon area on the other side, and if those holes are drilled, then maybe there's no need for anything else to go into some sensitive area. On the other hand, those holes may not solve the problem, in which case, that would be the example of when you might want to go into this very protected sort of area.

But, I appreciate his offer that there could be some reasons why we could mount some recommendations that this be allowed, because of this unique problem. So, we'll wait and see maybe what happens with the other drilling program, unless you meant help get the drilling program started. That's a different problem than saying where they start.

BULLEN: If you want to speak, please come to the microphone.

FISK: Well, we've been working with Inyo County and the BLM on permitting the existing wells that are planned. And, so, Echo Canyon well within the park is part of that effort, as is one that was drilled last April on the alluvial fan above the Furnace Creek area. We've also worked with Inyo County on access and permitting to BLM lands immediately east of the park in the Amargosa Valley as far as it's a BLM decision, but we've helped out with some of the coordination, if you will, and information on that.

Now, if we have to get into the area where the dam is, which is in the heart of the Funeral Mountains, then it becomes logistically difficult. It's hard to get a four wheel drive rig in there in some areas, and there are people, different points of view, naturally, within the park right now, the superintendent is very focused on water resource issues, both locally and regionally. There are other people within the park who believe it's blaspheme to bring a drill rig into an area, even a non-wilderness area, if we go into the Funerals, then we're looking at botanical, archeological, biological surveys, and the whole gamut of issues. So, it just makes it more complex.

But, we may find what we need and hopefully, we'll know that answer within--you know, drilling was supposed to start last fall, October, November, and now with Inyo County's contractual difficulties, shall we say, it's hard to say, it will be the heart of summer probably, and they'll want ice chests out there and fans, and so on.

BULLEN: Thank you very much. Seeing no other comments, and now I have a question, a point of order. We will declare the open forum closed. But, since I have the list of public commenters, do you want me to do it, Mr. Chairman, or will you? Okay. Mr. Chairman, I will continue.

We only have one public commenter who would like to speak, and she very graciously deferred from this morning, Judy Treichel, would you please come forward, and choose your microphone.

TREICHEL: I don't need the list. I need the last one.

I totally disagree with the last question on there. I think it shouldn't be on there, and I think it's inappropriate. How much could the technical basis be improved by 2010? Well, throughout this program, we've had just in time engineering, we've had just in time science, and talking about just in time, you know, the magic year 2010 is when the trucks, trains, barges, and whatever, are supposed to be pulling up at the door of Yucca Mountain. So, I would say that's waiting awfully late.

And, I think if there needed to be an improved technical basis, that probably the Board's letter at the time of site recommendation should have been a little tougher. I know we've been through this, and that's water over the dam. But, if the Board is asked by Congress or the NRC or anybody, when it comes down to DOE's license application, what do you think, maybe you should say well, we think maybe the technical basis should be improved, and perhaps that should happen not before 2010, but before any license application is accepted. So, I needed to get that said.

I found that in the various meetings that I've been to, and Lord knows that I live at meetings, that in this one, there was very little importance given to, or attention to climate change. It was mentioned a lot. It was talked about a lot. But, you've got studies that have just come out from the Pentagon and from a big one done in the UK, and these are not alarmist organizations by any means, and they all have their own agenda, but they find climate change to be very dangerous, to be a big deal, and to be coming very quickly.

I think the Pentagon study is talking about within the next few decades.

And, my question would be if they're right, if it's going to be a big change, if it's going to happen very soon, what does that do, how does that change everything that goes on out from there? What DOE did was to go back a few hundred thousand years, see what happened, and then flip the chart over and use that going forward. But, it would seem to me if there's going to be some dramatic change that's unexpected and has not happened before, that it might affect everything coming after that.

I always get up here with laundry lists, so I don't have any segues. I just go down the list. In the discussion of Pena Blanca, I think that's interesting because it's always brought up as being an analog, and it may be when you're tapping on the rocks and when you're looking at how the materials work out there and what the temperature and the setting and so forth, but from the public perspective, it's not an analog. The comment was made that it might be hard to know where the, since it's in a uranium mining zone, it might be hard to know where the pollution in the groundwater came from, because it could have come from one of the other mines, or the other places where there's uranium.

Well, that's not the case in Amargosa Valley.

Amargosa Valley isn't a uranium mining district. It's a dairy and farming and residential district. So, we're going to know where the pollution came from there, and you've got clean water now. I don't know if you have clean water at Pena Blanca. I don't know if anybody else shares the water with that site. But, I think I would like to know, and I'm sure that the people in Amargosa Valley would like to know.

There's been a lot of talk about Dr. Bredehoeft's presentation. I was so grateful for that presentation. It was a real breath of fresh air, and I told Priscilla I think that if I had any say over what the Board does, I would include Dr. Bredehoeft in every session that you have. He was asked how his information would feed back into what the project does. Well, once again, going back to where the time table and the sacred schedule is that DOE operates off of, I don't think it does feed back in. The site has been recommended, and they're in a race to get a license application in. I'm sure that some of the data, or a lot of the data, is probably frozen. They talk about work that they've got coming up, but if they're going to get a license application in and have the licensing support network all full of their 40 million pieces of data by June, it's probably not going to get back in there.

So, the only place where I disagreed with Dr.

Bredehoeft was when he said what's the rush to closure. And, my question would be what's the rush to opening. But, of course, I've been asking that for a long time.

In the area of model validation, where I have absolutely no training, but I listened to the discussion, and there was sort of the philosophical talk about validating models and proving models. I guess I agree that you don't prove the model is right. What you find out is that it's not wrong yet, and when you run it, you can have confidence building, and you can feel good that you're not wrong yet, you're still right, but not that you've got everything right completely.

So, I know that in one of the talks, the statement was made that we still have conceptual uncertainty, and I suppose that goes back to that statement right there, and that issues need to be pursued. If that's the case, they're not ready to apply for a license. And, not everything fits into performance confirmation. I'm sure that the performance confirmation superman is going to fly in to save the day, and everything will go in that basket.

There's a whole lot of things being thrown into the science and technology basket that were never supposed to be there. This thing has changed 180 degrees from when Bob Budnitz stood at one of these podiums and told us what it was about. And, it's just going to blend right in to the rest of the just in time science, and that's not what it's there for.

And, I guess, last, I would like very much to thank DOE for refusing to participate in this last session. That was wonderful. It opened up the microphone to a whole lot of things that we would have rather heard. So, I'm very grateful for that, and thank you.

BULLEN: Thank you, Ms. Treichel. Are there comments from anyone else in the audience? Seeing none, I guess I would just like to reiterate our Panel chairman's thanks to both the staff, and to all the presenters and all the participants in today's meeting, and yesterday's meeting. We always get a great deal of information when we come to Nevada for these meeting. And, I think we are adjourned until May 18th, in Washington, D.C. Is that right, Mr. Executive Chairman? What's the date of that meeting? Okay, the third week in May, Washington, D.C.

Thank you very much.

(Whereupon, the meeting was adjourned.)