UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

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Longstreet Inn
Amargosa Valley, Nevada

BOARD MEMBERS PRESENT

Dr. Jared Cohon, Chair, NWTRB
Mr. John W. Arendt
Dr. Daniel B. Bullen
Dr. Norman Christensen, Jr.
Dr. Paul P. Craig
Dr. Debra S. Knopman
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek, Session Chair
Dr. Alberto A. Sagηes
Dr. Jeffrey J. Wong

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I N D E X

P R O C E E D I N G S

8:00 a.m.

COHON: Good morning. We are now reconvening the meeting. Let me remind those who may be interested or inform those who are joining us for the first time, that there will be a public comment period at the end of the meeting today at approximately 5 o'clock, and I emphasize approximately. It will start whenever the regularly scheduled part of the meeting concludes.

We ask that you sign up in the bank of the room if you are interested in making a public comment, though that is not necessary.

Chairing today's meeting is Richard Parizek, a member of the Board and professor at Pennsylvania State University. Dr. Parizek?

PARIZEK: Thank you, Jerry.

Many of you have commented on the view one gets of the desert looking to the west from the Colorado Rooms 1 and 2 here at the Longstreet Inn. But, you know, in a similar manner, board members get a view of the Amargosa Valley region, and it gives us an understanding in the region of ground water flow field, much of today's topic.

The flow field has discharge areas in the Franklyn

Lake Playa that we visited. Several of us have done this several times, and the whole Board was here earlier last year. We've looked at the Death Valley area, some of us. Some of us have seen the Ash Meadows discharge area. And after having visited these areas, you can't help but get a totally different impression of the desert. There's a lot of water coming out in the desert. It's a totally different perception that you get if you stand on the crest of Yucca Mountain and get the feeling that this is really a dry desert.

We get another view when we go into Brown's Room in Devil's Hole. It's a window into a paleoclimate record, which is outstanding and very useful to the program. And yesterday, Russ Dyer gave us four performance attributes of a repository. One was limited water contacting waste packages and design containment, the waste package lifetime question, slow rate of radionuclide release, in part due to engineered barriers, and ultimately, the concentration of reduction of radionuclides during transport in the unsaturated and saturated zone. Well, that's today's topic, the saturated zone, and we'll focus on efforts to understand the local and regional hydrology of the saturated zone.

In time, some radionuclides inevitably will reach the water table and be transported to accessible environment, and so the question is in the early days when we thought the mountain was dry, you wouldn't have to put any attention on regional ground-water flow, but today, we know that's an important part of the story. There's a higher flux rate in the mountain at present, and there was higher flux rates in the past, and surely there will be again in the future with climate change.

We know there are some fast flow paths that exist within the unsaturated zone, and surely there must be similar fast flow paths within the regional ground-water flow system. So one has to then understand the transport and flow of water in the saturated zone. There's certain questions that have to be answered. What is the ground-water path like to the accessible environment? That may be in part set by rules of the EPA, but on the other hand, ultimately water is going to come out somewhere.

What are the expected ground-water travel times within the ground-water flow channels that might receive radionuclides that are released from the repository? And what are the respected retardation mechanisms that will slow down or eliminate some radionuclides that as a result will never get to the discharge areas? And how much mixing and dilution can we expect during transport within regional ground-water flow field, and how much mixing and dilution can we expect at points of possible future withdrawal of ground-water, either from small capacity or high capacity wells?

And how might future climate change change these conditions in terms of flow path rate, velocities of ground-water flow, volumes of flow, and points of discharge?

Now, several targets are available for calibrating and validating models, and we'll hear some of these today. But, one, you could use point hydraulic head data in the regional flow field, or patterns of the water table configuration on a more regional scale. Or we could look at the spatial distribution of hydraulic conductivity distribution in rocks and alluvium. And then there's a spatial and temporal variation of recharge throughout the whole flow field, as well as the discharge areas in the flow field. And transport prediction, on the other hand, is even more difficult because we have the question of understanding the velocity of ground-water flow, and then eventually transport processes are tied to that. But will plumes that inevitably develop under the mountain broaden and spread and result in good mixing and dilution, or will it be really a narrow track or focused flow where we really can't count on much dilution?

And will colloids be a significant transport mechanism in the saturated zone? And we heard yesterday there will be efforts looking at the unsaturated zone, but both saturated and unsaturated zone transport is important. And for models, what are the boundary conditions that we have to deal with? Do we really need to know what the steep hydraulic gradient is caused by in the northwest portion of Yucca Mountain, and do we need to put major faults in the flow field in the pathway analysis? How uncertain are the head distribution and conductivity distribution within the regional flow field? And does it make any difference at this stage in the analysis.

And so can we reduce uncertainty in our understanding of the flow field and transport models in the short time that remains between now and recommendation of a site or for LA? But if we don't understand it, what does that do in terms of slowing down the whole process of getting a license that finally will work?

Stay tuned to all of today, and we will learn all about these questions, we hope. We expect answers on all of them, and if not, then we need to know what additional research needs to be done in a timely manner to support a decision to go forward with Yucca Mountain and to deal with the question of the license application.

Today, the speakers are going to be varied, and we'll have an overview of a saturated zone program and it's objectives, we'll look at the three dimensional ground-water flow model for the Death Valley Basin, which is a very large study area with discharge areas throughout the window beyond you there. We'll look at significant hydrochemical domains in the saturated zone at Yucca Mountain. Surely there are fingerprints that show something of the history of the water, the age of the water. What do we know about that? And we have to model for major ion chemistry in saturated zone waters along flow lines through Yucca Mountain.

There's tracer tests at the C-Well Complex. You'll learn more about that, and we're going to have a three dimensional finite- element model discussion for saturated zone in the Yucca Mountain area, which is a site scale model, current status of the saturated zone flow and transport model. We discussed State of Nevada studies for the saturated zone, Nye County proposed saturated zone work, some questions and answers at noon, if some of you have to leave. We hope we'll have time for discussion then.

And then we have the whole elicitation process, the expert panel process, and that will occur later in the afternoon. We have saturated zone flow and transport conceptual model and parameter issues that will be discussed, and late in the day, our expert opinions on the part of two people who served on these panels. And then finally total system performance and what to do with the saturated zone in that regard, and thermal testing updates, and finally questions late in the day, a very full agenda.

The hope is not to cut off discussion if it seems very active and vital, but at the same time, to try to cover all of these speakers, we'll have to kind of do this in a business-like manner.

So the first presenter today will be on an overview of the saturated zone program and its objectives from William Boyle, and we heard from him yesterday, so I'll not go further with introductions.

Thank you.

BOYLE: Good morning. Thank you for being here.

I'm here to present an overview of the project's work on the saturated zone. Some of the talks this morning will be by others, Nye County and the State.

For those of you who are not a scientist, I'll provide a little background on the saturated zone, for those of you in the audience. The soils and rocks are saturated if every hole or crack in them is full of water. In contrast, usually the overlying rocks, if there's any air there, they're called unsaturated.

At Yucca Mountain, the saturated zone is very deep. Here in Amargosa Valley, it's not. For those of you who have never been over to Ash Meadows, I really recommend it. If you see the saturated zone, it's quite a pretty area, too.

In contrast to Russ Dyer yesterday, I will talk to all my slides. I really don't have that many.

Saturated Program Objectives. This slide largely talks to what are the objectives. What is it the studies want to get? What's the ultimate goal? We want to understand the saturated flow system in the vicinity of Yucca Mountain. How does the water move around? More specifically, provide ground-water flow fields. That's, in layman's term, how much water is moving, where it's moving, when is it moving.

The first two bullets deal mainly just with flow of water as a material. The last bullet is also concerned with the saturated zone studies, and it's more chemical in nature for the most part. It's what happens to materials in the water. Are they diluted or do they precipitate, or are other materials dissolved? That was the what of the saturated zone.

You can think of this slide as, well, who's going to use the results. And in short, the entire project over time. The results are used in the viability assessment. They're used in license application and ultimately if the repository is built, in performance confirmation.

This slide is power to the data collector, and you'll hear a little bit about all of these things from the other speakers today. But how the data are collected; there's water-level monitoring, aquifer testing, pump testing, if you will, how much water can be pumped out of the saturated zone over a given amount of time. Samples are obtained for chemical studies, and then as with any data collection activity, there's always associated modeling.

This is a summary you could look at the speakers to follow, at least from the project speakers; these are the topics that they're going to discuss today. You can read them as well as I can, and I'm just here to set the stage for the speaker.

In the agenda, there was no time for questions for me, which is okay. I don't think I'm over schedule yet, so there's something I'd like to bring up from last night. Many people here this morning weren't here last night, and a question came up about study plans and the site characterization plan, but it was specifically related to saturated zone studies, and it was a question of essentially why aren't we doing what was in the study plans or the SCP, and how do we explain why we're not.

And Russ Dyer addressed it from a project control point of view, how we take care of planning our project in the absence of the study plans. Chairman Cohon stated that it was a good question, then answered it himself and gave a very realistic answer, that the project needs to focus on certain activities and make tough choices and maybe some things won't be done that people once thought would be done.

Now, it is a very good question, as the Chairman pointed out. It's also a question that the Department has to answer in the license application. There's a specific requirement in Part 60 that we describe what was in the study plan, what we actually did, and why we did it differently. And we're not waiting for the license application to explain this. As part of our twice yearly progress reports, there is an appendix in the back, Appendix A, that explains what was originally planned and what is being done and the status thereof.

One final remark on the question; perhaps I'm too sensitive, but as people ask why aren't you doing what was in the SCP or the study plans, there may be an implication that the SCP and the study plans were somehow right and what we are doing now is wrong. I would put forth that there is no right answer in site characterization. There is no manual to go to to look up that says you need four bore holes, not three, not five.

So as people ask this question, I hope they do realize that it's a tough issue to deal with; that there is no cut and dried, right and wrong answer.

Now, having said that, are there any questions?

(No response.)

PARIZEK: The Board is quiet. Staff? I guess we'll just get at you later in the day I'm sure one way or another.

The next presenter is Frank D'Agnese, who's going to report on regional three dimensional ground-water flow model of the Death Valley Basin. And we've got to make a distinction between the regional flow model and the site scale model, and that will become clear as we go along today.

Frank has been on the regional ground-water flow model study for quite a long time. He's had experience in the desert area sampling paleospring deposits, and we're happy to have you make your presentation.

D'AGNESE: Thank you. I was originally asked to give an overview of the region, and then they told me I have 15 minutes to do that.

What I'm going to do I think is to spend about 15 minutes here kind of giving you an overview of the goals, the objectives and some of the basic conclusions of the regional modeling studies. For those who are interested in detail, the report for this study has just gotten off the presses, published as USGS WRI 96-4300, and that is available.

Now, this overhead here shows the outline of the Death Valley Basin, and in general, it includes about 80,000 square kilometers. It's a pretty large area that straddles Southern Nevada and Southeastern California, and Yucca Mountain is located approximately in the north-central part of that area. Death Valley is the ultimate discharge area of that flow system.

Dominantly, this basin is closed, and that means that we don't have any ground-water fluxing into that basin, except for maybe a little bit of water that enters the basin from what is known as the White River or Pahranaget Lakes area. So in our effort to characterize the regional flow system, this is the basin that we concentrated on.

And our objectives of our modeling exercise were to define subregional and local boundaries, major regional flow paths, regional recharge and discharge locations, as well as rates to help define flux through the system.

We're studying the effects of this very large carbonate aquifer which controls flow throughout the entire region. And then once we've done that, to use the model to assess the effects of climate change, water-use changes and structural changes to the system.

Our approach was to characterize the hydrogeology of the region and discretize this hydrogeology down into small packets that we could then simulate ground-water flow through. By calibrating our regional model, we were testing various conceptual models of the flow system. Once we had done that, we did a series of evaluation steps or validation steps on our model. From that point on, we can develop either recommendations for improving the model, or then use the model for some scenario testing. And I'll give you an example of one of those.

To characterize the hydrogeology, the first step was to develop a whole series of geologic cross-sections throughout the region, and we see here a whole slew of cross-sections developed by several geologists that describe the distribution of the hydrogeologic units within the region.

We took this information and we took those geologic cross-sections and we placed them into a digital environment, the computer, so that these cross-sections could be hanging in their three dimensional space. And here, we start to see the distribution of these very complex units in 3-D space. We took that information and correlated between those cross-sections to develop a solid three dimensional representation of a one conceptual model of the hydrogeology for the Death Valley Region.

And so what we see here is this very large three degree by three degree area of a digital representation of the hydrogeology. Las Vegas would be located here. This is the Spring Mountains. Death Valley would be located here, Amargosa Valley, Yucca Mountain to the top there. And so what you see in these little squares here is just basically the resolution of our digital model.

Along with geology, we characterized things like potential recharge areas. These are the recharge areas. The major recharge area for the flow system would be the Spring Mountains out here to the east, the Sheep Range, Pahute Mesa and Delta Range, and even areas like Timber Mountain and Shoshone Mountain to the north.

In addition to recharge, we characterized major discharge areas, not only where discharge is occurring, but also to quantify the amount of water coming out of these discharge areas, because this flux is going to help us define flux through the system, and that gets us an answer into potential dilution and travel times.

Major discharge areas for this region include Ash Meadows, Death Valley, areas like Oasis Valley up by Beatty, Sarcobatus Flats north of Beatty, and then some smaller areas to the south in the areas of Shoshone and Tocopa.

Dick Parizek had mentioned that there's a whole series of water level observations. This is a distribution of water level observations in the region. Obviously, the largest number of water level observations are in Las Vegas. Unfortunately, Las Vegas is not in our flow system, so we can't really use that information in our flow modeling.

The concentration of observations occur at Pahrump Valley, Amargosa Valley, Yucca Mountain, Yucca Flat and Pahute Mesa.

So while there is a lot of data on a local scale, on a regional scale, we really have some major gaps in our data set, particularly up in the northeastern part of the flow system, what is traditionally known as the Ash Meadows Flow System.

Using all of this different information, we took the Death Valley Basin, and we broke it down into what we call three subregions, and the areas that you're seeing in white here are the areas that we included in our ground-water flow model. We broke the system down into a northern subregion, a central subregion and a southern subregion, and these subregions are defined as such because there's very little flux or ground-water movement between these basins.

The central Death Valley subregion includes Yucca Mountain, it includes Beatty, Amargosa Valley, and what is traditionally known as the Ash Meadows Flow System. To the south, we have sometimes we call it the southern subregion, the Pahrump, Shoshone subregion because that's flow from the Spring Mountains through the Pahrump and Shoshone areas. And then to the north, that includes areas like Lida Junction, Scottie's Castle, that type of thing.

We characterized the system and broke it down into what we called ground-water basins and sections. Each ground-water basin is named for the major discharge area. So our Pahute Mesa, Oasis Valley ground-water basin, major discharge at Oasis Valley, our Ash Meadows ground-water basin has major discharge at Ash Meadows. And then our Alkali Flat-Furnace Creek ground-water basin has major discharge at Alkali Flat and Furnace Creek.

This not, you know, earth shattering, ground breaking information here. This is basically a summary or work that had been done in the past, but we're kind of packaging it and breaking it down so that we have a better way of doing an accounting when we do our ground-water budget. Characterize major ground-water flow paths. A similar exercise was done for the northern subregion. Here, major discharge in the northern subregion to Grapevine Springs out by Scottie's Castle. And then in the southern subregion, major discharge to the areas of Shoshone and Tecopa by way of Pahrump.

In our ground-water flow modeling, while we're calibrating, we're evaluating various conceptual models, and we're changing our interpretation of the hydrogeology, and these are some of the features that we pinpointed as key controls on the flow system, things like northeast and southwest trending high K zones, large hydraulic conductivity, high permeability zones. Okay, these are fault zones, big regional fault zones.

Northwest, southeast trending low K zones, the presence of a shale confining unit known as the Eleana on the test site, also very low permeability rocks in the Funeral Mountains right outside the window here, as well as Precambrian basement rocks. Critical to defining the flow system is this configuration of the carbonate aquifer which underlies the majority of the area, and that's the control, that's the key.

Along with the calibration, once we had flow lines out of our calibration, we took a flow path. We plotted that onto a map, and then we also pulled up our hydrochemical data base, and we tried to see where we had hydrochemical sampling. What this does is it gives us an idea potentially of how hydrochemistry evolves down a flow path. And so what we're using is we're using our flow model, and we're using an independent check to see if that flow model flow path is a likely representation of the flow system, and we're using hydrochemistry as an independent check, and I believe Zell Peterman will also talk some more about that.

So what did we learn? Well, we were able to characterize in a three dimensional manner the system, locating regional and subregional boundaries, flow paths, discharge areas. We noted the importance, the very, very significant importance of the Death Valley salt pan as the major discharge area in the flow system, and how critical it is to measure that flux from the salt pan.

We noted the significance and the complexity of the framework and how changes in our framework interpretation can change the results of the flow model. So understanding the framework is the key to getting a good representation of the flow system, and then also the configuration, the geometry of this carbonate aquifer.

One of the first scenarios that we used the flow model for was to use it to evaluate changes in climate and how that would affect the flow system. We had two simulations. We did a simulation of a climate for 21,000 years ago. This was the close of the last major glacial. And then also a simulation of a potential global warming climate.

In the past simulation, we noted that recharge over the entire domain increased about five times. We got a water level increase over the entire domain, but this was dominantly within the upper layers. This was in their shallow system. What we noticed was that at Yucca Mountain, at the repository, we saw a 60 meter rise in the water table, north of the large hydraulic gradient, the 150 meter rise. This is well below the repository horizon. So based on this simulation, climate change, even a five time increase in recharge, would really have no effect.

On the future simulation, we saw 1 1/2 per cent--excuse me--a 1 1/2 times increase in recharge, and we only saw a 15 meter rise at the repository, again very well below.

The latest efforts in regional modeling have been to sort of combine data and information being conducted by a lot of other programs, federal programs within the Death Valley Basin. Along with Yucca Mountain site characterization, DOE folks on the Nevada Test Site have been conducting regional studies as part of their environmental remediation program, as part of the defense programs. The

National Park Service, BLM, State of Nevada, Nye and Inyo Counties are all out there conducting independent studies. And what the USGS, Yucca Mountain project and DOE have begun is an effort to combine all of these resources and data interpretations into a single comprehensive regional data base and flow model, cooperating with state, federal and local agencies. And the ultimate goal is not only to have a tool for site characterization, but to have a tool into the future that can be used for a ground-water management tool within the basin. So when we worry about or have concerns about development, increased ground-water withdrawals, this data base can be used for it.

And we see here on this slide, in solid line, you see the outline of the Yucca Mountain model, and then in the dashed, you see the outline of the model developed for the Nevada Test Site program.

The schedule for this study is to complete a revised steady-state representation of this flow model, similar to the one I just presented to you. This would be done by the year--by the end of fiscal year 2000, in time for the license application at Yucca Mountain, and then to develop a transient model for three years out. So we've developed a very good, very reasonable, very detailed representation of the flow system. That's being used currently.

And based on that model that we develop, we recognize the room for improvement, and we've begun the process of improving that model to develop an even better flow model. So part of the science effort here is to constantly improve our interpretations. And that's what I've got.

PARIZEK: Thank you, Frank. Paul Craig?

CRAIG: Yeah, Paul Craig, Board. A couple of questions.

This last slide suggests that you don't at the moment have a transient model. Does that mean you can't say anything about flow paths from the mountain once it hits the unsaturated zone--or the saturated zone, is the first part? I'll let you do these together. The second is a longer range question and the one I'm really interested in, and that has to do with stability of knowledge. We've heard a lot about changing interpretations of various aspects of the mountain. And my question for you is do you believe that understanding of the model is now stable in the following sense? You don't expect major conceptual surprises if you do additional research, and your values with the associated uncertainty are stable, in the sense that there is a high probability that the uncertainty bands won't change significantly.

And what I mean by stable is that if you were given an unconstrained research budget and unlimited time and no management constraints, you wouldn't get any surprises of the sort that I described. That doesn't mean it wouldn't happen. But given the present state of knowledge, you don't think it would happen.

D'AGNESE: Sounds like a dream I've had several times.

Okay, the first question, we currently do not have a transient mode for the regional model. We have a steady-state model. A steady-state model does give us flow paths. It does define ground-water flow paths for us. What we don't have is a concept of travel time. We can build that into the flow model. It's not currently in the current published version. On the site scale, which John Czarnecki will present later on this afternoon, that model will have built into it, I believe he is calibrating in a transient mode, carrying it out many time steps.

So we can define flow paths currently. We don't have our finger on travel times yet. That's what we're looking to get with the additional studies.

We do not expect to see any new major changes in our interpretation. We do have some areas within the flow system where, you know, there's just absolutely no data, and unfortunately, some of these areas are relatively close to Yucca Mountain. One point that I'd point to is the Timber Mountain area just north of Yucca Mountain. We still don't have a lot of subsurface information north of Yucca Mountain. And so any new information we get there reduces our uncertainty.

CRAIG: Do you anticipate no surprises if you were to go there? The question has to do not with collecting data, which one can always do, but rather with anticipation of surprises. If you drilled a hole there, is there any reason to believe you would be surprised?

D'AGNESE: I think that in some of these areas, there is potential for surprise because in some areas, we may think that we would not expect to see the carbonate aquifer in the Timber Mountain area because it's a resurgent dome, you know, an old volcano type of thing, we might locate carbonate, and that would change the flow system in that area significantly.

So there's still some key areas where we may not have a high level of certainty. Now, what we do in our flow modeling is we track uncertainty in our observations as we're doing our ground-water modeling. And so one of the major tasks in our modeling study, along with just doing a model calibration, is also to track this uncertainty, and so we're constantly trying to reduce that uncertainty in observations. By reducing that uncertainty in our observations, we can reduce our uncertainty in the model. And so we will have at the end an assessment of what we think the regional flow paths and travel times are, and what our level of uncertainty and certainty is. So that is a product. That's built in.

PARIZEK: Dan Bullen?

BULLEN: Bullen, Board.

Could you go back to your geochemistry slide, Number 16?

D'AGNESE: Okay.

BULLEN: And as I understand it, this slide basically tells you the change in the chemistry of the water as it flows through the flow field; that's what the significance is?

D'AGNESE: Yeah. What we're showing is that the discharging area is here at Grapevine Springs, and what we see are areas like at Lida Valley Well, Carter Spring, O'Hara Spring and Bonnie Claire Airport, those are further up the flow line toward the recharge area.

Now, as these move toward the discharge area, they mix and the chemistry changes, and ultimately, we end up with a chemistry that we would see at Grapevine Springs.

BULLEN: I guess the question is with your future predictions where you noted water table rise, would you be able to predict the change in the geochemistry of the water, or are you going to go into a different stratus so that this data won't be relevant if I'm 40 meters higher at Yucca Mountain, or 150 meters higher, you know, north of the Timber Mountain area?

D'AGNESE: What we noticed when we did our climate change scenarios and we saw the water table rise, what you would be concerned about is does the configuration of the water table change? Do the dominant flow paths change? And we really don't see that happening. Even at the 21,000 year ago climate scenario, you don't see the major regional flow paths changing. In other words, there's some really dominant structural geological controls that even under increased recharge, increased flux through the system, those flow paths are not changing. So the type of scenario that you're describing, you really would not see that occurring. You wouldn't have to worry about any dominant changes in flow paths as a result of climate change.

BULLEN: Thank you.

PARIZEK: Jared Cohon?

COHON: Cohon, Board.

On your Slide 11, you presented the subregions that you created, and you said that there's very little flux between these subregions.

D'AGNESE: That's correct.

COHON: What's the basis for that claim?

D'AGNESE: What we look for are major framework controls, major geologic structures that would be like the distribution of low permeability rocks along these boundaries. And what these boundaries represent are the continuation of very low permeability structures, land forms within the region. And we also look at the water level observations. We develop an interpretation of dominant flow direction, and we see that these boundaries that we've defined here are pretty much what we would call a ground-water divide. So it's a high point in the regional potentiometric water table configuration.

We then take that and we stick it into the flow model, and we let the flow model calculate what the trends of ground-water flow are. The ground-water model again emphasizes that these are divides, ground-water divides within the system, and we only see very small amounts of flux in these areas.

COHON: Is this bearing up as you're starting the process of revising the model in light of the other work that's being done by DOE and DOD?

D'AGNESE: I didn't hear the first part of the question.

COHON: Are these subregion boundaries standing up as you're starting to do--

D'AGNESE: Oh, yeah.

COHON: So you're getting confirmation of that?

D'AGNESE: Here's a good example. The area you see dashed is the boundary used in another DOE NEVOO, Nevada Test Site model, and you can see that these used as their flow model boundaries, pretty much the same exact ground water divides that we were using. And what we're finding is that their interpretation--their simulation within the same domain where the two models sort of overlay, are similar.

COHON: I'm assuming that these subregion boundaries are developed primarily from those cross-sections that you showed us. And are they using the same data to create their subregion boundaries?

D'AGNESE: With the Nevada Test Site model, they used the same data, but they also had supplementary data on top of that. So they had more recent data. They had--I hate to bring it up--but they had a larger budget. So they had more geologists.

So what we find is that there are no major changes in geologic interpretation with their data set and our data set, but there are some minor configuration differences. But those minor configuration differences do potentially have an effect on some of the flow paths in the area.

COHON: Okay. Second question. On the climate change simulation that you did, you assumed two times CO2 global warming.

D'AGNESE: Right.

COHON: How did you go from that to rainfall at Yucca Mountain, or on the region?

D'AGNESE: As part of the Yucca Mountain Site Characterization Program, climate studies were conducted by the National Center for Atmospheric Research. They developed a global model where they input--

COHON: That's fine.

D'AGNESE: Basically a global model, increased CO2. They developed the precipitation distribution.

COHON: Last question. This is not a well formed question because I don't fully understand it. I'm trying to understand the relevance of your model to Yucca Mountain itself. My feeling is that what you've got here is going to be gross in a spatial sense, and therefore, undoubtedly valuable in knowing where discharge areas are. I mean, we sort of know that, and you're confirming it. But, for example, if I wanted to predict dilution from discharges coming from the repository itself, is your model going to support that?

D'AGNESE: Whether we want to admit it to ourselves or not, Yucca Mountain is superimposed on a very significant carbonate system. The flow within the local Yucca Mountain system is controlled by that carbonate system. What we're doing here is helping to define the localized boundaries of this Yucca Mountain flow system. Okay?

What we're also defining is the flux into and out of that flow system, and the only way we can get at that is by developing a model of this scale. If we just put a box around Yucca Mountain and modelled it as if the rest of the world didn't matter, we wouldn't get that amount. We'd have to back calculate it. And that's nothing to, you know, rest your hat on.

So dilution comes from flux. Flux at local scales come from flux at regional scales. It's the only way we could get at it. So, yes, it has a very important--it's a very important control on our understanding of flux and dilution.

PARIZEK: Debra Knopman?

KNOPMAN: Knopman, Board.

Just following up a little bit on that, in the Yucca Mountain area, you don't have the saturated zone; it's above the carbonate aquifer.

D'AGNESE: That's correct.

KNOPMAN: Okay. And so you've got some presumably then different flow regime, different controls on flow in that area. What, and maybe you said this, I know you've divided your model up into these zones, but what sort of discretization in at least the Yucca Mountain area does your regional model have? And this question is leading to the water budget questions of really how much do we know is coming into that area and going out, coming through that flow cell?

D'AGNESE: We get at this by two different ways. Geologically, what we've done with the regional scale model is we've broken into major what we call hydrogeologic units. Within the model, particularly around Yucca Mountain, we have the carbonates. Okay, that would be the blue. We've also broken down the volcanic system into three major units, dominantly the welded volcanic rocks and the non-welded volcanic rocks that you hear so much about at Yucca Mountain.

Spatially, what we have in the model is a discretization of one and a half kilometers. So each one of these little blocks that you see here are one and a half kilometer blocks. In the vertical, our regional model is broken down currently into three layers. Those three layers are at 500, 750 and 1500 meter thicknesses. So your 500 meter would be somewhat thinner, somewhat thicker and then the thickest would be at the bottom. And the attempt there is to represent local, subregional and regional flow paths that are superimposed on each other.

Another approach may be to just take the model and to slice it into as many layers as you possibly can. The trouble there is as we get deeper into the system, we have less knowledge of what's going on there. So it's more appropriate to discretize larger chunks of mass.

So the discretization that we have there is appropriate for this scale of modeling. We develop a water budget for the system at that scale, and then we assess how much water is moving into or out of our Yucca Mountain domain. And then later this afternoon, you'll learn more about that site model and how that represents the detail at

Yucca Mountain.

KNOPMAN: Okay. Knopman, Board.

From mod flow, you can tell a water budget per cell?

D'AGNESE: That's correct.

KNOPMAN: And then you have associated with that, some uncertainties?

D'AGNESE: That's correct.

KNOPMAN: Okay. But at present, you don't really have a handle on, or do you, can you give us an idea of what sort of uncertainty is associated with the water budget in the Yucca Mountain area of your regional model?

D'AGNESE: Yeah. The only way that we have of getting the dominant or the modeler's module would be Q in equals Q out, flux in equals flux out. So the only way we could get at the flux out of the system, or the flux into the system is to go to each of these natural discharge points and measure. Okay?

We had the opportunity to measure sites like Ash Meadows, Oasis Valley as part of either Yucca Mountain studies or other NTS studies, and the measurements that we're getting there are plus or minus 20, plus or minus 15 per cent. Okay? So that's about as good as it's going to get when you're measuring evapotranspiration off of some of these sites.

We still have some large uncertainties associated with like the Death Valley salt pan. Estimates in the past have ranged an order of magnitude, and so we still need to tie those down to reduce the uncertainty.

So I would say right now in the budget, in some parts of the domain, we may have an error as much as--you know, as small as 15 per cent. In other parts of the domain, we may have it as large as 50 per cent, perhaps larger. The only way we could reduce that is to actually go out and measure.

PARIZEK: Parizek, Board.

When you talk about the 1998 combination of data bases, is that mainly for the site scale model with the regional model, or does that also include, say, the NTS modeling efforts?

D'AGNESE: At the end of '98, what we plan to have is a combination of Yucca Mountain and NTS regional data bases.

PARIZEK: Okay. And then for the 21,000 year ago simulation, that was steady state. How close did you get, say, in terms of the water table configuration projected versus, say, known paleospring deposits? That's a calibration possibility--

D'AGNESE: Yeah, and we noted that. During that 21,000 year ago simulation, we did have discharge in all of the paleospring discharges that we had dated as being 21,000 years old. So it was a direct match.

PARIZEK: Okay. And will the model be used to project, say, rate of water table configuration change with climate change? You go from 21,000 years ago to, say, present, and then with global warming. But in the future with the climate change, you could have a slow response to regional water table rises, and that drives the whole flow system.

D'AGNESE: Ideally, what you would like to see is to evaluate a gradual change of the system, the response of the system over time to those climatic changes. Is that what you're getting at?

PARIZEK: Yeah. Is that planned?

D'AGNESE: It's being proposed.

PARIZEK: Proposed, okay. And then pumpage, say, in the Amargosa farms area, you have a short period I guess of withdrawal implied in the transient model. Will there be a longer term greater volume of water withdrawal effect?

D'AGNESE: What's proposed in this timeline that I've just showed you now is to conduct a transient simulation during the entire historical record. So starting in about 1913, out in Pahrump, we began pumping. We will track ground-water withdrawals throughout the entire basin from about the turn of the century to present. So all of those time steps will be included.

PARIZEK: But no future--

D'AGNESE: It's proposed, but it would probably be, you know, further down the line.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

This might be a little naive, but there's general extension going on as an ongoing process in the basin range area. To what extent can your model accommodate the changes in hydraulic conductivity and maybe even changing in discharge points or flow paths that might happen as a result past or future on the overall hydrologic model that you've created?

D'AGNESE: That's an excellent question. We've incorporated into this flow model, a structural model. What we've noticed is that the major discharge points throughout the entire region, Ash Meadows, Furnace Creek, these discharge areas are typically where a northwest-southeast trending, low K zone, which is in relative compression, is intersecting with a northeast-southwest trending zone that's in relative extension. Those zones are placed in the model explicitly so that we can actually go in and change the hydraulic conductivity, the permeability of these zones, to account for exactly what you're describing.

So it was all part of the original objective to evaluate structural changes within the region. So that whole scenario has been built into the flow model so they can be conducted. We have not yet conducted it, but the opportunity is there.

NELSON: And do you expect to include the idea of localization that I could imagine, particularly with carbonate has a head on it that causes upward flow; isn't that correct?

D'AGNESE: Yeah.

NELSON: So that you could actually introduce some discharge points or turn them off?

D'AGNESE: That's correct.

NELSON: As a function of this.

D'AGNESE: Yeah, you would expect to see that.

PARIZEK: Other Board questions? Staff? No staff? We do have one member of the public who has a tight schedule, so we'll take his question now. Please state your name.

DALEE: My name is Michael Dalee. I will try to be back later, but I don't think I can. I just had a specific question.

I was looking for numbers. You mentioned something about discharge. I was looking for how you came up with any figures for recharge, if you just used earlier studies, I think most all of which go back to the Maxey-Eakin method of the Fifties? And if you have any acre feet numbers, I'd be very interested to hear those.

D'AGNESE: We can't directly measure recharge, and so we get at recharge from reducing our uncertainty in discharge. The method used in the regional model was based on the Maxey-Eakin, although it's a modification of Maxey-Eakin. I don't have volumes, acre feet volumes off the top of my head.

My recommendation would be to get a copy of the report, WRI 96-4300. What we do is we break down each one of the surface water basins of the Death Valley Basin. We have estimates of recharge and estimates of discharge. So what we've done is a water budget accounting of the entire basin by what we call hydrographic area. But that is available.

COHON: If he gives you his name and address, could you send that for him?

D'AGNESE: I will do that.

COHON: Thanks.

PARIZEK: I think we ought to go on with our next speaker. Thank you very much, Frank.

While Frank is getting unhooked there, I'll introduce Zell Peterman, who's going to talk about significance of hydrochemical domains in the saturated zone at Yucca Mountain. This is a fingerprinting of masses of water to see if it could help in any way to verify ground-water flow models, and also should help us with some understanding of the ages of the water, as well as evolution of the water in the flow path.

Zell has been on the program and active with the U.

S. Geological Survey for a number of years, and brings a lot of credibility to the program.

PETERMAN: Thanks, Dick.

Yesterday, you heard Russ Dyer mention a couple of times the free emphasis of hydrochemistry in the project, and today, I'd like to summarize the USGS part of that, which has been in effect now for about three and a half months. In addition, right after me, you'll hear What Los Alamos is doing in terms of EH pH measurements and hydrochemical modeling. And I should mention a couple other efforts that are being valuable to the Yucca Mountain project, the NTS Environmental Restoration Program has a very aggressive hydrochemistry isotope effort on site being conducted, and this is mainly up at Pahute Mesa, Yucca Flat, conducted by Lawrence Livermore, and off-site, there's just a newly started Oasis Valley project which involves a number of participants, including Livermore, Los Alamos, USGS, both Nevada District and sub-district, people from the Yucca Mountain project branch, some of my people are working on it, DRI, UNLV.

Last summer, twelve new wells were drilled up north and northeast of Beatty. So this is a big effort. This is going to help us dramatically to understand the flow systems on a regional scale, and contribute to Frank's refinement of the model, the regional flow model.

Dick Parizek outlined the major saturated zones, or some of the major technical issues that relate to the saturated zone, in his introduction, and some of these same ones are shown here, of course, the amount of recharge through Yucca Mountain and the amount of water going through the saturated zone, reaching the water table at Yucca Mountain, extent of mixing. Some of the saturated zone experts have told us not to rely too heavily on mixing below the potential repository and also down gradient in terms of how much we can expect from dispersion. Matrix diffusion is always an issue in a fractured media, what sort of interaction is there between the matrix and the water flowing through the fractures.

Of course leakage from the carbonate aquifer, which has been mentioned already, delineation of up and down gradient flow paths, travel times; all these things are critical. I think the hydrochemistry and the isotope studies will contribute to a better understanding of many of these key issues.

What's USGS doing in the program? Our efforts for this last three and a half months have been sort of two-fold. We've been sampling and analyzing water from WT-24. I might mention that we do have analyses now from the first round of pumping at WT-24, and both dissolved ion chemistry, stable isotope, strontium isotopes, uranium isotopes, all suggest strongly that that first water that was encountered is indeed perched. It has all the same chemical and isotopic attributes as the perched water at UZ-14.

So we're sampling wells. We'll be collaborating with Los Alamos and sampling some of the older WT wells. We're kind of behind schedule on that. WT-17 was scheduled to be sampled late last year, but it's been delayed mostly by the slowness of getting discharge permits.

We're also looking at the existing data. We're constructing an integrated isotopic/hydrochemical data base, and somewhat interpretative, there's a lot of data out there for some wells, there's so much data for the casual user, it's hard to know how to use the data. So we're evaluating the data. We're coming up with our best analyses. We're going to have a best composition of J-13, for example, which integrates the dissolved ion and the isotopic data, that sort of thing. So that's our data base approach.

One way we can use this data is what I call mapping. The Survey is good at mapping, of course, and we basically want to map out hydrochemical domains, and then of course to use these maps to delineate flow paths and to integrate that information into the regional flow model.

In terms of what I'm going to talk about today, late last year, we had our Level 4 models going, which integrated isotopic and dissolved ion chemistry for Yucca

Mountain proper, and this year, we're expanding that to the south, east, west and north. And basically, I'll just take that same Yucca Mountain rectangle and I'm going to move it south, which is what you see over on this other slide.

Last year, we were down to hear, and now we're in pretty good shape down this far. The slide on the left just is an image slide that shows relative elevation, because you don't get a feeling of elevation from the shaded relief map. So just to show that like everywhere else in the world, water pretty much runs downhill and does really follow regional topography.

The flow lines there on the left-hand diagram I think are pretty much consistent with the potentiometric contours in the regional flow system.

I want to mention these discharge sites were mentioned by Frank, Ash Meadows here where the regional carbonate aquifer discharge is, Franklyn Lake Playa where something discharges, I'm not sure we fully understand which system or what combination of systems, and then Death Valley, which is off this area here. Now, hopefully by the spring, we will have extended our refined data base both to the east and the west, and also to the north to take into consideration all of the new data that Livermore is generating up on Pahute Mesa.

The type of data that we have of course are dissolved elements, minor and trace elements. Let me step back one point. When I mentioned the other work that was going on, I forgot to mention the work that's being conducted by UNLV, by Klaus Stetzenbach and his group over there. They're doing some very interesting regional studies using trace, dissolved trace elements, in particular things that they view as semi-conservative. So this is also going to be a very useful data set that will be incorporated into our thinking.

So we have the major elements, some minor and trace elements. We have the standard stable isotopes, oxygen, hydrogen, carbon isotopes, the radioactive isotopes, tritium, Chlorine 36, Carbon 14, a lot of interest in Chlorine 36. And I should mention that there's really a fairly large amount of data developing on Chlorine 36 in the saturated zone ground waters, and mostly on-site by Los Alamos, regionally by Livermore, and the patterns that are emerging are very interesting. I'm not going to talk about that today. There's just not time. But interesting regional patterns, and of course it was, you know, proposed years ago that Chlorine 36 might be a chronometer for ground water dating. It does not appear that that's going to be useful in this area. Obviously chlorine is not conservative on a regional scale.

Radiogenic isotopes, strontium and uranium; again, we're developing a rather large uranium isotope data base, but there's really not time to mention that either.

One of our major analytical needs of course is a reliable method to date ground water, and this of course is not unique to Yucca Mountain. It's a widely needed technique. Carbon 14 of course is--conventional Carbon 14 without interpretation is inadequate. In order to interpret it, you have to model it, so there's always questions on how you model the conventional Carbon 14 ages.

I think there is one hope, and that is this technique of separating out the dissolved organic carbon, which should come from recharge from the dissolved inorganic carbon, which is acquired along the flow path, and then if these can be separated, then you date the dissolved organic carbon. And from the work of Jim Thomas in Southern Nevada, including work on Ash Meadows, to me it looks very encouraging.

The travel times for the Ash Meadows system have gone from as much as 30,000 years ago, 30,000 years two decades ago, to probably less than 2,000 today. The other thing that argues that that dating is appropriate there is the fact that such a crisp climate signal is sequestered in those carbonates at Devil's Hole. If you had a long flow path or multiple flow paths of different lengths, you would blur that signal, and that signal is just as crisp as the

Marine isotopic signal.

Okay, let's push on here. Stable isotope data; this is all the stable isotope data that exists in that rectangle. And interesting things here; current precipitation of course would have a wide range from down in here to up here, and average, you might say average current recharge would be somewhere in this region here. The interesting thing here of course is the results from Franklyn Lake Playa, which show this classic evaporation curve, tremendous evaporation occurring there, which modifies the chemistry.

I think with this sort of relationship, we have the hope of taking this sort of thing, calculating how much evaporation has occurred, going back to the dissolved ion chemistry and then reconstructing what the apparent would have been, and then hopefully using that reconstruction to identify what water was in the Franklyn Lake Playa. The question of course is it Ash Meadows water from the region of carbonate, is it Yucca Mountain water, or is it water coming down the Amargosa, or is it some combination of any of those three.

Let's look at the hydrogen isotopes. Of course hydrogen and oxygen are conservative, or considered to be conservative from recharge. I apologize for these maps. Had I known the room was this long, I would have tried to make these illustrations larger. And I won't spend much time, except as I go through these, what you'll see is different isotopic and hydrochemical domains emerge, and then we have to ask the question what do these domains mean.

Our first step is trying to map these out. Let me just point out a couple of things. There are several features on here. This well over here and this closed contour produces very little from late protozoic clastic, very tight late protozoic clastic rocks. It looks to me like that water is not moving very fast, has a deuterium content less than minus 110, 112, I think it's minus 115. It's probably the oldest water in the region. Clearly, it was recharged back in the Pleistocene in order to get water that late.

There's another closed contour up here in Crater Flat and the west side of Yucca Mountain less than minus 104. There's much heavier water coming down Forty Mile Wash, which is much more like modern recharge. So clearly, we're seeing these domains, these closed screwy looking contours down here of course reflect the evaporation that's occurring at Franklyn Lake Playa.

And I should say these data have been gridded. Obviously they're not optimal for gridding, and I should, you know, blank some of these out. I wanted to show the geography. So, you know, there are edge effects, in other words. Things as you get out near the edge of the map where there's no data, there's strange things happen and the program wouldn't be too concerned about it. We'll trim those out eventually.

Oxygen of course is coupled with hydrogen and there are no surprises here. Again, you see a very light oxygen down here at this well that's in the Precambrian. You see the effect of evaporation. You see this closed contour up here at Yucca Mountain. Oxygen varies eight times less than hydrogen, so you don't get the same numerical resolution that you do with hydrogen or deuterium.

Strontium. Ground-water attains a strontium signal at recharge, and then as it moves along a flow path is slowly modified due to interaction with the aquifer minerals. There are some very systematic patterns here. We get low values at Yucca Mountain. We get a difference here between the main part of Yucca Mountain and Forty Mile Wash, which shows up in a number of these parameters as we go along.

An interesting thing down here at Franklyn Lake Playa. Suddenly all this variation that we get up gradient converges, and the Ash Meadows water has a very limited variation in strontium, and it may be that it's due to mixing, and it may be related, you know, to density driven mixing in the playa itself. The absolute values of strontium at Franklyn Lake Playa are intermediate between Ash Meadows and the larger values coming down in the Amargosa here. So it could be that that data is telling us that we have a mixed signal coming out of Franklyn Lake Playa.

Chloride is often considered to be conservative, and I think I'd rather use the term semi-conservative. With this slide now for chloride, we start to see some things that you'll see in subsequent slides. You start to see there is a tongue or plume or whatever you want to call it of low chloride water. I've had to use the log of chloride here because from Yucca Mountain to Franklyn Lake Playa, there's a four orders of magnitude increase in concentration. So if you use a linear plot, you know, basically that will bulls eye down here, and nothing else in the whole map.

So there appears to be this plume coming southward. Now, I want to point out one thing. We've got lots of data down here in springs and supply wells in Amargosa, and quite a bit of data at Yucca Mountain. We've got a big gap in here, so we're really extrapolating some of these features through an area where we don't have a whole lot of data.

Other things; this string of points over here is the Ash Meadows discharge spring line, and that will show up in a number of chemical parameters. The difference between these southern springs and the rest of them also will appear.

Sulfate is often also considered to be what we call semi-conservative. The same type of pattern emerges here.

You have low sulfate values at Yucca Mountain. For some reason, the contouring program did not connect this low here with this low here, and it was certainly reasonable to do so, but it didn't connect it, again, because of lack of data here.

Two other things up here at the latitude of Yucca Mountain. Over in Jackass Flats, two wells were initially drilled. J-11 was drilled as a supply well. It turned out to have such high sulfate contents that it was abandoned. It couldn't be used for drinking water. So it's distinctively different than the Yucca Mountain water.

Over here in Crater Flat, there are two wells less than two miles apart, VH-1 and VH-2, and they have markedly different water compositions, in spite of the fact they produce from the same volcanic unit. We're seeing an enrichment of sulfate down here at Franklyn Lake due to evaporative concentration. Franklyn Lake is also a sink for certain elements because there are minerals precipitating, mostly calcium and magnesium bearing minerals, so you can't use the uncorrected, say cation ratios, to try to trace back water compositions, because things are not staying in solution.

This is just a summation of the alkalian earth's calcium and magnesium. Again, you see this what appears to be a tongue or plume of water coming southward. You're seeing some strange things here at Ash Meadows, with some high calcium magnesium. I think these are wells here. And then when you get to Franklyn Lake Playa, you see low values, because calcium and magnesium is precipitating out as calcite, montmorillonite, that sort of thing sepiolite.

The alkalis are a little different. Again, you see that--

PARIZEK: Excuse me, Zell. Do you want to maybe stand to one side a little bit? Because you're apparently blocking our view.

PETERMAN: Okay. You see this same plume or tongue or whatever coming southward. You see an enrichment again. This is an evaporative enrichment down here at Franklyn Lake Playa. So the same patterns are emerging in these different chemical and isotopic parameters.

Look at the ratio between those two elements, which is always useful. You get a little, even a different perspective, and it shows up a couple things which I want to mention. Here again there's a low, there's this difference between eastern and western Yucca Mountain in terms of the ratio, the western Yucca Mountain samples tending to be more sodic than the eastern. A marked difference over here at Jackass Flat, tremendous difference here between VH-1 and VH-2, and this is where I think you have to start to look at the chemistries as certainly suggesting the possibility of compartmentalized flow.

The geologic control here is that, you know, there's this line of craters, volcanic little cinder columns over here on Crater Flat from whence the name comes, and one of the wells is on the west side, VA-2 on the west side, and VH-1 on the east side. Now, it's certainly possible there's some sort of feeder drive going up in Crater Flat and it's causing a hydraulic impediment to communication. I don't believe that when you construct the isopleths here, of course you get these gradients. I doubt that there's a compositional gradient. I think there's probably a compositional discontinuity between those two ground-water domains.

Let's see, what else? The other thing that stands out very nicely of course is the Ash Meadows discharge and a high calcium-sodium ratio is characteristic of the regional carbonate water compared to the water that's in the volcanic aquifer. And, again, the ratios decrease down here because calcium and magnesium are precipitating out as calcite and sepiolite.

So with the domains that we have, I think that we can currently define on the basis of the hydrochemistry. I think I've tried to point them out as we've gone along here. Jackass Flat, where we have the high sulfate in water of J-11, Forty Mile Canyon, which has stable isotopes closest to modern recharge, has the youngest Carbon-14 ages. It's probably the youngest water in the system. It's certainly a preferential pathway. The corrected Carbon 14 ages suggest that water from the upper reaches down to J-12 or J-13 is probably taking several thousand years.

We have the Yucca Mountain domain, east and west half, with a difference in the alkalis and alkaline earths, the Crater Flat domains. Certainly the eastern part of Crater Flat, VH-1, is indistinguishable from the Yucca Mountain waters. The western water in VH-2 has water that looks very much like water from the regional carbonate aquifer. It may simply reflect some degree of recharge through Bare Mountain, which is immediately to the west. Down gradient domains, the Amargosa Desert; in a number of the maps you saw there were closed isopleths down there, which indicated complexity and we're probably looking at mixing between the Forty Mile Wash system, the Yucca Mountain system and the Crater Flat system.

The Ash Meadows domain of course is discharged from the regional carbonate aquifer, and the Franklyn Lake Playa domain is a domain that's characterized by evaporation and concentration of dissolved ions and modification of stable isotopes.

So I think what we're looking at this year, we want to continue on with getting our refined data base together that integrates the isotopes. As we went through the maps, you saw that we have a lot of dissolved ion chemistry. We're really weak on isotopic data, especially down gradient. So what we want to do is in the next few months, come down here, try to resample a lot of these supply wells, build up our isotopic data base. I think that's going to help us probably as much as anything in trying to delineate these down gradient flow paths.

We'll continue to sample the new wells up at Yucca Mountain, SD-6, saturated one. We hit the saturated zone at WT-24, plus next year, SD-9 and SD-11 and 13. We want to try to get back into some of the WT wells, Los Alamos will resample those.

Basically, we're aiming at better delineation of flow paths, and of course we have to move up gradient also and incorporate that data. We have to understand what is coming into the system from the north, and that's mainly--we won't be gathering any new data up there. We'll be integrating what Livermore has produced for the environmental restoration program.

PARIZEK: Thank you, Zell. It's a lot going on in a short time, but it's based on years of background work.

Board members with questions? Yes, Paul Craig?

CRAIG: Craig, Board.

There was a lot of information. That was wonderful. For our purposes, one of the things we're most concerned about is the travel time through the saturated zone, obviously, and you gave us a set of numbers that had something to do with that. What I'd like to ask you to do is to give your best estimate, given your whole body of knowledge, on what is a reasonable travel time for Yucca Mountain water once it hits the saturated down, down into the valley, and what kind of standard deviation would you assign to that?

PETERMAN: I'll probably equivocate on that. There's a lot of conventional Carbon-14 data, there's a lot of Carbon-14 data for the wells up here in the Amargosa. They're running from, uncorrected, 10,000, 15,000 years. Clearly, they've incorporated dead carbon, so that has to be corrected out.

Ed Kwickless has been working on modeling the conventional Carbon-14 ages at Yucca Mountain. He generally reduces the ages by modeling by, oh, say several thousand years. In terms of flow velocity, I'd have to calculate that. Coming down from the upper reaches of Forty Mile Wash, UV-19, A-1 and 2 are up in here, and Jim Thomas at the district office is currently doing Carbon-14 on the dissolved organic carbon, and we think he's going to get zero. We think there's recharge up there.

And then you've got wells down here that are giving corrected ages of 4,000 to 5,000 years. So whatever that distance is, and I know--well, I've got a scale here--whatever that distance is, divided by--or divide 5,000 years by that distance, and that's the best control we have. That's only on Forty Mile Wash. Forty Mile Wash is a domain in the system. It's probably a preferential pathway. So I'm not going to give you a figure.

CRAIG: Well, what is emerging from what you just said is a time like 5,000 years.

PETERMAN: Yes. What I'd really like to say is I think we've got hope in answering that question. I don't think we have the answer right now. I think we get in there, get dissolved organic carbon, C-14, we're going to get some good realistic ages on the time between recharge and where that water is now, and then, you know, incorporate that into the model, we are going to be able to come up with some answers. I don't think we have them right now.

CRAIG: What's the probability that it's a thousand years or less?

PETERMAN: I think that's extremely remote based on what we see at Yucca Mountain. I think Forty Mile Wash is probably as fast as it gets, and there we're talking about 5,000 years over a few kilometers.

CRAIG: Okay. That's pretty helpful.

PARIZEK: Alberto Sagηes?

SAGγES: Yes, Sagηes, Board.

Down at the Franklyn Lake Playa that will be presumably one of the areas where some of the radionuclides may be emerging, what is the likelihood that there will be a concentration of those radionuclides at the surface of the playa?

PETERMAN: Well, anything that's in the water going into Franklyn Lake Playa is concentrated by evaporation. So anything that's dissolved will be concentrated.

SAGγES: Can you envision any scenario where a good fraction of the radionuclides will end up piling up and reconcentrating?

PETERMAN: I think that's something that with the refinement of the hydrochemical and isotope data, attempting to incorporate that into the regional flow model, right now, there's some debate what proportion goes to Franklyn Lake, what proportion goes to Death Valley. With a long enough flow path, of course it's sort of immaterial. By the time it gets to Franklyn Lake Playa, you know, you may be talking 5,000, 10,000 years. So much of the material will already be decayed. You'll have some exotic elements, but they're certainly not going to be a hazard.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

I must admit I'm a little bit confused. You've got a lot of well information there, and then contours made. Are all the wells developed in the carbonate aquifer in all cases?

PETERMAN: No. I should have mentioned that. This approach to try to display the data using these isopleths does not consider the third dimension. Now, down here in Amargosa Farms, all the wells produce out of the alluvium, and there's certainly a depth difference, too, and that's something we have to look at. Frank's group is developing a parallel data base where they will try to incorporate all this third dimensional information into their data base.

For Yucca Mountain, you know, the wells produce from different volcanic units. Basically, as you go from west to east, the saturated zone is moving into the--let's see, how does that work--it moves from the older into the younger, tends to move from the older into the younger volcanic units, although it's all chopped up by faults, so there's not a real consistent pattern. Right now, we're not considering--our isotopes, say our strontium isotopes, which you would think would be sensitive to water-rock interaction, it doesn't matter at Yucca Mountain whether we're producing from the Calico Hills or the Prow Pass or the Topopah Spring, and we have wells from all of those, the isotopes are the same, which suggests that water-rock interaction is very low.

But to answer your question, there's only one well at Yucca Mountain, and that's P-1 that goes into the Palezoics. All these others are in either the volcanics or alluvium, with the exception of the spring discharge at Ash Meadows, which everybody agrees is the regional carbonate aquifer discharge.

NELSON: Okay. When you were talking about Forty Mile Wash and travel times, whatever, what unit was that travel time developed through?

PETERMAN: The wells J-12, J-13, those supply wells, JF-3 in Forty Mile Wash in the lower part there are all in the Topopah Spring. JF-3 produces--that's about the only neat well we've got. That was designed to only sample the water in the Topopah Spring. J-13 gets 90 per cent of its water from the Topopah and about 10 per cent from another spring, which is down in the Prow Pass. J-12 gets most of its water from the Topopah.

In the upper reaches, UV-29, A-1 and 2, those are in the alluvium. The deeper one goes down into bedrock. So, you know, you never quite have everything you want when it comes to wells and where they produce water from. But there's an awful lot of useful information.

PARIZEK: I was going to ask a question, Parizek, about the twelve wells up at the Beatty flow system. That's a lot of commitment. You mentioned a whole group of people doing that work.

PETERMAN: Yes.

PARIZEK: Is that driven by NTS concerns? It would seem like Yucca Mountain flows are more apt to come down in the Amargosa Valley area where we are here. Why not 13 or 12 wells on 95 and somewhere in that area to resolve some of the things you point out where you have data deficiencies; why Oasis Valley emphasis?

PETERMAN: The Oasis Valley project is driven by the concern about the possibility of movement from the northwestern part of the test site, Pahute Mesa.

PARIZEK: So it's a test site driven concern?

PETERMAN: Yes.

PARIZEK: Should Frank stay alert and ready to go back to the drawing board in his regional model, based on the new data that you're putting together? I mean, are there any surprises that you sort of see coming based on your isotopic work and all the chemistry work?

PETERMAN: We've worked with Frank and been through one cycle, what, a couple years ago, of selecting certain flow paths and then testing, using the hydrochemistry to test. So, no, I don't think so. I think one of the big questions is this discharge, down gradient discharge sites at Franklyn Lake Playa, Death Valley, what system is feeding what, or what combinations, those sorts of things that I hope we can de-convolute by getting a better look at the isotopes and the hydrochemistry.

PARIZEK: The present concern is Franklyn Lake Playa is probably mixed waters; it's not a simple system necessarily. I mean, flow systems are coming from more than one direction.

PETERMAN: Yes, that would be my current feeling.

PARIZEK; And the time when you may get some documents together based on all of this activity, there's a lot going on in a short time, there's very powerful data, do you have some idea of when your deliverables might be expected?

PETERMAN: We're pushing--well, of course the sampling of the new wells depends on the drilling and when we can get into some of the old WT wells, which Arend will probably address.

In terms of the data base, you know, I'm hoping that by late this spring, we'll have pretty much everything that's available in this new integrated data base. I was just looking at the schedule, and somehow, I don't know how it happened, but we escaped having a milestone on that this fiscal year. I think it was part of the multi-year planning process where we encouraged not to pay attention to fiscal years, but pay attention to the work that needs to be done and when it needs to be done. So our milestones for the data base are next fiscal year, but that doesn't mean we're not going to continue to push it just as hard as we can, because we need that data base in order to guide some of our new work down gradient.

PARIZEK: All right, thank you. Staff? If not, I think we ought to go on. It's an important discussion.

Our next speaker is Arend Meijer, and he'll talk on model for major ion chemistry of the saturated zone waters along flow lines through Yucca Mountain. He's with Los Alamos National Laboratory.

MEIJER: I have to apologize that the actual title of the presentation is going to be inferences from saturated zone ground water chemistry and implications for transport parameters. The original presentation was going to be on a model for major element chemistry, but it was felt that coming up with these inferences and implications was more appropriate for this group.

I do have some slides concerning a model for major ion chemistry, and if there's interest, we can talk about that in the question and answer session.

This then is the title. The first slide may be a little imposing. I don't intend this to be a discussion of detailed chemistry, but we do need to discuss some chemical reactions, because they have implications for transport. And here, I basically reviewed some of the major chemical reactions that are likely to control water chemistry in the flow system that goes through Yucca Mountain. And this is one of the reasons I borrowed a slide from Zell, and that is the discussion I'm going to present today is focused on this tongue of the flow system, and basically including the area to the north. That's the recharge area in Pahute Mesa and Rainier Mesa. So I'm going to concentrate all my discussion sort of on a flow line from the recharge area, down into this Amargosa Valley region. So I'm not going to worry too much about the carbonate aquifer. I'm going to concentrate on the volcanic aquifer.

That being the case, precipitation/dissolution reactions are important, particularly in the soil zone, but also in the UZ, and also particularly in recharge areas, because that's where most of the water chemistry for the waters that are found along that flow path are imposed.

Ion exchange is quite important. Ion exchange on zeolites, hydrogen exchange on glass and Feldspar, and then finally, hydrolysis reactions are important. This is actually probably better termed acid base reaction, but it's also hydrolysis reaction. These two reactions are probably too slow to have much impact on the water chemistry in the flow system that we're talking about.

Now let's talk about some of the inferences. In the next slid, I'll show that--well, first of all, the way that I set these up is I've presented some observations, some data, drawn some inference from that, and then given an implication of that data in the inference.

First of all, the data suggests that the major constituent concentrations, that is, the major cations, anions and silica, show only limited variability in waters along flow lines from the recharge areas, Pahute Mesa, Rainier Mesa, down through Yucca Mountain into the Amargosa Valley. The inference is that these major constituent concentrations are buffered by water-rock reactions. The only reaction that seems to go on continuously is this one right here.

The implication is that the variations in concentrations of major constituents in saturated zone waters are unlikely to have a major influence on transport parameter values in the saturated zone.

The next slide is actually the following slide in your xeroxes, and what I've done here is just given you the compositions of major ions, major cations and anions and silica in three separate waters--actually four, but three of them from the flow system that we're interested in, these three right here.

This is water from a well in the recharge area. The well happens to be TW-8 right at the south end of Rainier Mesa, I believe. It's in tertiary volcanics, so these waters reflect a chemistry that interacted with tertiary volcanics.

The second water is a water that's halfway through the flow system at Yucca Mountain, and certainly Zell has referred to this. This is J-13 water composition. Then, finally, here's a water composition that occurs further down the flow system in this region here. Actually, right in here. So I could have also taken additional compositions further down the flow path, but I chose one right down here. J-13 is up in here, and then TW-8 is up above the highest black dot that you see there.

The point that I'm making is that these three water compositions are actually quite similar, so that all along that flow path, there is not a whole lot of water-rock interaction that goes on to change the chemistry. What does go on is the pH increases significantly, also sodium increases somewhat, by a factor of two or so, and that's from this sodium-hydrogen ion reaction that I showed on the second slide. So the argument then is the water chemistry seems to be set in the recharge area, and from then on, there's not a whole lot of change until you start mixing waters from other flow systems, such as the carbonate aquifer water.

The one thing that we don't have good information on is redox potential, and I put question marks here. We're hoping to get additional information on redox potential in a number of wells at the site this year, and in fact hopefully within the next month or so. And I'll talk a little more about that later.

Okay, another set of data, inference and implications. The data is that the water along the flow paths, the ages of the water along the flow paths are whatever they are; I put up to 20,000 years BP here. That's a number that I've seen over the years. The number might actually be 15 or 10, and we can argue about that. My point is that the ages range up to some number like that.

The inference is that the time of infiltration of the water is not a major factor in controlling water chemistry, because we saw from the last slide, water chemistry doesn't seem to depend on where in the flow path you are, whether you're in the recharge area or down in the Amargosa Valley. So time of infiltration doesn't seem to be important. So the implication is that climatic variations aren't going to have a major impact on major element concentrations in the waters in the saturated zone within Yucca Mountain.

This slide talks to the relationship between the presentations in this meeting and the presentations that might be made in a strictly transport meeting where we talk about the potential transport of radionuclides along the flow path. Because in order to calculate transport, you need to have values for transport parameters, and the question is how do you get those values and what kind of water compositions do you use to get those values.

Well, the fact is that the project has used J-13 water for a lot of experiments to obtain transport parameters, and I've said here J-13 is sort of in the middle of the flow path. The inference is that J-13 water is representative of the major constituents in saturated zone waters in the volcanic aquifers. The implication is that J-13 can be used in these experiment basically because the waters along the flow path don't seem to show that much variation.

Related to this is the fact that pH in the saturated zone waters does show substantial variation from 6.5 to about 9. The inference is that there's something controlling it, and I've said partial pressure of CO2 is a factor. There may be other factors as well. The implication is that we ought to do these laboratory experiments over at least this range of pH values in order to get transport parameters that can be applied generally.

This slide starts off the rest of the presentation. This gets us into an area that we're concentrating on at the moment and will for the rest of the fiscal year and possibly into the next fiscal year.

Here, the data is that the oxidation/reduction potential in saturated zone waters at Yucca Mountain shows a range from minus hundred something or other to 365 millivolts, but we have very few data points, and those data points were collected under conditions that are not necessarily optimum. And I'll get into that in more detail in a minute.

The other data point is that methane is found in some wells. So there is a suggestion there might be reducing conditions, at least in some wells.

Inference is that based on that range of values, the oxidation/reduction potential, or Eh, of waters in the saturated zone may be sufficiently low, that is, the waters may be sufficiently reducing so that the lower oxidation states of redox sensitive radionuclides are stabilized.

The implication is if those lower oxidation states are stabilized, then nuclides such as Technetium 99 and Neptunium 237 would be greatly retarded relative to the situation under oxidizing conditions. So it's quite important to determine whether the Eh of waters in the saturated zone is sufficiently reducing.

The data that we have to date, and I mentioned the range earlier, but basically this data was obtained by Al Ogard in 1984. There's a Los Alamos report here that presents that data. The data was obtained basically on pumped water samples, samples that were pumped from depth using metallic tubing and using a metallic pump and various other things that aren't necessarily optimum for obtaining the redox potential of the water that's 1,500 feet down in the ground.

However, even under those conditions, they did see negative redox potentials, and overall, the average of the numbers that they obtained is something like 225 millivolts, and then there are also some lower values in some "thief" samples, basically dip a bucket down the well and pull up the water.

This average of 225 is very close to the potential, that is, the redox potential at which Neptunium goes from plus 5 to plus 4, and Technetium goes from plus 7 to plus 4. So the possibility exists that these radionuclides would be in the reduced form in the saturated flow system. So the rest of the talk then is about what we're going to do to try and resolve that.

This slide is thrown in here just to give you an additional handle on the radionuclides that are particularly pertinent to this discussion, and I won't go into the details there.

What we plan to do, and as I said, in the very near future here, we're going to make measurements on the redox state of saturated zone waters. We're going to make these measurements on waters that are pumped from the wells. However, the pumping systems are going to be essentially non-metallic. We'd have fiberglass tubing. We've got a non-metallic pump, and then the measurement equipment at the surface will all be non-metallic, except of course for the platinum electrode.

Wells are going to be cleaned and purged beforehand, and the wells that we're starting on are WT-17, WT-3. These are two wells in Dune Wash down gradient from the potential repository. So those are important locations to have this sort of information for.

The pumped water will be monitored not only for the platinum electrode potential, but also for a number of other redox sensitive parameters. And those include various redox couples, including total iron, Iron-2, Sulfate, Sulfide, Nitrate, Nitride. And then with Klaus Stetzenbach at UNLV, we're also intending to do Selenate, Selenite, Arsenate, Arsenite, and a number of others, so that with that whole collection of data, we can bound the redox values in the saturated zone at the locations at which we sample it.

Zell talked about the work on major constituents, environmental isotopes and other work that he says the USGS will carry out.

In summary then, the variations in the major constituents along this flow path from recharge area to Amargosa Valley, or to the accessible environment boundary, wherever you want to put that boundary, the variations are unlikely to have a major impact on transport parameters, except perhaps for pH and Eh. We're doing the experiments over a range of Eh, so we'll cover that base, and then the Eh measurements, we're going to do here shortly. And what that will require then is a more detailed analysis of how the observed pH values relate to the transport parameters. And that will be based on laboratory data.

Variations in climate are unlikely to have a significant impact, as I mentioned earlier. Water from Well J-13 seems to be close enough to representing the average of the waters in the volcanic flow path that it can be used. And then, finally, the redox potential in saturated zone may actually be sufficiently low to stabilize less mobile forms of some very important radionuclides that are now a problem for dose calculations.

Thank you.

PARIZEK: Questions from the Board? Paul Craig?

CRAIG: Craig, Board.

I'm struggling as a non-geologist to put these pieces together. So let me tell a story and tell me what's wrong with it.

You're telling us on your graph four that the time of infiltration isn't a major factor in control, which is encouraging. And then in addition, there's encouraging information in that Neptunium and Technetium, that they may not move as fast as the water. Zell has told us that 5,000 years is a plausible time for the water motion. If you can only get a factor of two out of your hold-up through the chemistry, you're now up to a time of travel in the saturated zones which is comparable to the times that are emerging in other pieces of the project, 10,000 years for the canister, and so forth.

If that's true, the saturated zone becomes an important and possibly even a determining hold-up time. What's wrong with that reasoning?

MEIJER: I like it. Well, I mean I don't necessarily disagree with it, and in fact I think I agree with it. Basically, I think that the hold-up time in saturated zone may be more than a factor of two, particularly if the redox potentials are low enough to stabilize plus four neptunium and plus four technetium. So if the question is how could the retardation times be less than a factor of two, based on the water chemistry, about the only thing I could come up with would be pH, because that shows a significant range, and under low pH conditions, radionuclides stick less to the rock surfaces than under high pH conditions.

But the fact is that the pH increases down gradient, or seems to, based on the data we have, and so that doesn't seem to be a problem. So I don't see a problem with the conclusion at the moment, but I'd like some more time to think about it. But at the moment, I don't see a problem with it.

PARIZEK: Debra Knopman?

KNOPMAN: Knopman, Board.

Wait a second. In interpreting your data from really just a couple of points here, you're sampling from the basically matrix waters as opposed to water that might be flowing through fractures where you're getting some mixture of those waters and you really--you're not talking about--or let me phrase this as a question. What really can you say about retardation between the repository site and discharge areas from, you know, five data points or less?

MEIJER: Your point is well taken. First of all, I can't say anything about retardation based on the major element chemistry. I mean, that's for a talk on the effects of major element chemistry variations on sorption coefficients, on solubility and all that. This talk is not that talk. Okay, that's the first thing.

The second thing is that I've used three data points, but the fact is, as Zell showed, we have probably a hundred data point--well, a hundred may be a little much but certainly 50 data points along that flow line. So if you look at all the data, all the data would be consistent with this argument, you know, plus or minus some error, the argument that I've made. So those are the two main points.

With regard again to the retardation, I'm also sort of calling on a previous life I had in sorption coefficient determinations and transport parameter derivation, so I'm adding in here information that I haven't given here today, but that's in the literature that you have available and probably in other talks that you'll hear on that. Not here; not today.

PARIZEK: Parizek, Board.

Do you have any idea about the origin of the methane that you observed in wells? Is it really sedimentary bedrock units, or was that in the volcanics?

MEIJER: Well, first of all, I'm not the one that found it, so I don't know exactly the conditions under which it was found. But with that caveat aside, there are suggestions here and there that there are sources of methane somewhere down there, and the fact is that there are sources of petroleum fluids and such not too far north of the test site. And so the Paleozoic aquifer, that is the carbonates, have the potential of having some reducing substance in them, including methane. And so perhaps this stuff is coming from the paleozoics, I don't know, but it has been observed in more than one place, and beyond that, I can't really make any statements.

PARIZEK: But it would be good news in terms of the hold-up of, say, technetium and neptunium?

MEIJER: You bet.

PARIZEK: But the iodine then would be the last ringer in terms of long-life nuclides?

MEIJER: Right. Iodine is actually interesting. We've taken the position to date, and again I'm talking about my previous life here, that iodine has no retardation whatsoever, and in fact may be ion excluded and, therefore, travels faster than the water on average. But once you start including an alluvium into the calculation, it turns out that iodine can be held up in alluvium and it seems to be a reaction in which iodide is converted to iodine, I-2, and that iodine seems to react with organic materials, even a small amount of organic material that somehow gets deposited with alluvial material.

So the possibility exists that as water comes out of the volcanic aquifers and goes down into the alluvium in the Amargosa Valley, that organic materials in that alluvium could retard the iodine. That's just a speculation on my part. But based on data that we have in the literature, in the peer review literature, and also an experiment done on alluvium from Yucca Flat by Kurt Wolfsberg in the Seventies, he saw retardation. So even iodine has got the potential for being retarded.

PARIZEK: Other Board questions? Staff questions? If not, we thank you for your presentation. And the next presenter before our break, and we still may make a break, has to do with hydraulic and tracer testing in the C-Well complex by M. J. Umari and Paul Reimus from the U. S. Geological Survey.

UMARI: I'm M. J. Umari, the PI for hydraulic and tracer testing, conservative tracer testing at the C-Holes, and my colleague, Paul Reimus, is the PI for reactive tracer testing at the C-Holes, and he will be basically--I'll be presenting you with summary of hydraulic and conservative tracer results, and then he will be talking to you about reactive tracer results.

I've placed the C-Holes complex here in a large aerial context basically to point out two things. First of all, that the hydraulic tests that have been conducted here at the C-Holes complex have affected are area much larger than the complex itself. They've affected WT-3 down here at about three and a half kilometers, WT-14, H-4 and C-1. So this is the first reason for placing it in the large aerial context like that.

Another one is to point out that all the hydraulic and tracer testing that has been done at the C-Holes is not intended to be the end product. The C-Holes were not constructed only to determine hydraulic and transport parameters at the C-Holes complex, but to determine methodology for characterizing fractured rock in the saturated zone, and that those procedures would then be carried out at other locations at the site. And that will be a context for thinking of the second tracer complex which Paul Reimus will tell you about. The idea of that would be essentially to carry on the methodology that has been developed at the C-Holes to locations other than the C-Holes.

I'd like to basically tell you about a few hydraulic and tracer tests that were done at the C-Holes by having these two overviews up, one that gives us the geohydrologic cross-section at the C-Holes, and this helps to illustrate or to point out that most of the tests that have been done at the complex have been in the combination of the lower Bullfrog and upper Gram interval, which is a highly conductive zone.

There is a plan, and we are in the process of starting to implement it, of doing hydraulic and tracer testing at the Prow Pass interval, which is a low flow zone higher up in the interval, and I'll talk about that a little bit later on.

Another thing this illustrates is that there are faults intersecting the bottom of the C-Holes complex, and that is the reason why there are also some proposed studies of the hydraulic and transport properties of the fault zone that intersects the bottom of the C-Holes.

You have in your handouts following this overhead, you have a list of bullets that I will not show here, but I'll talk about those. Essentially, I'll talk about them by looking at this triangle here and say that the test that you're looking at there, all the hydraulic and tracer tests listed were done with this well, C-3, being the pumping well in terms of hydraulic tests. In May of '95, this well was pumped and both C-2 and C-1 were in open condition. The packers were uninflated and we got hydraulic characteristics of the combined interval, the combined saturated interval at the C-Holes.

Then in June of '95, we inflated the packers in C-2 and C-1, still pumping C-3, and that enabled us to get hydraulic parameters of individual intervals tested. Even though we were pumping from this interval here, we got responses in the Calico Hills and Prow Pass and the Tram and all of them, so we were able to obtain hydraulic parameters of those intervals.

And then from May of '96, all the way until November of '97, we conducted a very long hydraulic test, again with pumping C-3, and that long-term hydraulic test is the one that affected the far away wells that I mentioned. And the hydraulic test was the basis over which were overlaying a series of tracer tests that we'll be telling you about.

The first of those tracer tests was one in which we injected iodide as a conservative tracer from C-2, again pumping C-3. That was in February of '96. And then in

January of '97, we conducted another conservative tracer test in which again C-3 was being pumped, while 2-6 difluobenzoic acid was injected in C-2, and pyridone was injected in C-1. These are tracer tests that I will be talking about later on in the talk.

So like I said, the next overview, I will not show. You have it in your handouts.

This handout that you have essentially has a summary of all the tests. This is the May 22 test, the June 12, the February, '96 test, which is a tracer test overlaid over a hydraulic test, and this is the long-term test. I want to point out here that although it says that the stop pumping is March 26th, it actually was pumped all the way to November of '97. This date indicates the end date for which the data were analyzed. That's what that means. But essentially here, this just gives you an overview of the tests.

And then in the overhead following that, I have some results of hydraulic properties at the C-Holes and at the distant wells. ONC-1 is one of the Nye County wells, and then there is H-4, WT-14, WT-3 and the combined rocks.

Maybe I can put this back up here while recapping these to point out that--no, that's not the one I intended to put up. It was the geohydrologic one that shows the aerial location of the wells that I thought could help us talk about them.

So basically, ONC-1 is very close to the C-Holes that are not shown here. H-4 is up Antler Wash here. WT-14 is at the top of Fran Ridge. WT-3 is down here at the bottom of Fran Ridge near Busted Butte. And the other numbers are for the combined interval at the C-Holes.

Now, the hydraulic results essentially indicate that we have a cone of depression that is elongated in a northwest, southeast direction. These are two snapshots, one at 30,000 minutes, which is 20 days. The other one is essentially at 321 days. And in both cases, you have information in terms of the well number, as identified here, a slash, with the dry-down indicated in centimeters.

Now, the cone of depression we feel is elongated in that northwest, southeast direction because of alignment with a series of fractures along and faults along Antler Wash.

All of the distant wells from the C-Holes were plotted on a drawdown versus t over r squared for all of those wells. And, you know, interestingly, they all kind of overlay each other and give credence to the fact that the whole area, the whole area that extends all the way to WT-3 can be analyzed as a combined single saturated zone aquifer.

Starting to talk about the tracer testing, these are three conservative tracer tests. The first one in black is the iodide tracer test from injection into C-2, and the right one is the PFBA injection into C-2 also. And these were both done by the USGS without recirculation of the tracer of the pumped water from the pumped well, and this was a tracer test that was done by Los Alamos Labs in preparation for their reactive tracer tests in which they had it in recirculation mode so it arrives earlier. But as you can see, apart from the earlier arrival, essentially all of them confirm the same shape of the tracer test, or the break-through curve of a conservative tracer going through the rocks at the C-Holes.

Here is an analysis of the February, '96 iodide tracer test in which we applied an analytic solution of advective dispersion equation by Alan Moench, and that particular analytic solution gives us a dispersivity value, a flow porosity and a storage porosity value.

In addition to that--so this is the line essentially that has, of course the jagged line is the data, and what we did was we did a hand fit using that analytic solution of Alan Moench's and obtained porosities and dispersivity. But in addition to that, we developed a parameter estimation routine to identify or to quantify the uncertainty in the data, and this parameter estimation routine was implemented in conjunction with this analytic solution of Alan Moench's of advection dispersion equation. And what we did was we took a hand solution and then perturbed it, and the parameter estimation routine converges to a solution, and the advantage of that is it gives us, in addition to the parameters, it gives us a 95 per cent confidence interval, and we did that because we were trying to get at the concept of quantifying the uncertainty in the parameters.

In this particular case, we have a longitudinal dispersivity value of two and a half meters, a flow porosity value of 8.6 per cent, and a porosity, a storage porosity of 16.3 per cent. Similarly, we did the same thing for the DFBA test. Again the same thing, applying that analytic solution and then using the parameter estimation routine for those two tests.

Now, I'd like to put back that triangle real quick here if I can find it. We're getting two overheads, and it's an advantage and a disadvantage at the same time. You have to be very coordinated.

After the tracer test that I just described, all of the ones that I just described, the tracers were injected in C-2, and the pumped well was C-3. This particular tracer test is a result of injecting Pyridone up there at C-1 and pumping at C-3, and this particular test took a long time, 300 days, and we did not reach a peak and we got poor recovery, but at the same time, we feel that it still has quite a bit of value in terms of the initial arrival time.

And if we make a guess as to whether, you know, we are at the peak or not, we can come up with some numbers for the dispersivity values. But these are very iffy as a result of the fact that we don't know where we are.

For example, if we were to take the data from only the first 200 days of the test, it would appear that we had a curvature, and we could match it like that. But then when we took the whole data set up to the point that we arrived at, then a different set of numbers were obtained. So at this point, we really--well, the judgment as to what it indicates in terms of dispersivity and matrix diffusion parameters is ambiguous, but I think it will give us a fairly defensible value for the porosities, because they are to a large extent influenced by the arrival time. And of course the shape of the rising curve is influenced a lot by the Peclet number, which the dispersivity is a part of.

And so at any rate, this test has not been analyzed formally in a milestone yet, but there will be a level four milestone probably at the end of the year that we're going to try to add to the system that really doesn't exist at this point.

This is a summary of results from the conservative tracer test. The three that I first showed on one plot produced these numbers here of dispersivity values, flow porosity values and storage porosity values. And this is the pyridone test that I showed at the end that is from a distant well, C-1, and this is the one that has not been analyzed formally yet. And also I'd like to point out that this number here can be ignored because this particular test so far has been analyzed assuming a single porosity medium, not a dual porosity medium. And so, therefore, values of matrix or storage porosity are--these could be ignored at the bottom right corner.

Essentially, we feel that the results of the C-Holes, tentative results from the C-1 well taken into account also, indicate that the relationship between dispersivity and scale as presented here, for example from Gelhar and others in 1992 with only a few points selected that indicate fractured rocks as opposed to other kinds of media, we feel that the C-wells at the distance of C-2 and at the distance of C-1 are consistent with these dispersivity versus scale relationships.

Future testing plan are to go up to the Prow Pass interval, the low flow zone, that will give a range of parameters not only for highly conductive zones, and it's also one of the first zones to be reached by radionuclides from a breached repository. We have to build special equipment for that, and we're working on that now.

The hydraulic testing in the fault zone has been proposed, and it's deferred for now for budget reasons, but it's planned in the future, and also conducting hydraulic and tracer testing at other locations will probably be through the second tracer complex which Paul Reimus will tell you about.

Very quickly here, I'd say that the equipment redesigned for the Prow Pass testing conceptually and mainly involves the fact that we're going to be pumping water out of the zone, out of injection zone, being able to sample it at the surface, cool it and inject it back in in this concentric fashion, the idea being that if you look at the interval being pumped here, you can take the water in the central portion here all the way to the surface, around that central part pipe, and then reinject it back in. That way, we will be mixing the tracer in the injection zone, so there wouldn't be any issues about the tracer being only in one portion of the injection zone. It will be mixed throughout the zone, and at the same time, it would enable us to sample at the surface.

That is it. And I was going to ask for the questions for both of us, for myself and Paul, to be combined together. So, Paul, if you'd come over, you can continue.

PARIZEK: While paul is coming over, for lunch, there will be beef tips over noodles with lasagna. Buffet will cost $5.00, and it will be served downstairs for lunch.

REIMUS: Thank you, M.J.

Well, let me begin by summarizing the results of the tracer test that Los Alamos conducted beginning in October of 1996, which involved the simultaneous injection of the four tracers whose curves you see on this plot here, and whose properties are summarized in the table at the bottom of this viewgraph over here.

I believe the Board was presented the preliminary results of this test last year, so I won't go into a lot of detail. And since then, we've added several thousand hours to the testoration, but that only amounts to about an inch here on the viewgraph, so it doesn't look terribly impressive on a log scale.

I want to point out a few key features of the test. First of all, you note the double peak behavior. We attribute that to multiple flow pathways in the roughly 300 foot long interval that was tested, and this is a combination of lower and central Bullfrog tuft from C-2 to C-3 with partial recirculation.

A few key points to mention. The difference in the peak concentrations of the pentafluobenzoic acid and bromide, which are two conservative tracers that have different diffusion coefficients, we attribute, and it's very consistent with matrix diffusion, it doesn't look like a significant difference here on the log scale, but that is about a 15 to 20 per cent difference in their peak concentration, and this is to be expected because pentafluobenzoic acid has roughly the effect of three lower diffusion coefficient than bromide and, therefore, does not diffuse as readily out of fractures and into the matrix as the bromide does.

Lithium, which is a weakly sorbing by ion exchange cation is attenuated relative to the conservative tracers due to sorption. The attenuation appears as a lower peak concentration in this first peak, and a lower and delayed peak in the second peak. This is consistent with sorption in the matrix and also sorption probably in fracture flow pathways in the second peak.

And, finally, the microspheres of 360 nanometer diameter polystyrene spheres that serve as surrogates for sub-micron size colloids moving through the saturated zone, and we see that their response is significantly attenuated relative to the solutes, but however, a significant fraction of them do arrive even earlier than the solutes, and they persist throughout the test.

We interpreted this test using a dual porosity conceptual model that's implemented very similar to the Al Moench model that M. J. mentioned, and I want to emphasize that these break-through curves were all simultaneously fitted, that is, we constrained the interpretation such that the flow parameters, in other words, residence time, dispersivities and mass transfer coefficients for matrix diffusion for all three tracers had to be the same in each flow pathway since they were all injected simultaneously and should have followed the same flow pathways. And by doing that, the model accounting for differences in the response of the tracers gives us parameter estimates for matrix diffusion and sorption of the lithium.

And I won't go into all the details in this table here, but this summarizes all the transport parameters, the flow and transport parameters that are deduced from the test. And basically the two halves of the table here are two different ways of treating the lithium behavior. Either it's equilibrium sorption or rate limited sorption.

The important point is we get estimates of dispersivity presented here as Peclet number in various pathways through the system. The two pathways here basically account for the two peaks that were observed in the tracer responses. This is a mass transfer coefficient for matrix diffusion. We get an effective flow porosity, which is derived from the mean residence times of the tracer through the system, and also estimates of the sorption parameters for lithium.

And I want to point out that the sorption parameters deduced for the lithium are quite consistent with parameters derived from laboratory scale testing, and in fact if anything, there was more sorption apparently occurring in the field experience than will be deduced from laboratory experiments.

And we feel this is important because it shows that the laboratory measurements are providing first a good indication of sorption at field scale, or at least a scaling that we can understand, and secondly, if anything, we're over estimating--or I should say under estimating and being conservative in applying laboratory scale sorption parameters to field scales. And this is important because we can't test radionuclide transport in the field and, therefore, we have to rely on laboratory derived sorption parameters for radionuclides to predict field scale transport.

Very quickly, I've got a quad very similar to what M.J. had showing ranges of dispersivities. These are longitudinal dispersivities from our test, superimposed on the plot taken from a paper by Shlomo Neuman in Water Resources and Research from 1990, plotting a variety of data from tracer tests at different length scales and also numerical modeling results. And you see that the dispersivities we get out of the tests more or less fall within the 95 per cent confidence intervals that Shlomo identified in his paper.

Okay, now I'll shift gears somewhat and talk about some laboratory experiments we've done to try to better constrain the interpretation of our tracer test and ultimately reduce uncertainties in the parameters obtained from the tracer test.

One of the things we're doing is attempting to measure diffusion coefficients of the tracers that we used in the field test in a laboratory experiment, and the experiment apparatus is shown here called a diffusion cell system. We basically separate a large reservoir containing high concentrations of the tracers of interest from a smaller reservoir that's initially tracer free with a block of intact matrix material of known dimensions.

This reservoir is continuously stirred and continuously flushed to a fraction collector, and we essential attain a break-through curve of tracer diffusing through the rock.

And example of some of the data is shown here for Central Bullfrog Tuff. You see resulting break-through curves for bromide and pentafluobenzoic acid, and also model fits to the data. I want to point out that this discontinuity in the fitting is due to changes in the flow rate through the outlet collection chamber, which caused these changes in slope and discontinuities.

The important point is we can back out from this simple one dimensional numerical modeling diffusion coefficients through the block of matrix material, and we've done this for both Central and Lower Bullfrog Tuff. And there's a couple important points here. First of all, we see there is about a factor of two to three difference between the Lower and Central Bullfrog Tuff diffusion coefficients for both conservative tracers, and the higher diffusion occurring in the Lower Bullfrog Tuff.

This is not difficult to understand. When you look at the porosities of the specimens, the Lower Bullfrog Tuff had a much higher porosity, so the diffusion coefficient seems to correlate with porosity.

We also know that there is roughly a factor of three difference in both tuffs between the bromide and pentafluobenzoic acid diffusion coefficients. This is important because when we initially did our interpretation of the field data, we assumed a factor of two difference based on literature values. It appears that based on these experiments, it's really more like a factor of three difference, which will have somewhat of an effect, relatively minor effect probably, but nevertheless, an effect on the interpretation of the test.

Okay, another thing that we've done recently that has not previously been reported is interpret the microsphere response from our combined tracer test. This is a rather involved exercise that I won't get into the details on, but essentially we have a number of--what I've done is apportion the mass of microspheres to different sets of pathways that have different filtration parameters. This is a simple model that's shown here in its mathematical form on this viewgraph that involves a linear forward filtration rate, and a linear resuspension rate.

What I found is that you couldn't explain the microsphere response in each of these peaks with a single filtration and resuspension rate so, therefore, the mass is split up into pathways that have different rates. And the results are summarized here, and this is certainly not a unique fitting exercise or a unique interpretation, but nevertheless, we are providing some parameters, or at least a range of parameters, that performance assessment can use for colloid transport through the saturated zone.

Future work, very briefly. M.J. mentioned the Prow Pass test. We want to and plan to conduct a test very similar to the test that was conducted in the Bullfrog Tuff, which would involve multiple tracers. This time, we would like to use three different sizes of microspheres to better constrain the microsphere interpretation and hopefully get some more meaningful colloid transport parameters for performance assessment.

We want to complete sorption and diffusion cell tests in Prow Pass Tuffs. These were initiated last fiscal year and are ongoing right now. I haven't talked about the sorption tests, but we are measuring lithium sorption to the Prow Pass tuffs that are going to be in the interval tested.

We want to go back and reinterpret the Bullfrog test results using some of the information derived from the diffusion cell test. And this should say Paintbrush Canyon, but M. J. mentioned the Paintbrush Canyon fault zone testing that's been deferred to FY-1999.

Okay, finally, I want to talk very briefly about an activity that was just started this fiscal year which involves Los Alamos, USGS and Lawrence Berkeley Laboratory staff to recommend a site for a second testing complex at which hydraulic and tracer tests could be conducted for the project. At this point, we're hoping to complete this activity actually within the next few weeks, and provide a recommendation to the project and then basically wait for comments and response from the project on that.

The group that's convened, or the group that's doing this activity has currently more or less converged on recommending a location right in the immediate vicinity of SD-6, which is up on Yucca Crest, and as you can see, within the repository block itself, or actually we'd be testing beneath the repository block. There are several reasons we've come up with this preliminary recommendation.

First of all, this would involve testing relatively unfaulted rock, which is in contrast to the setting, the geologic and hydrogeologic setting at the C-Wells and several other locations that have been proposed. Also, it's obviously relevant to the project, in that we'd be testing saturated zone directly beneath the repository block where radionuclides will first encounter the saturated zone.

Also, there's a number of existing wells, H-3 through 6 that I've shown here, and there's others that I haven't shown, that could be used as observation wells to get hydraulic responses, long-term hydraulic responses all across the block.

Another aspect of this proposal is to locate a couple of wells, which I haven't shown here, somewhere in Solitario Canyon, probably within a kilometer of the SD-6 cluster, and these wells would be intended to investigate the hydraulic and transport properties of the Solitario Canyon Fault, which is of interest since the Solitario Canyon Fault appears to be some sort of a pounding fault, in that there's a significant head gradient to the west of the fault, and basically a very flat gradient to the east of the fault, and responses over those kilometer distance can certainly be looked for when pumping one location or the other at the other location.

With that, I'll wrap up and take questions for both M.J. and I, I guess.

PARIZEK: Thank you. Questions from the Board?

Parizek, Board. There's a question about the next test that you run. Will you run that more than 200 days so you can actually get the break-over or the peak? It's unfortunate in a way that you really didn't get the peak on the past test. That's kind of vital data. So now the question is how many days would you have to go with a new test? Is it planned that you'll keep going until you finally get that peak?

REIMUS: Are you talking about the Prow Pass test?

PARIZEK: Right.

REIMUS: That certainly would be the hope. Maybe I should let M.J. address that since that was really a USGS test. But we do have a number of constraints that are placed on us that sometimes, you know, we can't test optimally, and there are schedules to meet and so forth. So I guess the answer is yes, we would definitely want to go well past any peaks in any subsequent tests. That's always subject to constraints that the project has as far as schedule and money and so forth.

PARIZEK: One other question about the spheres arriving ahead of the solute. It's like a fisherman falls in a creek and he gets downstream before the muddy water he stirs up.

Give us a little understanding of how that might be possible. I mean, obviously it suggests something about diffusion in the matrix or some other mechanism to slow down solutes.

REIMUS: Sure. Yeah, you've definitely touched on my response. Basically, the spheres would be expected to be confined to fractures. They have a three order of magnitude lower diffusion coefficient than the solute tracers. They're much larger in size, so they might actually be physically excluded from the matrix. But essentially, communication with the matrix should be minimized, if not completely eliminated, for the spheres and they would tend to be, at least the early response would tend to be more indicative of true fracture flow only pathways.

PARIZEK: Go ahead.

COHON: Cohon, Board. I'll go first since mine builds directly on that.

Your multiple pathway model that is a way to explain the spheres, is there any physical--can you explain those five pathways specifically, or are those simply mathematical constructs to make it fit?

REIMUS: For the spheres?

COHON: Yes.

REIMUS: Yeah, those are basically mathematical constructs, and what I've done is really split up--essentially what's probably really the case is that there's a non-discrete distribution of filtration and resuspension parameters that describe the colloid transport behavior and interactions with the fracture surfaces, and I've tried to capture that with discrete breakup basically of this continuous distribution in a way that at least matches the response of the microspheres. So I tried to do it as simply as possible with as few discrete packets of filtration and resuspension parameters.

PARIZEK: Alberto Sagηes?

SAGγES: Sagηes, Board.

I like the diffusion cell apparatus. What is the size of the rock specimen?

REIMUS: The dimensions at this point of course are variable, but the rock wafers that we've tested so far, we call them wafers, are about a centimeter thick and about ten centimeters diameter, basically the diameter of the core from the C-Holes. I should point out these were, you know, cores taken directly from the interval that was tested.

SAGγES: How do you select the specimen? Like what if it has a fracture running across it?

REIMUS: Yeah, we try to pick specimens that are representative of intact matrix, so we try to avoid any fractures or features that may affect the measurement.

SAGγES: And what level of duplication or multiple specimens do you use to account for what no doubt would be significant variations in the transport across the specimen?

REIMUS: Currently just one. We would like to and have certainly proposed to do a number of these tests on the same material, varying thickness, varying surface area. You know, budget concerns basically keep us from doing everything we'd like to do.

SAG$_{\gamma}$ES: Thank you.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

How long does one of these tests take? I mean, is this getting to a point where if you wanted to do a suite of these, it would be a fairly significant time investment right now?

REIMUS: You're talking about the diffusion cell test?

NELSON: Yes. It seemed like you had some with--

REIMUS: I can best answer that by looking at the response.

NELSON: --200 hours or 400 hours for the test, something like that?

REIMUS: I think that's right. I think it was on the order of 500 hours, if I can find it. You could probably look in your packet and find it faster than I can.

NELSON: I think it was--it seemed like it was on the order of like a month to do a test.

REIMUS: Right.

NELSON: And how many do you wish to do?

REIMUS: I'd like to do at least three to five on each matrix rock type of varying thickness and possibly surface area. I think thickness may be more important.

NELSON: So that sounds like you might have like two dozen tests, or something like that that you'd like to run?

REIMUS: Yes.

NELSON: And you have one cell? How many sells do you have?

REIMUS: Oh, no, we make as many cells as we need to do as many tests as we want.

PARIZEK: Other Board questions? Staff questions? Jeff Wong?

WONG: Jeff Wong, Board.

I have just a short question for M.J. On the pyridone tracer test, because your recovery is so low, the extrapolation as to what that curve is so you can then generate your parameters is really uncertain; that's what you're telling us?

UMARI: I think the recovery isn't as much of an issue in my mind, in that once you get some material between the two wells, then essentially you have a slug of material connecting the two wells, and the shape of the break-through curve is going to give you, if analyzed properly, the characteristics, transport characteristics of the medium, whether it's one gram that links the two wells or ten kilograms.

So I don't think that is the issue as much as the clear definition of the peak, because essentially for conservative tracer tests, you're trying to determine flow and storage porosities and dispersivity values, and the rising shape of the curve at the beginning is influenced a lot by the dispersivity. So a lot of that is there, but there's going to be uncertainty because we haven't gotten to the peak proper, so we don't really--you know, there's going to be ambiguity about coming up with that parameter.

And also the final number for the porosities is also dependent on the actual existence of the peak. So those will have uncertainties because of that. But it's not because of the amount or mass recovered.

WONG: Okay, thanks.

PARIZEK: Other Board questions? Staff?

(No response.)

PARIZEK: Okay, the good news is we have coffee break, but the bad news is we're only going to allow five minutes. We're running behind and we have a lot of important papers to still hear before lunch. So let's try to get back here in five minutes.

(Whereupon, a brief break was taken.)

PARIZEK: Let's get organized here. The time is short between now and the lunch period. If we could reconvene? So if you could bring your coffee on board?

The next talk will be given by John Czarnecki from the U. S. Geological Survey, who has been associated with the program for a number of years, and he'll talk on preliminary 3-D finite-element ground-water flow model of the saturated zone at Yucca Mountain. John?

Could we have everybody's attention? We're beginning our next presenter.

CZARNECKI: Thank you for that introduction. It's a pleasure to be back in the Amargosa Valley. And thinking back to the time I came here first in 1983, I think this is the very first time I've worn a tie in the Amargosa Valley.

I noticed also that there are a number of new residents in the valley, namely the camels. And they also join various other forms of wildlife, ostriches up in the north end of the valley, and I'm wondering where Dr. Doolittle is with respect to all this.

That aside, I'd like to share with you some results from a preliminary model of the saturated zone at Yucca Mountain. Before I get into the details, I'd like to acknowledge my co-collaborators on this effort. Claudia Faunt from the USGS has been involved with building the hydrogeologic framework model which was used in the regional model, and was sampled at a finer scale, or at a scale pertinent to the site. Carl Gable from Los Alamos has been involved in helping to take that stratigraphic hydrogeologic information and grid it into a form that we can use for numerical modeling, and George Zyvoloski, also from Los Alamos, has been involved with me in actually doing the numerical modeling.

I want to put up a slide that is not in your package just to show you the relation between the site model, which is this box, rectangular box, and the regional model that Frank D'Agnese described earlier. It's embedded just about in the middle, and Yucca Mountain is in the northern half of the site model.

Why was that rectangular domain selected? Well, it was selected first and foremost to be coincident with the grid cells in the regional ground-water flow model such that the base of the site model was equivalent to the base of the regional model.

The second reason for selecting this domain is that it be sufficiently large to minimize the effects of flow and pressure boundary conditions on estimating permeability values at Yucca Mountain.

An additional reason for the domain's shape and size is that it be sufficiently large to be able to assess ground-water flow at distances 30 kilometers down gradient from the design repository area. Also, we wanted to make the domain small enough to minimize the number of computational nodes used in the model.

Also, the domain was selected such that it be thick enough to include part of the regional Paleozoic carbonate aquifer, which is in the base of the model. And, finally, the domain is large enough to include well control in the Amargosa Desert at the southern end of the model.

This illustration shows the hydrogeologic units as would be met from the surface. The northern part of the area is dominated by volcanic units in and around Yucca Mountain. The yellow depicts basin fill alluvium and other basin sediments. These sediments are tertiary to quaternary in age. These units in brown and blue represent the Funeral Mountains which are in the northwest, quartzites, and then down in the south, carbonates. But the box shows that it's a fairly simple depiction at least from a map view.

Well control in the vicinity of the site model is shown on this slide. Again, the site model area depicted by this box. Notice, and we saw this in other talks, the number of wells in and around Yucca Mountain and in the Amargosa area. These form the basis for calibration. We try to use water level data from these wells in calibrating our flow model.

A little closer detail, this time the site model is the perimeter of this figure. Each of the points represent well control. The black lines represent the potentiometric surface one could construct using water level data from these wells. Yucca Mountain again is in the northern half and it's in an area where we see a bunching together of these potentiometric surface lines.

I kept the contour interval uniform to emphasize this particular feature. It's referred to the large hydraulic gradient, and we have several possible explanations for this feature. It's not something that we haven't considered over the years. Some explanations include that the large hydraulic gradient was caused by faults that contain nontransmissive fault gouge or that juxtapose transmissive tuff against nontransmissive tuff.

Another explanation may be that a large hydraulic gradient shows a different type of lithology that is less subject to fracturing.

Another possible cause may be that we have a change in the direction of the regional stress field and a resultant change in the intensity, interconnectedness and orientation of open fractures on either side of the area with respect to the large hydraulic gradient.

Another explanation may be that what we're seeing at the large hydraulic gradient is an apparent gradient resulting from a disconnected, perched or semi-perched water body.

And, finally, another explanation we could invoke is that a highly permeable buried fault drains water from the overlying tuff units into a deeper regional carbonate aquifer.

Now, what's attractive about each of these explanations is that they could be used to formulate various models that we can then test these hypotheses.

I've included this slide, which is a fence diagram of the various hydrogeologic units going from north to south and from west to east, and this results from a 1,500 meter sample spacing of the hydrogeologic framework model. It is perhaps less detailed than we would like to see, and we recognize that, and in our current revision of this model, we're going at a 250 to 500 meter sample spacing. But we took this particular sample distribution and used it to produce the finite-element grid, which I'm about to show you, which represents each of these three dimensional objects in tetrahedral finite elements.

The different colors represent the different material properties used in the model. We keep track of these with the numbers on the left side of the figure. The red represents alluvium. These yellow and green units represent volcanic units. And the darker blue represents I believe the lower carbonate aquifer. This dark blue represents a volcanic pluton, as I recall.

So in the model, we keep all of these units uniform and in their properties. We haven't broken them down into variations within a given unit from north to south or across the model.

This slide shows the distribution of hydrogeologic unit permeability values. It shows the different units used within this model, and on the right-hand side, the values that were used in the model to obtain the best fit to hydraulic head.

In the middle, we have high and low values that were available to us from the literature to help us constrain values used in the model. Not every one of these values fits within the values shown between high and low values.

A good example of that is the lower carbonate aquifer where the value used in he model is about 4 darcies, four times ten to the minus twelve meters squared. I compared that with the literature value that we had, and these values probably represent numbers derived from core for that unit.

When we take the values of permeability using the model and look at them in three dimensions, this is the picture that emerges. We're going from red units, which are about 10 darcies, to the blue units, which are ten to the minus 18 meters squared, a microdarcy. So we have quite a range in permeability.

One feature that was used in the model which is not a hydrogeologic unit is this zone shown as a blue plane. That plane was added to the model to represent the large hydraulic gradient. We could not represent it accurately without inclusion of this additional zone.

There's another zone that you can barely see. It's a green, slightly higher permeability. That's Solitario Canyon, and that was added to the model.

Now, if we look at these permeability units in fence diagram, we get this distribution. Now, the large permeabilities in the model show up in the bottom. We have the lower carbonate aquifer which is about 4 darcies, and the volcanic units here around Yucca Mountain are about a hundredth of a darcy to a tenth of a darcy. This blue line represents a barrier to north-south slow across the large hydraulic gradient.

These zones are shown again in map view. The black diamonds represent the Solitario Canyon barrier and the black crosses represent the east-west barrier across the large hydraulic gradient. These open circles represent nodes that were used to specify recharge at Forty Mile Wash.

What one can do with a model of this sort, and any model that one produces, is to take each of the potential parameters and vary them so that we see how the model responds to each of the individual parameters. And I've done that in this slide, and we have a ranking of the individual parameters, and I've got them named here. The top three, the first one is the Forty Mile Wash recharge, in part because where we're specifying that recharge is in a wash where we have an observation node, so naturally when you increase the recharge, you're going to see the impact at that observation node. But it's quite dramatic.

The next one is the Solitario Canyon Fault zone, and the third one is the middle volcanic aquifer, also known as the Crater Flat Tuff. So what one can do with a list of this sort is to prioritize where one should go to the field or emphasize resolution of the model.

I need to emphasize that this list is non-unique. It's highly non-linear. Any changes in the model likely will result in a different order of ranking for any one of these parameters.

The simulated hydraulic head and residuals are shown on this slide. The colored region shows the potentiometric surface going from highs of around 1,200 meters, to lows of about 600 meters down in the Amargosa Valley. Each of the symbols represents a range that the observed and calculated hydraulic head fall into. And by and large, around Yucca Mountain, we're going from about plus 5 to minus 15 in matching the hydraulic head.

We'll get a little better representation of that in this slide if we plot simulated hydraulic head against measured hydraulic head, we see a reasonably good fit for this particular model. There are a few busts at this point in particular where we have difficulty representing some of the points in the vicinity of the large hydraulic gradient.

The correlation coefficient is quite high for this particular model, perhaps in part because of the range that the data span. We have a .97 or .98 correlation coefficient. If we looked strictly at the area around Yucca Mountain, that number decreases to about .84, indicating that we can do a little better job there.

A histogram of these residuals shows that points are well distributed about zero. How did we come up with such a good fit? Well, we didn't do this strictly by hand. We used an automated parameter estimation scheme which can be used with any model, and it eliminates some of the bias that one might have, or might occur if one were to do this calibration by hand. Again, some of the outlyers are shown here, this roughly 100 meter discrepancy occurs in the area of a large hydraulic gradient.

The model is also useful in that it can tell you graphically what the direction of ground-water flow within the model is. This is a plane which slices horizontally through the model, and each of these vectors represents the direction of ground-water flow.

Let me stress that each of these vectors is uniform in length to its neighbor. They're normalized, and the colors depict the different magnitudes of the flow. If one were to non-normalize this, which I didn't bring, the vectors in this area are quite small.

I'm going to close with a final slide indicating the limitations of the model as it stands. We recognized that the model discretization is coarse, and as a result, causes incomplete definition of the hydrogeologic units.

Secondly, the permeability is known to vary spatially within individual hydrogeologic units.

Also, an average temperature for the entire saturated zone contained within the site model has not been calculated. One of the sensitivity analyses that I did was to look at the effect on flow as a result of changing temperature.

Also, the hydraulic head conditions that were used to specify boundaries for this particular model are based on a process of extrapolation and interpolation of the extant potentiometric data.

We make a steady-state assumption which may be invalid in areas in which ground-water withdrawals are occurring, particularly in the Amargosa Desert.

Also, the no flow specification along the base of the model is likely inappropriate, but because of the need to get this model out, we took an expedient route.

Finally, the representation of the large hydraulic gradient remains inconclusive. And I mentioned that we can test various models of the gradient. That was done, and I can elaborate during the question period if there's an interest there.

PARIZEK: Thank you, John. Board questions? Debra Knopman?

KNOPMAN: Knopman, Board.

Two questions. One, I apologize for coming in late on your presentation, but I'm wondering if you could go over a little bit more in a more--in some detail about how your model is consistent with the regional flow model in terms of its visualization of the flow regime? That's question number one.

And question number two has to do with the consistency factor between your model and your conceptualization of flow and M.J. Umari's conception of flow and the kinds of parameters they're backing out to describe that flow relative to your model.

CZARNECKI: Two excellent questions. The connection between the regional and site models is such that we've compared fluxes from the site model with those of the region. Not everywhere did the fluxes agree. The idea behind taking the domain that we selected was so that we could take fluxes from the regional model, and this is still the plan, to map those fluxes onto the boundaries as they occur and specify at least on one side, and maybe other sides, where those fluxes occur.

We recognize that there are disconnects because of the boundary conditions that we impose, and we're working to improve those.

Regarding the permeability distribution, we have in the model, particularly for the Crater Flat Tuff and the tests at the C-Holes, it has been suggested, and I agree with these suggestions, that we look at larger values of permeability in the Crater Flat Tuff and try to pin those values within the site model and calibrate the model without changing those parameters. That's something we can pursue. That was not done.

PARIZEK: Chairman Cohon?

COHON: Cohon, Board.

You offered several possible ways to explain the large hydraulic gradient, and then you tell us how you treated it in the model. Is there any correspondence at all between that model treatment and any of those possible causes?

CZARNECKI: Yes. The current model, the one that gave us the best fit where we impose a barrier to flow, might be considered as a fault zone which juxtaposes transmissive or nontransmissive units, or has fault gouge. It's a barrier to the flow.

One other model that we tested that did not hold up was to treat that zone as a drain in which water flows downward down the drain, and out through the carbonate units. But the heads on the upgradient side cannot be sustained.

COHON: Are we doing any field tests to substantiate the current treatment in the model?

CZARNECKI: Not the current treatment per se, but the work at WT-24 is encouraging because it's looking at the potential mechanism behind the large hydraulic gradient. As Zell Peterman pointed out, the initial indications show that the sampled water may be that of a perched zone. Now, that's a very preliminary analysis. Also, the configuration of the hole is such that we're only seeing the upper part of the saturated zone, and I am very eager to find out what happens as we continue drilling and test lower parts of that part of the mountain.

COHON: Thank you.

PARIZEK: Paul Craig?

CRAIG: Craig, Board.

This morning, we heard that the lifetime of the water might be 5,000 years, and if the retardation time is a factor of two, that brings us to 10,000. If the retardation is a factor of 20, that brings--that may make the saturated zone the most important hole up in the whole system. But there's the problem of cracks. So the question for you is what fraction of the water is going to go through the cracks and possibly be fast paths? And how do you build that issue into your model?

CZARNECKI: In this particular model, we assume that the permeabilities represent a bulk rock matrix combination, or an effective continuum. If one were to think of flow through fractures only, one has to think about much faster flow than a combination of matrix and fractures. The permeability is one story. Porosity is the other component. And then in terms of velocities, the porosities for the fracture system are much, much smaller, translating into much faster velocities.

I did a calculation years back using a much simpler two dimensional model, and at that time, I had to estimate what the potential range in porosity might be, and I think we've done a little better at Yucca Mountain in terms of coming to a bracketed value for porosity. I think now we're within, oh, I'd say at most, two orders of magnitude. Certainly for the C-Holes much less than that, probably within an order of magnitude.

But at the time I did the velocity calculation, I was out to about three orders of magnitude, which gives a very big spread in terms of the potential travel time from the repository out to the accessible environment.

I don't know if I answered your question.

CRAIG: I remain confused on how to think about this. What fraction, let me phrase it differently, what fraction of the water do you think might go through fast paths?

CZARNECKI: Let me add. Yucca Mountain, my conceptual model is that fractures dominate flow at Yucca Mountain. I don't think I'll get any argument there. In terms of ground-water flow, if you want to drill a well, you're going to produce water in the fractures. If you don't hit transmissive fractures, you won't nick water.

PARIZEK: Parizek, Board.

Do you plan any transient studies in this site scale model?

CZARNECKI: I didn't put up the slide that we have planned work. Transient analyses would be useful, as Lynn Gelhar has advocated, in using the fluxes from hydraulic tests, and using those to constrain values from hydraulic tests. Other than that, probably not.

What we would like to do is take a different data set, completely different. I failed to even give the preamble of what code we're using and what it feeds. Bruce Robinson is going to talk about transport simulations, which have to use transient analysis. And the code that I'm using is passed directly to Los Alamos so that they can do transport. So there's a transient component there. We want to take advantage of temperature data, which is an additional independent constraint which can be simulated using this current code and the grid.

PARIZEK: Parizek, Board.

Last night, you raised a question about the study plan and where these are up in the dusty attic of the program. What elements would you put in from the old study plan that you think ought to be put in in order to do additional work of the type you're doing here with the site scale model, or for that matter, how we link it in with the regional model?

CZARNECKI: We have very little data up gradient of Yucca Mountain. We have begged, borrowed and steal data from the other work at the test site. Any time there's a new hole at the test site, we try to use that data in any of our work outside of Yucca Mountain. It's very clear to me that we need additional control on the up gradient side. It has been suggested, perhaps rightly or wrongly, that the large hydraulic gradient has no impact on flow down gradient from it. I disagree. I think that because of the uncertainty in permeability associated with units at Yucca Mountain, depending on how you construct the large hydraulic gradient will result in a different order of magnitude, or different rate of flow on the down gradient side of Yucca Mountain.

PARIZEK: The report earlier on the C-Well complex high porosities that were mentioned, are they reasonable? And if you were to do a transient model, you've got to do something obviously with the storage properties. These are high numbers. Are they physically possible for the rocks that were being tested?

CZARNECKI: I think M.J. Umari would like to comment on that, and I will defer the question to him.

UMARI: Can I, please? Because that was something I should have made clear in my presentation. The numbers that we have for flow porosity are not fracture porosities. We are conceptualizing a medium in which the primary flow path is in fractures and segments of matrix connecting discontinuous fractures. And then there is a storage portion of the matrix that just functions as storage. But the dual porosity conceptualization that we have is a medium in which there's a primary flow advection, you know, transport by advection in fractures, plus segments of matrix connecting discontinuous fractures. Okay? And then there's a storage portion which is in some dead spaces and portions of the matrix.

So when you see a 30 per cent, I'm not saying that the fracture porosity is 30 per cent. Fracture porosity cannot go more than probably 3 per cent. So those numbers are for a combination of fractures and portions of the matrix that contribute to the main flow in order to do a porosity conceptualization.

PARIZEK: Paul Craig?

CRAIG: Sorry to come in with a second one, but I think something important is going on here.

You just told us, John, that you think that fractures may be most of the flow. If in fact that's true, the fast path is the slow path, is the dominant path. If that's the case, so we've got big cracks through fast paths, plus hold-up someplace in whatever reservoirs are there, that's the model that Umari is talking about, if that's the case, then a conservative viewpoint, which is the fast path dominates viewpoint, gives you long hold-up times. That's really good news for the repository, and that's not a picture I've heard before. The picture we've heard before is the fast paths are a big problem. With your information, the fast paths make it a small problem, and that's really good news. Now, what's wrong with that picture? Am I missing something?

CZARNECKI: I'll have to think about that.

CRAIG: It seems to me that everything we've heard today supports that view.

CZARNECKI: I'm not sure I can provide a--

COHON: Paul, you have me confused now. This is Cohon, Board. Are you saying that the fast paths tend to be slower than you might have thought given their name fast path?

CRAIG: Well, fast path is a name which is used to distinguish matrix flow from flow through the cracks.

COHON: Right.

CRAIG: If it turns out that the matrix flow is of relatively small importance, for simplicity let's forget about it completely, everything is going through the big cracks, that's what we just heard from John, if everything is flowing through the big cracks, that's the dominant mechanism.

COHON: And if it's slow, then that's good news.

CRAIG: And if that dominant mechanism is slow, that's really good news, particularly if there are big hold-up times.

CZARNECKI: Can I make a comment? This comes back to a fundamental question that we've had, and that's regarding the gradient and the associated permeability at Yucca Mountain. There's two ways you can interpret that gradient. One could be that we have big flows going through large permeability zones, or we have very little flow coming through, and that supports this very small gradient. So it's open ended, and we still I think need to resolve that.

PARIZEK: Other Board questions? Staff? Leon Reiter?

REITER: Leon Reiter, Staff.

John, Frank in talking about the regional model, emphasized a critical role of the carbonate aquifer. How important--I was looking at your chart--how important is the carbonate aquifer in your model? Particularly I was thinking that we think the water coming through to Amargosa Valley goes through the tuff into the alluvium.

And just one other question. Yesterday, Russ Dyer when he was presenting work on TSPA, talked about using multiple lines of evidence to do reality checks on models. Are you doing any of these?

CZARNECKI: I'm going to use a viewgraph to help with the comment on the carbonate.

I think it's useful to review the distribution of the permeability within the carbonate. Since there's four darcies and it's at the base of the model, we get large flow occurring in this unit. It's also backed up in the vector diagram which just cuts through the middle of the model horizontally. But because this is such a high permeability within the model, it does have an impact.

The question related to reality checks, I have co-workers that are constantly reminding me of the geology in the area, and that vertical barriers that represent the large hydraulic gradient have not been found. And that kind of a reality check I think is useful because it forces one to look at other models. Another model that I'm looking at for that is to consider changes in permeability as a result of thermal alteration to the north, and instead of assuming a uniform permeability within a given unit, we'll break it up into sub-parts. And that will allow us to do other types of models that the current assumptions with the individual units will not permit.

Another reality check is to compare what's coming out from Zell Peterman's work to look at other reinforcement from completely independent data sets. And this is one that we have begun to study. The hydrochemistry is supporting the types of results that we're seeing with respect to flow in and around Yucca Mountain, very valuable tool.

PARIZEK: Dan Bullen, Board?

BULLEN: Bullen, Board.

Just to maybe reiterate what Paul is saying and ask the question in another way, if water usage in the Amargosa Valley increases and the gradient increases, then the fast flow pathways become fast in Paul's definition?

CZARNECKI: That would be a consistent interpretation. There was a study done at our request to compile hydraulic head data for the Amargosa Desert. The report is one that was written by Kati Kilroy, and it compares conditions in 1950 with those in 1987. And when you look at the potentiometric surface, you see cones of depression near the pumping centers, but they tend not to spread out too far, which is I guess encouraging. There's considerably more pumping in '87 than there was in '50, I believe, and a lot more wells. So perhaps this phenomenon will be more localized than you or Paul may be concerned with.

BULLEN: Thank you.

PARIZEK: I think we ought to go on with our next presenter and thank John. This will be Bruce Robinson with current status of the saturated zone flow and transport model.

ROBINSON: There's been a lot of questions, comments and discussion on what some of these data collection and modeling efforts have to do with transport of radionuclides, and that's what I'd like to focus on in my presentation in describing the current status of the saturated zone flow and transport model. This is a collaborative effort in which John Czarnecki and colleagues at the USGS have been working with us at Los Alamos. And in addition, some of the modeling that I'll present today was the result of numerical model runs that were developed at Sandia National Laboratories.

This is an outline for what I'm going to be talking about today. I'd like to briefly describe the flow and transport models that are being used. It's fortunate in that I won't have to spend too much time on that given that the modeling that we're doing is very consistent between the USGS and Los Alamos, using the same code, the same model geologic framework and numerical grids to do the calculations.

And I'd like to present some radionuclide transport studies of two radionuclides of interest, technetium and neptunium. I'd like to show how the saturated zone system behaves under one set of parameters for transport, and then focus the majority of my talk on some sensitivity analyses that look at some of the uncertainties associated with making a prediction of transport in the saturated zone. They're listed here. I'll get to them during the talk, so I won't discuss them any further right now, and then I'll summarize with some future work that we intend to do with this modeling.

This is a figure in your packet. I put it up only to keep you in place if you're following along in the papers, because you've seen this layout before, in fact in John's previous talk. This is the domain for the site scale model, the box that's shown here. There are a variety of models that have been used to predict flow and transport in the saturated zone. The largest scale model that's been used to predict radionuclide transport on the project is the site scale model.

It's a good model for determining general flow directions and transport directions. It has some limitations in its current setup that I'll discuss and talk about in the future work, but basically the resolution of the model is insufficient at the present time to capture the transport processes near Yucca Mountain, and that will be corrected in an updated version of the model that goes to a much finer grid resolution. But in the meantime, what we've done is to try to develop transport calculations based on two models, and it's kind of summarized in this schematic.

The box is the site scale model domain and schematic over here. Embedded within that model for the purposes of this calculation is a sub-site scale model domain in which much greater resolution in the model was possible because of its smaller model extent, both aerially and planned view and also in depth. So I'll present transport calculations and results at both scales, but many of the simulations that I'll show are actually on the sub-site scale model domain in which we really have a finer discretized model that allows us to study things near Yucca Mountain in greater detail.

So the process here is to take results from a sub-site scale model, feed it into a site-scale model and come up with transport predictions, which I don't expect you to be able to see, but I'm going to discuss this slide in a future slide, discuss those results.

This is a permeability distribution in the sub-site scale model that I just described. It shows permeability values that coincide with the major hydrogeologic units that are present in the saturated zone. This is a model that was developed by Bill Arnold and collaborators at Sandia National

Laboratory. They developed the flow part of the model. I'll be running transport calculations and presenting the results of those in this talk.

You can also see that as John Czarnecki showed you in the site scale model, several features typically if one models the large hydraulic gradient as low permeability zones, then those need to be placed in the model, and this figure just shows where those are present in this model domain.

I just want to point out one other thing. This line that you see here is not an outline of the repository. I just wanted to point that out. It actually rests somewhat in here. It's an unfortunate plotting glitch and I don't want you to get the impression that that is the outline of the repository in this picture.

In order to make predictions of transport through the saturated zone, you have to have a source term, and the source term that I'm using for these are calculations of the mass flow rate of radionuclides through the unsaturated zone, which combines into the results of unsaturated zone flow and transport modeling, which is not the topic of this talk or this meeting. But in bringing you through these calculations and describing them, I'll focus on them for just a moment.

What you see on the left are the mass flux, the moles per year of radionuclide predicted from the unsaturated zone models for technetium and neptunium, the two radionuclides that I'm focusing on today.

So this is what is reaching the water table according to the present models of flow and transport in the unsaturated zone. What's built into these calculations are a 1,000 year canister lifetime, so that I'm trying to outline for you the assumptions that go into these curves, a 1,000 year minimum lifetime for canisters, followed by releases from the near-field over in the case of technetium, 3,000 years, and 30,000 years for neptunium, and these are the break-through curves at the water table that serve as the source for the saturated zone calculations. So we're going from unsaturated zone, we're hitting the water table, fed into saturated zone transport models, and the results are shown on the right-hand side.

The different curves are for different infiltration rates. They kind of give you a feel for the amount of uncertainty based on one of the key parameters in the unsaturated zone where infiltrations and how those play out in terms of mass flow of radionuclides hitting the water table.

Now, on the right-hand side are the calculations of the concentration out at five kilometers using the sub-site scale model that I described. And this is for technetium and neptunium. Now, there are several cases involved here.

Basically, the three curves here are put into the saturated zone model and the break-through in terms of concentrations at the five kilometer point that was studied here are shown in these curves.

One of the key parameters at least for radionuclides such as neptunium is the sorption coefficient. Even values of sorption coefficient that chemists would describe as small, two cc's per gram, result in significant delay in radionuclide travel time. So that's one of the sensitivity analyses that I'll show in a minute.

Now, the result from the sub-site scale model are in a sense digitized and placed into the saturated zone site scale model to perform calculations out to 20 kilometers, more in line with the current thinking on the compliance point that you would be computing and predicting.

This red curve was a five kilometer compliance point, similar to one of the curves that I showed in the previous slide. When you go to the longer travel times, you get additional dispersion and mixing according to these models that result in essentially lower concentrations as a function of distance.

Now, this is one of the key uncertainties that I think we need to look at in greater detail, and that is the nature of dispersion in this system, because models will typically always predict mixing and dilution caused by the dispersion of a contaminant as it travels along the flow pathway. We need to look at that model in detail and make sure that we think it's a realistic assessment.

Now, there are different curves on this plot. Basically, there is a sensitivity also to what you assume for the porosity of the medium. This is getting to some of the questions that were discussed earlier. I'm going to talk about the porosity in some of the sensitivity analyses.

Let me give you a flavor for all of the sensitivity analyses that are described here. I'm not going to have time to talk about them very much, except to touch on them.

One can either do a very detailed job in placing radionuclides at the water table as a function of position and try to make those predictions based on an unsaturated zone model, or you can just lump it all together and put it into a model uniformly over the entire domain.

What I'm showing here is a comparison of radionuclide that hits the water table at different locations in the sub-site scale model. Depending on what unit is encountered by percolating radionuclide, Prow Pass, Bullfrog and Tram, one gets somewhat different plume migration directions. And so the conclusion here is that the plume trajectory and the concentration hitting the water table needs to be considered at least in terms of which rock unit is encountered when those percolating radionuclides hit the water table.

Probably this effect is most important if one is focusing on something five kilometers downstream, but as we get to greater distances away from the repository, the whole thing looks a lot more like a point source at the repository and at Yucca Mountain.

Next is the effective porosity. What I show on the left here are sort of generic transport calculations, the response of the saturated zone system to a constant input of radionuclide. That's on the left. And what I'm showing is the break-through time basically is controlled by what one assumes for the porosity of the medium.

If you assume that radionuclides travel only within fractures, one would select a porosity something like .0001 in the extreme, or perhaps .01. What I'd like to point out is that the travel times that are backed out from that sort of calculation are on the order of one to ten years, out to five kilometers. Is this consistent with the data that we have, the hydrochemical data that we have for the site? I believe no. I believe that porosities more like matrix porosities are more appropriate based on the hydrochemical evidence that we have so far. However, I think we need to do more in the area of study and actually getting into our numerical models the influence of or the constraint, the additional constraint of the hydrochemical data. What that will allow us to do is to constrain and narrow the bounds of possible porosity values that we put in these models.

Now, the porosity, the effective porosity that one assumes has the greatest influence on the arrival times. It perhaps doesn't have as great an effect on the amount of dilution that one would predict from a model, because the porosity will affect the travel time more so than the dilution of radionuclides entering the saturated zone and becoming diluted with water that's travelling past it in the saturated zone.

COHON: What is the base case value? Is it .3?

ROBINSON: In this calculation, it was on the order of .3.

COHON: Was it .3?

ROBINSON: I believe it was .3. I'd have to look it up. But it's very close to that. Clearly, greater than .1. I think it's .3 is about right.

SAGγES: I don't understand the colors in the technetium predictions curve on the right.

ROBINSON: Okay.

SAGγES: Which one is the base case in that curve there?

ROBINSON: Red.

SAGγES: And then as you go to lower porosities, it moves faster?

ROBINSON: Absolutely. Think of it as a displacement process in which the travel time is governed by, in essence, the flow rate--or excuse me--the volume of fluid that needs to be displaced, divided by the flow rate of that fluid. That will give you travel time. So the lower the volume of fluid, i.e. the lower the porosity, the more rapid the travel time.

SAG$_{\gamma}$ES: Right. Because you don't have to fill the pores and the cracks.

ROBINSON: That's right.

We saw that there was uncertainty in the perceived or computed fluid flow rate through the saturated zone. This would be under base case or present day conditions, but also factoring in the effect of a changing climate and the uncertainty that that gives us in terms of the flow rate.

This is just an example of what happens in the model calculations if one assumes a fives times greater flow rate. Greater flow rates in the saturated zone result in lower concentrations on this sort of a calculation, which again is a constant source at time zero. And that gives rise in terms of a prediction to a lower concentration.

So there's a trade-off, if you will, in terms of how flow rate or flux through the saturated zone affects these model results. If everything is going to get to an accessible environment point within a compliance period, then the higher the flow rate, the greater the dilution, and that's a good thing.

If, however, one's predictions say that the travel times are greater than the compliance period, then anything that speeds that up and brings it to the accessible environment sooner would be, in essence, bad from the standpoint of performance. So the flow velocities impact both the travel time and the amount of dilution that we predict in these models.

Next topic is the influence of repository heat. We ran calculations in which we imparted, gave a temperature wave that hits the saturated zone due to the repository heat effects, and this is just an example of temperature distribution within the saturated zone, both in plan view and side view. The types of temperature rises at the water table that have been discussed are on the order of 50 to 60 degrees C. These, according to this model, are transported somewhat through the saturated zone. So you do get, in essence, a plume of fluid that's somewhat higher temperature than the ambient for a period of time, and it abates as the heat from the repository abates.

The question here is what influence might that have on saturated zone flow and transport? The predictions on the right-hand side, if you focus on just the two colored curves, those are concentrations of technetium under similar assumptions for all the transport properties, but one in the influence of the heat and one without. And it turns out it has a very minor effect on the predicted transport through the saturated zone.

Now, the assumption, the key assumption made here is that there would be no durable changes in the hydrologic properties due to this heat. We know that there will be some mineral redistribution associated with the flow of water and the migration of a thermal plume, and this might influence in fact the calculation of transport properties, because the hydrologic properties may change. Those are not included in these simulations. Our initial looks at that suggest that that would also probably be a minor effect, but I think I'd like to make that a soft conclusion at this point, too, because we haven't looked at it as much as we perhaps should have.

Sorption in the saturated zone is another aspect of transport of radionuclides that is clearly important. What we do to make these sorts of predictions is to identify in the case of the important radionuclides the minerals that are implicated in the sorption. And in the case of neptunium, sorption to zeolites appears to be the primary mineral, clinoptilolites for sorbing and retarding neptunium.

We take the mineral data bases that have been put together, embed them or superimpose them on top of our numerical grids, and then perform the calculations of transport, assuming a retardation factor that is, for this simulation, 2 cc's per gram in the zeolitic zones, and zero elsewhere.

That sort of transport Kd parameter has a significant retarding factor for the case of neptunium, in this simulation, on the order of 10 to 20 delay in travel times of neptunium relative to a conservative radionuclide.

Colloids is the next topic. This slide is a sensitivity parameter, or a sensitivity analysis on what we think is probably the key parameter that will decide for us the importance of colloid facilitated transport of radionuclides such a plutonium.

This is an example of a simulation of plutonium concentration at a compliance point versus time for different values of a partition coefficient, Kc, which I'll describe now. Kc basically is the relative partitioning of plutonium onto colloids versus that in the aqueous phase. So if there is no partitioning onto colloids and/or those colloids are not mobile, one predicts very long travel times for plutonium through the saturated zone, in the saturated zone alone, would delay plutonium, because of the high Kd that's assumed.

However, what happens if you assume that most of the plutonium resides on colloids, and furthermore assume that those colloids are mobile, transport times to the accessible environment come within 10 to 100,000 years out to the 20 kilometer compliance point. So the work that's going on now is attempting to come up with parameter values for this parameter.

I think that currently, the numbers that are being estimated for this partition coefficient are in fact closer to zero than certainly they are to 99. They tend to range from about zero to one in most estimates of this partition coefficient. That would place plutonium into the longer travel time range that we assume in the absence of colloid transport.

Final sensitivity analysis is the effect of dispersivity. We saw some estimates from the C-Wells experiments. those estimates provide kind of a starting point for estimating the dispersion coefficient or the dispersivity in the field scale models. One has to scale those for the distance of travel. You're talking about 30 meters or so of travel in the C-Wells experiment versus a 5 or 20 kilometer travel distance in these simulations.

So these are predictions of break-through and concentration at a compliance point for different values of the dispersion coefficient. Now, these are dispersivities longitudinal and transverse. And the point here that I'm wanting to make is that it's actually the transverse dispersion, something that we don't know a whole lot about, that really is going to be a key consideration in computing what the--how much dilution can be attributed to dispersion effects within the saturated zone.

This is just a rundown of some of the things that we're going to do in the future. Combining the sub-site and site scale models I think is an important thing that we're working on at the moment to try to eliminate the step of having to piece different models together. It's a computationally intensive problem that we're working on at the present time.

I think constraining a model with hydrochemical data of the sort that was discussed this morning is an important next step. One can run chemical transport calculations to provide further constraints on flow models, and I think that's one of the things that we're focusing on at the moment.

Dispersion is extremely important. We need to start incorporating into our models more heterogeneous systems to look at dispersion in more detail. Dispersion in the saturated zone is shown to be a key element in the predicted dilution of radionuclides, and so it needs to be looked at in greater detail.

Some of the alternate conceptual models for flow related to the large hydraulic gradient and other things; I believe that those need to be looked at in terms of implications for transport. Even if you argue that they're up stream from the repository, I think that we could surprise ourselves if we assume that just because it's up stream of the repository that it's really not going to have any influence on migration away from the repository.

And, finally, looking at elements of colloid-facilitated transport is a key component in the transport studies that we're working on right now.

That's my conclusions. I'd like to open it up for questions and just provide the conclusions as part of your packets, and address any questions you might have.

PARIZEK: Debra Knopman, Board?

KNOPMAN: Can I just ask a question, Dick? What's our schedule going to be with regard to lunch?

PARIZEK: Well, we're set up for more or less a quarter past 12:00 is what's on the schedule. But let's take questions while we have a speaker available.

KNOPMAN: Bruce, I found the sensitivity studies very interesting. However, they're of course contingent on whether you parameterize the model correctly and whether the conceptual model has any validity in and of itself.

In terms of these transport matters, what can you tell us about the kind of testing you've done of alternative conceptual models, and what effect that would have on your particular sensitivities and some of these maybe key results?

ROBINSON: I think one of the key aspects that's been brought out in the discussions today is this idea of the effective porosity that's available for transport of a contaminant, a radionuclide, as opposed to flow of water. Now, underlying the parameter sensitivity studies that we've done here, we've looked at conceptual models that are more idealized, but allow us to get at the kind of time scales that are required for, say, a radionuclide travelling through a fracture to diffuse into the rock matrix and, hence, be delayed relative to one that just squirted through the fractures, if you will.

That sort of analysis implies that the fracture porosity is probably quite a bit lower than the effective porosity that one would have in the presence of, say, matrix diffusion. That's why the results of the C-Wells experiments I think where matrix diffusion has been identified as a process that is occurring in those tests is very important, because we can do all the calculations we want and say that, well, it looks to me like it should have enough time to diffuse into the matrix and, hence, we should have longer travel times. Credible field evidence of that has been hard to come by, and that's a way in which that sort of data is being used in our thinking and in our development of these models.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board.

Thinking about the importance of dispersivity, I could see how it's important that, for example, a five mile compliance point, particularly when you look at a point source effectively. But if you look at a distribution of point sources where the radionuclides might be delivered according to some distribution of fast paths or whatever from the unsaturated zone to the saturated zone, how important is it that efforts be taken to evaluate the dispersivity?

ROBINSON: I think the dispersivity is an extremely important component to a first approximation--let me back up before I say to a first approximation. The unsaturated zone transport models when combined with models for how the canisters fail and that sort of thing, usually give you a very long time period over which radionuclides are going to be hitting the saturated zone. Okay?

Now, the only thing that will dilute them beyond that in the saturated zone is the rate of flow of fluid. If you conceptualize the saturated zone as just a pipe that's transmitting water and radionuclides, it's the relative flow rates of the downward percolating radionuclides with the flow rate in the saturated zone. There's that factor.

There's also the spreading of that plume as it travels down gradient. The conceptual models range from, well, I think that a radionuclide plume really stays pretty compact and it just kind of bends with the flow, it doesn't really spread out that much, versus one in which dispersion or dispersivity really does spread it out and results in lower concentrations. I think pinning that down on a scientific basis will really constrain our calculations of the performance in the saturated zone. It's very important.

NELSON: But if there's many, many point sources--

ROBINSON: You mean at the repository?

NELSON: Just all over the projection of the footprint onto the UZ/SZ interface. And you don't know where these are, but there may be many, you know, over time arriving and entering the saturated zone, so that I guess I'm wondering, it seems like if you're talking about many point sources all starting at different times perhaps somewhat, that the importance of dispersivity as a parameter to try to evaluate might be somewhat reduced.

ROBINSON: I don't think so, because the way I think about it is you have a repository, and if you assumed a first approximation that the footprint of the repository that you project down onto the water table, maybe that's where the radionuclides are going to be basically travelling, downward, vertical flow in the unsaturated zone. That's about three kilometers or four kilometers by one. You're talking about travel distances out to 20 kilometers. That repository footprint begins to look a lot like a single point source when you go way out to 20 kilometers.

COHON: And that's my question. Cohon, Board.

You're going from a sub-site model to a site model to a regional model. And this comment about the repository appearing to be just a single point source from 20 kilometers away, how much of that is model, the fact that you're going from one model to another, and the last model has very gross spatial scale, relatively speaking? And what effects does that introduce into the conclusions?

ROBINSON: I don't think it's model dependent. I'm making sort of an argument based on, you know, just the geometry of the repository layout and how that might be imprinted on the water table as a source term for radionuclides versus the travel distance. I don't think that's dependent on the models. To be sure, we want to have ideally a single model that would capture the resolution needed to really get the detail near Yucca Mountain embedded within a larger scale model that maybe doesn't have as much detail out at the peripheries of that model, right out to the site scale model boundary. That's what we're working on now. There aren't any plans to go and actually embed that within the regional scale model. Those two modeling activities right now are distinct and we go back and forth from one to the other for consistency. Basically, the regional scale studies in my mind tell us, in the simplest sense, tell us what we think the flux might be through the site scale model domain.

COHON: Okay.

ROBINSON: And those are in fact pretty consistent.

COHON: Just one last very quick question. You said that the site scale model will be converted to a spatial scale that you're working at now at the sub-site model. When will that be done? When will that be accomplished?

ROBINSON: I'm going to tell you the current progress, and then when it will be accomplished will have to be a little bit of a prediction.

COHON: That's fine. Just what's your prediction? You don't have to give me the details of what you're doing; just what's your prediction? A month?

ROBINSON: Okay, probably--no, no, it will be more like four to six months.

COHON: Thank you.

PARIZEK: Parizek, Board.

The importance of the colloidal transport mechanisms, I know in the C-Well tests or tests like that where you're injecting and pumping, youi really induce artificial gradients in the system. The natural gradients are very gentle in this sort of area. And so the question is how do you get at the colloid migration that really occurs under natural conditions, natural gradients? It may be a question that's hard to answer, but I can leave it hang for later discussion this afternoon.

ROBINSON: I think there's tremendous uncertainty, and if I would say so, confusion out there right now about how important colloid facilitated transport is. I'm not saying everybody else but me is confused. I'm trying to put it together myself as well. I think experiments like the C-Wells kind of give you a ground truth, but they need to be augmented by much more detailed laboratory studies that look at the properties of real colloids as opposed to--let me say natural colloids as opposed to microspheres, to look at the transportability, if you will, of the colloids.

And there's another factor to consider when considering migration of radionuclide on a colloid, and that is how strongly attached is a radionuclide to the colloids. So it's this relative partitioning between the aqueous phase and the colloids that's important as well. So it's the transportability of the colloids, and also the chemical interactions of the radionuclides with the colloids that's going to be the final arbiter in how important colloids are.

PARIZEK: Alberto Sagηes?

SAGγES: Yes, Sagηes, Board.

When you talk about the heterogeneous model formulation, do I understand correctly you're talking about models in which you have like fractures, and then porous materials on the side of the fractures and, therefore, you're considering transport through the fractures and then the demands needed to fill up the surrounding rock? Is that what you mean by the heterogeneous models?

ROBINSON: Not exactly. Basically, the heterogeneous simulations that are being performed are attempting to take into account fracture densities and that kind of thing, but to do calculations using a continuum representation. In other words, a block of rock still has a given permeability; it's just that what we want to model is variations in permeability that you might see due to these fractures, but on a smaller scale than has been done so far in these models.

SAGγES: But the presence of like the cracks surrounded by a sponge type of model which you see in some other disciplines, that's not in the plans right now?

ROBINSON: Well, that can be an element of the transport simulations as opposed to the flow simulations, and in fact we tend to use continuum models more for the flow calculations, and when we really go to transport, we need to include some of these concepts like the matrix diffusion concepts.

SAGγES: Okay, thank you.

PARIZEK: Questions from Staff? If not, we thank you very much. Our next speaker will be Linda Lehman, State of Nevada studies of the saturated zone, and she's president of Linda Lehman and Associates and comes to us from Minnesota.

LEHMAN: Good afternoon, everyone. Thank you for the opportunity to present the State of Nevada's research in the saturated zone.

Most of this work has been performed over the past eight to ten years of working for the state. What I'd like to cover today is some of the work that we did on the water table frequency analysis, the response of the water table to rainfall or whatever natural phenomenon occur that change the water label.

We looked at the water table wells, and we also looked at the response of Devil's Hole. We also did an analysis of the response of the water table to the earthquakes which occurred, the Landers Earthquake and the Little Skull Mountain Earthquake in 1992, and the results of these studies helped shape our conceptual models of the saturated zone.

And with respect to the conceptual models, the flow models, I'd like to present our conceptual flow model and the numerical model that we've done, and then also what we feel is needed in addition to what we've done in the past.

The water table analysis was done in the late 1980s, about seven or eight water table wholes, which at that point in time had data. Our analysis basically was to first remove any linear trends that we saw in the data, for example, this declining trend. After that trend was removed, then we wanted to do a spectral analysis, essentially, and we looked into Forier transform analysis and found that we would have a problem applying any of those because the data were not evenly spaced in time.

So we simulated the Forier analysis by fitting a cosigned function, and we used a code which we developed called FIT.M. So basically, after removing the linear trends, then we fit a cosigned function to the data to see if we could get any cyclic movement.

This is WT-1 analysis, and as you can see, we did come up with some cycles. These cycles are very small. However, they do give you some information.

On most holes, we basically could group two consistent groups of responses. the first was WT-7 and WT-10, and I'm going to put these up over here while I'm talking about them so you can see it.

This has about an average of about close to a thousand day period, maybe 970 days, something like this, and the phase shifts were quite similar.

The second group that caught our eye was WT-1, 11 and 16, and they all sort of hovered around an 870 day period, with a phase shift of about 250 days.

When you look at these on this viewgraph, what you'll find is that they are spatially distributed as well. For example, the longer period of response fell to the west side of Yucca Mountain along the Solitario Canyon. The shorter period, the 870 day period, was on the right-hand side, or the east side of Yucca Mountain, and also was in a linear trend, which is parallel to the extensional faults zones that we see in Yucca Mountain. The very long periods, as you will see, coincide with like the crest of Yucca Mountain and other elevations.

Seen another way, looking across the mountain from west to east, we have the longer period response on this side, shorter period here, and then a very long response under Yucca Crest. And this was our first indication that the flow field on the west side of the mountain was different than the flow field on the east, and as Zell Peterman has told you, there are some major ion chemistry differences as well, and Nancy Matuska at the Desert Research Institute at that time had done some major ion chemistry and supported this idea of separate flow fields.

I think today we're still doing most of our modeling as if it were one flow field, but I think we now have evidence that it is probably separate systems, at least loosely connected.

We looked to see if there was something that would be causing these roughly two and a half to three year cycles, and John Fordham at DRI in Reno provided us some information about the annual precipitation, and what you'll see is that the deviation from mean of the annual precipitation follows a roughly two and a half year pattern as well. So we think that possibly this is some signal in response to rainfall.

So basically, our conclusions from the frequency analysis were that there is linearity in these responses, and we believe that it was structurally controlled, and that the flow field and frequencies were different on each side of the block, and this two and a half year rainfall distribution.

We also applied this analysis at Devil's Hole, and this is just a map to orient you to where Devil's Hole is. It's not too far from here. Devil's Hole is a spring, for people who don't know about it, emanating from the carbonates. And before the earthquakes, we had done an analysis to look at the water use there and had found that there was a significant declining trend which really started--our analysis started way before '89, but this is a plot from '89 to '92, before the earthquakes, and it was very much declining.

After the earthquakes now, we did this same analysis again to look at the trend, and what we found was that the trend was quite different. It had now flattened out, and this was not only a function of the earthquakes, but we believe it reflected the increased rainfall that came in the winter of '92 and early '93. So this later part here of the analysis actually flattened this curve out quite considerably.

With the earthquake data, that bottom line there was without the earthquake data, this is the response at Devil's Hole, this decline from the Landers and Little Skull Mountain Earthquakes. As you can see, it was really a steep decline, and then it recovered again to higher levels.

So whether we use the earthquake results or not in here, it really made no difference. It was pretty much a level line.

So after removing that small linear trend, we then looked at the cosine function using FIT.M once again, and we came up with a period of approximately 350 days, so we assumed that that was pretty close to an annual cycle that was reflected in Devil's Hole.

In addition to the annual cycle, we could also see a larger cycle, once we subtracted this annual cycle out, of about a 3.8 year period reflected in there as well, and we don't really have an explanation for that cycle.

We also looked at the response of wells in the region, not just Devil's Hole, and some interesting conclusions came out of this work. What we found was there were basically four types of responses to these earthquakes. The first was a sharp increase and then an immediate recovery. The second type was a sharp increase with no recovery. It was a sustained deviation. And the same in the reverse, decline and recovery, or decline and sustained decline.

We first noted that it was interesting that Devil's Hole had a sharp decline and a sustained one for a short period of time, and then right next to it, all the adjacent wells showed an increase in water level. So we started wondering, well, why could this happen, and we remembered that there is a fault zone there and we believe that these structures actually are controlling the response.

So we looked around the region and plotted these wells in regard to the different fault zones, and what we found was that in the extensional zones, which are the dotted lines, that consistently we saw a water level drop, but in the transform faulting, the shear zones, we consistently found rises, and these are these zones this way.

So basically our observations were, as I just told you, the shear zones increased and extensional zones decreased. Again, this kind of reinforced our structurally controlled idea of the flow system, and at this point, we decided we would attempt our own modeling exercises to see what we could come up with in terms of flow paths at Yucca Mountain. And in doing so, we started looking at the available data.

Well, basically there are a couple of types of data that you can use that were available at the time upon which to calibrate your models, and the first is potentiometric data and the second is temperature data. Temperature can be a very good indicator of flow paths.

We looked at the potentiometric data, and this was a 1984 potentiometric surface by the U. S. Geological Survey. After this surface was done, they decided to go back and re-level a lot of the holes. There was some discrepancy whether or not these elevations were correct. And they also corrected the water table measurements for temperature and density, and when they did that, they published a new publication, 1995, and in this publication, you can see that the water table surface is quite different. I'm going to put these both up here for a moment so you can see that these embayments up near Drill Hole Wash and below the repository footprint had disappeared essentially from this new potentiometric surface.

This essentially, if you were going to use this as your guide for flow paths, you would assume that flow would be moving pretty much to the southeast from the northwest, or diagonal to these lines.

We wondered what happened to the embayments here, so we went back and looked at the actual data that they used in the report, and in the report, they had a paragraph, a caveat that says basically that a lot of these data here in this area, they didn't use it because they couldn't explain it. They felt there was no physical explanation for the data. And we felt differently because we felt it was coincident with some of these shear zones, so we went ahead and took their actual data from the report and plotted it. And I want you to notice particularly this 730 meter contour line. And their data when potted, you can see that it is coincident with the traces of the Sundance Fault and the Drill Hole Wash Fault. Each place you have these embayments.

This embayment here when we plotted it, we thought well, maybe there could be another fault in here, and I believe Bill Justice told me when they did the C-Well test, they actually did discover that there was a fault here, but I predicted it on the basis of this embayment.

I've also lately been told that the Ghost Dance Fault trace does not extend beyond--I mean, the Sundance Fault trace does not extend beyond the Ghost Dance. But to me, that is not so important. I think what's important here is that we do have fault intersections that are controlling the direction of flow.

So if you were to take this map to calculate your flow paths, it would be quite different. You would see flow coming into these faults like this, moving into it rather than straight across. So it's quite a different flow path than had you just used this straight potentiometric surface.

So we're quite concerned that anyone doing the modeling should use the correct potentiometric surface, and don't be too quick to throw out your data points. Look at them first.

As I said, temperature is another indicator and another data base that can be used to determine your flow paths. So the available temperature here was done by Sass in the late 1980s, and as you can see what's happening here, I hope, is that there is a very high heat loading here to the west on Solitario Canyon, and this is close to those volcanos that exist, 38.8 degrees centigrade, essentially, compared to the center point of the mountain itself, and here is the footprint, but you can see a cold water plume moving down the center of the mountain. And at some places, this is 30 degrees C., so we have an eight, almost nine degree temperature drop within like a kilometer--two kilometers at best.

Then again a heat source here following Midway Valley extensional zone, and then over here in Forty Mile Wash, we have another cold water zone.

So given that temperature distribution, we then came up with some flow paths that would be likely based on that temperature, and saw that essentially the flow is coming down from the north, the cold water is up here on the north side of the hydraulic gradient, what little data we have, we know it's cold, moving down almost coincident with the Ghost

Dance Fault trace, and then moving out into the Midway Valley area. There may be some intrabasin flow coming through the lower part of this graphic here.

I do want to just put one thing back up here for a moment, and maybe you can see some similarities. I noticed these similarities right away. If you'll look at the stress field and assume this is a transform faulting situation, what you have is this zig-zagged pattern where you have shearing, shearing, extension, and then on this side, you have both plates moving at the same, together.

So I see this pattern here in this flow path. How real it is, I don't know. We have to construct some models and do a lot more work, but at the same time, it does give you a visual indication that it is moving along these fault zones and is structurally controlled to some extent.

I have to apologize for this slide. It may not be too clear. But this is our grid that we developed for the 2-D model. We used A-TOUGH, which was developed by Parvis Montazer. We can run it on our PC. We were using V-TOUGH and had to run it on the Cray at UNLV, so we really were appreciative that Parvis took the time to put this on a small computer.

Basically, we included the faults explicitly in our model. The scale here is somewhat smaller than the sub-site scale model that you just saw from Bruce. This is the trace of the Ghost Dance Fault and the Midway Valley fault zone, and over here, the Solitario Canyon Fault.

Now, we do have different permeabilities assigned to these because as we said before, the different fault zones, you know, are not all transmissive. Some are quite tight. We did simulate the hydraulic barrier that John Czarnecki was just talking about with a very tight zone here. And then because it was tight in going into a regular tuff type of permeability, we had to have a transition zone so that our model would stabilize.

But basically, this zone is more transmissive. This zone is transmissive. And these northwest shears are less transmissive, as is the Solitario Canyon, and you can see the numbers that we have used for permeability in these various zones.

Some of our initial runs, and we didn't get to do very many runs before our funding was cut, but we felt that we actually did accomplish quite a bit with these few runs, here's the actual potentiometric surface. This is the 730 meter contour line, and as you can see, we did manage to get those embayments shown on here, maybe not exact, but at least we got them in.

And then with our temperature match, here's the actual temperature and here's our simulated results. We did manage to get a cold water plume, this is the 31 degree contour, and then the hotter water here, and hot water here again.

However, what we realized was that this was not really adequate, because with the significant temperatures, we felt this water was up-welling and probably from the carbonates, and work a little bit later on in time, the last year or two by John Bredehoeft for Inyo County, John took some of the data from P-1, the only carbonate well, looked at the temperature and calculated through the use of earth tides what would be the bulk permeabilities and the head relationships from the carbonates, and he calculated basically that there was a 20 meter head difference. The carbonate was higher by 20 meters than the tuff aquifer.

So we feel that there is a significant component that needs to be accounted for. And as you saw, so far, no one is using temperature as an indicator. We feel temperature is very critical, and we don't also agree with this no flow boundary condition on the bottom. We feel like we have to account for upward flux.

So the next step in my opinion would be to take this lower temperature boundary, and this is from Fridrich from the USGS, and it was recently used by John Bredehoeft in his water resources research article on this topic. This is looking from the south to the north. This says to the north, but it's from--to the south, but it's from the south. This is Solitario Canyon, the high heat loading here, the 38 degree temperature. This is the Midway Valley, extensional fault zone, and then here is Yucca Mountain with all of this cold water down through the middle of it.

So we feel that we need to incorporate this now into a three dimensional flow model which accounts not only for the correct potentiometric surface, but also this temperature difference, because it is significant across the site in a small horizontal distance.

So that's where we stand now, and if you have any questions, I'd be happy to answer them.

PARIZEK: Questions from the Board? Questions from Staff?

(No response.)

PARIZEK: Okay, we thank you for your presentation. We have a chance for one more, and this has to do with Nye County proposed saturation zone EWDP. Parvis Montazer will make the presentation, and we'll try to finish as promptly as we can to make it to lunch, and we'll have to postpone getting back here until 1:30, 1:35, depending upon how long the presentation takes.

Thank you.

MONTAZER: Thank you. Can you hear me?

I'm Parvis Montazer, Consultant to Nye County, and I would like to take this opportunity to thank the Board for providing the opportunity to make this presentation. I'd also like to extend Nye County's appreciation to DOE's cooperation in allowing us to get access to the site and invite us to a lot of closed door meetings as far as technical is concerned, and keep Nye County aware of what's going on in the project.

What I'd like to do is first apologize for not providing you with handouts. We'll mail handouts to you shortly after--by the end of this week. But I'd like to just briefly go over Nye County's plan on providing an early warning system, monitoring system, in conjunction with the Yucca Mountain study. And as part of that, we have a more extensive saturated stone study that I'll briefly go over.

Our saturated zone modeling effort that is ongoing, and I'm not going to go into the results because they're all preliminary at this point. The question is why Nye County is developing another model.

As everybody in this room, the basic primary interest is, Nye County also is interested in protecting the resources and lives in the county. The thing is that more often than not people in the county come and ask the representatives as to what if this, what if that, and what's going to be the impact. And we often times have to wait until we get the results from DOE, or pose those questions to DOE before we can provide a response to the citizens.

So we're taking this upon ourselves to basically recreate or copy, if you will, the DOE's saturated zone and unsaturated zone models into one combined model that we can basically run and do scenario analysis.

The flow of information is slow. That's not being critical on the project. That's the nature of the project, and we can't wait for a lot of the time for the final results to come out, so we have to--we want to be able to do a lot of the scenario analysis on a realtime basis. And as the project data comes in, we want to be able to evaluate these different alternatives.

And right now, there's really no monitoring system that's designed for the site, so we want to use this model to provide a tool for us to see if we can design our monitoring system, and at the same time, really find out where the data needs are.

Our approach as far as model is concerned, our philosophy basically is that the model is no substitute for the data, but it can be used as a tool to see where the data gaps are, and that's where we're heading.

Just to briefly show you, I don't know if the grid shows this, this is the same picture you've seen from D'Agnese's model. Our grid is more focused. The 32 VOC which we're going to be using basically is TOUGH code with some transport capabilities in it, and I'm not going to go into detail on that. But it has a flexible mesh just like TOUGH-2, which is used for the unsaturated zone, and it has the advantage of being coupled with unsaturated zone readily, so we can basically decouple or couple the saturated zone flow with the unsaturated zone.

As you can see, we have emphasized our focus. This is a blow-up of the same mesh. You can see in the Yucca Mountain and the Amargosa Valley and down to Pahrump Valley, we are trying to emphasize the grid in that area where most of our interest is as far as what is going on with the system.

Based on this saturated zone investigation and reviewing all the information that's available, we have basically come up with a monitoring system, and the purposes are relatively clear. Number one is off-site hydrogeologic system not well known. As you heard from Zell, there's a relatively large data gap right south of Yucca Mountain, and the off-site ground-water monitoring programs are not satisfactory. Actually I said basically don't exist. And we don't see it really feasible for DOE to install an adequate monitoring system based on the economics of the project.

And last but not least, the ground water in the Amargosa Valley is becoming more and more valuable as the demand goes up.

This is our preliminary proposal for installation of a phased drilling program. Yucca Mountain, this is the repository area, and we've designed this as phased, every year there will be two deep holes drilled and five shallow holes will be drilled to characterize the alluvium and the immediate bedrock. The deep wells are designed to as deep as the carbonate aquifer and figure out how the flow system or what's the stratigraphy and how the flow system is behaving in that area.

This well configuration is not cast in stone, and the reason we have it phased is we want to be able to, as we drill one hole, be able to predict and decide what we're going to do with the next hole. All the information that is going to become available from this is going to be used to planning the next location for the next hole. And we have plans and we have discussed cooperative efforts with the U. S. Geological Survey both on the geophysical activities and on the geochemical activities. All the samples that we collect will be available for analysis.

Just to show you the purpose of why we have the shallow and deep system is as far as monitoring is concerned, this is a cross-section north to south, going toward the Yucca Mountain, and this would be the Yucca Mountain area, and this is just a very simplistic scenario, what if you have a situation where basically either your pathway misses the deep holes--the deep holes are going to be spaced very far apart, therefore, it's very easy for us to miss any narrow plume going through there, but once we get into the alluvium, they'll be dispersed enough for the shallow wells to be able to detect it.

Therefore, the primary detection mode is going to be the deep wells that are going to basically, the front row, if you will, and the shallow wells are basically backup just in case the deep wells miss the plume.

And that's all I have, and thank you.

PARIZEK: Thank you. Questions for Parvis? Parizek, Board. A question about your time frame for drilling; is that something that's likely to happen within a year or two years? And will it be quality control type well data which would be useful in licensing or support licensing data source?

MONTAZER: I'm going to attempt to answer, and I'd like Nick to jump in. Our hope is that to actually get funding to start this year, and the first set of holes we'd like to be able to install this year. And as far as the data becoming available for licensing, I can't--I'm not sure if that's Nye County's position to say whether that data is going to be usable for licensing, because the primary purpose will be the monitoring system, but the data will be quality assured. Nye County has got its own quality assurance.

Nick, do you want to mention--say something?

STELLAVATO: Nick Stellavato, Nye County. All the data we generate, we put in our own QA plans, and NQA1 program, so that we could take any data we do generate into the license if we needed to. So everything we do is NQA1, and it falls into NQA1.

PARIZEK: But it surely is data that would be useful; it's near the alluvium bedrock contact where a lot of action would occur from fracture flow domination in the bedrock all of a sudden to a more dispersed flow in the alluvium, so it's kind of an important data base, it seems like.

MONTAZER: Yeah, we understand that.

PARIZEK: John Czarnecki?

CZARNECKI: John Czarnecki, USGS.

You mentioned that you would drill down to the first bedrock. Do you have any plans, say if you hit bedrock at a very shallow depth, to keep going and try to hit the carbonate aquifer in a number of these holes? We have an example of one of the holes which I think is planned to be converted to a multi-port observation well. That was Felderhoff-Federal 25-1. That hit the paleozoic carbonates at about 2,000 foot depth. Do you have like a target depth to work with, and if you have more depth to go, youi would drill that deep?

MONTAZER: Let me first clarify as far as hitting the bedrock, that was for the shallow wells that are going to be in the alluvium. We'll try to penetrate the alluvium and hopefully select a thick section of alluvium and penetrate the upper 100 to 200 feet of bedrock, whatever that be, most like volcanics. The deep wells however, they are targeted to go at least 3,000 feet. If we don't hit the carbonates, depending on funding, we're going to go as deep--we want to make sure that we hit the carbonates, at least 100 or 200 feet, and install--all of these are going to be on multi-port observation wells, and some of the wells, deep and shallow, we are going to plan to install a facility to be able to do pump tests. So there may be a possibility to do at least a single hole test, if not cross-hole testing in some of these holes.

PARIZEK: Thank you. I think we would say that any further questions for late this afternoon, we have again a public session. It's now time for beef tips over noodles and lasagna at the buffet, $5.00.

Can we come back by 1:30? That gives us an hour to eat, and we're then only 20 minutes behind schedule?

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

PARIZEK: We have one minute to assemble for the afternoon session.

Putting the pieces of the puzzle together, the first presenter will be Robert Andrews. He's Deputy Manager, Performance Assessment Operations. He'll talk about the saturated zone flow and transport conceptual model and parameter issues affecting total system performance assessment. It's how much of what we heard this morning needs to be known about in detail, where this all fits in

ANDREWS: I'd like to walk through a few things today and of course put this all in perspective of why this is important from a performance point of view, and it's always easy to list the issues. I think we did a good job of listing the issues, and this morning, Dr. Parizek listed some issues when he started out. Those will be very similar, as you'll see, to a list of issues that come up later on here.

What we're maybe sometimes not as good at is quantifying those issues in a way that then can be made in a predictable evaluation forward looking mode, and then to evaluate the consequences or the significance of those issues to help define additional data collection, et cetera. So I'm going to walk through very quickly from a performance assessment perspective the whole natural system, then zero in on just the saturated zone components, and then talk about some technical issues associated with its incorporation in performance assessment, and then a series of technical issues which then will feed into Kevin Coppersmith and our two experts, plus a follow-on discussion of things that were elicited in a series of workshops last summer and fall.

You can't have a PA talk without talking PA a little bit, just to make sure everybody understands that we have a full system here. This might be a little bit of a blow-up focusing in on one component--or all the components of the system, but here's all the engineered components of course which haven't been discussed at all today, all the other natural components in the unsaturated zone which haven't been discussed at all today, and what we've been focusing on is just what happened in the saturated zone.

Putting it in more of a modeling visual, we're looking at just this little bubble down here, which is in the saturated zone, the flow, and ultimately transport of radionuclides to some receptacle located at some distance down gradient from that.

The next slide talks about the natural system in very, very general terms, but ultimately, what the saturated zone does is this very last bullet. It provides for a given release from the EBS from the unsaturated zone, provides some reduction by either dispersive dilution or retardation processes and some delay in the arrival time between the repository point to some distance down gradient.

The next slide really talks about what we want. Ultimately, and I think Bruce Robinson--well, I guess the last project speaker this morning, discussed this at some length. But ultimately, we want to take a release of radionuclide Y, so we look at some key radionuclides, the release of that from the base of the unsaturated zone as released into the water table and into the saturated zone, what is the time of arrival and ultimately the concentration of that radionuclide when it reaches any potential withdrawal wells if that's the ultimate point of discharge.

You know, our current understanding from what EPA is talking about and what NRC will probably ultimately implement is that it will be some kind of a well withdrawal type scenario, because it's closer to where we are now than it is to any point between here, which is about 50 kilometers from the site, back to any other regulatory type boundary. Whether that regulatory boundary is 30 kilometers or 5 kilometers or 20 kilometers, which is the current bases for the calculations that are going on for the viability assessment is TBD, of course, based on what EPA does and what NRC does.

The focus then is on a few key radionuclides, and these are no surprise because they've been coming out in other people's performance assessments, as well as project sponsored ones, are the very highly soluble and generally unretarded species, although Arend Meijer gave a talk this morning talking about the potential for technetium and neptunium to be potentially highly sorbed due to changes in geochemistry, and he even gave the potential for iodine on the organics in the alluvial system to also be sorbed.

So what I have here is generally they're treated as non-sorbed species and highly soluble, but may not be in fact the case in local spots in the saturated zone.

An important point I think somebody mentioned this morning is that the releases from the unsaturated zone are not point releases. They are in fact distributed releases. They are distributed over space, because it's a relatively large repository area, and they're also very distributed over time. The degradation rates of the engineered components of the system are time varying functions. Those time varying functions and the degradation mechanism and the transport mechanisms all tend to spread out the release that arrives at the water table in time. And, of course, as we've talked I think this morning, taking those releases and translating them into concentrations at that 20 kilometer distance is also temporally and in fact spatially variable.

What we've assumed this might be right here, in the absence of EPA telling us where that well is, we say let's assume that well is in the zone of maximum concentration at that 20 kilometer fence. So regardless of whether or not that is exactly the maximum concentration or not, that's the assumption, and I think many of you have read what the NAS panel said. They had two kind of alternative models, one which had a stochastic kind of likelihood of interception of plumes, and the other one said no, make it simple, just deterministically assume you're going to hit that plume with that well. Don't take any credit for the likelihood of intersection or non-intersection of a plume. But that's an issue that's going to be discussed I think by our two experts, certainly an issue that came up in the expert elicitation.

Some major groupings of saturated zone flow and transport model issues or uncertainties, and these come from a lot of different places. NRC of course has reviewed this and they have issues or sub-issues associated with the saturated zone. The project has sponsored a number of workshops. We did go into the viability assessment. We said, well, let's get all the people together that know something about this, and ourselves come up with what we think are the key issues that need to be addressed. And that does need to be addressed mean? It means through somehow, either in a sensitivity study or as part of an elicitation or direct incorporation in the viability assessment, we need to quantify and address the significance of these issues. And if that means for L.A. purposes going and collecting additional data, then that's what it will point to. If it means the significance is small and maybe you don't need additional data, and the sensitivity showed it was relatively insignificant, then you say, you know, no point in going on with significant additional data collection on issue X.

So this just more or less lists these issues. Some of them are really geologic issues. Some of them are flow related issued. You've talked a lot about both of these this morning. Some of these are more transport related, and some of them are more modeling related, coupling of the unsaturated zone and the saturated zone, looking at coupled effected, like climate changes and tectonic effects and thermal effects.

Another issue down here at the bottom is what kind of scenarios might be used to actually simulate the water withdrawal from the well. Does the well tap the whole aquifer? Does it tap the zones in the aquifer? What's the likelihood that it tapes a zone that has no contamination versus contamination? Those sorts of issues.

One of the things that came out of that workshop that we conducted in the spring of this year was more or less a series of activities to try to address in a quantitative fashion some of the sensitivities with the process understanding and look at alternative ways of modeling or alternative ways of addressing particular technical issues.

And so a series of studies in fact were conducted last fiscal year. Bruce this morning talked to you about this one. Bruce and Bill Arnold worked together. Bill will talk to you a little bit later about how the two zones are being connected effectively for TSPA purposes. You heard a lot of discussions already about the C-Wells and those data, and interpretations of those data are being used directly in the TSPA for the viability assessment.

Frank talked to you at some length this morning about the past and present, and a little bit about future and the climate impacts. You didn't hear about this one, but there have been other studies done by LBL to look at structural effects on flow.

Some of the things that Linda Lehman was talking about this morning have also been addressed by the project looking at structural effects and temperature effects on the SZ flow system.

All of those issues and all that knowledge was then kind of consolidated into I have eight, I think actually when it was elicited it came out to be ten issues which were then, with all the relevant data that the project could bring to bear, or others could bring to bear, was given to a series of I think five experts for them to help us quantify the uncertainty in key aspects that we all agree were key aspects of the saturated zone flow and transport model.

Kevin is now going to walk through the process of that elicitation, walk through the results of that elicitation, and then you've asked two of the experts to give their own feedback and their own personal reflections, and hopefully you'll ask them the hard questions that you asked people this morning, too. That's what they're paid the big bucks for.

So with that, I'll stop, and if there's no questions for me--

PARIZEK: Wait. We have to at least allow for questions.

Any questions from the Board for Bob Andrews? Priscilla Nelson?

NELSON: And you can defer this to a later speaker.

I'm interested in the source term characterization for saturated zone analysis. Will that be covered in terms of what it looks like that's coming out of the UZ into the SZ in time?

ANDREWS: Will it be covered in here? No.

NELSON: Is that going to be discussed here?

ANDREWS: No.

NELSON: Like is Dr. Arnold going to discuss that today?

ANDREWS: He's going to talk the process of coupling the UZ transport model with the SZ flow transport model. Now, the output of that UZ transport model is a function of all of the engineered components of the system and the degradation of those engineered components and dissolution and transport in the engineered components of the system and transport in the unsaturated zone, which is another couple of days of talks, which we don't have right now.

NELSON: Okay.

PARIZEK: Alberto Sagηes?

SAGγES: You have in your transparency Number 6, what do you exactly mean by source term? It says Y is determined by source term, and then EBS release and UZ transport--I thought that EBS release was--

ANDREWS: Yeah. I mean, the source term in that particular thing means, you know, the waste form, degradation of the waste form, the cladding, release from the package itself, and the EBS is the additional engineered components that may or may not be in the drifts.

SAGγES: But they're also studied?

ANDREWS: They're all related to, yes.

SAGγES: Okay.

ANDREWS: Often we get a release at the edge of the drift, and that release at the edge of the drift is transported through the unsaturated zone, yeah.

SAGγES: Okay, thank you.

PARIZEK: Chairman Cohon?

COHON: Cohon, Board.

Is TSPA/VA done, frozen, in terms of what's in the model, both model structure and major parameter values?

ANDREWS: Model structure is for 95 per cent of it. There's a few--

COHON: What about with regard to saturated zone?

ANDREWS: It's frozen.

COHON: And in terms of what we're about to hear as the expert panel, did any of that influence the saturated zone representation in TSPA/VA?

ANDREWS: Yes. You'll hear that from Bill Arnold, I think.

COHON: Okay.

ANDREWS: How it was used.

PARIZEK: Other questions from the board? Staff?

(No response.)

PARIZEK: Okay, thank you, Bob.

Our next speaker is Kevin Coppersmith. He's vice-president with Geomatrix Consultants, and he's sort of been the driving force behind this whole expert elicitation process. I call him Mr. Certainty, seeking project uncertainty in the program, and he's not looking for consensus, he'll tell us, but I see also an analog for Kevin. He's sort of like to the expert elicitation process, like a grape press is to wine making. He gets to squeeze the experts for their opinions and their uncertainties, and we'll see a couple of grapes after Kevin's presentation.

COPPERSMITH: Sometimes I think the expert elicitation process is sort of the project irritant. We irritate the technical PIs that are trying to get work done and milestones in, and we cause them to come and give a series of talks. I think Frank D'Agnese, for example, gave three talks in a single one of our workshops.

We have the experts who have to do far more than they would if they were on a peer review panel. They actually have to quantify uncertainties and go through a very torturous process of drinking out of a high powered fire hose, and that's very tough to do, and then others in PA are asking when are you going to get your results to us so we can use them in the viability assessment. So it is a real challenge, and I won't continue on with the wine making analog.

Let me go through in general the purpose, the objectives of these elicitations, and I want to make it very clear there are a couple of issues, procedural issues, that I'd like to cover in the course of this as we go through it that I think are very important.

Number one, the goal of these elicitations in the saturated zone is one of a series of five elicitations being conducted for purposes of the viability assessment is to quantify uncertainties. So we're focusing in on a characterization of uncertainties. Now, that characterization can be quantitative in the sense of uncertainty in parameter values, or it can be more qualitative in terms of the relative degree of belief that exists in alternative conceptual models, or it can be much more qualitative than that just in terms of the overall conceptual model or important processes that might need to be considered in the process.

So this, to my way of thinking, is the focus. It's not to supplant data collection or other types of things, but to characterize uncertainties.

A panel of experts, we have members on this expert panel, as I'll show in a minute, who have been involved in the project to some extent, and we've had those who have not been involved in the project. The idea is, and the question always comes up why not just involve project experts. My definition of an expert is an acknowledged--an individual with acknowledged specialized knowledge and experience, and that acknowledgment comes obviously from publications and other peer review types of things. The specialized knowledge and experience speaks for itself.

Obviously, we have those individuals on the project. There are experts. There's nothing special about that, other than the fact that by bringing people in from outside and focusing them on uncertainty characterization, we might have an opportunity to get a broader perspective, a better representation of the diversity of views that exists at some particular point in time. So that's not to say that there aren't experts within the project as well.

It is a snapshot of uncertainties. My view of life, if I can give it briefly, is that technical interpretations consist of two parts, data, plus judgment, and I think that data at any particular point in time, there's a ratio of data to judgment that go into the interpretation. I think there is uncertainty in data. Even if we have a hell of a lot of data for some particular aspect, we still have uncertainties, at least variability or dispersion and other measures. There's uncertainty in judgment. Alternative conceptual models might be an example. And ultimately, our responsibility for something like TSPA is to characterize the uncertainty in the interpretations. We do that by characterizing those uncertainties in the data and judgment.

The question often comes up where you do use the elicitation to supplant data, or in lieu of data, or don't be tempted by the possibility of being able to use elicitation to supplant data.

From my point of view, expert elicitation is the process of characterizing uncertainties in the interpretation, the degree to which at any particular time, the data to judgment ratio can vary. And we asked this panel of experts for that snapshot this summer, basically in the August, September time frame, and that was the point they had that they were able to look at the data, try to drink from the fire hose of information they heard from multiple talks and written materials to make that assessment. And it's a very difficult assessment to make.

This is part of a series of elicitations, and I think again the distinction may be for another talk to this group. This is very different from a peer review where you're asked to come in, talk about what's being done, why it's being done, grill the technical PIs and then talk about what should have been done or could have been done or will be done. These guys first have to make an assessment of uncertainty. To do that, they need to assimilate a huge amount of data in a small amount of time. We go through a process of characterizing those uncertainties, and then we do ask the question at the end what do you think can be done to reduce uncertainties. And that's an opportunity for them to be as specific as possible about those types of activities that can be conducted. And we'll hear some about that in the discussions that follow me.

I should point out one thing that I've only read the written materials. The Board, as you know, brings the experts in, so we're not involved in that process. But sometimes we're questioned about whether or not a process we follow somehow biases or unduly affects the judgments of the experts, and we work very hard at not doing that, and I think good evidence of that is Lynn Gelhar's written comments where he basically draws us across the coals for some of our procedural aspects. So clearly, we haven't affected him in any way.

The steps that we follow, and again these are the steps we followed at other elicitations. This follows what's called a formal elicitation process, and it's not formal in the sense that we all were ties or we use a rigorous questionnaire type of approach that's very formalized. In fact just the opposite. Those of us who have been in the game here know within the jargon formal means that you basically have a set of steps that are followed. The NRC technical position on expert elicitation, DOE guidance that's been developed all specify this type of series of steps that deal with selecting the experts, carrying them through with dissemination of data, discussion of alternatives, interactions at multiple workshops, going through an elicitation process, feedback, documentation. That's what we mean by formal as opposed to an informal process where it isn't clear quite who made that assessment, where the judgment came from, what the technical basis was, whether or not it was designed through uncertainty, and so on. Informal technical judgments are made all the time and are part of the process of evaluation. Formal allows this process to be more clearly defined according to these types of steps.

By the way, I was talking with Dick Parizek, and during the course of these workshops, I was sitting down and thinking about it. I think we've had on the order of 500 talks, or so, and every one of the times I was the guy sitting there with the watch. So I know how horrible that process is, and I probably won't save you much time as we go through this.

This is the members of the SZ expert panel, again composed of a number of acknowledged experts, and some include information or past experience related to the project. Others have not been directly involved to any extent.

Let me just briefly go through. You have in your packages my viewgraphs obviously, and you can look at these in more detail, but let me just step through some of the issues that we asked the experts to address. But remember that again we're looking for not only quantitative uncertainties in parameter values, and so on, but also looking at some of the conceptual issues, conceptual models, how these things should be developed. And some of the members of the panel are modelers themselves and spent quite a bit of time exploring the regional hydrogeologic framework model, looking at the way it couples or doesn't couple with the site model, some of those other detailed aspects of the process.

In terms of the conceptualization of SZ groundwater flow, a couple of important issues that were address, for example, the down gradient flow that we were talking about this morning, appear to be largely within the volcanic aquifer and the alluvium, not much evidence for actual down gradient flow within the carbonates. Highly permeable flow regime; channelization is a very important issue and we'll hear more discussion about that. It appears to be a highly interconnected system of faults and fracture system, again, consistent I think largely with the discussions that we heard this morning by the technical PIs on the project.

Multiple hypotheses have been proposed; large hydraulic gradient. I'd say the panel in general was split in terms of the importance of that feature. Two, saturated zone flow and transport issues. It obviously is an important issue from the standpoint of our understanding of the regional hydrogeologic framework, but it's important to things like flux of the site or to down gradient flow and transport issues can be debated, and maybe there will be a little bit of discussion about that after me.

But basically, the panel boiled it down to two alternatives that actually are very close in my mind in terms of the causative mechanism for the large hydraulic gradient. We asked them for the possibility of short-term or transient changes, and those were deemed to be very unlikely.

The key issue of course is the flux at the top of the saturated zone beneath Yucca Mountain. It was defined as specific discharge, the product of hydraulic conductivity and hydraulic gradient. We're dealing with issues, scales that are the scale of the site scale model. John Czarnecki talked about on the order of a kilometer by a kilometer by perhaps 100 meter type of scale in making this assessment. They considered the uncertainties in hydraulic conductivity that were presented to them, both in situ type single hole, multiple hole, laboratory analyses, and so on, in arriving at their assessments.

There's quite a bit of uncertainty in the hydraulic conductivity, particularly at these scales, and basically we're dealing with sort of effective of bulk type of assessments over those scales. And of course because of those scales, the C-Wells data and other types of field tests that deal with characterization of the scale you're asked to make the assessment were deemed to be very important tests, and I think we'll probably hear more about the importance of those tests and other types of tests that can be conducted that are similar.

Velocities and porosities were provided by some of the experts, average linear velocities on the order of meters per year, with a good healthy range, and also some of the kinematic porosities, again a range of values.

The influence of climate change was discussed. Again, this is one that's perhaps less quantitative or qualitative, what do you expect to happen. Some of the evidence, geologic and otherwise, for past water table changes was deemed to be a reasonable way to estimate the potential for future water table rises beneath the site. Evidence was presented for past water table changes due to glacial episodes on the order of 80 to 120 meters. That seemed to be reasonable based on the data and presentations that were made to the panel.

We also asked them about the problem or the issue of transient glacial periods. Could this likely lead to different flow patterns or different types of processes that might perhaps affect dispersion and dilution. And there's some disagreement on that, and maybe I'll talk a little bit about that, but whether or not in fact that climatic change condition, there will be long-term transients, but whether or not they're short-term enough to lead to true changes that would lead to more of a dispersion type mechanism, there's kind of a split on that issue.

Let me show one example of just some of the assessments that were made for, for example, for specific discharge, Shlomo Neuman dealing with the issue of specific discharge. We have some uncertainty in the hydraulic gradient, but most of the uncertainty in this assessment comes from uncertainty in hydraulic conductivity of the lower volcanic aquifer. And this is, for example, an example of an assessment that was made where he specified a particular type of distribution and the parameters to that distribution. In other cases, point estimates were made, cumulative distribution and functions and so on.

If we put those together, I want to use this just as an opportunity of showing where we have multiple assessments of a particular parameter value, and there actually aren't very many that we made in this elicitation, that we have this type of spread, expert to expert types of distributions, and the aggregate distribution that's shown in here in dark basically incorporates and treats all the experts equally and aggregates through a summing process across the CDFs.

Now, there's an opportunity, people have asked the question about what about if you have very few respondents, you have, for example, in sorption coefficients, we have one person making an assessment, in other cases, maybe two people have made an assessment. We're after uncertainty and we're after--there's some component of expert to expert, what's called diversity, that we're trying to capture. Obviously, in the process, if we're not--if we have one person or two people responding, we don't claim that that provides the full range of diversity that we're after.

These are not point estimates. We're not asking individual experts to be a proponent basically of a single view. We're asking them all to represent a range of views. But nevertheless, the law of low numbers comes into play.

In these cases, we use the information that comes from elicitation to supplement characterizations of uncertainty that are made from the PA anyway. So the TSPA has to characterize uncertainty in all of its input parameters in one way or another, the degree to which the elicitation can help with that process. If it provides only a qualitative assessment or provides just one or two distributions, that may not be sufficient to characterize the uncertainty. It will be used as a supplement; the uncertainty assessment will be documents in the TSPA.

Just a couple more. I need to finish. By the way, Dick, in every one of those talks, everyone promised they would be--oh, this is going to be quick and I'll be done way under my allotted time. 90 per cent of time they're wrong.

Conceptual models. Again, this deals with the transport issue. I think most of the discussion here is similar to the types of discussions that Bruce Robinson and others dealing with the transport problems, there's agreement there, but I think in general across this panel, it was felt that in fact the proper way a conceptual model for the down gradient flow is one that is more contained. It likely would be within flow tubes. This is a highly permeable type of flow system, and the scales that we're dealing with, in fact there are a few mechanisms that will lead to substantial mixing or dispersion along those flow tubes out to distances of 5 to 20 kilometers, or so. And I'll leave some of that discussion to the presentations by Allan and Lynn.

The same thing with dilution factor. When we deal with regional distances, again we're dealing with the scales that are appropriate to scale the modeling. We're dealing with dilution factors on the order of about ten, perhaps with a range, but again very little, in my mind, very little transverse dispersivity is assessed.

Now, to show just again, here's an example of a few of the assessments of the dilution factor, quite a range, and that range of uncertainty that could be used in the model.

Just a couple others, and then I'll end. We asked about effective fracture density, some of the transport parameters. Again, when we got into the discussions of things like Kd values for particular radionuclides, our hydrochemist, Don Langmuir, basically was the one who provided the most information related there, the only quantitative information. And, again, that will then be used to supplement the assessments that the project makes of Kd values. But basically, he made the point that, and all of the experts really made the point that the issue, a key issue is the residence time in the matrix, and again there were discussions about this this morning, the degree to which the C-wells data shows that type of dual permeability type of behavior is very important.

In fact, do we have field evidence that would allow us to deal with how much time or whether or not significant portions of the flow reside in the matrix for any appreciable period of time. That's where a lot of the reactions will occur and a lot of the retardation.

Some of the issues, and perhaps, Lynn, you'll comment on this, has to do with the use of the laboratory sorption data. There were some questions about its usefulness. It obviously is some of the best data that exists. But how representative of the field conditions is it in the types of tests that are carried out?

Let me just jump to the end, because again, one thing I've found through the course of these elicitations is that the recommendations are the areas that experts love to respond to the most, because it's an opportunity for them to focus back on the uncertainties and say that not only is this an uncertain quantity, but here's something you can do about it specifically.

So I just put those into categories that deal with the recommendations. In fact, in elicitation summaries, the experts are very specific about the types of tests that could be carried out to help reduce uncertainties. They deal with multi-hole types of tests, in situ types of tests that provide the proper scales for making the measurements, interference tests at C-Wells complex and recommendations there. Again, the issue of fault zone properties is one that is very important. How do faults, what are the hydraulic properties specifically of fault zones, how important are they to the flow regime? It's a very important issue, and the more that can be done in situ, the better off it will be.

Calibration and coupling of the regional site scale models, a very strong emphasis, particularly for someone like Shlomo Neuman who feels that they need a tighter coupling to make them usable. And, again, a few of the experts dealt with the issue of ground-water chemical data, potentially additional isotope data of the type you heard about this morning.

It's all in there and open for questions.

PARIZEK: Thank you, Kevin. Questions from the board? Alberto Sagηes?

SAGγES: Yes. Sagηes. How do you handle aggregation of magnitudes which are given on a--like the example that you show here with the volcanic aquifer velocity? Do you handle those things linear or logarithmic?

COPPERSMITH: They're handled all--you put them all into whatever space you want to have them. If they were collected in linear space, which they often were--actually, this panel was more logarithmic space. We can combine them that way.

SAGγES: But then you combine them logarithmically?

COPPERSMITH: If they've been provided that way. We're dealing right now with equal weights. We don't have any differential weights expert to expert in the combination scheme. They are all equal. The way we deal with that in terms of numbers is if a particular expert does not want to respond to an assessment, it's out of their field of expertise and so on, then we don't have an assessment for them on that issue. So we're not combining that.

SAGγES: Well, if an expert says point one and another one says one, do you average it as .3, which would be the logarithmic average, or do you average it as .5 or point whatever?

COPPERSMITH: It's linear.

SAG$_\gamma$ES: Linear average. If you ask for pHs, for example, one says pH of 4, one says pH of, say, 5, the pH average would be--you can average 4.5, or you can average to something else, depending--

COPPERSMITH: We do it linearly. The other important thing is that you'll see on all these, that we show the assessment that each individual expert makes, because that is important. But, you know, getting to the logarithmic, earthquake magnitudes for example, Richter magnitudes and all the magnitude scales are logarithmic one way or another, and they're all done linearly. It's clear when you have the assessment, individual experts also, you can see what their actual assessments were.

PARIZEK: Chairman Cohon?

COHON: Cohon, Board.

I also continue to have questions about the combining process, and I'm sure there are questions that I'll have forever, and I'm sure there are questions you have about it as well. But I want to try to get a sense of threshold, that is, it's clear that one expert is not enough, and the more experts the better, up to some number I guess where it becomes difficult to handle. What I'm really curious about, and it came up I guess in the unsaturated zone process as well, is when you have a relatively small number of experts who sharply disagree, in that case, the combining leaves one really kind of queasy, at least me.

Here's another example, and we'll wait for the individual experts to talk. I want to hear especially what Allen Freeze has to say about it, but when you have distributions which almost don't overlap at all, but instead of making this, say, instead of preaching, let me try to pose a question, is there some threshold of disagreement beyond which you'll want to step back and said wait a minute, I'm not comfortable here combining them at all?

COPPERSMITH: Well, there's a whole--there's different ways to answer that. One, though, is basically you're falling into a couple of camps and there's a bimodal type of distribution and you're basically combining such that the application, the use is going to be of, say, a mean value, then you're going to be wrong, by definition, you're going to fall between the modes of the distribution and it's going to be a problem.

These distributions are using the entire probability distribution, the entire combination. I don't think that there's any plan in PA to basically use some single value to represent the aggregated combination of assessments that are made by the experts.

But the other issue sort of how many, you know, the difference between one, two, three, four, to move up, how much more do you gain, has been examined. And normally the measure, or one measure that we've done in the past is to look at the contributions to uncertainty. If you have a case where you have a large number, say you have ten, like we had in the volcanic hazard, and the question came up how many did you need to have to capture essentially all that uncertainty or diversity, you can work your way back down through a random sampling process or bootstrapping process, down to eight, nine, and so on, and look at the way, say, the 90th and 10th percentiles come in. And what you see as you do those, if they don't come in significantly if you get down to numbers of like five or four, and there's right around at that point, you begin to really lose a significant part of the total uncertainty, say you lost 30 or 40 per cent of the total, and that's one way to do a test of that. I know the NRC has done similar types of tests when they were dealing with the issue of do they have enough experts.

COHON: But in the previous case, when you seemed to have two distinct modes, and you said by definition, it would be wrong for you to take a mean value, what would you do in a case like that?

COPPERSMITH: I would take the total combined distribution, and not a central value, and would display the individual assessments by the experts, so that anyone looking at the data can see where the mass of the data actually lay, and here's what the aggregate looked like.

COHON: But wouldn't your statement about the average of two point estimates being wrong apply to two distributions as well? That is, wouldn't a combined distribution in some sense be wrong?

COPPERSMITH: Well, it might not be wrong in the sense it depended on the application. If you wanted an assessment of hazard or something, then it might in fact be correct. But if you at some point need to dissect the details of a problem and see what the contributors were, you would need to see the details of those modes of the primary distribution at one end.

COHON: Okay, thanks.

PARIZEK: Other Board questions? Staff?

(No response.)

PARIZEK: We thank you very much, Kevin. And we'll continue with two of the five experts that made up the panel, Allan Freeze being the first. We'll call him a grape in this regard. He comes to us from Canada. He's had a distinguished career and continues to have one as owner of his own company, Allan Freeze Engineering, Incorporated. Before that, he was at the University of British Columbia, and before that, University of California Berkeley. Allan, we could say he's a straight shooting Canadian, but he talks fast and he'll get his in in a short time period, I think.

FREEZE: Well, maybe I'll start with my definition of an expert. My definition of an expert is somebody that can say whatever he wants without any responsibility for all the trouble he causes every all around him.

In my case, coming from another country, I'm kind of doubly protected, because I don't even pay the taxes that this might lead to.

You're not going to hear anything really new from me. I think everything that I'm going to say has been said by somebody else here in these last couple of days. Basically, we were asked our opinions. We were given all these possibilities, and asked in a sense which ones we believed. So, you know, I could give this talk by saying, well, I agree with Frank on his third slide and I agree with somebody else on their fourth slide, and Item 2-B of somebody else, but I'm kind of going to go through that, so I hope you're not looking for some kind of whole new ball of wax. You're not going to get it. You're going to get all the same ideas and me saying, well, in my opinion, you know, it's more this way than that way.

The issues that I thought I would try and zero in on today are the role of faults, the large hydraulic gradient, and the potential dilution, those three. And in a short time, I think those are three of the more important ones. I'm not going to say too much about the transport parameters, although I will mention my thoughts about retardation kind of in passing.

I think you have to start if you're going to talk about the role of faults, you have to start by thinking about the hydrogeology on the scale of the site itself. And, again, we've heard a lot of talks about this, but I just wanted to tell you where I'm coming from.

I see the different scales, the regional scale, the site scale and the sub-site scale, and each of the diagrams you might put up would look a little bit different. Here's a kind of a very simplified diagram of the regional scale, representing the lower carbonate aquifer, the volcanics up above and the alluvium down in the end.

You'll learn most of us believe that the flow paths from Yucca Mountain are likely to stay in the volcanics and go into the valley fill, but clearly, there's some possibility that the carbonate aquifer could be a pathway, although not perhaps very likely. But I agree with the people that say that it's an important controlling feature on the entire hydrogeology of the system.

You can move down a scale, and we've all seen this diagram, and now we have those two sets of names that we have to be thinking about all the time. The one set of names are the formations, the Topopah Springs and the Prow Pass, and all that sort of thing. On the other hand, the hydrogeologists have divided these into the upper volcanic aquifer, which is basically the Topopah Springs, the upper volcanic confining unit, which is basically the Calico Hills, lower volcanic unit and lower volcanic confining unit.

Much of what we talk about today is going to be concerned with the lower volcanic aquifer, the Bullfrog Tuff in particular, and the issue then is kind of what is the role of the faults in the movement of water through this system. We have to move down yet another scale I think before we can really begin to answer that question totally, and here's some logs which I'm sure you've seen before from the C-Wells, which show that there's a lot of internal basically horizonal stratigraphy within each of these formations, these geologic formations, and within each of these hydrogeologic units such as the lower volcanic aquifer.

There are welded and non-welded embedded sections, and I think all of us that have seen all this data over the years have slowly been educated towards the fact that it's the bedded--it's the fractured--let me start again--the fractured, welded, middle portions of the flows that represent a potential horizontal high permeability aquifers. That's the stratigraphic control on things.

So when I picture a leak happening to one of these canisters some century down the road, I picture it entering into some place in this whole system, but first of all, it has to get to here, this highly fractured, welded, potentially migration hosting horizontal stratigraphy. So the first step is it's kind of got to make its way there. Then it's liable to flow very quickly. But then of course it's going to encounter one of these faults which block these things all off into these little blocks. And the faults themselves then, we start thinking about them, and they also occur at several different scales.

At the regional scale, we have these big vertical faults, basically north-south trending, Solitario Canyon, Ghost Dance, Bow Ridge Faults, spacing about a kilometer, laterally continuous for hundreds of miles, offsets of hundreds of meters.

The next scale down, which you might see in a map like this one, are these ones in red, and there's even been some suggestion that the Sundance Fault down farther this way, a little different orientation, likely to have a little different properties. They don't go for as far and their offsets are more on the order of, say, tens of meters.

So, once again, we could picture that particle of water then having kind of gone through the matrix it enters, this sparsely fracture rock, getting into the Bullfrog, heading off somewhere, hitting one of these two different types of faults, and depending on which type it is, it might be a bigger or smaller event in its pathway.

If it hits one of the big ones, does it then just sail right off the site or not, or what do we expect those big ones to look like? Now you're starting to get some personal opinion. I have a fair bit of background in the geotechnical world in my experience, and have looked at a lot of faults around dam sites and things like that. The faults that I've actually seen, that I've actually mapped and looked at in the field, often are this kind of layering, if you want. I mean, this makes sense from a structural point of view, from a tectonic point of view, that when the faults move, on the one side--there's a moving side and a stationary side very often--on the moving side, you might get brachiation of the rocks. It breaks up. You get fracture and you get higher permeability, a plane of high permeability parallel to the fault.

On the other hand, on the stationary side, it tends to grind the stuff away. And if the formations that it's grinding on have a lot of play, and that kind of material that can be ground into gouge, then you get gouge along that side.

So there's a question of whether these faults are high permeability or low permeability. I suspect most of them are both, that they have high permeability parts and they have low permeability parts, sometimes both in the same place. But in other cases, I expect both those events to be patchy, too, because picture that the faults are actually breaking down through that cross-section of stratigraphic formations, some of which are highly brittle and some of which are more likely to create gouge.

So I picture on that plane, a kind of a checkerboard, if you want, of high and low permeability things, but somewhat layered, so therefore somewhat anisotropic in the plainer dimension, high permeability parallel to the plane, low permeability across the plane.

Now, that's all kind of a conceptual model that I think should be built into these models as best can be done, but many of the models so far that have been developed by USGS are at a bigger scale than that.

The newest model that's sort of appearing, at least just appeared on my desk, is the one from Berkeley Labs. It's called the S-4 Zedd model. I don't know what that stands for, S to the fourth Zedd--or Z, I should say.

What does it stand for, Dwight?

HOXIE: Yeah, it stands for the sub-site scale saturated zone model.

FREEZE: There we go. Okay. You know, I gather that Bruce Robinson was also using a model of that scale, and I think it's at that scale that we could begin to think about introducing some of these things. Clearly, they can't be introduced on huge 1.5 kilometer, 500 meter thick blocks.

But to my mind, when we're starting to think about travel times and dispersions and dilutions and all that sort of stuff, we have to think of the role of faults in a somewhat more complicated manner along the lines that I've been trying to describe.

The faults and the fractures are often stratabound. The fracture density depends on the unit. Hopefully, you can read the rest of this stuff as we go through this.

Now, I think there is some evidence that the major and secondary faults do act, you know, as high permeability pathways in the direction of flow. This is Linda Lehman's diagram which she showed you earlier today. I think it has some merit. It doesn't surprise me that there might be things like that, that the equipotential lines would be splayed out and around these various faults.

As you can see, if this were the case, as she has drawn it, then it means the flow is in toward these things, and there's a flow down this way. I wouldn't be surprised to see one come this way and go off that way. That would be that it's coming in from one side and not the other perhaps. So, you know, I think these faults certainly play a role.

There's abundant evidence that most of the in-flows to drill holes, this is just one of the C-Wells, that the flow coming in comes in in very bounded locations, and it's exactly usually where those welded middle Bullfrog stratigraphic units are, or where faults cross the system.

So this comes up over and over again. You get the flows coming in. You get the temperature moving. I think there's a lot of evidence to suggest that there's concentrated and focused flow. But I don't picture it, Dr. Craig, as being one of these--you know, as being open pathways or something with space in between. I think of them as being shear zones, fracture zones of some width of higher permeability with a lot of this high/low checkerboard permeability controlling it.

There's also evidence that they act as low permeability barriers in some places. This is a diagram that George Barr showed to us. I don't know if you can make it out. I just copied it from him. This is supposed to be the Solitario Canyon Fault over here, and there appears to be a drop in head across the Solitario Canyon Fault, which implies that it's some kind of a linear barrier parallel to its plane.

On the other hand, we have all this other evidence that they act as conduits parallel to their plane. I think a lot of it of course has to do with what direction the gradient is going at that point in time.

He had also kind of hypothesized some other drops up to the north that might account for the large hydraulic gradient. Those are obviously more contentious.

So in summary then of the sort of my opinions, if you want, about the fault zones is I think they're a very important feature, together with the welded highly fractured Bullfrog. I think that the flow paths will be ones that angle their way down through these things. I think any given flow, too, will experience a large variety of different hydraulic conductivity domains in its path. I doubt very much if there are high conductivity conduits that take you from point A all the way down to Amargosa Valley, just shooting it down at the highest possible permeabilities we've ever measured. I think that's highly unlikely. I think the flow paths would experience a lot of different permeabilities.

The stratigraphy then across the faults I think would be anisotropic. The stratigraphy itself I think would be anisotropic horizontal to vertical, and the faults anisotropic planer to cross-planer.

Some of the calibrations that were carried out with this new S-4Z model showed that the Solitario Canyon Fault could be calibrated as a low permeability unit, and the other faults as high K units. That sort of thing wouldn't surprise me, but I suspect it's somewhat more complicated than that.

Linda Lehman also drew attention to this paper by John Bredehoeft, which appeared in WRR last year, which seems to me to have merit, where he's used these earth ties to try and back out a calculated value for fault permeabilities for the faults that go down into the carbonate aquifers. And he came up with a number that's about an order of magnitude higher than the tuffs aquifers that overlay them. So, again, he's starting to get some ballpark figures, but these are going to be among the higher permeabilities.

So that's what I was going to say about faults, and I'll be open for questions later.

The next topic is the large hydraulic gradient. Maybe I should preface all this by saying I suppose some of the things I'm saying, my opinions will be popular in the program and some of them will be unpopular. I don't know where that one on faults stands.

This next one I suppose is going to be popular because I'm one of the people that doesn't think the large hydraulic gradient is all that important. First of all, of course we draw attention to the fact that there's a lot of large hydraulic gradients all around this area. They're a feature of the hydrogeology of this basin and range area.

The one that we're looking at is this one, but there are lots of them all over the place. So they probably share some kind of a common cause, in my opinion.

The second point I would make is, and this is just a cross-section taken from the paper by Fridrich, et al, is that the so-called large hydraulic gradient is not a huge thing, you know. It's just a somewhat higher gradient. It's actually, if we believe it's a fully saturated situation, it's just a slightly larger slope to the water table. I don't even like the term large hydraulic gradient too much, because we don't really know what exists down three dimensionally beneath it. It's a somewhat higher slope to the water table, and in actual fact, it's coupled with a less than average slope east of the Yucca Mountain site.

So the real question in my mind isn't where did the large hydraulic gradient come from; it's why is there this break in water table slope just at the north end of the Yucca Mountain site. One might have expected that it would have been more even right across the whole thing. You know, in a certain sense, it's not that big a deal.

Nevertheless, there has been mentioned a couple of different interpretations, a saturated flow system or an unsaturated or a perched flow system, let's call it, and I just want to make the point that the configuration of the water table beneath the land surface any place in the world is a function of the topography, the recharge patterns and the geology, the hydrostratigraphic units and the structural features.

The topography here steepens to the north. The recharge increases to the north. So even if there was no geology at all, one would expect the water table slope to increase to the north in this area. And over here, I've tried to show you what these numbers look like. The topographic gradients change by an order of six times kind of thing, you know, as you move up these hills. The mean annual precipitation changes tremendously. The calibrated regional model suggests that the recharge should increase by maybe a factor of ten or something as you go up there. So there's lots of reasons for the water table slope to become steeper, but perhaps not this much steeper, and I think perhaps you do need to have some geological control on this.

Bill Dudley presented to us in the elicitation a tremendous summary of all the possible geologic arrangements that could lead to this. But just for the purposes of a meeting like this where not everybody is a hydrogeologist, I think they can be sort of simplified down to about three, and that's the three I've shown here. Some kind of a near vertical low permeability structural feature, and that was mentioned today, John Czarnecki made the point that he would have to introduce such a thing into his model in order to calibrate the heads.

Another possibility is a near horizontal low K feature, that should be stratigraphic feature. And, in fact, we have such a feature at the site right at the right location, and it's the Calico Hills formation. I'll keep this diagram out.

But as you can see in this somewhat simplified again version of the geology, the water table comes across just kind of under the Calico Hills, and then at some point, it cuts up across the Calico Hills. That makes sense, because the Calico Hills is a low K horizontal aquitard, and we'd expect the water table to be steeper as it cuts across there, and then maybe flatten out again when it gets up to the top.

So to my mind, the one in the middle of this diagram may be the most likely one. One could also get this by some kind of a deep drain, for instance if the carbonate aquifer in some sense pinches out there or there's some portion of it pinches out or something, that could create a water table rise up above. The water table can kind of feel what's happening down below.

But I think the more likely of these is the one in the middle with the Calico Hills acting like this. And the point I want to make is what is the difference between a fully saturated system and a perched system? Well, if this system, the way I've drawn it here, I would imply it's fully saturated and the water table goes up across there like that.

The other possibility is that this water table up at the top here kind of comes right out to the edge, creates little springs there, and this water table then would come up to here like this, and these would then join up across this way. It would be kind of an upside down water table, if you want, and in the middle here, would be an unsaturated wedge of rock with a saturated zone up here, and the fully saturated zone down here.

The question I would ask is what are the moisture contents likely to be in this unsaturated wedge that lays between these two saturated portions? I believe that it would be very highly saturated, almost saturated, in fact.

So my argument is that the difference between the two models, if you want to call them that, is not all that great. In one case, you have a water table that goes up like this, and the pores are 100 per cent filled with water, and in the other case, you have an unsaturated wedge and the pores are 95 per cent filled with water.

You can ask me questions about that if that's not clear.

So in my mind, the implications for the total system performance analysis are similar for both of the systems. I can't see answering the question as having a huge implication for the millirems per year that come out down in the Amargosa Desert.

Whether perched or saturated, the large hydraulic gradient is simply a manifestation of the local flow system controlled by the interactions between the topography, geology and recharge. It's not a cause for alarm, in my opinion.

The likelihood of a break-through of water, dammed in some sense behind the LHG, to my mind, is zero. It's just not the way hydrology works. And I really can't think of any long-term transient readjustment of gradients that is likely to occur, even under some kind of tectonic influence. There's just a lot of controls on that keeping it up there. I just don't see it as a big deal.

In my opinion, the impacts on total system performance assessment would be much greater from the repository heating on the unsaturated and saturated hydrologic regime, or even on increased future pumping down in the Amargosa Desert, which will introduce transients into the flow system at the downstream end that could have quite an influence on travel times and things. To me, those two issues are much larger in scope.

Okay, to move on to the third issue, the third issue was the dilution issue. And of course the dilution issue requires that we first think a little bit about the flow net, exactly what it looks like. The models have provided us with a pretty good idea of what it looks like, but you can get some ideas of what it looks like just by thinking about it on the back of an envelope and scoping calculations, and so on. And for that reason, I wanted to put up this little diagram. This is again of that same sort of location, again, a very simplified version of the geology showing the upper volcanic aquifer, the upper volcanic confining unit, that's the Calico Hills, the water table going across it, and in this case, I've favored my own interpretation, which was a bit of a minority interpretation on the panel, that it's probably fully saturated. If it turns out to be perched, I won't be embarrassed. I gave it 30 per cent or something chance of being perched. I can live with that.

The point I wanted to make here is this one measurement of hydraulic head down in the carbonate aquifer at UE-15p, Number 1, is made right down here, and it's around 750 meters, I don't remember the exact number, but that one hydraulic head point constrains the flow net tremendously for the whole system. One measurement is worth a lot. There's a lot of talk about we need a lot more of these measurements. That's probably true. But that one measurement, if it has a 750 hydraulic head, there has to be an equipotential that goes from that point up to where the water table has an elevation of 750, and the only way you can draw that is somehow like that.

It's going to be more or less vertical across the aquifer and much more sloping across the aquitard that underlies it, the lower volcanic confining unit, and right away, we see then if this is 750, 740, 760, whatever, there's upward flow from the carbonate aquifer. It shouldn't come as any surprise to anybody. That one measurement proves it beyond a shadow of a doubt. If that number is right, there's a lot of upper welling from the carbonate aquifer. So that definitely is part of the truth of the system here.

So in terms of the flow path off the site, we then go to the models I guess to try and integrate all our knowledge of this whole thing. We carry out this flow tube analysis, first of all saying, well, I think the steady state approach makes reasonable sense at this site. There's no evidence of any major transient sort of occurring as we speak. There's certainly some thoughts about climatic transients over a long term time frame, and we'll talk about that in a minute. We have the upward flow from the carbonate aquifers.

In any case, what the model shows is some kind of a regional flow tube that goes off to the southeast from the site in the volcanics, kind of turns the corner, may thin up a little bit, may spread out a little bit, may thin up again, but like most plumes that all of us hydrogeologists work with out in the Superfund Program, solvent plumes and plumes that we see at other sites in the world, we anticipate this one to be a long skinny snake. We don't think of it as something that's going to spread out wildly. Lateral dispersion, so-called, is likely to be small.

In my opinion, dispersion is simply a fudge factor for all the things that might possibly make a flow tube spread out. It's not a scientific construct of any great sophistication. This is from the work done by Schwartz and Sudicky for EPRI actually, but I'm sure that the USGS models would show similar things.

Here was one that was done for the Nuclear Regulatory Commission. It shows spreading out and then a focusing of flow lines here. So the whole idea of whether the flow lines are going to definitely get wider and wider apart, myself, I feel they could get wider apart in some places and come back together in other places, in other words, focusing and de-focusing, both are possible.

The general feeling is that hydraulically, we would anticipate the flow tubes that come out of Yucca Mountain to more or less keep their flow tube pretty much the way it is.

Now, the one thing that could cause them to spread around in my mind would be long-term transients due to climatic changes. If the climate is changes in such a way that the water table distribution under Yucca Mountain is changing in its shape, then obviously the flow tubes are going to be changing with time over this very long time frame. Now, maybe that's what accounts for the geochemical measurements that seem to indicate mixing down in the

Amargosa Desert, because otherwise, there's a bit of a disconnect there between the hydraulics experts where I kind of number myself, and the geochemical experts who see mixing as somehow--you know, they see the geochemical signatures suggesting quite a bit of mixing. And we in hydraulics don't really have an explanation for that, and we don't observe it in the short time frame plumes that we work with in the Superfund Program.

There is some agreement between the geochemists and the hydraulics people. This is the age dates. This is taken out of our elicitation and prepared by Don Langmuir. And if we start to look at the ages and the advective flow rates and so on that we might expect, it seems like the geochemists are telling us that the water is, you know, thousands of years old, thousands of meters away from the site, which would suggest advective flow rates of a meter a year, or something like that. And those are the kinds of numbers that we come up with hydraulically as well. So I think there's some gross type of agreement there.

I'll let you have a look at this slide. I won't go through it. But the gist of it is that in my opinion, the rates of flow of unretarded contaminants are likely to be on the order of a few meters per year overall. Keep in mind I picture them going faster and slower and faster and slower as they go through these various, you know, each flow tube hitting the various fault zones and Bullfrog and into the alluvium and getting hooked around in the alluvium, and so on. I was one of the ones that estimated I think between 0.3 and 30 meters per year for the unretarded advective flow.

Now, I'm not going to speak much about retardation today, because it's not my area of expertise particularly. I don't know whether Lynn is going to say much about that or not. Maybe I'll say just a bit more than I was going to.

I guess it's my opinion that the retardation is likely to be quite important. I mean, radionuclides are not mobile elements in general. They generally sorb quite a bit. There's generally a fair bit of matrix diffusion. We heard this morning of some of the geochemical controls that might enhance some of those retardations. But I guess, again, just an overall gut feeling is that these contaminants will be retarded significantly, maybe by a factor of ten or more, I don't know. That would probably take them out past the 10,000 years, if they're not out there even before that.

There have been other advective flux estimates by other people and they're kind of all in the same ballpark, so I guess I think we're sort of starting to zero in on that number as being something we could live with. Here's what the panel came up with. That's the one that you already showed, Kevin.

So coming back to the dilution then, to kind of summarize it, if I define it as the contaminant concentration at the release point, divided by the highest concentration that's obtained at some potential receptor location, if I assume the sources are sort of, you know, quite large, that we have a steady non-decaying source, the receptor is about 25 kilometers away, and we think about concentrations measures thousands of years after the release, then the mechanisms that are going to potentially control that dilution or lack thereof are the focusing of the flow tubes, which will make the concentrations higher, the de-focusing and splaying out of them, which will make them lower, the possible transient flow tube wandering under climatic changes, which I think is certainly possible, longitudinal dispersion which seems to be--there is some kind of longitudinal things going on, perhaps matrix diffusion and other things going on, the fudge factor, not much lateral dispersion, in my opinion, not much mixing of the type that seems to be underlying some of the geochemical models, I don't see the waters coming in and somehow glomming together and mixing.

We hydraulic people, we think the flow tubes come along, if one comes from above, it will push the one below it down a little farther, and off they go. Pumping well capture could create quite a bit of mixing. Each pumping well creates a capture zone and sort of brings water into itself and mixes it in the well. I can't imagine you get any credit for that, but that will happen, you know, and it could complicate the flow systems down in that area.

The panel, I don't think Kevin did show this one. This was what the panel came up with for dilution factors. We hedged a lot because we don't know too much about this, and some people I think, including myself, even had an upper bound way up as high as like 100 full dilution. But do notice that almost all the most likely values are, you know, sort of less than ten, and one is certainly in the ballpark. There might not be much dilution at all.

I guess I'm just about out of time. I've got just a couple more here.

Changed conditions with respect to climate, I think maybe I'll just let people read these. My feeling was that the program has a good handle on the climate changes, at least as good as could be expected.

The impact of repository heating, I'm probably influenced by having been a former member of the thermal hydrologic peer review team. I learned a lot that few months, and I became concerned that no all the implications of the thermal hydrologic saturated and unsaturated system was understood to the degree that it made me feel comfortable, and I felt there was still some possibilities that these temperate effects could extend in the saturated zone in the formation of convection cells or changes in properties.

Bruce Robinson this morning said that their initial conclusions are that it's not likely to impact the saturated zone unless there are changes in properties. So maybe we're reducing one of the uncertainties there.

And that leads to a summary of conclusions, which I think I've pretty much said. I guess in a nutshell, I think faults are very important. They're not fully understood yet. I think more work needs to be done to make sure we understand them. From what I learned yesterday, it seems like a lot of that work is ongoing.

The large hydraulic gradient in my opinion is not hugely important. I certainly wouldn't be adverse to spending some more money on it just to get the credibility of saying we can understand it. But I don't think it's something that will affect performance assessment, and I don't think the dilution is likely to be very large.

The implications for the Yucca Mountain program then, for the field program and the computer modeling, I think follow from what I've been saying. And I'll just leave it at that and let you have a look at those.

And I don't think I'll comment on the elicitation process. I think Lynn is going to say something about that, and I can jump in if I want to in his discussion.

Thank you.

PARIZEK: Thank you, Allan. Questions from the Board? Debra Knopman?

KNOPMAN: Knopman, Board.

Allan, in your first conclusion, you basically say that the Yucca Mountain flow system is not outside the bounds of hydrologic analysis as it's currently practiced. It is a tough site, but there are others that are as difficult. But can you give some summary view, your view, of what the bottom--what constitutes irreducible uncertainty in this saturated zone flow system? That is, how low can we go and how far can we go in reducing uncertainty beyond which really no one, you know, there's no site that--

FREEZE: I don't know if I have the right answer for you. I believe there's always a level of irreducible uncertainty at all sites. You go through kind of an iterative site characterization program, and I think that's being done here to some degree, and more so in recent years than in earlier years where you measure things in the field, you model them, you accept that every time you model them is not the absolute final answer, but it's the best you can do now. You carry out some kind of a design for your repository, you look to see what the implications of that are in performance assessment, you try to decide whether additional data would have any worth, you know, you carry out some kind of a data worth system as to whether it's worth doing more modeling or getting more data.

And I don't know that you ever reach an irreducible level. I guess I think that's a political concept. I think the--or techno-political, or whatever you want to call it, that at most sites, the decision to note take any more data and to actually make the decision to go ahead and build the damn thing, or whatever, happens in the public domain by groups like this. You know, it doesn't happen, you don't ever say, well, we're going to be satisfied with a delta phi of 2 per cent, or something like that, you know.

KNOPMAN: Okay. I guess that's what I was trying to get you to say, was that you would really not be able to get much beyond some estimates that were bound maybe within two orders of magnitude.

FREEZE: Well, I don't think I'd be willing to put numbers on it. I mean, you're coming at this thing from both sides, and the program at one time or another has flip-flopped in the way it comes. On one side, you come up from the bottom trying to understand all the processes, and you're nervous if there's processes you don't understand. We're nervous if there's processes we don't understand as the technical people. And that might be matrix fracture interactions in the unsaturated zone. I think we all have a bit of a feeling that we maybe don't quite fully understand that as well as we should, you know. Maybe matrix diffusion in the saturated zone; is that important or not. So there's that way.

But then coming down the other way, there's the performance assessment approach where they're doing these things and they're saying well, it's not sensitive to this, so why should we waste our time, you know, this is not a scientific project, it's an engineering project. We're trying to put a repository in the ground.

So somewhere they meet in the middle at I think at a political level, where both sides aren't hollering too loud, you know, something along those lines.

PARIZEK: Paul Craig?

CRAIG: Craig, Board. I have two questions.

Linda Lehman mentioned temperature, and I'd like you to offer an opinion. It hasn't appeared anyplace else in the conversations I've heard. What significance should we attach to that, is the first question?

And the second question has to do with focusing effects or jetting effects. Of course an average is wonderful, but what will really hurt you is if there is an argument that there is some significant amount of material that gets focused on just to the place where someone wants to put in a well. Now, I think you used the language highly unlikely when you were talking about this kind of thing. I may have misunderstood. But in any event, what I'd like you to do is to address the technical issue as to how you go about constraining that kind of a phenomenon. What kind of data can you collect? What kind of models can you use that will help you understand and put limits on it?

FREEZE: Okay, to the temperature question first, I guess my feeling, and I think all of us would share this on the panel, that any data that's available that's pertinent to understanding the processes and ultimately the directions of flow is valuable, and the temperature information is one of those sets of data, and I think you're right, it probably hasn't been fully integrated into the whole hydraulic scheme. But there certainly have been attempts to do that.

We had some very interesting presentations by Bill Dudley and Dr. Sass and John Bredehoeft has worked with some of that stuff. And, you know, we've read all this, and my feeling is it's helpful and useful in helping us understand the nature of the flow systems. If we can show that there's heat plumes, they're in a sense a tracer, you know, and that would tell us a fair bit. And I think they should be, and probably will be used by the modelers in calibrating their model.

The focusing of flow, what you're asking, if I understand it correctly, is how do you avoid, I don't know, spending too much time worrying about the absolute worst case where somebody suggests a fracture that goes all the way to the well, you know, with a two year travel time, or something.

CRAIG: Yeah, how do we decide whether this is something we should worry about or not worry about in a situation where the mean times with retardation included may be sufficiently low effect or--

FREEZE: Well, I guess that's the first step. The first step would be that if even that worst case doesn't lead to anything dangerous, then maybe you don't have to worry quite so much about it.

CRAIG: Well, the worst case, it seems to me, though, we heard this morning and you seem to be confirming it, leads to a pretty good situation.

FREEZE: Well, first of all, don't quote me on that. I have never looked at the millirems per year, you know, calculations in enough detail for me to comment whether those are safe or not, and I'm also not a toxicological expert.

CRAIG: Well, let me--

FREEZE: So back off from that one.

CRAIG: Let me stipulate that these fast paths lead to long enough time lags and with retardation, it could be a significant element. So then we say if that's true, then what are the real flaws that you have to worry about? And it seems to me that the one flat which you've absolutely got to deal with is the possibility of some kind of a focusing effect that produces a highly vulnerable outlayer. If you can't limit that reasonably well, then you're in trouble with this.

FREEZE: I suppose one way to do it would be to try and look probabilistically at, you know, what you're saying is there would have to be a link-up of a bunch of highly permeable sets of conditions, you know, the high permeability--the release would have to come in the high permeability fault. It would have to head right towards a very high K flow path in the alluvium or something like that. I suppose one could look at that. There are geostatistical techniques, you know, to look at the natural occurrences of these things, and the probabilities of all that sort of thing, or you'd be multiplying probabilities, and perhaps you'd be able to show that the probability is very, very low. But I don't know, you would never be able to show its zero, I suppose.

CRAIG: It won't be zero.

FREEZE: Yeah. I don't have a direct answer, but I would think there ought to be some methods, when we know the offsets on these faults and we know how continuous these aquifers are and how likely it is that they get bumped up against something of high K or something of low K and how long they are, you know, I would think there should be something. And I presume the models would have a significant role to play in producing that kind of thing for you.

CRAIG: Well, if somebody around the room has an approach on this one, please come and talk.

CHRISTENSEN: Christensen, Board.

It strikes me that that's the historic trade-off and risk between concentrated risk and the probability of hitting the hot spot, that is, the more focused it gets, the probability of actually hitting it goes down, but the consequences when you do are much greater. And I think that the ultimate importance of that probably comes down to having greater guidance in terms of what the actual standards are going to be, how we deal with those standards.

CRAIG: Yeah, but through the statistics, too, we have estimates for where the faults are of some sort. So it seems amenable to the statistics.

FREEZE: It seems to be there's a bit of a catch in that one, though, and that is that if Amargosa Valley turns into Las Vegas, which none of us would have predicted 100 years ago, and there's heavy utilization of the water from the alluvion aquifers in the valley, that will create capture zones that will suck this water. I don't think it will change the travel times a whole lot, but it will change the receptor locations. It will mean that the chances of a well going into a hot spot, if such a hot spot were to be there, would be almost one, probably.

CHRISTENSEN: Since you raised that question, let me ask a question that I've been wondering about in this process that doesn't get mentioned, and it may simply be a different way of restating the climate change scenario, and that is imagining down slope from the area, which is a fairly likely scenario in any population growth situation, and whether that is equivalent to a climate change that would increase the slope of the gradient.

FREEZE: Yes, I believe it is. In fact, it might even be more effective in providing the mixing mechanism that I guess we would all like. But as I say, it's kind of twinned. It mixes it, but then it sucks it in. It doesn't mix it and let it get away.

PARIZEK: Chairman Cohon?

COHON: Cohon, Board.

Allan, I'd like to ask you a question or two about this picture with the three conceptual models of the large hydraulic gradient. Let's stipulate that a large hydraulic gradient doesn't matter.

FREEZE: Okay.

COHON: I mean, I'll stipulate in the sense that we'll accept your feeling that it doesn't.

FREEZE: I was going to say, yeah.

COHON: Well, I'll keep talking while you're looking.

FREEZE: You keep talking and I'll try and listen while I find that.

COHON: The top one, the top model is the near vertical low K structural feature, and that's basically the way it's currently represented in the models that--

FREEZE: Well, I think it's been introduced by some of the modelers almost against their own will.

COHON: I understand. I'm not saying they have adopted that model.

FREEZE: Okay.

COHON: That conceptual model. But to make the model work, so to speak, that's what they've done.

FREEZE: Okay.

COHON: I mean, that's a fair characterization of what's in the model.

FREEZE: I wouldn't--is that true? I think so. Bruce?

COHON: Okay. Now, suppose, however, that the one you favor is in fact correct, that is, the middle one?

FREEZE: Well, it's a very simplified picture of a complicated--go ahead.

COHON: No, I understand that. And the question is we're trying to get the benefit of your insight into what implications having the top representation might have for the model, when in fact reality is the middle one.

FREEZE: That's a good question. Probably the calibrated model would look pretty much the same, perhaps.

COHON: Okay, good.

FREEZE: Because there's very few layers in the model. If there were more layers, if we had this represented by eight or ten layers and there was a layer in the middle and it was there with three layers and we did it backwards, you know, but I'd just like to say that the question about whether--I said I didn't think this mattered a whole lot. But the point was made this morning that if these gradients are different up here than these ones down here, then it's going to be a different flux down here and this could lead to be important. That's the counter-argument, and maybe John is going to make it right now, I don't know.

CZARNECKI: This is John Czarnecki from the USGS.

The model that I presented this morning started out as the middle diagram, which we could not represent without introducing yet another feature, such as the vertical barrier on the top.

I should also point out that the bottom model was also part of the current model, in that we've got the carbonate as a very high permeability zone, and just using those units alone was insufficient.

I would add a fourth model, and it would take the shape of the water table that he has on the bottom, only along the curved portion, you would have a lower permeability zone extending up gradient as a result of thermal alteration. And that's a model that we call a spillway model, if you can use that term. And then I think it makes some sense based on the thermal regime.

FREEZE: There is an overhead in the--that I didn't put up I think this morning that sort of summarizes Bill Dudley's many, many suggestions and tries to group them in these three groups, and he judges them from a much better position than I can, much more expertise, as viable or unlikely or, you know, uses words like that for them.

I mean, I don't disagree with John. John and I talked about this lots of times and, you know, as I said, when we were asked in the elicitation to put some numbers on this, I said 70-30, or something like that. We don't know.

But the point I guess I did want to make is that I don't think the absolute, you know, this goes back to this irreducible uncertainty, I think we should understand before this goes to licensing, I think DOE would be well advised to have some understanding that's better than the current understanding of this feature, just from a credibility point of view, and from a science point of view. But my personal feeling is it won't impact the TSPA particularly.

COHON: The other question I'd like to ask you is about this combined probability distribution. You put it up, and it's the same one that Kevin put up.

FREEZE: I just took it from Kevin's report. I didn't have anything to do with producing those.

COHON: No, I know. But I'm just trying to identify it by saying you put it up as well as Kevin.

FREEZE: Yes.

COHON: It's for Q, the volcanic Q, and your individual probability distribution is very different from the others.

FREEZE: I noticed that.

COHON: And from the combined. So that's why I wanted to ask you in particular. And this is a rough estimate from eyeballing it, but it looks like the expected value of the combined distribution is a value that you wouldn't even find credible. That is, it seems to be almost off your distribution, that is, the expected value of the combined looks like about .5, and the upper bound of your distribution is about .1. So the question is how do you feel about that?

FREEZE: To be quite frank, I didn't give it any thought particularly. I anticipated that we understood how the system was going to work. We were all going to give our opinions. I honestly expected there to be much wider divergence of opinions than turned out on all those diagrams. And, in fact, the fact that there isn't worries me more than the question you've just asked me, because it makes me worry, we were warned that we might be, like all experts, in love with ourselves and think that we knew more than we did and that our distributions would all be too tight.

My own fear was the opposite. My fear was that we were all going to try and cover our ass to such a degree that we'd never be embarrassed by these things in the future, that we tail them way off out to the ends. And so I purposely tried to kind of not tail mine quite so much as I might otherwise do. And that's maybe why mine tend to be a little tighter than some of the other guys perhaps.

COHON: So you feel that for something like Q, this particular parameter, that this kind of--

FREEZE: Yeah, they seem pretty reasonable to me. I mean, I'm obviously a little lower. Kevin kept trying to get us to let the other people inform us. So I maybe should have moved mine over a little bit, having learned that four people whose opinions I respect differed by an order of magnitude from me, I should have moved over a quarter of an order of magnitude because I'm a good Bayesian; right?

PARIZEK: Other questions from the Board? Staff? Questions from the Staff?

(No response.)

PARIZEK: If not, I thank you very much, Allan. I guess probably the jury is still out as to whether it's first of all good wine or a bad wine. We have good wine and bad wine opinions according to how it drives the program, but thanks very much.

Our next expert is Lynn Gelhar from the Massachusetts Institute of Technology, who spent time is New Mexico and New Mexico Tech, and also previously before that at MIT, and was one of the five experts and he will give us another look at the review process that he was subjected to.

GELHAR: Thank you. It's good to be here today, and I'm going to provide a sort of contrast with the discussion that Al Freeze gave us, in that I'm not going to get into a lot of technical detail. I will look at a few technical points toward the end of my discussion, but I'm going to focus a little bit more on what I call possible weaknesses or limitations of this kind of process, and maybe some suggestions about how things might be improved to get a better overall product out of this kind of exercise.

So the topics that I will be discussing are some concerns about the elicitation process itself, first of all, and then just a summary of some of the key technical issues, especially ones that I focused on in the elicitation process, because each of the experts sort of concentrated and made presentations on selected portions of the information, and I'm going to give you a summary of some of that. But I believe the Board will have available the full report of the group, and there are 25 or 30 pages just presenting my summary. I'm not going to try to cover all the technical points in that summary. But then I will finish up with a few suggestions about reducing uncertainties.

So let me say a little bit about the process first of all, and the framework that's involved here is the use of probabilities. And of course we all know that there are different perceptions about what probabilities actually are, or there is a kind of subjective probability which is more the concept that I believe is being used in this elicitation process, probabilities as reflecting degree of belief. This is the Bayesian concept of probability.

And a second more classical kind of view of probability, which views this as a quantitative representation of repeated experiments, or kind of inference based on repeated observations, and that's the classical kind of frequency view of what's involved.

The overall process that evolved here I think is a kind of mixture of these two. But I tend to be in the classical camp, if you'd like. In other words, I'm interested in there being a base of information of data that we can sample in a repeated sense to try to get some information about statistics. I recognize that in some cases, degree of belief has a place, but I tend to, if I had to pick a camp, I'd be in the frequentist camp.

And of course the kind of concern that many have expressed already is one that I also am concerned about, that this kind of degree of belief, subjective probability, should not become a substitute for actual collection of data and being able to look at repeated experiments. They might not be experiments directly at the site. It might be using closely related information that we can apply to this kind of situation.

Now, another concern that I had was the relationship of the elicitation process to performance assessment. I felt that since the product that we're producing, these probability distributions are primarily to be used in performance assessment, that I would have been more comfortable with a process that more directly told us about how the performance assessment was being done and how this material really fed into that process. I mean, for example, one concern that I have is something I call here probabilistic dilution. I've made a little picture to give you the sort of idea that I'm thinking about here. And I believe some of these ideas were coming up in some of the earlier questions.

But let's talk about effective porosity, because effective porosity is one of those parameters that many feel is important as it has to do with how long it may take for a contaminant to move through this aquifer system. And what I'm using here is--or what I'm looking at is the probability density function for a kind of effective porosity, or might be a fracture porosity, plotted on a log scale. And just as a hypothetical, I've said we have five experts, and we don't have very much information, so the experts can just, well, make sort of a wild guess or depending on their bias or experience, come up with, in this case, I would say they're non-overlapping, and the averaging process is just dividing each of these by five and putting them together on one graph, and I have a probability distribution. It's kind of a log uniform probability distribution for effective porosity.

And, in fact, these aren't--the values that come here are not out of the realm of realities that have been discussed or have been possibly used, because what we heard this morning is from the C-wells tracer tests, we're seeing values that apparently are up in the range of ten to the minus one, maybe even larger than that, experience with fracture porosities. In some volcanic materials, the Basalt flow tops at Hanford have produced ten to the minus fifth for effective porosities.

The value that was used in the LBL simulations, I'm not sure where they came up with this, but this S-4 kind of model used a value of ten to the minus four. So there is a potentially wide range just from overall experience of what effective porosities might be.

And suppose that we had a true distribution. If we did a large number of tests out at the site in the volcanic system, we would come up with a distribution that looks something like this.

The question I would have then is having this wide distribution of possible values, is this representing in a meaningful way the influence of uncertainty? Or does it under estimate the possibly important influence of a narrower distribution that has a much more severe consequence, and how is this kind of difference really accounted for in the process that one goes through in this situation?

So to me, it's sort of a question in my mind. I'm a little bit uneasy about just spreading out over a wide range of possibilities and what in effect dilutes what might be an adverse consequence.

And along the same lines, I think--I mean, this is another reasons why I'm more interested in the link to performance assessment. Because performance assessment is just a kind of transformation of these probability distributions into another space. It's a non-linear transformation, and because it's non-linear, just because this distribution looks like this, the distribution that comes out for, say, concentration at a downstream point of impact is not necessarily going to look at all like the distribution you started with. And parts of this distribution may be much more important than other parts of the distributions in the case of a non-linear transformation, which is what PA is.

So that's supposed to be a little explanation of what I mean by probabilistic dilution. Is there something like that going on in this kind of process?

So I felt that the performance assessment, or our process of elicitation would have been more enhanced if we had a more direct focus on performance assessment, including maybe some sensitivity kind of information on overall performance assessment, or just the notion of a more direct link between performance assessment and site characterization so that we can be using performance assessment as a way to set priorities about site characterization, and also have a more clearly documented connection between the parameters and conceptualizations that are needed in performance assessment, and the sort of observable characteristics that we have in a field data collection program.

I feel that there's a kind of gap in this overall process between what kind of data are collected and how this produces performance assessment parameters that if you're going to have a more defensible process, you want to be able to close that kind of gap.

So that's the point here regarding the link between site characterization and performance assessment.

Now, I also have some concerns about just the nature and availability of information, possibly because I've taken maybe a little bit different perspective about how I looked at the information that is presented, because we were given an awful lot of information, and I said some of it--well, I think a fair amount of it was ill-focused, maybe purposely ill-focused, and we were intended to provide a focus or clarify things in one way, and maybe there's nothing wrong with that, but there is also a focus, a framework, that could have been used more where we could see how the project researchers see their work giving information about performance assessment, or parameters needed in performance assessment. Tell us; here is how you're going to use this to come up with this parameter that you need in performance assessment, and here is the uncertainty that you think applies to this estimate, and use that kind of framework as a starting point for an assessment of the type that we've developed.

One of the reasons that I found this to be an attractive kind of thing is that I went through with a similar kind of process in terms of the modeling and performance assessment, risk assessment on the Nevada Test Site about a year before this process, and that was entirely built around performance assessment, all the way from the site characterization data to the results of the performance assessment were the focus of that kind of process. So looking at the whole picture. And I found that to be a good way to focus in on the things that are important.

Now, I also have, just from long experience with oral presentations and transparency copies and things like this, found that it's difficult to sort out reliable information from presentations of that type. It's very important to have reviewable material, as far as I'm concerned, and I think we all appreciate that. To what extent we put that as a constraint on how we look at this information may vary. I put that as a fairly severe constraint. So if I didn't have written information that I could review, if all I had was some transparencies or presentation, I did not place as much weight on that kind of information. I just--I'm not as comfortable not being able to see the pertinent details about how these things develop.

There is also a question of timeliness and organization of an effort like this, because in several cases, I did not feel that the information got to the panel in a timely fashion. They're too late in the process to really, in a comfortable way, be able to assimilate the information in a way that the panel could interact about the nature of the information. I mean, the C-Well test is one thing I've complained about before, and complain about again, because we got the C-Well tracer tests and hydraulic result just a few days before we had to put together our kind of group, preliminary group assessment.

So it seems to me if you're going to go through what is a fairly time consuming and costly process, there should be the courtesy and the like of providing information before you have workshop meetings, say a couple weeks before workshop meetings. We know what the agenda is and we get the information before it's presented so one can effectively discuss and question and the like. And I don't see any reason why those kinds of things could not have been done in this kind of setting.

So those are sort of the complaints for Kevin and others which are probably--there are probably many good reasons, scheduling and the like that produced this kind of situation.

Then I wanted to talk in a summary sense about a few of the technical issues that are involved, and maybe give some contrasting perspectives with Al Freeze' comments. And what I will do is just give these summary notions about a few of the components of the overall project, starting with the regional flow model, which we heard some update on this morning. And the regional flow model I am skeptical about even if you do the best that you can to reduce uncertainties in the regional flow model, that it can be expected to provide you tight information about what's going on at the site.

There's so many unknowns in the regional flow model, the amount of recharge, the spatial distribution of permeabilities, that it's so far from being able to specify very much detail from a model like that. Its role is for a bigger picture kind of thing, and its role is to address things like the rough idea of what climate change effects could be, and can we represent those in a reasonable way. Linking it directly to a smaller scale model, I think is not as likely to be feasible as what I'm talking about here in the site scale model where the--my concerns in the site scale model are getting more faithfully into the site scale model and representation of the major geologic features. And I believe that is moving in that direction, but with the discretization, a kilometer and a half discretization that has been used I think there are lots of questions about whether you're really getting the important geology into the system.

I would also be concerned just about the geologic information, how far down gradient do we go before we actually get into the alluvium, and do we know decently where the water table goes from the volcanics into the alluvium, and I haven't seen a clear demonstration that that's very well known.

So I would not see it as a very likely prospect of really being able to reduce uncertainty about local site conditions by linking to the regional model. The way I would look at it, rather, is that we use things like the aquifer tests, which fortunately are stressing and influencing quite a large area, out to several kilometers, as the way to impose a flux on this model in the areas that we're really concerned about, the areas down gradient from the site, to understand the nature of the flow and the properties in those regions.

So that's the point here. You should use flux imposed by the long-term aquifer testing in the calibration of that model.

The advective flux vector, that's the Q that we're looking at the probability distributions of, and I believe the reason that--or I think those probability distributions are actually quite tight. I mean, Al is almost in agreement as far as uncertainties in ground-water conditions are concerned, and I believe the reason that it's tight is that we have a tight measurement on hydraulic gradient. We know what the hydraulic gradient is, and we have a decent aquifer test in the pertinent area just down gradient of the site.

So where the information is coming from, there isn't much to debate. A number of aquifer tests have been done, and the numbers are in a range that's reproducible. So this characteristic of a specific discharge of a half a meter per year I think is quite well established.

Now, just to present something that is not well established, however, is how you convert that into a velocity, because that goes to these effective porosities and things like that, and I just picked out, I'm not advocating this number at all, ten to the minus three, but if you divide that into the Q, you've got extreme rate of movement, and I don't know how well we can, you know, acknowledging that there is geochemistry and so forth, I still don't know how well we can determine what the velocities of the water is in this. They're kind of an average velocity in this kind of system.

There are a large number of single hole hydraulic tests, and some of the most important kind of information, as far as I'm concerned, is that that comes from the old bore hole flowmeter logging tests. You just put a little propeller--you pump your well and put a propeller down in there and you see where you get water coming into the hole as you extract water from the hole, and Al showed I think one of these spots, but you will find pictures--well, here's an example from a--that's Bill Arnold's presentation. The patched zones here show the zones whereby the flow metering, the in-flow was predominantly occurring in each of these bore holes. And in my mind, this brings in what I might want to see in this kind of thing, is some kind of continuity in these pictures, and it's unclear whether the attractive idea that lithology determines the degree of fracturing and the distribution of permeability in these systems is really a valid idea.

I have not seen the information put together in a way that demonstrates that each of these in-flow zones that has been measured here is related in a simple way to lithology so you can recognize where the permeable zones would be just by knowing, say, a welded tuft. And that seems to me to be a very important question.

Now, maybe someone knows if it's resolved. But as far as information that we got, it was not clear. So that's the basis for my suggestion here that it may not be clear that the high permeability zones within the strata are easily related to lithology. And these kinds of questions are important because ultimately, you're interested in sorptive characteristics on the pathways where the water is moving, and if we don't know what those pathways are, and it seems that the strategy of characterizing sorption is to relate sorption characteristics to lithology, and if we don't have this relationship between flow and lithology, then we don't really have I say a workable strategy of characterizing sorption characteristics.

The single hole hydraulic testing, more traditional hydraulic testing, provides only relative information. It seems to run one or two orders of magnitude lower in hydraulic conductivity than something like the aquifer test at the C-Wells. Well, it's not clear what's producing this kind of difference.

The aquifer test, the C-Well work is very important work, as far as I'm concerned. And the fact that we're seeing responses out to several kilometers from pumping these wells, where we know the flux and we can see it pretty much follows an equation, means that we have a fairly transmissive system here. You know, the transmissivity is in thousands of meter squared per day, and that is consistent with the low gradient.

The advantage of having this kind of aquifer test is that we have a known flux so we can eliminate the question, and this makes less important the question of what's happening upstream, because we use the measured conductivity and the measured gradient and we know how much water is going through that part of the aquifer.

Where exactly it comes from, I'm not so sure how important that is, but we know how much water is moving through that part of the aquifer, or that's the way I am interpreting this information.

Now, the tracer tests, I think it's still an open question about what these mean. I would be more comfortable if we were seeing more dramatic differences. Say with regard to matrix diffusion, the differences are really quite small, and I'd want to make sure that we understand what could be other complications in these tests that might be producing these differences.

The fact that you have two peaks and apparently two pathways means that the kind of classical interpretation that has been used is not necessarily an adequate interpretation, because as soon as you have two different pathways with different velocities, then there's a question of transverse diffusion between the different pathways that is not really represented in these kinds of models.

So there are questions like that, and I'm still not sure what these large effective porosities really mean. I mean, what you're doing in this tracer test testing first of all a very small part of the overall system that you're interested in. The wells are, what you're testing is a portion of the aquifer between these wells that are 30 or so meters apart, and it's not clear to me whether some complications in the flow field, some three dimensionality or anisotropy or something in which the flow field has a more complicated pathway than a flow field, which is assumed in this kind of analysis, is what's producing the larger effective porosity, or what it is. It's important to understand what the limitations might be in the interpretation of this.

I mean, it would be a gross speculation to try to extrapolate from the 30 meter spacing tracer tests to what's going to happen over many kilometers. So there's an important difference between the tracer tests. The tracer test samples adjust this interval between the wells. The aquifer test influences a large area in kilometers and produces a flow in that area, and produces information on a scale that's closer to the scale and is more integrated, because the difficulty of a complex system like this is if you're going to try to say, as Al is maybe suggesting only conceptually, that there is a very detailed and complicated distribution of permeable features in this system, doesn't seem very likely that you're going to be able to map those out in complete detail.

You might know their geometry, but to map out their hydraulic properties would be a very challenging kind of thing to do. What you'd rather be able to do if you can is make integrating measurements. If you can make integrating measurements that give you the properties approaching the scale that you're interested in, I view that that's a more direct attack on the characterization kind of problem.

Well, Victor sort of suggested that I say a little bit about dispersion, dilution questions, so I thought I'd add it. There is a single sheet. If you didn't get that, there's a single sheet in the back that gives this additional information, and I want to give sort of say the introductory lecture about dispersion and what dispersion is in a field setting. And it really builds on the concepts and descriptions that Al Freeze was providing, because he's suggested that there is a complex distribution of permeable paths where there are probably variations along the paths, that there isn't a single continuous path of very high conductivity that extends for many kilometers. And it's the interactions at several different scales that produce a kind of dispersive effect.

You can think of first of all the very largest kind of scale, and what I'm trying to do here is actually draw a distinction between spreading and mixing and/or dilution. That's the distinction that we want to be able to recognize. And you could think of having, say, a plume that's in a heterogeneous aquifer that's made up of two layers. It might be an aquitard and an aquifer, and say the conductivity contrast is just a factor of two, if you have a hydraulic gradient applied to this system, the part that's in the aquitard versus the aquifer is going to move twice as fast, and what you'll end up with is a plume that sort of bifurcates. You'll end up like a dumbbell type plume, where the elements of the plume have spread out because there's a variation in hydraulic conductivity, but you don't find significant dilution in this kind of situation.

That is in contrast to having a large plume in a medium that has fine scale variation that is small compared to the size of the plume. In that situation, the plume gets chopped up in a small sense and spreads out at the same time that the concentrations are decreased. And this kind of hierarchy of scales of variation in fluid velocity or hydraulic conductivity are what it takes to produce the kind of dilution effect. And, in fact, what we know from recent theoretical developments is the finest scale of variation in hydraulic conductivity has a very important role in this process of creating dilution, the reason being that in order to produce any dilution in any of these kinds of systems, we ultimately have to get down to molecular diffusion. The only way we can decrease concentrations is ultimately to have molecular diffusion act effectively.

And if you have lots of very fine scale variation in the flow field and in hydraulic conductivity, this produces a situation where you produce all kinds of little fingers, high velocities, low velocities, high velocities, and you greatly increase the surface area and the gradients, concentration gradients over which diffusion can actually act.

So it's this small scale chopping up of plumes that is needed to produce dilution. Otherwise, there would be no dilution at all. The plume would just move around and it wouldn't decrease its concentration. So it's a kind of hierarchy of variations, but the variations on a scale smaller than the plume and including those much smaller than the plume, which we will never be able to characterize completely are what we throw into the sort of dispersion description that's involved.

The dispersion effect we know is related to the variability of hydraulic conductivity and the scale of variability of hydraulic conductivity. And that has been confirmed by independent experiments where we measure this variation in hydraulic conductivity on an aquifer, like at the Borden Site in Canada or the Cape Code Site in Massachusetts, we have measured these kinds of properties and we know that we can predict this kind of characteristic.

So not that such measurements are actually practical in the Yucca Mountain setting, but what I have done in my assessment of the dispersion characteristics is really build on the data from many sites where essentially this dilution process has been measured by primarily looking at the behavior of plumes.

And what is identified on this graph is many different observations, field observations, at scales up to scales of interest, directly of interest in this Yucca Mountain setting. I put on here the value of longitudinal dispersivity that's coming out of the C-Wells, and you can see that that falls within the range that is involved here, and I've also identified with the open symbols here several observations in fractured rock settings where dispersion, longitudinal dispersion coefficients have been identified, and then shown on here the geometric mean, basically the X and plus or minus two sigma values which were the basis for the dispersivities that I assigned in this case. So that's basically how I arrived at dispersivity which ultimately relates to dilution.

Now, the transverse dispersion we know is usually quite small and it's more influenced by variations, unsteady flow characteristics, variations in the direction of the hydraulic gradient, for example. What we know from the Borden site experience, in contrast with the Cape Code experiment, is that the Borden experiment has a much larger fluctuation in the direction of the hydraulic gradient, and we understand hydrologically why that has to do with the discharge point of the ground-water changing seasonally.

Cape Cod has a smaller degree of variation, and a question that I looked at a little bit is to what extent on the Yucca Mountain site do we have, or what one might guess is that you have a relatively steady system in this arid setting, and it's borne out. If you look at numbers, the variation in the direction of the hydraulic gradient is maybe only two degrees, based on the triangle of wells that we can pick out at the site, whereas--well, it's not that much larger on Cape Cod. It's about three degrees for the same kind of thing.

But what we understand is that this influence of plumes sort of wandering around a little bit through a heterogeneous aquifer is what produces this transverse dispersion and mixing. And the other graphs here just show a summary of how I arrived at the ranges for the--this is the vertical transverse dispersivity, which is the most controlling value. And you see what we're dealing with in a geometric mean sense is values less than a centimeter. We're dealing with very small vertical dispersivity.

So overall, I agree that there is not going to be--or provided that the release is practically steady state, there is not going to be very much overall dilution, and you can make calculations of what the dilution would be, depending on the configuration of your source and the magnitude of these dispersivities.

The horizontal dispersivity again is not very large, probably quite a bit smaller than values that are being used or at least that Bruce was showing us this morning in terms of transverse dispersivities in the current analysis.

Now, another factor that I believe you need to think about especially if longitudinal dispersivities are coming into the picture is that they can be different for differently sorbed species. For heterogeneously sorbed species, you get a kind of spreading of contaminants that's due to the fact that the sorption characteristics themselves are heterogeneous, and that should be considered as well.

Well, I'd like to just finish up then by going through the list of points that I had included in my report about reducing--I guess I would say reducing my uncertainties about this. It may be that someone else has some additional information or has a different perspective. This is reducing my uncertainties, and these are sort of priorities that are set. A better way to set priorities would be to build on your PA, on your performance. Since I didn't have that, this is just sort of my biases about what I think is likely to be important.

So large scale hydraulic and tracer tests, and I mean large scale relative to what has been done, because what has been done at the C-Wells is 30 meters apart and I'd like to see tests getting out to a half a kilometer or a kilometer if that's feasible. Then we're starting to integrate many of these complex features, whereas you might be testing just one special characteristic, a small part of the aquifer with the small 30 meter or so test. So 500 to 1000 meter well spacing.

Dipole configuration. The dipole configuration has the advantage over the radial flow configuration that was used, in that you produce a force flow field and you can separate dispersion and retardation effects, and matrix diffusion is just a kind of retardation effect. So you want to be able to try to separate those things out.

Different tracers with more contrast and molecular diffusion coefficient are also desirable kinds of things to be included in something like that. And we're talking about a long-term and costly endeavor that should be very carefully designed, and this is the kind of thing where I think there should be external review of the plans, not looking after the fact at what didn't work, but looking at what could make a difference before this is done.

So this would be--this is a first priority as far as I'm concerned, to try to get at the crucial transport properties.

Now, I think you may be able to get more information out of the single bore hole tests, some kind of reevaluation, up to date, maybe three dimensional simulation, so forth, because they cover such a large part of the site. There's spatial information in those that might be quite valuable.

The improvements in the site scale model I've sort of alluded to already, better refinement and using the aquifer test and calibrations.

Another kind of measurement that may be suitable to try to resolve the matrix diffusion question is looking for evidence of matrix diffusion or the lack thereof in an ambient sense. And the reason I bring this up is that apparently this has been done successfully in looking at some of the perched waters up north of the site, where they have looked at waters extracted from fractures, and then from the matrix, and they see a difference, an order of magnitude higher chlorine concentrations in the matrix than in the fractures.

I don't know whether anything like that has been done at any saturated zone settings, but in portions of the rock where we expect significant flow and it's not clear how much matrix diffusion might be involved, it seems to me this kind of in situ evaluation of matrix diffusion is something that should be attempted. So that's the main point here. I mean, there would be complimentary kind of lab tests, but I think some of those have been done. But I would focus on natural fracture surfaces.

And then I believe you can improve or maybe build confidence in the C-Wells information by looking at alternative interpretations and better documentation of what's involved. I mentioned one, transverse dispersion effects. And, finally, I am not comfortable with the transferability of the lab sorption information into the field. I don't think the case has been made for that as yet, and I'd like to see the case made. That's really what it amounts to.

So I think that's what I have to say. Thank you.

PARIZEK: Thank you, Lynn. Questions from the Board? Alberto Sagηes?

SAGγES: Sagηes. Very quickly, just to clarify this, the units of dispersivity are in meters or--

GELHAR: Correct.

SAGγES: Then it's the property of a system and not property of--not intrinsic property of the medium?

GELHAR: Well, we multiply this length by the velocity to get a diffusion like coefficient. I mean, it's property of the medium, although it is also influenced by the history a little bit.

SAGγES: That's fine.

PARIZEK: Other Board questions? Staff?

(No response.)

PARIZEK: If not, I think we ought to have our break. But we're going to again have to cut this break short in order to allow time for the remaining presenters and still round up at the end. So how about about eight minutes for break? And just bring your coffee back to the table. It's better that than if we truncated the end or stay here till dark.

(Whereupon, a brief recess was taken.)

PARIZEK: We should reassemble. We will see the sun set, I believe, from this room.

Our next presentation will be by Bill Arnold from Sandia National Labs, and he will look at the saturated zone flow and transport analysis in total system performance assessment for Yucca Mountain.

ARNOLD: Okay, thank you. What I'd like to accomplish here is I'd like to give you a little bit of background, some more detail on implications of some of the issues we've been talking about today for performance assessment calculations. I want to give you a very brief summary of how we're performing these calculations for the saturated zone in the TSPA, and also try to link some of this information that's come from the saturated zone expert elicitation to what we're doing in TSPA.

The processes that are important in terms of performance for TSPA, first of all is ground-water advection because this is the process which moves contaminants from beneath the repository in the saturated zone to the accessible environment. diffusion of dissolved radionuclides into the rock matrix, because this is really an issue that or a process that influences how much of this median is available for storage of a contaminant during transport. Mechanical dispersion of dissolved radionuclides because this leads to a reduction in concentration. Geochemical retardation by sorption on mineral grains, which is a retardation process which slows the movement of contaminants, and potentially dilution at a pumping well would reduce the concentration that's actually delivered to the biosphere.

And what I show here is a flow diagram of a portion of the TSPA calculation. All of the upstream components shown here are feeding into unsaturated zone transport model. The link between the unsaturated zone transport model and the saturated zone flow and transport modeling is a radionuclide mass flux history. So this is the coupling upstream from the saturated zone.

Downstream from the saturated zone component of the analysis is a radionuclide concentration history, which is then fed to the biosphere model, which is used to calculate radiological dose. And this radionuclide concentration history would be for several radionuclides. Also, I indicated that climate change history influences other components of the system such as unsaturated zone transport and saturated zone transport. What's being done in the TSPA calculations is we're considering three discrete climate states; present climate, a long-term average climatic conditions roughly corresponding to conditions 21,000 years ago, and some sort of very wet super pluvial condition. And I'll try to indicate how that influences the saturated zone modeling.

Just a figure to show you the area again that we're talking about. The outline of the repository is shown in red. General direction of ground-water flow in the saturated zone is shown by the blue arrows. The 20 kilometer boundary from the edge of the repository is also indicated here. This is being taken as a compliance boundary, the primary compliance boundary for calculation of concentration as passed to the biosphere model.

The way in which these calculations are being performed, we're using flow and transport modeling at two separate scales, sub-site scale flow and transport modeling for transport out to 5 kilometers, and then the site scale model as described by John Czarnecki earlier today for transport out to 20 kilometers. The need for two models here is chiefly driven by lack of geologic and numerical resolution in the currently available calibrated site scale model. So a sub-site scale model was used that incorporates a more detailed geologic framework model immediately down gradient of the repository, and also utilizes a higher resolution grid and the hydrostratigraphic definitions for transport out to 5 kilometers.

The two models are coupled at the 5 kilometer fence through the radionuclide concentration term. So radionuclide concentration as simulated by the sub-site scale model is passed to the site scale model at that location.

We're using an abstraction method here for the TSPA realizations. The full Monte Carlo TSPA runs will use the convolution integral method to approximately radionuclide transport in the saturated zone, and this is a much more numerically efficient method than running the full three dimensional flow and transport models for each realization in the TSPA runs.

Just to show you what the simulations look like, this shows the outline of the sub-site scale model. The blue crosses here are located 5 kilometers out from the repository. This is a plot of simulated concentration at the water table over this area, assuming a unit of mass flux source at the water table beneath the repository. At the bottom is a cross-section taken along this 5 kilometer fence showing the distribution of simulated concentration in the vertical direction. So this is a steady state, or nearly steady state plume after some long time period following initiation of the source beneath the repository. So this is the sub-site scale modeling.

The site scale model picks up transport at the 5 kilometer fence. It's indicated here. And carries it outward to the 20 kilometer fence, 20 kilometer boundary. Again, the outline of the repository is shown in red, and this is--the colors indicate a simulated concentration on the log scale. You can see that the general direction of flow is to the south, southeast from this 5 kilometer fence area. At the bottom here, I also have a cross-section showing simulated concentration along that 20 kilometer boundary. The highest concentrations are shallow, simulated to be shallow in the saturated zone. The zero depth here represents the water table, so this is depth below the water table.

And in the TSPA calculations, we're using a single concentration at a hypothetical pumping well in the center of the plume. Here you can see that the plume, the simulated plume depth is about 500 meters.

I mentioned earlier that we're using the convolution integral method to approximate flow and transport in the TSPA runs. This is a slide. This flow chart explains that method. We use an assumed pulse input of radionuclide mass flux and feed that into the three dimensional saturated zone flow and transport models, both the sub-site scale model--well, at the repository footprint in the sub-site scale model, which is subsequently passed to the site scale model. At 20 kilometers, we simulate a generic break-through curve for this assumed unit pulse input, and this process is carried out numerous times for multiple realizations of the system, including parameter uncertainty in the transport model.

So we end up with a library of these generic break-through curves for different radionuclides for different realizations of the system.

Now, within an individual run of the TSPA calculations, the UZ transport model simulates a radionuclide mass flux history at the water table. This is combined with this generic break-through curve, with one of the generic break-through curves from this library of realizations, through the convolution method, the radionuclide concentration history 20 kilometers down stream is calculated as a function of time.

We've broken down the TSPA analyses that we're going to perform into a set of Base-Case calculations and sensitivity studies or auxiliary studies that we're going to perform. In the Base-Case analyses, the focus is on uncertainty in transport characteristics of the saturated zone. The transport characteristics being deemed to be first order in terms of their effect on performance of the repository, and the first of the important uncertainties that we're evaluating here is uncertainty in effective porosity. And we're using the effective porosity conceptual model here. This is a single continuum representation of the system for transport.

You can think of this effective porosity as representing that fraction of this medium that is available for ground-water flow and/or solute storage and sorption. This is a parameter about which there is a great deal of uncertainty. It is a lumped parameter which includes really different processes, including matrix diffusion or lack thereof. If we have complete matrix diffusion, then basically the entire matrix porosity is available for solute storage. If not, we have some lower fraction of the medium available.

This parameter also accounts for heterogeneity or flow channelization of the system. If transport through the system bypasses large volumes of this medium, then that would tend to reduce the effective porosity as well. We're using a broad distribution here encompassing the expert elicitation estimates.

Next parameter is dispersivity, which was discussed by Dr. Gelhar just before my presentation. We're using distributions for longitudinal and horizonal transverse dispersivity taken from the expert elicitation. Low values of vertical transverse dispersivity that Dr. Gelhar mentioned will be evaluated in the sensitivity studies. This is because the numerical models that we have available for use in TSPA are not adequate for evaluating or simulating transport under--assuming these very low values of dispersivity. And I'll discuss that in a minute also.

Okay, the process of sorption, we're assuming a linear sorption model. The distributions of effective Kd values for all the volcanic units are taken from the work at Los Alamos. The distributions of effective Kd values for alluvium and the carbonate units are taken from expert elicitation, this expert elicitation and from the literature.

And, finally, colloid-facilitated transport is being simulated using an equilibrium model which Bruce Robinson describe briefly earlier today. This is the partition coefficient model. This is also a parameter about which there is a great deal of uncertainty, as well as conceptual uncertainty about the model itself. And we are incorporating a relatively broad distribution of this partition coefficient, this Kc value in simulating colloid-facilitated plutonium transport.

Okay, the sensitivity studies that we have planned, I should mention that this is very much a work in progress. The Base-Case calculations are in progress now, fairly well firmed up. The sensitivity analyses, we're still working on exactly how we're going to do this, some of this work. But uncertainty in the permeability distribution and ground-water flux will be examined in the sensitivity studies. We want to evaluate the effects of the uncertainty in the distribution of permeability and in ground-water flux as elicited from the expert panel. So we want to look at both uncertainty in the ground-water flux through the system, as well as flow pathways. Right now, we're using the calibrated sub-site scale model and calibrated site scale model, so there is no uncertainty in the flow path in the Base-Case analysis, but this will be assessed in the sensitivity studies.

We also want to look at the influence of heterogeneity and flow channelization through the use of geostatistical simulation of permeability within the system, including both intra-unit heterogeneity and the possible influence of structural zones, faults, so forth, on transport through the system.

And the small values of vertical transverse dispersivity are going to be evaluated using analytical solutions. These analytical solutions, however, require an idealized representation of the system, single velocity, uniform flow field through homogeneous medium, but there are both steady state and transient solutions that we can make use of for this analysis. And we also plan to include an evaluation of dilution in a pumping well for these analyses. Very small values of vertical transverse dispersivity would indicate very thin plume, on the order of meters or tens of meters, versus the simulations that I showed you before that indicated a plume on the order of 500 meters thick.

So if we have a very--if we're simulating a very thin plume, it's reasonable to assume that there would be some dilution, some mixing of that concentration in a pumping well, and we plan on evaluating that.

Some additional considerations relative to the results of the expert elicitation, we're going to make quantitative comparisons of dilution factors simulated in the TSPA calculation with the expert elicitation results. You saw the dilution factors that were presented earlier.

And, finally, there were several issues which we decided to exclude from explicit evaluation in TSPA, in part on the basis of the expert elicitation results. For example here, uncertainty in the model of the large hydraulic gradient; this will not be evaluated explicitly in the TSPA calculations, or the influence of disruptive events on changes in water table height and changes in the flow system.

So that's all I have.

PARIZEK: Thank you, Bill. Any questions from the Board? Yeah, Dan Bullen?

BULLEN: Bullen, Board.

Could you go back to your convolution integral method, Viewgraph Number 7 of 11? And maybe I'm a little dense in trying to figure out exactly where you're going to use the expert elicitation inputs, but could you sort of describe that to me with respect to how it fits into this? And maybe another comment might be that when you're finally presenting this in the VA, transparency is sort of a key, and I know that mathematically people would understand what convolution integrals are, but describing how you're going to mix the two at the interface, or whatever, is going to be an important factor. So being a little more transparent might be a little helpful. But maybe you can start by telling me what expert elicitation data will be used and where, and then just a little advice on the transparency issue.

ARNOLD: Okay, yeah. I wasn't really clear in my explanation of this. This is not really related to the expert elicitation. This is just an explanation of how we're performing the calculations. So there is no direct input of the expert elicitation in this.

BULLEN: Okay. And then I guess the follow-on from that would be that you showed us a nice example, but it seemed to me the experts didn't have plumes that were 2 kilometers wide when they got out into the area. But you were just giving us an example of a sample calculation. The real calculations will have thinner, smaller plumes that will ultimately hit the accessible environment?

ARNOLD: Not in the Base-Case. What I showed you is what's being simulated for the Base-Case analyses.

BULLEN: So the red plume that comes out for the Base-Case is pretty big; is that not correct?

ARNOLD: by the time it gets to 20 kilometers, it's on the order of 7 kilometers wide and 500 meters deep. You have to remember the footprint of the repository is on the order of 2 to 3 kilometers wide.

BULLEN: Right. But as I understand the site scale model today, it looked like there were a lot of fractures that might have been flowing through there. How do you basically take a fast fracture pathway out of the repository horizon and take it to the saturated zone and then get it out? I mean, the indications of maybe the effects of faults on the focusing of flow and things like that this morning don't look like they're represented here. And so when you see something like--well, I'll throw it to the experts and ask the experts if they think there's going to be a plume for a Base-Case analysis that's a couple kilometers wide?

Allan, do you think so?

FREEZE: Well, I guess it would depend on the nature of the source. That's always been another source of uncertainty. The question might be are you going to be looking at different sizes and sources.

ARNOLD: I could provide a little more detail on what we're doing here. Think of the whole thing, all going at once and they're all, you know, the whole--

BULLEN: Well, I guess my perspective is that even if they all go at once, what's going to come out is going to come out where it flows. And if it comes out where it flows, then it looks more like, you know, sort of discrete sources as opposed to some smeared source over a couple of square miles.

ARNOLD: Well, let me give you a little more explanation to this figure. That's what we're doing; we're considering six different sources at the footprint of the repository. This is a composite of all six of those zones, which effectively gives you a source that's 2 to 3 kilometers wide. But we have sub-divided this at least to the extent of defining six different zones, and transport from those six different zones is evaluated separately in the calculations.

BULLEN: Okay, maybe I'm asking something--

FREEZE: I think the point is well taken. It doesn't look like those diagrams that I showed from Whitmeyer and from Schwartz and Sudicky, which kind of had tubes, you know.

BULLEN: A few hundreds of meters wide, not thousands of meters wide or something like that. That looks a lot different than the representation that was shown earlier, I guess, is the concern I have.

ARNOLD: It is in the sense that this is not considering the limited transverse dispersion that--

BULLEN: That the experts have come up with?

ARNOLD: That the experts are proposing. But that will be evaluated in the sensitivity studies.

BULLEN: Okay. One more follow-on question and then I'll stop. You did make a comment about when you're doing your sensitivity analysis, that in the Base-Case there is no uncertainty in the flow path?

ARNOLD: That's right.

BULLEN: Could you explain that to me? How could there be no uncertainty in the flow path for the Base-Case? You know exactly where it's going to be and what the shape is and the distribution and all that? I mean, just based on the models that you've picked, that's dictated--you can't introduce any uncertainty; is that what you're saying?

ARNOLD: That's right. The way we're doing the calculation, we are not introducing uncertainty in the flow path in the Base-Case analysis. We're not claiming that there is no uncertainty.

BULLEN: Okay. I misinterpreted what you said, because you said there is no uncertainty, and I was going, boy, I'm still uncertain. But you're not introducing any uncertainty, but you will do sensitivity studies that will address the issue?

ARNOLD: That's correct.

BULLEN: Okay, thank you.

PARIZEK: Alberto Sagηes?

SAGγES: Sagηes. In this transparency that shows the 20 kilometer plume, is the area of highest concentration aware from the repository because presumably the repository has stopped ejecting--

ARNOLD: No, the sub-site scale model is used to simulate transport from the repository out to this 5 kilometer fence. The plume is just picked up here and simulated in the site scale model from this point.

SAGγES: Oh, I see. So conceptually, one should combine the two plumes to get the entire plume?

ARNOLD: At the same scale, I could superimpose those two figures and show a single plume.

SAGγES: I see. And then on the one that you have next, the one on the convolution integral, just to make sure I understand this, you called that--it doesn't show very well there, but it shows up in the printout, when you call it--it looks more like a step and the response at the bottom looks also like a response to a step, not to a pulse. Am I understanding that correctly?

ARNOLD: Perhaps I used the wrong term. It is a step input.

SAGγES: A step. But how can it be a step and then when you have the accumulated response, how could it come down with time?

ARNOLD: What's actually done in the convolution integral method is this break-through curve, the derivative of this curve is taken at discrete time intervals, and it's the derivative that's used in the convolution of this signal with the transport calculations.

SAGγES: Okay. On the flux, which would be what, moles per meter squared per second or something?

ARNOLD: Yeah, or grams per year. It's really an arbitrary--actually in the TSPA calculations, it's taken in grams per year.

SAGγES: Okay. And once it's turned on, it keeps going on forever basically?

ARNOLD: Well, to derive this generic break-through curve, yes, that's true.

SAGγES: The one I don't understand is the one to the right.

ARNOLD: This is another component of the analysis. This is the unsaturated zone transport model, and its output is this radionuclide mass flux as a function of time.

SAGγES: Yeah, that would be the sum of pulses. I interpret that as the sum of pulses. So that I understand. But I don't see the sum of steps.

ARNOLD: It's just a numerical integration method that takes--that breaks this history down into small intervals in time and tracks the response of the system based on this generic--

SAGγES: For each one of them; right?

ARNOLD: For all of these pulses, and integrates the result.

SAGγES: Right. Okay, I don't understand it if you have a true pulse in the top and then a curve of linear response with a pulse underneath. Maybe that's another transparency question to come up later.

ARNOLD: Yeah, overall, this is a short-cut method that saves us computational time. What we could do is we could take this radionuclide mass flux history, feed it into the saturated zone transport model, and go directly to this. But that would be much more computationally expensive, and we've done validation runs that show that this method yields an approximation that's very accurate.

PARIZEK: Debra Knopman?

KNOPMAN: Knopman, Board.

Could you elaborate a little bit more on how you're evaluating dilution in a pumping well? First of all, what does the Base-Case look like for that? And then what sort of sensitivity analyses are you in fact running?

ARNOLD: For the Base-Case, we're not including any additional dilution at the pumping wells.

KNOPMAN: Okay.

ARNOLD: We're just taking the maximum concentration.

KNOPMAN: Okay.

ARNOLD: For the sensitivity studies, that's ongoing work at this time. The NRC has actually done some numerical studies to look at this question as well. Their results should be published very soon. We want to take that into consideration. The basic answer is we're not sure yet.

KNOPMAN: Okay.

PARIZEK: Other Board questions? Staff?

(No response.)

PARIZEK: If not, thank you very much.

HOXIE: Can I ask one question?

PARIZEK: Yes.

HOXIE: Dwight Hoxie, USGS, just one question, Bill.

On the spreading of your plume, did you take into account numerical dispersion, or is there some of that in there? Because you're spacing down gradient is 1,500 meters.

ARNOLD: That's correct. There's certainly numerical dispersion in the simulation.

PARIZEK: Thank you, Bill.

The next presenter will be Dwight Hoxie, program activities at Yucca Mountain, addressing key saturated zone issues. This ought to look at what ongoing studies are being planned for the program.

HOXIE: Good afternoon. I'm glad to be here. About 24 hours ago, I was actually wagering that there was a fifty-fifty chance that I was going to have a voice today, and so if I suddenly--if you see my mouth moving and you hear nothing, you'll know that something went wrong. But anyway, I kind of apologize for being kind of gravely, and I was very much afraid I was going to have to give this talk in sign language, which is a language I don't know.

Anyway, we're at the end of presentations on saturated zone studies, and so this is kind of a wrap-up that I have put together, and the way that I would like to address this is to kind of review some of the things that we discussed today to try to pull it all together, and I want to do this in the framework of the key issues that the expert elicitation panel were given to assess, and so I'm just going to go run through these, and what I would like you to do is to say okay, this is all well and good, these are issues that not only were important to the expert elicitation panel, that are important to PA, but they are important to the project as well. So the question is is what are we doing about them?

You heard some of the activities that are taking place, and I just kind of want to go through this a little bit to let you know that, yes, we agree these are important and we are doing things. So I'll just go through them one by one, just say a few things. This will not be an exhaustive talk at all.

So anyway, first of all, our conceptualization of saturated zone flow, we heard a lot about that today in various contexts, and of course the way that we develop our conceptual models and refine them, first of all, is to collect data on the basis of which we make inferences, and then of course this is something that I think everyone needs to realize about the modeling that we're doing, both the saturated zone flow modeling and the transport modeling, is that we can use the models sort of in reverse. We can use them as heuristic tools to try to gain a better understanding of the system, and I will give you indications of where we're doing this as I go through the talk.

We heard a lot about the large hydraulic gradient today. We have ongoing studies, some that have been completed in Bore Hole D-2, for example, which is in the large hydraulic gradient. We are conducting testing as we speak actually in WT-24 and we have plans to do additional testing in WT-18.

The kind of testing that we are doing is hydraulic testing. We are also doing geophysical logging, which will give us some idea of water content profiles vertically down the bore hole so that we can look for saturated versus unsaturated intervals, and we're also doing core analysis, again, in order to measure saturation so that we can look to see if in fact we have a water table and a saturated zone beneath it, or if we have a perched water system at that particular location.

So we are trying to determine, trying to discriminate between some of the various hypotheses that have been presented today regarding the nature and original of the large hydraulic gradient.

Another issue that of course is very important to the transport folks is how much water is moving and how fast is it moving beneath Yucca Mountain. We've heard a lot of discussion of that today, even from the experts themselves. The thing that I think everyone needs to realize first of all, the flux is moving slowly enough that it's not really something that we can measure by a flux meter, for example.

What we have to do instead, and this is what was done, and Allan Freeze discussed that, is that the first thing we have to do is know what the hydraulic gradient is, so we can refine our evaluations of the potentiometric surface again by measuring water levels and bore holes.

I just want to mention that we have two new bore holes that are going to be penetrating the saturated zone shortly, we hope, and that's WT-24 up in the large hydraulic gradient, and SD-6 that we talked about earlier today, which is on the crest of Yucca Mountain. Once we have the potentiometric gradient, we then have to have hydraulic conductivity measurements. We heard talks today about the work that's being done at the C-Well complex to actually measure hydraulic conductivities in situ. Of course we have a back log of data from previous testing in bore holes that we also can access, and we also, as you heard today, are planning a second testing complex on a large scale, I think much larger scale than the C-Hole complex, at least according to the planning to date, and that this will give us more refined values of the hydraulic conductivities.

So once we have a gradient and hydraulic conductivity, we can multiply the two together to get a flux. And the way that we normally do this in order to get a spatial distribution of flux, and we're running transient simulations, a temporal distribution of flux is to use our ground-water flow models. So we are proceeding on that front.

The influence of climate change. We heard about that today. A lot of the simulations that have been done that include or incorporate climatic change have been based on models, climate models, future climate models that were done by NCAR. We discussed that a little bit earlier today. But the other thing to do is to look at the past as an analog to what future climates might be.

We've had an ongoing paleohydrology, ecological set of studies, and we're continuing those. Right now, we're looking at core from Owens Lake to the west of us here a little bit to try to get a handle on what was going on and the details of the climate in the past 10 to 100,000 years.

Another activity that is ongoing that is continuing is to look at both modern springs and paleospring discharge sites to try to infer what climatic conditions were, what water levels were in the past. And another study that is going on is to look at the morphology and the geochemistry of calcite at the water table beneath Yucca Mountain. There are some, what do I want to say, some anomalies there that may indicate a fluctuating water table over a period of time. So this may give us additional data, new data, on what water levels beneath Yucca Mountain might have been. Our best estimates right now is that in the past, they may have been as much as 80 to 100 to 120 meters higher.

And then of course this is the program that we have that we talked about earlier this morning where we take our ground-water flow models, impose a climate change on those models, that is to say, higher precipitation at higher elevations, for example, in order to assess what the impacts might be on the flow system.

In talking about our conceptual models of saturated zone transport, I think one of the exciting things that is coming out of our program currently, and you heard about it from Zell Peterman this morning, is looking at the ground-water chemistry and isotope geochemistry in attempts to infer what flow pathways might be, flow domains might be, to give us some kind of ideas what the pathways are down gradient from Yucca Mountain.

Again, we can use our transport modeling in a heuristic mode to do tracer studies, simulated tracer studies, and particle tracking kinds of studies in order to delineate potential transport pathways.

And I might just put in a plug. I understand that people have been getting reports on the so-called--now I've got to remember what it is--S4Z flow model. It's a sub-site scale model that we developed a year ago, and we have done some, again, heuristic modeling with faults, and discovering that in fact we can identify some kinds of at least in theory anyway, preferential flow pathways that are structurally controlled.

We heard a lot about that today. This is a program that is not ongoing at the moment, but is probably going to be resurrected shortly.

In terms of trying to get at these quantities, and I'm hearing that they're in some sense maybe kind of nebulous quantities of dilution factors and dispersivities, again, the way we get at these kinds of numbers, as you heard this morning, is doing testing, tracer testing, for example, at the C-Wells, and again probably at the planned second testing complex.

I think we can also probably get some ideas of what the dilution factors and dispersivities might be by looking at our ground-water isotope geochemistry, attempting to use the isotopes as tracers themselves, perhaps using our transport modeling capabilities to try to invert the problem and back out the kinds of transport properties that we might expect.

We talk about effective fracture density, and I think that the important thing here to recognize is that we feel that the primary flow pathways within the welded tufts down gradient from Yucca Mountain are certainly in the fractured tufts, the fractured welded units. And it's the fractures that are probably providing the flow zones in the bore holes, for example, at the C-Holes complex, and at other bore holes at the site, so that the flow tends to be channelled into preferential pathways by the fracture systems and also by the fault systems, and by any kind of zone of linear zone, for example, of increased or enhanced hydraulic conductivity.

But in terms of actual fracture data down gradient from Yucca Mountain in the volcanic aquifers, anyway, we have very little data. Our best data right now is of course in the ESF, which is actually mostly in the Topopah Spring, which is the upper volcanic aquifer, not the Crate Flat Tuffs, which are lower volcanic aquifer. But we can perhaps make some transfer just on the basis of from one welded tuft to another welded tuft. But we have a great deal of fracture data from the ESF.

The other thing that's probably important from the ESF is that we've identified distinct fracture zones, often times associated with faults. Perhaps we can transfer that data, at least heuristically, down gradient to try to incorporate some of the concepts of channelized or focused preferential pathway type of flow. I don't really refer to it as fast pathway so much as preferential pathway.

Hydrochemical transport parameters, again, a lot of this has to come out of field testing at places like C-Wells and our second testing complex. We heard this morning from Arend Meijer about another aspect that might be important to transport, and that is the oxidation state of the water, or actually the redox potential of the waters, and that we are going to try to do this, try to make actual field determinations of what the redox potential might be in some selected bore holes by going back in and conducting very careful experiments.

The other things that we are doing, and I know Lynn Gelhar has some misgivings about the transfer value of this, but we are continuing laboratory evaluations of first of all the solubilities of radionuclides to make sure that, or try to determine just how soluble they are going to be in the saturated zone environment that we anticipate beneath Yucca Mountain to try to determine effective Kd's to put into our models to quantify the retardation of radionuclides, and finally, conducting experiments on colloidal-facilitated transport. That's been going on at LANL for quite some time and is continuing, and also is going to be done--some work is being done at Livermore in order to look at actually further up in the system, the formations of colloids in the engineered barrier system that then could be transported through the UZ and into the SZ.

I think I would like to simply say that we do have a program that recognizes a set of key issues. We are trying to address those key issues by a variety of means, both field testing, laboratory testing, and through our modeling program, and the entire intent is to--well, we're just simply targeting in on trying to reduce uncertainty of the key issues, key parameters that are going to be input to our SZ flow and transport models.

And I would just like to make a comment on an observation that Lynn made today that concerned the performance assessment and the site characterization side of the house are not well integrated. I think that we've made a lot of progress in the past couple of years by having these abstraction testing workshops involving both site and performance assessment personnel working together, and I think we're further going to be enhancing that by going through a revision of our modeling program in order to bring not only PA, but design, into the actual guidance and direction of our site process models, the models that feed into like the SZ flow model, the UZ flow model that feed into performance assessment.

And so with that, I would like to say thank you, and I'd be happy to entertain any questions.

PARIZEK: Board, questions for Dwight Hoxie? Yeah, Debra Knopman?

KNOPMAN: Knopman, Board.

I have two questions. One has to do with timing. For the project to work through the resolution of these various issues and your agenda, what's your estimate on when the bulk of this work would be completed?

HOXIE: I think we're targeting much of this certainly in the time frame for the license application. Some of this will obviously extend beyond. For example, I'm sure we'll be doing a lot of testing for a number of years at, for example, the second testing complex, so that would be going into what we would call our performance confirmation program. But much of this testing is, for example, large hydraulic gradient should be completed prior to the license application.

KNOPMAN: Prior to?

HOXIE: 2002.

KNOPMAN: So the work would be done by 2002?

HOXIE: Right.

KNOPMAN: Okay. Can I ask one more question?

On your Slide 7 in your discussion about conceptual models, you used the term heuristic transport modeling. Would you explain what you mean by that? I know what it is, but I'm not clear on what you're really doing here.

HOXIE: Okay. What I'm saying is is that we introduce a generic tracer artificially into the model. It doesn't have to be anything. It could be particle tracking, for example, I would say would be heuristic, just to identify potential transport pathways.

KNOPMAN: Okay.

HOXIE: That's what I meant. I was distinguishing that from, say, simulating neptunium transport, per se.

PARIZEK: Other Board questions? Yeah, Allan Freeze.

FREEZE: Allan Freeze.

Dwight, there's two areas in your program that strike me as being sort of easy to say and hard to do. One of them has to do with the testing procedures that you're going to use in G2-WT-24 and WT-18 to provide an answer to both whether it's saturated or not saturated. The method you're proposing, hydraulic testing geophysics and moisture contents on cores, are all the same methods that have been used in other holes to try and answer the same kind of questions, generally with kind of equivocal results, it seems to me.

The second area is the tracer testing at the C-Wells and the new testing facility. Again, will it be possible to interpret those results in terms of sorption versus matrix diffusion versus dispersion and so on, or will there still be the same kinds of uncertainties that we've had in the past tracer testings that, you know, provide some information, but don't really answer these kinds of questions? I guess I'm worried that it's more of the same. I wonder if you have sort of new methods that are going to solve these problems, or are they going to be equivocal results again?

HOXIE: Well, I think I will take a hint that I heard just a little while ago actually from Lynn Gelhar. That may be the thing to do. We're developing a plan for the second testing complex, and maybe the advice should be taken that we should take that plan outside of the Yucca Mountain project, and gain the opinions of other experts like yourself, for example, to see if we are planning tests that will provide that kind of information.

FREEZE: I'm not sure there's answers to these questions.

HOXIE: Well, then there may not be.

FREEZE: What about the perched, did you feel the geophysical testing or moisture measurement, is it going to answer the question or is going to leave us with lots of uncertainties still?

HOXIE: Well, let me talk about history a little bit. SD-7, for example, we measured saturations on cores, and of course once we did that, we got a saturation profile that does indeed correspond to the perched water, and I think from geophysical logging, we probably can't have that high a resolution, we can't discriminate between 95 and 100 per cent, but I think from core analyses, I think we now have honed our skills well enough that we can probably determine between 100 and 95 per cent. I think we've done that. I mean, I think that we've encountered certainly in many bore holes, especially in some of the neutron holes, the shallow neutron holes, where we see infiltration events simply based on core analyses, for semi-perched zones, quasi perched zones at the base of, or at contacts between units, for example.

So I think that from looking at the cores, we can do that.

FREEZE: I'm not sure about the geophysical logging.

HOXIE: But we've also inferred the possibility-- historically geophysical logging was done in G-2 and it does sort of indicate that there may be an unsaturated zone beneath the normal water table. That was encountered in G-2. So I think we have a hope of that.

PARIZEK: Czarnecki?

CZARNECKI: This is John Czarnecki from USGS.

I just wanted to comment about the G-2 work. If we can use UZ-14 as a comparison to what we saw in G-2, the fact that we stopped the upper water producing zones and drilled through them with a substantially large interval of non-producing bore holes, if we see those kinds of things in G-2, I think that will be a very revealing condition.

The fact that G-2 is a completely open bore hole and water could be coming in from the top and we haven't tested the bottom part, I think we have something to learn. But only the first third of the tests that we've proposed for G-2 have been done.

PARIZEK: Parizek, Board.

There seems to be a distinctive chemistry of the water under Forty Mile Wash, and we saw that again with Zell's presentation today. Is there an opportunity to use that as an analog more or less of what happens to that mass of water in the direction of regional flow to the south under Forty Mile Wash to get out some dispersivities?

Another way to look at a natural analog; I pushed for this for maybe the Crater Flats water, you only have two wells there so you can't really do much with it at this time, but is there some possibility of using Forty Mile Wash chemistry as a way to find out how that mass of water evolves going southward?

HOXIE: Well, I'd probably have to defer to the chemists, of which I am not. But one thing I'd just comment is that it seems to me anyway that it looks like Forty Mile wash is a preferential pathway for flow. So it would probably entail some kind of structural control, but that's not answering your question.

PARIZEK: No, I was looking for the dispersivity possibility.

HOXIE: Right.

PARIZEK: But just let it maybe be in the program thought process and we'll ask you next time we see you and see whether there's any merit to that.

Another thing is about this water level 80 meters or 120 meters higher than the present sometime in the past. It seems like in the evolution of Yucca Mountain before faulting, during faulting with mountain uplifting, Basin range development, and a whole change in the water table configuration, there should have been a water table signature higher up in the mountain than presently, 80 or 120 meters. Is there any evidence of that? It just surprises me that that doesn't sort of show up somewhere as fuzzy data somewhere up in the unsaturated zone.

HOXIE: Well, I think I can address that, and I would appreciate comments from the audience. But I think that actually the faulting and the topography of Yucca Mountain probably was pretty well established 10 million years ago because if you go down to Dune Wash or Rainier Mesa, there's a deposit at Rainier Mesa tuft there that must have been filling in a topographic low. So I think that the speculation right now is that the 100 meter rise or so represents something that is probably a 10 million year old type of feature.

PARIZEK: Thank you. Staff? Any Staff questions? Yes.

REIMUS: Paul Reimus, Los Alamos.

I'd like to address Dr. Freeze's question, as well as some of Dr. Gelhar's concerns, at least with respect to the C-Wells tests and the second testing complex.

Specifically to address your question about new ideas, new thoughts, there's actually a lot of ideas and thoughts that aren't entirely new, been around a number of years on the project, that we would like to pursue at both the C-Wells and the second testing complex to address these issues of, for instance, matrix diffusion and sorption.

One of those ideas, just as an example, is to conduct tests at similar in configuration but at different flow rates to sort out matrix diffusion effects.

Dr. Gelhar's comment about using tracers with greater contrast and diffusion coefficient is another thing we would like to pursue. There are some definite physical and chemical constraints there. As you try to get larger and larger diffusion coefficient contrasts relative to say a simple anion like bromide, you're talking about bigger and bigger molecules, bigger and bigger molecules always tend to sorb more, and so forth, and so there's that problem.

But there's also, you know, another way of looking at matrix diffusion that a lot of people advocate, is doing single well injection withdrawal tests. These have been proposed on the project for a number of years. They've been done successfully by the WIPP project, for instance. So these are other ideas.

Also, the thoughts of going to larger length scales and doing tracer testing and doing more recirculation, approaching what Dr. Gelhar suggests, basically a full dipole test, are certainly thoughts on the table as well. These things do boil down of course to time and money, and that all has to be considered as well.

PARIZEK: If there are no other questions, we'll go to our final presenter, Robert Yasek, who'll talk about thermal testing program and an update. It's above the saturated zone, but it goes to the large heater experiment, and a chance to look for some new input or data. After that, hopefully it will still allow time for public comment or input before it all closes down. And the sun is setting, but I hope that everybody gets a new appreciation of the desert based on today's presentation. A lot going on below our feet as we travel through this country. Bob?

YASEK: Good evening. My name is Bob Yasek. I work for the Department of Energy on the Yucca Mountain project, and I will be updating the Board on the thermal testing program at Yucca Mountain.

Basically, I'll be going over the large block test, the single heater test and drift scale test, and some of the results that we've seen from each of them, and the progress of them.

Here's a picture of the large block test. The objectives of the large block test are to look at the thermal effects of the hydrology of the near-field environment and look at such things as dryout and condensation, looking at condensate refluxing and some of the chemical and mechanical responses to each of these.

The schedule for it currently is we are in the cooling phase, or we will be initiating the cooling phase later this month or in early February, and that will continue through the end of the fiscal year. Post-test characterization for this will begin in early FY-99, which will be in October.

This is the layout of the large block test. In the diagram on the left, the dominant face is the east face here into which the heaters are placed, and the dominant face here is the north face looking to the south.

Currently, the maximum temperatures are being maintained between 135 and 140 degrees C. And we have levelled off as seen in RTD at TT1-14. This, if you are looking at plan view with the north to the top, would be roughly within a meter of the center of the block to the northwest of it, and basically it shows where it levels off at roughly 135, about 135 degrees.

In TT2-14, it shows similar. This is out closer to the edge on the eastern side of the block, and it shows a little lower temperature, which could be due to the fact that it's closer to the edge, getting some boundary effects there. And both of these are from vertical bore holes.

Some cooling in the power ramp down period is seen, but the temperatures are now relatively constant. This is through the 10th of December, and we see this levelling out. This break here is at about New Year's, and this is just before Christmas and indicates some internal event.

Looking at the hydrology of the large block test, using ERT, electrical resistance tomography, they suggest that there's a region of dry rock has formed around the heater where up to 80 per cent of the original water has been lost. And in some vertical features down here on the bottom here, we have a saturation model, a region of increased saturation is seen locally and situated vertically. And this came up during a time of possible significant hydrological event, which occurred about June 13th. This was taken on the 25th, this tomograph, which was shortly after it. And the middle row indicates the temperature within the block.

Moving on to mechanical effects, this, which doesn't show up very well, this face right here is the eastern face. Some of the results seen in the mechanical; all instrumented fractures have opened since the start of heating, and a sub-horizontal fracture, which is basically this plane here, this is the east face, this is the south face, has shown movement to the east, top to the east.

Also, a north-south feature which runs roughly here has shown significant movement with the west block moving down relative to the east, which is moving up.

Thermal perturbations happened at about 2,500 hours, which is roughly about the June 13th event, and was preceded by an acceleration of fracture openings and sliding. This deformation is known to increase the fracture permeability, suggesting mechanical response, may have contributed to thermal hydrologic behavior.

Moving on to the single heater test, the objectives of the single heater test remain to assess the thermomechanical response of the Topopah Springs to a linear heat source, enhance our understanding of coupled processes in an intermediate field scale test larger than the large block test, and shakedown of our instruments to be used in the drift scale test.

The cooling phase currently has ended and the insulation has been taken off. That was taken off in early January, on the 5th, and that acquisition system will continue through early February when post-test characterization will start. And some of the significant activities we'll be doing with that will be overcoring of heater bore holes and hydrology bore holes, and looking specifically at the Bore Hole 16 where water was taken out of. Air injection, gas tracer, Goodman Jack testing, rock bolt pull tests, both on the ambient and on the heated side of the drift, all these to compliment studies that were done prior to and during the test itself.

Okay, other information, other studies to be done in the single heater test, post-characterization, mineralogy-petrology analysis, laboratory hydrologic properties, evaluation of heater and instrument performance, perhaps to help in the drift scale test, and laboratory thermal-mechanical properties. All this will be wrapped up in a report that will be done next January. And here's a layout of the single heater test.

Some of the things seen in the predictions versus measurements are that the measurements agreed quite well with predictions, and show that heat transfer is dominated by conduction, but it's important to incorporate hydrology into the model predictions and analyses.

This is looking to the south--or rather into the block, the heater, if you were standing at the heater source here looking in. Drift scale tests will be down this drift and the alcove goes around this side here. This is predictions and measurements. And as you see, they agreed. They agree well.

Looking at a single thermocouple, the measured response is in red, and it agreed reasonably well with the predicted data. Permeability was not included in this and we were to assume that only conduction was involved in the heat transfer. They tend to over predict, so there would be likely a line higher above here. So it's clear that something besides just conduction alone is involved in heat transfer.

And looking at measured versus predicted for mechanical, again we see responses that are consistent with predicted.

Looking at ERT for the single heater test, we show significant dry-up occurred around the heater during the heating phase, and re-wetting conditions continue to progress during the cooling phase.

These first two, the first line there shows the resistivity ratio, and then temperature. And then the saturation model, the first model is assuming that conduction of electricity is primarily through the pores, and model two assumes surface conduction. And model two generally seems to provide better results, and these are based on Waxman and Schmidt's equations.

Looking at the geochemistry of the single heater test, hydrology and geochemistry, what was collected on four occasions from Bore Hole 16-4, approximately five liters each time on three occasions and one and a half liter was collected on February 27th, probably because it was only less than a month between collections from sample two to sample three.

There was no significant accumulation of water after the heating phase was terminated. However, note that it did go up slightly here, and then on this--this was collected on the 22nd and less than a week later, the heaters were turned off. There was some up turn. Then as soon as the heaters were turned off, it dropped off very sharply.

But the water chemistry was consistent with condensate origin and interaction with fracture-lining minerals along the flow path. Length-scale of the flow path was on the order of meters, roughly 3 to 6 meters, and based on reactive transport modeling.

And, finally, I'll update you on the drift scale test, which the objectives are to predict and measure coupled processes at the scale of an emplacement drift, looking at temperature distribution and heat transfer modes, propagation of drying and re-wetting regions, changes in the water chemistry and mineralogy, and thermal expansion and deformation modulus.

There's what's commonly called the "Pick up Sticks" diagram of the drift scale test, showing all the multiple bore holes and their purposes.

This test was started on December 3rd. Current plan calls for a four year heating process. Data collection and analysis is ongoing. Our first report is due this September.

Some early observations out of the drift scale test show basically no surprises. Baseline and initial hearing phase measurements have been recorded. They show anticipated behavior. All the equipment appears to be working in the manner consistent with what we expect. And basically the instruments are still within an area that is within ambient. So thermal perturbation hasn't reached the outermost part of the instrumentation yet.

This information, the temperatures range from 75 to 82 as of January 5th. As of this past week, I would say that would be about 84 and 90 degrees C, roughly, and of course rising. And so far, we haven't seen any responses in any of the hydrology bore holes that indicates water accumulation like we saw in the single heater test, those types of responses.

The power has remained relatively constant. The wing heaters in red above are operating at 100 per cent of power, and the canister heaters in the drift itself are operating at 80 per cent of power. This will continue at least through one year, and it will be evaluated whether or not the power should be adjusted at that time.

Trace and data from several bore holes, the first graph shows data from within the drift itself on one of the canisters and in the air, showing the heat-up.

SAGγES: Excuse me. I don't understand. Where is that--

YASEK: This is about halfway down the drift. One of the air ones, which is in blue, is a thermocouple that's just out in the air.

SAGγES: Like in between the two packages?

YASEK: Exactly. And I could get you more specifics on the exact location of that, but I believe it's between 4 and 5.

SAGγES: And the floor heater, what--

YASEK: And the floor heater, that's on the surface of the heater itself, the surface of the simulated waste container.

SAGγES: Now, the containers are simulated containers; do they have heaters inside?

YASEK: They have heaters inside of them, yes.

SAGγES: And what is the floor heater again?

YASEK: A floor heater, it's a metal mock-up, approximately the same size that we would expect a container, a waste container to be, and it contains 30 heaters located around the inside of it, evenly spaced.

SAGγES: So those are inside a simulated package?

YASEK: The heaters are inside the steel containers.

SAGγES: Okay. And that's a thermocouple in one of those heaters inside the containers?

YASEK: At the surface of the container, on the outside. There's a thin wall, probably a quarter inch of steel between the inside of the heater and the outside where the thermocouple is located.

SAGγES: So that would be equivalent to a container's surface temperature; is that correct?

YASEK: That is correct.

SAGγES: Okay, thank you.

YASEK: Okay, this graph was taken from data taken from this vertical bore hole in yellow located about midway down the drift, and it shows a response. These RTDs are spaced about 30 centimeters apart, the first RTD being at the collar of the bore hold, and shows a rapid response. As we get further in, the second RTD shows a much more subtle response, and beyond that, there may be a break in RTD-20, and by the time we get down to RTD-30, there's no response. But it is showing a smooth response.

SAGγES: I understand the blue curve, but the other ones, could you indicate in the diagram on the right where the thermocouples for those would be located?

YASEK: Okay. The thermocouples for those, let's see, the first one would be right at the top of the drift here. Right there. Thermocouples are located every 30 centimeters throughout this. So RTD-10 would be roughly 3 meters in. So this is a 20 meter bore hole, so it would be about a seventh of the way, it would be just below the T on drift there on that vertical bore hole.

SAG$_{\gamma}$ES: Okay. And the others are higher up?

YASEK: That is correct.

SAG$_{\gamma}$ES: I see. Thank you.

YASEK: On the bore hole that is parallel to a wing heater, and that would be on this bore hole right here, again we're seeing responses. Right here, RTD-40 has some unusual inflections and at this time, this is data that's probably less than three weeks old. It's just the only thing that I could tell you about that is that it indicates that there's something other than just conductance of heat.

This is from the bore hole that's parallel to the drift right along here. This RTD-1 here is right at the collar, and generally would show the ambient temperature in the drift. And then RTD-10 is further in. These are located at one meter spacings, and so RTD-10 actually doesn't reach the wing heaters yet. These wing heaters are coming out to the side here. RTD-10 would be somewhere in this region right here.

Now, once you see RTDs where you're reaching the wing heaters, then we show an elevation in temperatures, basically showing that there aren't any surprises so far, and that the drift is heating up.

These are along the surface of the roof of the drift in a line along here, and showing temperatures along the drift are increasing since turn on. And an interesting thing to note is that it appears that it's a slight bit warmer near the bulk head than it is toward the back of the drift.

And, finally, looking at a hydrology bore hole, which is this blue one right here, so far, we've seen responses which are not inconsistent with that which we would expect from barometric pressure. I haven't seen any responses like in Bore Hole 16-4 in the single heater test where we might expect to see some fluid build-up, some water build-up.

And that concludes my update of the thermal testing at Yucca Mountain.

PARIZEK: Thank you, Bob. Any questions?

BULLEN: Bullen, Board.

Can you go back to the large block test?

YASEK: Yes.

BULLEN: Interesting anomalous outputs, and describe for us, if you will, a little bit more about what you think happened at 2,500 hours and again at it looks like about 4,500 hours.

YASEK: Okay. At 2,500 hours, that was roughly June 13th, there was--this probably shows it best right here. There was a hydrologic event. It rained within the vicinity of Yucca Mountain. There wasn't a rain gauge right at the large block test, so we don't know exactly how much rain fell right there. But the response of this was believed to have been caused possibly by that event.

Again, there was at 4,500 hours, this was roughly Labor day, just after a Labor Day weekend on I believe September 2nd, there was another similar event where the temperature went down again. I also might add that at this point right here, there was a chiller failure. The heat exchangers that are on top of the large block failed, and so there's some possibility that played in a role in it, but also this event here, there was no chiller failure there. But at both of these, this event here I believe that Yucca Mountain had several inches of rain within a 24 hour period.

BULLEN: This was the hurricane that came in from the Gulf of California?

YASEK: I believe so. September 2nd, it was right after Labor Day, on a Sunday, I believe.

BULLEN: Could you comment on the other thermocouples that you had, the other RTDs, these are temperatures that are in the hot zone. What were the other kinds of temperatures that you had? I mean immediately below the heaters, what was the temperature response associated with these anomalies?

YASEK: Okay, I haven't seen those, but, for example, this is further off to the side. Generally, I believe that if they hadn't reached the boiling point, that we didn't see a response like here, however, at this one, since it had reached above the boiling point here, we're seeing some of this noise which is similar to that which we had over here. And also it shows the September 2nd event there as well.

BULLEN: So you think the noise could be associated with the heat pipe type effect where you actually have a boiling front that's moving back and forth along fractures nearby the RTDs? That's a lot of speculation on your part, I realize.

YASEK: That's a lot of speculation. Right now, like I say, we don't have rain data for right at the site there, so we can't positively say that it was water, it was rainfall that caused these events.

After this September 2nd event, the top of the block was covered to prevent any further rainfall from possibly getting any water into the--

BULLEN: One last question, and then I'll yield to my distinguished colleagues here.

On the drift scale heater tests, the initial data that you're collecting, does it also include relative humidity data, partial pressure of oxygen, partial pressure of CO2, partial pressure of water vapor within the drift, and do you have any information on that? Or that's a preview of coming attractions we can see at a later date?

YASEK: It does have relative humidity, and such. And that's probably something that we will be coming out with at a future date.

BULLEN: Okay. I just wondered. In all the data acquisition besides the temperature data acquisition, have you seen any surprises, is my question?

YASEK: I haven't seen data, so I can say no.

BULLEN: Okay. Well, I'll bother Bill Boyle about that later then.

PARIZEK: Paul Craig?

CRAIG: Craig, Board.

I'm interested in understanding, your level of understanding of the mechanisms that are going on, and there's two questions along those lines. Probably at the level of Yucca Mountain, the boiling point of water is maybe 98 or so C?

YASEK: That sounds reasonable.

CRAIG: And it looked from the graphs with the resolution that I could see from here, that on both occasions when you had your rain storms, the temperature instantaneously dropped to the boiling temperature. It sure looks to me like right there, it dropped to 98, and right over there, it dropped to 98 instantly on the resolution of that. You probably have higher resolution. That sort of suggests that there are some big cracks there through which the rain can get in very, very fast. That sounds like something one could model, so that's one question.

The second question is in talking about your understanding of these graphs, you said, if I understood you correctly, that everything could be understood--well, first of all, you said it wasn't just thermal conductivity; that it was more complicated effects. But interpreting what I thought you were saying, thermal conductivity, the heat capacity of the medium and the latent heat of the water would appear to be the primary physical phenomena, and that's straightforward enough.

YASEK: Right.

CRAIG: Are there other physical phenomena that you're modeling, or is that the full extent of it?

YASEK: Other--okay, we're also looking at fractures of the block. I'm sorry, I lost my train of thought.

CRAIG: The question is what are the physical mechanisms that you're modeling here, and do they include phenomena other than thermal conductivity through the rock and evaporation of the water?

YASEK: Yes. As a matter of fact, this event here was preceded by the mechanical event where there was a mechanical perturbation prior to that where fractures opened up, possibly allowing water to go in.

CRAIG: That's for the abnormal event.

YASEK: That's correct.

CRAIG: Presumably you're measuring that.

YASEK: Yes.

CRAIG: But for the normal behavior when you don't have a crack, are there any other mechanisms when you don't have a failure where you heat it up and it cracks, are there other physical phenomena that you're modeling besides the one that I mentioned? What goes into the physical models when you do all these--we measured this, we predicted that. I'm trying to understand what went into the model that produced the curve with which you compared the experimental data in order to say that the agreement was good. Is there anything that went into that beyond the thermal conductivity of the rock, the heat capacity of the rock, and the latent heat of water?

YASEK: Okay.

CRAIG: Specific heat of water, too.

YASEK: Okay, I'd probably have to get back to you on that one.

CHESTNUT: Duane Chestnut from Lawrence Livermore Lab.

There is no quantitative analysis yet of what happened on this test, but I think we have a reasonable speculation about what happened. There is a report available on CD ROM now called thermal coupled processes that has somebody's temperature curves and some discussion of what we think happened.

In addition to that, there are some videos showing some lab scale experiments that were done at Lawrence Berkeley Laboratory with pentane in an artificial fracture.

What's happening is you have a system that's gravitationally unstable. You're heating from below. You have a dry-out zone, and up above that, the water that has been evaporated is collecting somewhere up in the rock, and this first event here seemed to have been triggered by a failure of the upper heat exchanger at the top of the block, which I think allowed condensation to occur. It overloaded the capacity of the fractures to sustain that thing against gravity drainage, and so you have water cascading down into the hot zone and then evaporating rapidly and being blocked.

So we've got basically a percolator, thermal siphon effect going here, and I think that's why you see these extremely rapid--there's no other mechanism we could think of that would lead to such rapid changes in temperature, other than advective movement of water, followed by rapid boiling, and this is exactly the picture you see in these little lab experiments that I mentioned.

Now, in order to get a model of this thing, we would really have to have a very detailed dual porosity model with fractures in the matrix adequately represented in this thing, but we have to have the relative permeability characteristics of the fracture, so without the thermal hydrologic properties in addition to all of the basic thermal properties. So I think that's kind of where we are on the thing.

CRAIG: That's very helpful in the unusual event. Now, what about normal behavior when you don't have rain and you don't have cracking? What else goes into the model?

CHESTNUT: Well, what--

CRAIG: Are there any other physical mechanisms?

CHESTNUT: What do you mean by normal behavior? I mean, this--

CRAIG: Well, behavior excluding the events that occurred--

CHESTNUT: No one modeled this before we saw it happen. Okay?

CRAIG: Pardon?

CHESTNUT: No one modeled a temperature curve that looks like this before we actually saw this.

CRAIG: Well, take this section over here.

CHESTNUT: Now, right here, the system is--basically it's a conduction dominated system.

CRAIG: Are there any other physical phenomena besides the ones that I mentioned that you include?

CHESTNUT: What you have is a pure heating problem. And remember that up until we hit 100 degrees, we're really drying out that part of the rock. So that's essentially a single phase system. Now we've created a system where we've got a dry region building up around the heater. That water has to go someplace, and some of it goes above the heater plane and some goes below. What goes below doesn't bother us because it runs out the bottom of the system. It's the stuff that's up here that has this--it's been sitting there waiting to come down as soon as something perturbs it, and I think what happened is you see this plateau, you have a long period here where we're right at the boiling point. That's a two phase region where we have both liquid water and vapor present, and finally we dried out all the water in that particular thermocouple location and we started to come up with another conduction heating. So now we have a dry system essentially with water vapor in it, and that continued until something happened at the top that caused water to come back down into that hot zone.

CRAIG: And you were able to quantitatively model all those processes?

CHESTNUT: I'm saying we have not quantitatively modeled. This is part of the work that's in progress now. Now that we've seen the occurrence and we know we have to take this kind of thing into account--

CRAIG: Well, I remain confused because there was a graph that showed temperature distribution experimentally and temperature distribution observed, and the assertion was made hat the agreement was excellent.

CHESTNUT: In the single heater test.

CRAIG: Okay.

PARIZEK: I would recommend that since this is a progress report, a lot is going to be going on in the months ahead and we'll have a chance to come back to this, and it's more important now that we do allow time for public comment, and that will be chaired by our chairman.

Meanwhile, I want to thank all presenters for an immense amount of effort to get all of this across in the time period available. I said it would be dark when we were finished, and I'm right on schedule, but again I thank the audience for staying with us and being patient.

COHON: My thanks also to the many speakers that we had today, and to Richard Parizek for his fine job of chairing the sessions and for keeping us right on schedule. It is indeed dark.

Well, clearly, as we've learned today, the saturated zone is an area of keen and active interest with the program, as well it should be. It's also an area where there's a great deal of activity still shaking itself out.

I'm now going to call on people who have signed up to make comments or to ask questions. First, Sally Devlin.

DEVLIN: Here I am, Dr. Cohon, and of course I want to give you a toastmaster's evaluation, and that is that we really welcome you to Amargosa. We hope you'll come back. We hope next time it will be Pahrump and you can enjoy our hospitality there. And I hope you enjoyed the weather we brought you and I hope it stays this way.

Your joke is ready and my friend with the beard up in front has it for you. My comment on the program is that, one, it's much too long. Is there anything that can be done so that there isn't 15 presentations, lunch, breaks and so on? Can we tape it? Can you do it elsewhere and bring it all together? Can something be done electronically? I know I'm broken, and I can imagine how broken you all are, and then you have to fly out. I'm asking you that question.

COHON: Your observation is duly noted. I'm sure we're all feeling that way.

DEVLIN: Okay. And I think it's something to be looked into, because if we had the full day yesterday, perhaps it wouldn't have been so congested today.

COHON: It's a fair point. Thank you.

DEVLIN: The other thing is I give my best speaker to Lynn Gelhar. Is he here? I think he's gone back to MIT.

COHON: I think he might have left.

DEVLIN: Anyway, the reason that I'm doing it is he really had doubts about Yucca Mountain, and you know how I feel about you, you're going to kill us all and the entire world if you get water in those radionuclides, and so I really feel that you've had a grand time doing modeling, and it really rather disturbs me because there's nothing wrong with modeling. I took a computer course seven semesters ago and I have a 286 word processor. We have no wiring to get internet where I live, so I'm kind of stuck with that. But what is bothering the most, the first thing I learned from my teacher is garbage in, garbage out. And of course he went to jail for life. So I'm just saying I'm hearing too much of this modeling. I've seen too much of the modeling, and I'm concerned that you want reality to live up to your models.

When Russ said about, well, we're going to do the model before we go into the east-west drift, this really bothers me. Models have their purpose, and I think it's wonderful that you have probably many millions of dollars of computers, but I didn't see this is that color, this is that height, this is so on, and I want more heat testing, and I want more age testing, and I want more flow testing. But I want it no on a model. I want it in reality, because as far as I am concerned, and I go way back with John, when some guy did a 40 minute presentation with every crack, fissure, what have you, in the world at Yucca Mountain, and he said where did you do it, and he said I did it in the lab.

You've got to do these things in real life, and I do believe that the mountain is just one fracture, fissure, fast flow and so on, and if you had been here, and I will never invite you to this, and that is when we had the floods, and we had seven and a half inches of water or more in Pahrump. You had it out at the test site, too, not as bad as we had it, but on the desert, we have a different world out here, and I remember talking to John before they put the tunnel boring machine in, and I said for goodness sakes, drop four inches of water on the tunnel before you put it in, and he said we don't do that.

The next year, you had the floods. The roads washed out. The trucks washed out, all kinds of damage. So this is the desert and I'm an Easterner, but I've been out here full-time more than 30 years, and I understand this desert. You can have a cloud that cries and floods you away, and that is our desert, and I just want you to make you aware of it.

I always recommend, and that was my recommendation, and my commend is come on back. I see old friends, Dr. Chestnut and so many others. I'll have another joke for you next time. I'll see you at the next meeting in Vegas. But I really do feel that this is overwhelming, not only with information, but with modeling, and you might do a little more hard science, and I am afraid of the radionuclides and I want more cancer things. Nevada has the highest incidence of cancer for women, breast cancer, and lung cancer, and the worst in the entire nation, we're number three in the world, but the worst in the entire nation is the District of Columbia in every class of cancer. And I am looking for more radionuclide studies to let us know what these radionuclides do to our body, what one does to this one and that one, and so on, and why you die or I die, and nobody knows. And this is really what the public is I think interested in.

So thank you. Come again very soon.

COHON: Thank you, Ms. Devlin. Thank you very much.

Mike Williams. Mr. Williams, are you still here?

WILLIAMS: I spoke yesterday, sir.

COHON: Thank you. Earl McGhee.

EARL MC GHEE: I'm Earl McGhee. I live in Amargosa Valley, and I, too, want to thank you very much for having your meeting here. I appreciate it. I hope that you understand what the public is trying to tell you. However, sometimes when you rub elbows with a certain class, whether you're an engineer, a doctor or what have you, you think along the same lines and I happen to think a little different.

What Sally said about water, she was at the meeting at the Mirage in Las Vegas where I asked a question. When the man says oh, it will stand a 6.5 earthquake, and I asked him, I said, "What happens to the subterranean resource when you have that seismic activity?" That comes up, and there's no way to predict that you won't have a 6.5 or even a 7.5 in the future. There's no way that you can predict one way or the other. You have Mammoth right now that they're afraid may boil over and pop its cork. And if that does, you have your faults here that may become a little active also.

It reminds me of what the great--when something was not with authenticity and not proven, he said God doesn't roll the dice. It was Albert Einstein. So all the variables that you have studied I think you'd better take a second look at the values. I think you do a good job. It's obvious that you're working. Sometimes--well, it reminds me of an old story, if I may tell you. It's about a new teacher that went to a new school and teaching a class in theology, and the first day in class, he asked the boy, he says hey, Sammy, stand up and tell me who tore down the walls of Jerico, and the kid jumped up and screamed at him and says, I didn't do it. He says, furthermore, if I knew who did it, I wouldn't tell because I don't rat on anybody. Later, he was having lunch with another teacher, and he told the teacher about asking the question and the response he got, and the other teacher stopped him. He says wait a minute, he says was that a little fair haired boy with freckles? He said yes, it was.

He said, well, if he says he didn't do it, he didn't do it. And furthermore, if he knew who did, he wouldn't tell because he wouldn't rat on anybody. This disturbed the teacher, so he was telling the school administrator, the principal about it, and the principal listened attentively and when he was through, he told he, he said, boy, that is awful. He says it's the worst thing I ever heard. He said but, however, don't worry about it. I'll have the brick masons come around tomorrow and put it back up again.

Sometimes we can get lost in these things. Well, there is such a thing as the competent/incompetent, and sometimes you listen to the incompetent/competent, you might be better off.

On this water thing in Las Vegas at the Mirage, that's a very serious thing. When I brought up about the EIS for the test site, Amargosa Valley is in the ambience, and it's also in the flow pattern, and they needed an alternative water source, even though they had plenty of water. Now they're filing on Amargosa water. So think about it. When I told people about this after I read it, they wouldn't believe it. So people can stick there head in the sand or wherever, and they get nowhere.

But, again, I thank you and I'm not saying that you're not working. You people are doing an awful lot. There is such a thing as work with a positive construction rather than destruction. When you risk a certain area with wildlife and people, maybe that risk can be avoided. I think it can be. I thank you.

COHON: Thank you, Mr. McGhee.

Victoria McGhee could not stay for the public comment period and she wrote a statement and asked Bill Barnard, the Executive Director of the Board, to read it into the record. Dr. Barnard?

BARNARD: Thank you. "My name is Victoria McGhee. I live in Amargosa Valley. My comments are addressed to everyone at this meeting.

Ladies and Gentlemen, you are breaking new ground at Yucca Mountain. You have a wonderful opportunity to break new ground in this small community of Amargosa Valley. If Yucca Mountain is licensed, and I believe it will be, Amargosa Valley will be in the critical hazard area. I would urge you to rethink your payment equivalent to taxes obligations. Amargosa receives no benefit from these monies. The county has discounted and disowned Amargosa, choosing to build monuments to themselves elsewhere.

Break new ground for the existing population. Stop making victims of the surround population. That has happened in Fernald and all the other toxic contaminated sites. Chernoble residents never recovered. Their injuries are ongoing and they are still dislocated. There will be accidents. Remember Murphy's law. Take the lead for the nuclear industry. Use this opportunity to break new ground. Buy out the residents of this small community. Use Pahrump as an example of how Amargosa would have developed without Yucca Mountain, plus a relocation allowance. Live up to your responsibility to this critical hazard area. Rethink your obligation. Lead the way.

Thank you. Victoria McGhee."

COHON: Thank you, Bill. Steve Frishman?

FRISHMAN: In the interest of time, I just have one very short comment. And that's that I think regarding the viability assessment TSPA, I think it would be very important to know if there is a valid and technically supported reason for the cross-sectional dimension of the plume in the saturated zone and also what basis is there for not considering uncertainty in that calculation. I can think of an obvious reason that it's not technical. But I think it would be important for the Board and the rest of us to find out if there is a valid technical basis for doing that in the TSPA, the viability assessment.

COHON: Okay, duly noted. Is there any point in revisiting this uncertainty issue with regard to that one parameter and what is really meant by that? Who is the one that introduced that? Is he still here? Maybe not. Abe, do you want to--

VAN LUIK: This is Van Luik, DOE. What the issue was is that for the Base-Case, in order not to convolute too many uncertainties, the saturated zone portion of the model is going to use basically the main calculation, the calibrated calculation from the site scale flow model and the sub-site scale flow model, and then in separate sensitivity analyses, which will be in the TSPA/VA documents, we will show what the affect would have been had we thrown in the uncertainties in those models. But for the Base-Case calculation, we were going to show basically the ones that Bill Arnold showed, and I think it's a call that we're making at this point to not convolute too many uncertainties on top of each other. But they will all be in there and you'll be able to find every one of those uncertainties.

COHON: Thank you. Did you want to say more about it, Steve? I don't mean to foment any kind of discussion here.

FRISHMAN: Well, I think you pretty well got your answer. It's a call and they don't want to confuse people by showing the uncertainty and ultimately the dose that they're going to produce in the executive summary of the TSPA. They won't give out a dose from the TSPA calculation. And sure, you can find all the rest in there, but the summary of it is going to be just--is going to be the base case and it's going to be extremely misleading because over the last day and a half, I have seen that uncertainty has not been reduced in the unsaturated zone. In fact, it looks higher than I have seen it before in terms of the number of factors and getting down to an order of magnitude uncertainty here, another one here, stacking on each other. So we may end up with the Department very proudly announcing that at 20 kilometers we can meet what people might think is a reasonable dose standard, but not telling the people who are making decisions that that may have four orders of magnitude uncertainty attached to it.

COHON: Thanks for raising it. Yes, sir, please come forward, introduce yourself.

WILLIAMS: Yes, Dr. Cohon, I'm Mike Williams. You had called me earlier and at the time, I wasn't really prepared. I spoke yesterday concerning the containers with the two and a half inch I-beam on the shipping containers being faulty because of the welds. There was a couple more points I wanted to bring up that I would like someone to look into them if at all possible.

Evidently whoever was in charge at the time concerning these transportation containers at the test site, at the time, was using a material, now I do not know the technical name, but it's something similar to Kitty Litter they were putting in the bottom of these containers to absorb the liquid materials that would accumulate. Now, due to cost effectiveness, they changed it to a liner similar to the materials you see in your butcher shops that soak up the blood in your steaks and so forth, which I understand are not quite as efficient as this Kitty Litter material. I don't know who made that decision or why it was made.

The other thing, these containers originally were wooden containers and they are being placed in open trenches with the idea of them deteriorating naturally, going into the soil. They've been replaced with metal containers which are leaking, and as far as I'm concerned, I don't understand the real importance of them leaking other than being on the highway, if they're going to be put in an open trench anyway. But I've been told that they're going to return to these wooden containers and if this is true, I would like to know why and why these things are being put in open trenches, this low-level material. It's in Area 5 at the test site.

COHON: Fair questions. Low-level waste is totally out of the purview of this Board and we have no knowledge, specific knowledge of these low-level waste depositions at the test site. However, is there anybody here from DOE who might know where to steer Mr. Williams to find answers to these questions?

(No response.)

COHON: We don't, but we'll be happy to try to find out for you. Right? Yes, we will find out and get back to you with a name and a contact.

WILLIAMS: Thank you.

COHON: Thank you. Is there anybody else who would like to comment or ask a question?

(No response.)

COHON: Well, thank you all for participating over the last day and a half, especially to all of our speakers and to the commenters.

We stand adjourned.

(Whereupon, at 6:00 p.m., the meeting was adjourned.)