UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER 2001 BOARD MEETING

SCIENTIFIC AND TECHNICAL ISSUES

Wednesday, January 31, 2001

Longstreet Inn
HCR 70
Box 559
Amargosa Valley, Nevada
(775) 372-1777
Fax: (775) 372-1280

BOARD MEMBERS PRESENT

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

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

NWTRB STAFF

Dr. William Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Ayako Kurihara, Editor
Linda Hiatt, Management Analyst
Linda Coultry, Staff Assistant

I N D E X

PAGE NO.

P R O C E E D I N G S

(8:05 a.m.)

COHON: Good morning. Welcome to the second day of our Board meeting. We hope that today will be as stimulating and valuable as yesterday was.

Chairing today's meeting will be Board Member John Arendt. John?

ARENDT: Thank you, Jerry. Today's session returns to the Board's usual format. The session consists of three parts. The first part takes place this morning. The Board shall hear three updates from the DOE. The updates address the project's ongoing scientific and technical investigations, its work in the repository design, and a new study that attempts to characterize uncertainties in performance assessment.

The second part of the meeting begins just after lunch. The DOE will talk about two efforts that will be critical ingredients in developing a safety case for the proposed Yucca Mountain Repository. The first presentation looks at the issues associated with creating learning organizations. The second presentation describes the DOE's latest revision of the repository safety strategy. The third part of the meeting consists of two presentations by groups that are working with the DOE on characterizing and evaluating the proposed Yucca Mountain Repository. The Board will hear about the scientific investigations conducted by Nye County in cooperation with the DOE. We will then hear about a performance assessment of the proposed repository carried out by the Electric Power Research Institute.

We also will have two opportunities for public comment; one just before lunch and one at the conclusion of this session.

Our first speaker is Mark Peters. Mark is from Los Alamos National Laboratory, Testing and Engineering Support Office Manager. He has his PhD in geophysical sciences from the University of Chicago. He is responsible for integrating natural environment testing program. Mark?

PETERS: Thanks for having me again this morning. Thanks, Mr. Chairman. I'm going to give this morning, I think, what you all have heard me give several times now, the past several Board meetings; a whirlwind tour through the testing program. A lot of material, but I also have a lot of time. So, I'm going to try to march through it methodically. As always, if you have questions during, please ask. We've got a lot of time for questions afterwards, it looks like.

I'm going to try to cover the highlights of the program. So, obviously, I can't go into some of the details in the presentation, but we can talk about that in the questions and there is a lot of folks in the audience who can help answer some more detailed questions. A lot of the scientists are out in the audience if I need assistance in answering questions.

So, I've already given you the overview. I'm providing status on the scientific and engineering testing program in support of the process models and design, focusing on the key processes, and reducing key areas of uncertainty.

I should also say that yesterday Bo, Al, and Gerry touched a lot on a lot of the ongoing tests that we're using to improve our models and reduce uncertainties. There will be some repetitiveness. I'll also provide more details on some areas. I tried to match this up well with the presentations yesterday.

I should also say, as I go through, obviously, this isn't my work. I mean, I'm talking about work done by the national labs and the USGS. I'll try to mention names and organizations as I go. I sometimes forget; so, please, forgive me for those in the audience who I forget to mention. But, if you have any questions on who the performers are, please ask and I can tell you.

I've structured at this time, broken it up into the unsaturated zone studies looking at the ESF studies, the thermal test, as well as an update on ^{36}Cl validation which I know the Board is interested in hearing about. Moving into the cross drift with a lot of the work. Looking at seepage and flow processes in the Topopah Spring and also touch on what we've seen in the Bulkhead Investigations in the cross drift.

An overview of where we're at with Busted Butte, moving to the Calico Hills section that sits beneath the potential repository, and then move into the saturated zone. Have some discussion of lithostratigraphy results that we're getting out of the work and cooperation in Nye County in the early warning drilling program, and also an update on where we're at with the alluvial testing complex. Nye County will give a presentation this afternoon, as well. So, questions concerning their program, I may defer some of those to them this afternoon, but I'll be happy to answer as much as I can.

Moving into the engineered barrier system, two testing programs that are ongoing at the North Las Vegas Atlas Facility--I know some of you all saw those a couple days ago; I guess, it was on Monday on your way out here --the preclosure ventilation test, as well as the column experiment that's been going on for THC at the B4 building over at North Las Vegas. Very high level bullets on the waste package materials testing, I'm not going to go into any detail. Gerry covered that, I think, in gory detail yesterday. And then, just very, very brief of where we're at with waste form testing and then a wrapup.

So, starting with the unsaturated zone, the underground testing program, you've seen this before. This is a plan of the exploratory studies facility, north ramp, main drift, and south ramp; the potential repository block to the west of the SF; and the cross drift here in red going out over top of the block and across the main display of the Solitario Canyon.

I'm going to talk mainly today in the ESF portion about Alcove 5, the drift scale test, and also about the ^{36}Cl validation where we've looked at samples both across the Sundance Fault here at Alcove 6, and also the Drill Hole Wash Fault. I'm going to focus on--we've been focusing on the Sundance, but we've also sampled the Drill Hole Wash Fault structure in support of the ^{36}Cl validation study. I'll show a detailed blowup of the cross drift when I move into that section of the talk. So, we'll get into that in a little while.

First, the drift scale test diagram that I've used in all the presentations just to remind you all what the test looks like; an observation drift, a connecting drift, with the heated drift area here. Nine canister heaters end-to-end in the heated drift and the 25 wing heaters on each side that are heating up the rock with boreholes both within the heated drift, as well as off the observation drift.

I'd like to put this in just to remind everybody of where we're at. We're a little over three years into the heating phase. We're scheduled to start the cooling phase December of this calendar year. We've turned down the power three times now, total power three times, to maintain the drift wall temperatures at approximately 200 degrees Celsius. So, that's where we're at. So, all you're seeing here is time, power on the left, and temperature in degree Celsius on the right. The boiling front is about three meters into the rock right now and we're maintaining at that point.

I've got three slides here that give you a flavor for some of the data that we're collecting and how it compares to predictions. I'm focused here on THC processes, thermal, hydrologic, and chemical. And, also, I'll talk briefly about some analyses that we've done of the saturation data.

Here, I've got two parts, CO_2 parts per million by volume versus time for two boreholes from the observation drift. Both boreholes are drilled up. What you see on the plots are two predictions from the THC model. The base case fracture is limited number of minerals in the thermodynamic data set and then we have an extended data set that includes the aluminal silicates. That's not really important in the details. We can talk about that in the questions. But, we are doing a lot of different conceptual models for predicting THC processes not only in the test, but also in the drift scale THC model that was alluded to yesterday. But, two sets of predictions, along with measurements of CO_2 in the boreholes. And, you can see, particularly in 75, we see the increase in CO_2, we predict the increase in CO_2 and then the subsequent decrease, and we are, in fact, seeing that in the gas concentrations. Here, we see a bendover in the actual data, but in talking to Eric Solenthal, the person who produces these models, he says actually the predictions that we plotted here could be a function of where he picked within the node to plot the temperature. He's actually seeing this thing turn over, as well. So, I wouldn't make too much of the fact that we're seeing this turnover early and the prediction isn't. We do, in fact, see systematics that suggest that we're predicting pretty well the distribution of CO_2 in the concentrations.

Related to CO_2, of course, big control on that. How controlled is the pH of the water? A similar plot here, pH of water collected in the field for two boreholes, two up boreholes again, from the observation drift. Here, we're showing a whole host of conceptual models for the THC model, different ways of treating calcite kinetics, reactive surface area of endophyte and in some cases taking calcite out of the assemblage. Again, I don't want to get lost in the details here. Just know that we're doing a whole series of predictions, and in general, the pH varies. Much like the systematics in the CO_2 cause the variations in the pH, these two outlyers down here happen to be very low volume samples. So, they're probably samples that condensed in the line we were sampling. So, right now, I would ignore those. You can see, in general, we're again predicting very well the pH evolution of the water that's collected in the boreholes.

I'm going to take a couple minutes to explain this. This is a very busy slide, but I want to make a couple points. What we're talking about here is how well are we predicting saturation in the rock as a function of time through the test with three different techniques. You've got a bunch of data on here, but we're comparing different ways of statistically comparing the data, predicted versus measured. Mean difference, root mean square difference plotted over here, and difference from predictions. There's an error on this slide and I shouldn't have saturation here. This is actually the normalized difference by percent from predictions versus measure. Again, we're showing the three different ways. We look at saturations with neutron logging, electric resistivity, and radar. So, there's three different statistical ways of looking at the data and we're basically seeing how well we match the predictions as a function of time. So, you can see in normalized space, we're in a very detailed level basically predicting saturation to the 40 percent level. Okay? So, you look at predictions first as measured and we'll give her about 40 percent. Now, this is a very detailed look at it. This masks the gross redistribution of moisture. If you talk about the gross redistribution of moisture, we're moving it away from the-- we're drying out, no surprise, moving it to the sides and to the bottom. You see that at gross scale, but when you look at the very gory details of the saturations, this gives you a feel for how well we're predicting saturations. Three different techniques.

BULLEN: Bullen, Board. Mark, just a quick question then. Is this also on indication of how well you know where the energy went that you put in because of the saturation predictions? If you integrated all the energy in, what fraction do you know where it ended up?

PETERS: If you look at the temperature field, if did a similar plot for temperatures, we're about 15 percent. Okay? I think that's probably a better way of looking at it. Does that answer it?

BULLEN: So, in other words, you have a pretty good feel for where 85 percent of the energy went and the other 15 percent--

PETERS: Yeah, there's some going out the bulkhead.

BULLEN: Right.

PETERS: And, we're in the process of working through how to quantify that.

BULLEN: Well, I guess, the following question to that is it really the bulkhead or is it the mountain itself breathing or both or--

PETERS: We think a lot of it is the bulkhead.

BULLEN: Okay, thank you.

PETERS: So anyway, again, a lot of detail in here, but the three techniques are giving similar answers for saturation. That's another point. And, this is the kind of analysis that we're going through to try to understand how well our models are predicting saturation and temperature.

Moving into ^{36}Cl validation, I don't need to dwell on the objectives, but I will. We're validating the occurrence of "bomb-pulse" at two locations in ESF; the Sundance again down by Alcove 6, and the Drill Hole Wash up just before you come to the breakout for the cross drift. As you all heard, gosh, it's been, what, last May or June in Pahrump, Livermore and Los Alamos have been doing experiments on some of these validation samples and the data sets, there's some significant differences between the two data sets. So, we set up a path forward that involved collecting a reference sample from the ESF and--well, let me back up. We think a lot of that might be due to how the samples are prepared in terms of leaching in the laboratory. So, we've gone, collected a reference sample and done a series of leaching experiments. Those experiments in terms of looking at Cl and Br concentrations are complete. We have yet to analyze ^{36}Cl on those samples. So, we're not yet ready to say this is the common processing method that we'll use for the rest of the validation samples. We're a couple of months away from being able to do that. So, that's where that's at.

In terms of path forward, once we come up with a common processing method, we'll analyze the additional validation samples. Livermore is also developing a technique to do ^{129}I analyses. The USGS continues to do the tritium analyses that you all have heard about before and we're still planning on wrapping up with a final report later this calendar year.

Moving into the cross drift, something you've seen again before. Here's a blowup of the cross drift here. A couple of things to note. The black bold are testing facilities that exist where there's ongoing tests. The blue italics is facilities that are in the baseline plan, but yet to be constructed. Also, got the contacts for the different subunits of the Topopah Spring noted on the cross drift that you encounter as you go down the cross drift, the upper lithophysal, middle nonlithophysal, the lower lithophysal, and then the lower nonlithophysal up to the main display of the Solitario Canyon Fault running right there. The top update on the crossover alcove work; Alcove 8/Niche 3, also an update on the seepage experiments at Niche 5, and comparison of the air permeability measurements from Niche 5 to those that we saw in the ESF Niche 4 in the middle nonlithophysal.

Move in to talk about the bulkhead studies. As you all are aware, there's three bulkheads constructed in the ECRB; one halfway down, one just before the Solitario Canyon, and, more recently, we've put one up just at the back of the tunnel boring machine. That experiment, it continues. We've basically cut off the ventilation and are watching it return to ambient. We actually entered just last week and so I have some very preliminary observations from what we saw when we went in last week.

Starting with Alcove 8/Niche 3 crossover alcove test, again remember I'll show a diagram of what the test looks like, but this is a test where we're using the geometry. We've got an alcove lined off at the cross drift and we can then exploit the ESF that's underneath and we're doing a large-scale flow and seepage test in the Topopah Spring. Bo alluded to this yesterday. Again, about 18 meters of separation between the two. So, it gives a real good feel for the scaling of a lot of these processes.

This is a schematic diagram showing the way out of the test. Again, Alcove 8 driven off the left side of the ECRB and ESF Niche 3 underneath. We have boreholes drilled down from Alcove 8 and up from Niche 3 to do geophysical logging for monitoring the moisture front and also these holes here, I should point out, blast monitoring holes, we originally started this excavation with drill and blast and so we had a blast monitoring set up. But, this test is ongoing. The idea is there's an infiltration plot in the bottom of Alcove 8 and we're infiltrating water and seeing how it travels through the rock and also how much would enter or seep into Niche 3 underneath.

Here's where we're at with the infiltration test. Right now, we're doing a small--what I'll call a small scoping test. We were doing that on a fault that happens to be in the floor of Alcove 8, the back of Alcove 8. That began in August on this again small plot over a fault. We've applied on the order of 770 liters. You can see the average rate. Maximum rate was two centimeters a day. We've yet to see any seepage into Niche 3. Again, a very small plot on the fault and I want to talk a little bit about maybe why in the next slide.

The fault isn't taking up very much water. At least, here's the small plot that I was alluding to. This is the floor and back end of Alcove 8. There's a fault that runs across and we had this small 70 by 70 centimeter plot here at the fault. It's not taking up much water. There's a lot of smectite in the fault. So, we're having a real hard time. It's probably, likely, swelling up and causing some significant decrease in permeability and we're having a hard time getting it to take water. So, what we've done is recently we did a trench to expose more surface area to try to see if we can get the fault to take up water. Once we move beyond this, we'll also move into a much larger plot that will make up the larger part of the test as we move forward.

Moving to Niche 5, again seepage, of course, is a real key area for us and we've done a lot of work in the middle nonlithophysal in the ESF. Niche 5 is in the lower lithophysal in the cross drift. I want to give a series slides here. Bo alluded to the seepage test and the importance of that for calibrating and validating the seepage model yesterday on a brief update on some detailed data that we're collecting from the niches.

This is a similar pretty diagram showing what Niche 5 looks like, a cross drift coming here, portal is this way. So, we're headed down towards the Solitario in this direction. Remember, in ESF the niches are very small 10 meter niches. Basically, that would be the equivalent of this test area. In Niche 5, we actually excavated an access drift that we then, to get ourselves away from the cross drift, we then did the pre-excavation boreholes and an excavated niche. So, we do a series of air permeability tests, both before and after excavation, and then we're now in the process of ramping up to do the liquid release seepage tests from some of these boreholes above the niche, as we speak.

I'm going to focus on results that we've got from Niche 5 on air permeability pre- and post-excavation and what we see in terms of excavation effects and compare that to what we saw in Niche 4 in the middle nonlithophysal. A lot of what I already said, again, air permeability tests before and after niche excavation, the four niches in the ESF, and in the middle non, and then Niche 5 in the cross drift in the lower lith.

The next three diagrams are permeability versus position in a borehole. For a given borehole, air permeability pre- and post-excavation. Okay? There's lines drawn on that are kind of rolling averages, but I think I want to focus on the individual data points. For Niche 4, two different boreholes. The purple in both cases is pre-excavation and the yellow is post-excavation. You see a systematic increase in air permeability after excavation in the crown of Niche 4.

In the case of Niche 5, much less clear that there's any pronounced different in pre- and post-excavation in air permeability. If you look at the average maybe, but if you follow the individual data, it looks pretty much the same air permeabilities in the crown before and after excavation. If you go to the sidewall, in Niche 5, we have the advantage that we drilled some holes parallel to the niche along the sides and did similar measurements. And, again, indistinguishable, the pre- and post-excavation air permeabilities look very similar in the sidewalls of Niche 5.

So, to wrap up, some preliminary conclusions. Based on the Niche 5/Niche 4 comparisons, the lower lith may not be as sensitive as the middle non to excavation-induced permeability, the hydrological-mechanical effect. The permeability changes may be greater above the ceiling than on the sides due to stress unloading The air K tests are used by the seepage--using the seepage models to look at all the different processes that might affect seepage into the drift. Finally, we're moving forward now with the seepage tests in Niche 5 and we're working very diligently to try to control the relative humidity within the niche during the test so that we maintain as close to ambient relative humidity within the drift as we can. That's been a concern of some of the other tests that we had lower relative humidity and that might have inhibited some of the processes that we might normally see. And, we're doing some considering of looking at some ways to actually try to improve our mass balance; maybe actually excavating slots to try to collect more water to improve our mass balance on seepage. If you don't see it drip, does it go around? Well, how much goes around? That's also an area of uncertainty. So, we're considering options for maybe trying to improve that aspect of the test.

Yes, sir?

COHON: Cohon, Board. Mark, just to put this in context, if you could go back to one of the air permeability --that's fine. For the middle lith where there was difference, how much difference would there have been in comparison to these?

PETERS: Go back to--about an order of magnitude, order of magnitude and half.

COHON: Order of magnitude, okay. Thanks.

PETERS: Okay. In addition to the Niche 5 tests, North Berkeley in conducting a series of tests within the cross drift in the lower lithophysal again. But, here, instead of looking at one test location, Niche 5, we're doing a series of borehole based air permeability and seepage measurements along the length of the lower lith, at least the part that's not behind the bulkhead. So, over about 300 meters or so worth of lower lith, we're able to do regularly spaced boreholes and do borehole based measurements. So, we're conducting these tests in very long boreholes. There's a series of boreholes both drilled at low angles into the crown, as well as holes horizontal off the ribs that we're using for gas tracer measurements, etcetera. But, again, air permeability, liquid release, similar concepts to the niche tests.

This is a schematic of the setup. Again, this is the collar at the crown of the cross strip and you have very long, low angle holes that are packed off in as many as three zones and we're doing zone-specific air permeability and liquid release and then collecting that water in the crown of the drift. So, again, looking at the heterogeneity within the lower lith for the fracture properties and the influence on seepage.

Yvonne Tsang, the principal investigator for this test--and I believe that's Paul Cook, one of the associate investigators--both from Berkeley. This is just to give you a feel of working conditions in the cross drift, also what the layout looks like. This is the injection and control system and this is how they're collecting seepage. So, again, a hole drilled in the crown at a very low angle up into the ceiling basically and then they have locations where they're quantifying or collecting the seepage that drips into the cross drift above.

How is this data used? Bo alluded to this yesterday. When you combine the work with systematic, as well as the niche studies, the air permeability measurements are used to build a heterogeneous permeability field that's input into the drift seepage model. They assume initial values for the hydrologic properties. That's from calibration from the short duration niche tests. Then, they use longer duration niche tests to do the validation exercise with the model. And, we're calibrating a lot of the fracture properties for the lower lith using this data.

For the bulkhead investigations, again, three bulkheads in the cross drift isolating the whole back half about halfway down just before the Solitario Fault is the second bulkhead and then there's another one just at the back of the tunnel boring machine. Remember, in earlier presentations, we were seeing condensation in the cross drift, particularly near the second bulkhead and we evaluated that and decided that the tunnel boring machine, in particular, was probably producing a lot of heat at the back end of the cross drift and might be causing thermal gradients that were leading to the condensation. We didn't think it was dripping from the rock. So, we went in and constructed that third bulkhead. It's in there. It seems to be doing a very good job of isolating that heat source. We just went in last week and still see quite a bit of condensation in the section between the third bulkhead and second bulkhead. So, over about 100 meters right in the area of the Solitario Canyon Fault.

The working hypothesis is that we think it's still condensation and it may be due to the heat source still dissipating within the tunnel. These are very preliminary observations and we need to still evaluate in much more detail about what we're actually seeing. We don't think that there's evidence of a lot of dripping. We still think it has to do with temperature fluctuations and condensation.

I mentioned the bugs, the mold, the slime that was observed back there early-on. A lot of you all saw that when we had to dress you up in all those nice white pretty suits, but that seems to be declining in abundance. That's a qualitative observation.

BULLEN: Bullen, Board. A quick question about that. By declining in abundance, do you mean qualitative observation and the question is do you think that the food supply is going away which is why the bugs are going away or what's your observation as to why it might be declining?

PETERS: I wasn't in there. So, I probably can't answer that right off. We can get you--my guess is is that a lot of it is partly food supplies disappearing because they were feeding off of like stuff that was left behind by the miners, hydraulic fluid that might have been left behind by equipment, and stuff. We aren't introducing a lot of that material any more.

BULLEN: And, are you monitoring--are you taking data on the bugs to determine if that is the case? How are you analyzing the data that you have or is it just strictly observational?

PETERS: This one is strictly a qualitative observation on my part based on what I heard. We are analyzing the bugs. We did a lot of bug collection early-on and Livermore is looking at that extensively and trying to integrate that in.

BULLEN: Thank you.

PETERS: I won't dwell on this, but I talked about the observations that we saw within the tunnel itself. We continue to see re-wetting in the rock. We're doing periodic neutron logging in addition to the instruments that are measuring water potential in the rock and we continue to see re-wetting or returning to ambient conditions within the tunnel.

Okay. Now, moving away from the potential repository block and now down into the lower part of the Topopah Spring and the Calico Hills section, Busted Butte again, to the southeast where we're at with ESF in the cross drift, at the bottom of the Topopah, top of the Calico Hills. Here, we're into hydrologic Calico Hills; so, getting into bedded tuff. So, a much different flow regime than what you have in the Topopah Spring. Objectives of Busted Butte, you've heard these before. I won't dwell on them. Basically, looking at sorption data at the field scale compared to laboratory measurements for some of the key radionuclide analogs that we're using in the test, calibrating and validating the transport model, and again addressing scaling issues.

I have a more detailed diagram that will show up here in the next slide or two that shows the way out of a--just to remind everybody, it's a very short excavation, about 70 meter excavation. Portals here, main adit with a test adit. The Phase I tests which were smaller scoping tests, I won't talk about today. Those are complete. We've talked about those before. You've heard about those before. I'm going to concentrate on what's going on in the larger scale Phase II test block.

Just to remind everyone the tracers that we're using in the two phases of the test; Phase I tracers and again I'm going to focus on Phase II tracers. Phase II, we use these plus these. So, we have a whole series of analogs for some of the key radionuclides of interest at Yucca Mountain on neptunium analogs, plutonium, and americium. And, there was some colloid analogs, some microspheres injected, but at the field scale, we're actually not--we're having some problems with quantifying colloid transport in the test block. I think we can probably talk about in the questions. We're doing other things in colloids to try to address the issue, but the results of the colloid experiments at Busted Butte probably aren't going to be like what we originally hoped when we planned the test.

Detailed layout of the Phase II block. I've got this on an overhead. I think after I walk through here, I'll probably put it up so I can refer back to it. Again, Phase II, there's two injection rates--let me back up. Stratigraphically, what you're looking at; you're looking at the bottom of the Topopah Spring, the welded fractured vitrophere, and a less fractured vitrophere, and then the true bedded Calico Hills. That's the section that we're in here. So, the hydrologic Calico picks up, I believe, right here and down. These are litho stratigraphic nomenclature. But, we've got two injection rates; one in this upper fractured vitrophere and another injection rate down in the bedded Calico Hills. Off the collection, for the main adit, we have a series of collection boreholes that are drilled below the injection plane. Different injection rates for the tracer soup; 1ml/hr, 50, and some at 10ml/hr. And, again, these show how those break out. What else can I say here? There are some faults in the block which we're in the process of incorporating into the test specific model to try to understand how the faults influence the results.

Where we're at, the Phase II injection stopped at the end of October. So, we've called the injection phase over and we're in the process of going in and doing a post-test characterization of the Phase II block. We did a series of five overcores of injection holes. I'm not going to be able to pull these numbers directly out of my head, but we did two overcores on a 1ml/hr injection hole and two on a

50ml/hr injection hole. So, we're basically overcoring the hole and then chasing the tracer front as it moves down. We also tried one overcore down here in the Calico, but it comes out pretty much like sand and the coverage is very poor. So, it was real hard to get oriented core. So, we're not going to really be able to get much in the way of information from the overcore. What we've got planned right now to start actually any day is a mineback within the Phase II block. What we're going to do is we're going to excavate from back here into the block and then make a left turn and march down towards the injection array. And, similar to what I think you've all seen with the Phase I, we're going to stop periodically, map, take hand auger samples, and then analyze those core in the laboratory and compare that to the pad analyses.

Let me back up, these collection holes, remember have a liner system, and they have absorbent pads and we can harvest those pads and get as a function of time tracer concentrations as a function of time. Then, the core, we'll get us the picture at the end and we compare those.

Okay. So, that's the kind of information that we're collecting. Again, the mineback will start in February and we'll have a lot more information, particularly on the travel distance of the reactive tracers. We've got a lot of information on the conservative, but we haven't seen breakthrough of most of the reactive on the pads. So, the mineback is real key for that, particularly down in the Calico because of the problems with the overcore.

A lot of what I already said. Analyzing tracers in these cores what isn't straightforward. So, we've done a lot of development of technique to be able to analyze the rock samples for tracers. That's complete. We did some preliminary overcoring last fiscal year and we've already analyzed some of those core samples. I've got an example of some of that data in the next slide. And, we're going to start analyzing overcore samples immediately.

This is an example. I talked about we did some coring last year. We did a series of three quick cores off the main adit. This particular Hole 50 was drilled in this area here and what I was trying to get at is we were trying to get a picture, if we could, of how far the reactors had traveled so that we could make a legitimate call on when to call the end of the injection phase. So, that's why this borehole was drilled. And, what you've got plotted here is concentration versus concentration initial as injected into the borehole as a function of distance along the borehole just comparing core with pad measurements to show you the kind of data that we're going to be collecting, particularly in these overcores and also looking at in the mineback.

A couple points, in general, the core and the pad give similar answers for concentrations. This happens to be for a fluorobenzoic acid tracer. But, this is the kind of information that we'll be collecting from the cores and comparing that to the pads as we go through the overcore mineback program.

I showed last meeting some comparisons of results versus predictions for the test-specific model. This is just another example of that. Here, we've got concentration, again normalized concentration, for Borehole 46. Sorry to be turning this on and off so much. That would be this borehole here. So, it's along and below the lower injection array in the Calico Hills. What it is is a series of time slices as we get normalized concentration as a function of distance along the whole as a function of time. Two different models in red and blue and then the actual pad analyses in black. This happens to be--I'm sorry, I didn't even put that on there--it happens to be for lithium. I should have told you what the tracer was. It's for lithium. So, it's slightly reactive. But, there's two different model simulations. We assumed what the design injections were in the four holes, 24, 25, 26, and 27 above. The actual injections happen to be lower as measured in the field. So, that's why there's two different model simulations. One is an as-built to the injection array. In general, we do a good job of predicting. In some cases, we over-predict; in other cases, we actually do quite a good job of predicting the quantitative concentrations of the tracer. When we have differences like that, we're in the process of looking at our conceptual model to try to improve our predictive capability.

So, from the test modeling, I think right now the Los Alamos folks are making several conclusions. A good overall agreement between the models and the data. Actual measured concentrations, agreement varies. We are working on enhancing the grid. I talked about the fault and some other things that we'll incorporate into the model to improve our predictive capability for the test. There's some things that aren't yet in the test-specific model. Heterogeneity, that seems to be important to improve our predictions. Finally, this last bullet, it shouldn't be a surprise. When you look at laboratory measured hydrologic properties when you go do a field test, they don't always give the same answer. So, I think this underscores the need for doing field tests like this to improve our confidence in the laboratory measured hydrologic properties and understand the differences.

Now, moving on to the saturated zone. The focus of our program, as you all know, almost completely on work that we're doing in cooperation with the Nye County program. Nye County, I know, is going to talk this afternoon and so I will no steal their thunder, but we are working cooperatively with Nye County. We're collecting a lot of data as a project, in addition to trying to use some of the information that Nye County has collected to incorporate into the SZ model. You heard a lot about that from Al yesterday.

This shows a layout of the both completed and planned Nye County program. US-95 running up here towards Beatty, Yucca Mountain up here, defensive boreholes that you're familiar with along US-95. We're going to talk quite a bit about the alluvial testing complex. The centerpiece of that is 19-D which is located right here just to the north of US-95. Then, there's also plans to continue Phase III of the program and I think you'll hear a lot more about that from Nye County this afternoon.

Back up for a second. We're collecting a whole lot of data and I'm not going to be able to give it all the credit that it deserves. Today, I'm going to give you some slides on what we're learning in terms of lithologic distributions and how that's improving our understanding in hydrogeologic framework and also a little bit about sorption measurements in relation to the transport and hydraulic testing going on in the alluvial aquifer at the testing complex. Again, reminder, Nye County is collecting all the information; the project is, as well. This is all being incorporated into models, when appropriate.

Talking about the litho stratigraphy first. We're learning a lot about the distribution and how lithologies change, thicknesses, what pinches out, what doesn't pinch out as you move to the south of Yucca Mountain using information from the Nye County drill holes. This is a table that Rick Spangler provided that shows basically the stratigraphic units that we might encounter in the south of Yucca Mountain downward where the Nye County holes are being drilled, age, as well as thicknesses, and the different stratigraphic symbols. Bottom line is we're seeing a lot of these older tertiary tuffs in relatively significant thicknesses in the Nye County boreholes. But, in the case of a lot of the units that you're more familiar with up near central Yucca Mountain, they either don't exist or they're hard to pick out or they're very thin. No surprise we're moving away from the eruptive center and so the welded units are getting thinner and we're picking up more fall units, but we're also starting to get intercalated sediments in with these. But, this is the kind of information that we're able to collect and prove our hydrologic framework.

Correlation diagram for--go back to the map; that one right there. I'll show you a correlation diagram kind of running along through here. Okay? So, it's a stratigraphic correlation diagram looking at what we see in the boreholes and correlating that borehole to borehole. Okay. Go back now.

Again, this is basically up north on 95 and kind o moving to the south on 95 if I've got that right in my head, but shows the distribution particularly of those overtops and how they correlate between boreholes. These shallow boreholes, we didn't go deep enough, and in some cases, you see a lot of pinching out of a lot of these units. So, there's a lot of changes in stratigraphy as you move from north to south along 95. This is all work-in-progress. This gets incorporated into the hydrogeologic framework.

So, a lot of it, I've already said. The central part of Yucca Mountain, you get the major flow deposits separated by significant thicknesses of in fall deposits; you know, Topopah, PTN, that kind of relationship. If you move south down towards 95, you get a combination of fall deposits, you lose these significant flow deposits, and you get reworked sedimentary rocks within these fall deposits or you get just sedimentary rocks with no fall material. So, there's heterogeneity in the stratigraphic structure to the south. No surprise, but still important to characterize from an uncertainty perspective.

NELSON: Nelson, Board. We went on a field trip--which thank you very much if you had anything to do with it because it was wonderful--on Monday and really became totally immersed in the idea of how, I think, these units vary and in many ways why and how a lot of what we see is so dependent upon what the topography was at the time of an event and also the proximity to the eruptive center. So, the next result is, as you get more distant from the source, you start getting a lot of reworking as you're observing them and introducing a lot of heterogeneity. Do you have hopes to be able to bound that kind of heterogeneity in a meaningful manner to fit into an understanding of the hydrology?

PETERS: Well, yeah, I mean, the ongoing data collection, particularly in Phase II and Phase III, is going to reduce those uncertainties, particularly as we're talking right now, if we move further up the wash, that will reduce the key area of uncertainty. But, if you talk about the saturated zone model, the key is the whole alluvial uncertainty Al alluded to yesterday. Where does the alluvium pick up in the tuff? Where does it enter the alluvium and where are the flow past there? I guess, what I'm trying to say is, yes, we're going to collect additional data. The question for the modelers is when. You know, there's always going to be uncertainty. I'm probably not the right person to answer how much uncertainty can we live with in modeling and PA space, but the data we're collecting in the borehole is going to help with that.

NELSON: I almost suspect after having discussions out in the field on Monday that a few boreholes will actually introduce an appreciation of more variability. And so, it might become more complex and more difficult to predict, the more information you get.

PETERS: Well, right now--well, what should I say? Yes? I mean, this is science. I mean--

PARIZEK: Could I weigh in on this just a minute? This was for later, but what I see is the fact that the rock straight south of the footprint are becoming more alluvial-like, and therefore, that's good for transport. And, Al in a minute will say, well, his uncertainty box didn't spread that far to the west of Forty Mile Wash, but maybe the uncertainty there is to our benefit. It's more alluvium-like or unconsolidated-like as reworked fall deposits. This is what I'm seeing coming out of this.

COHON: Richard, what does good for transport mean? You mean, it's slower?

PARIZEK: Slow it down. We want to slow it down.

PETERS: Maybe we should talk about the questions. Is that okay or do you want to finish talking about that now?

ARENDT: No, let's take it in questions.

PETERS: Okay. But, I want to say one more thing. I don't care what you're talking about. We can always say that the more you do, you're always going to have surprises. I mean, I guess, you asked a very difficult question to answer and I know you know that. I guess, the distribution of alluvium, as Dick pointed out, is the key. How heterogeneous, for example, an air fall deposit is, particularly if they're all in the alluvial aquifer. Does it really matter? So, I think, you've got to overlay what really matters from a modeling perspective because we can always drill more holes and learn more about details. But, the question is does it really matter?

NELSON; Well, we can talk about this at the break.

PETERS: Okay. Moving on to the alluvial testing complex, again, this is just showing a select number of Nye County holes. US-95, 19-D here, the potential repository here to the north and one potential flow pathway. You can see 19-D is along one of those potential flow pathways and, as you'll see in the next diagram, it happens to have alluvium below the water table. So, this is a stratigraphic column for 19-D/D1, 19-P; the pair of holes drilled at that location just north of 95 showing the Valley Fill deposits with the--metric surface, as well as the tuffs and tertiary sedimentary section. This doesn't go to carbonates; the carbonates are much deeper, if we would hit them, at all.

Shown on the left hand side here are the different testing intervals that were screened off to do the hydraulic testing. Nye County did a open hole test. They can talk about that this afternoon. Again, this is the centerpiece of what will become the multi-hole alluvial testing complex. We isolated off four intervals within the alluvial aquifer and we've done isolated interval hydraulic pump testing. We're also now in the midst of doing single hole tracer tests. So, push/pull, inject/pump back type tracer tests. The plan is for Phase III, Nye County, they will drill a series of additional boreholes and will do multi-hole tests where this will be the pump well for that complex.

A lot of what I already said, the single-well hydraulic tests are complete. Hydraulic conductivities in this range, permeabilities are on the order of darcy to 10 darcies, in that kind of range for the alluvial aquifer here. Again, we've completed two of the three single-well tests. We're looking at fluorobenzics and bromide type tracers. As we move into the multi-hole test, we'll increase the tracer sweep and also include analogs for colloids, etcetera. The three tests have different shut-in times. We inject, leave it set, and then pump back. So, we just started over this past weekend and I believe it's got 30 days of shut-in and then a 60 day pump back. So, it's a relatively long-term single-well test.

Preliminary results, Al alluded to this yesterday, insignificant diffusion from flowing groundwater into the stagnant water. It's an advection-dominated system. There is some dispersion along the flow path. I think you saw that in some of the simulations that Al showed. I think you saw the carbon-14 being disbursed along the flow path. We're also working to quantify the effective porosity from the test results. But, implication for TSPA, this was touched on by Al yesterday. Use of a single-porosity continuum transport model is acceptable for alluvium based on what we're seeing at the ATC.

A lot of what I've already said, again Nye County will, I think, touch on their plans for this year in a lot more detail. But, the plan right now is to do a series of injection and monitoring wells and those will be installed this year for the beginning of the multi-well tests. And, again, looking at scaling and getting the same kind of parameters that we're getting out of the single-well test, but at a larger scale. And, also, trying to look at colloidal transport.

We're doing a series of batch sorption and dynamic column sorption type tests in the laboratory at Los Alamos to compliment the field scale studies at the ATC. We've done sorption experiments with iodine, technetium, and neptunium. Those have been the ones that we've concentrated on. Under oxidizing conditions, we basically see results that are real hard to distinguish from zero, in terms of sorption; the iodine, technetium, and alluvium, whereas with neptunium we do see some sorption, no surprise. It's dependent on the smectite and zeolite content. But, as we've concluded in prior experiments, when you look at a column experiment, it shows less retardation than you see in a batch experiment.

That's real important to understand and particularly to compare to the field experiments when you talk about what Kd's you're using in the process and PA models. But, again, this will compliment the field scale studies.

That was my quick tour through the natural system. Now, I'll do an even quicker tour through the engineered system.

In terms of ongoing testing, we'll start with the engineered barrier within the drift, the ventilation test that's ongoing at the Atlas Facility in North Las Vegas. Some of you all saw that on Monday. Again, here, we're looking at preclosure. We're providing date for validation of the preclosure ventilation model. We have a test design. I have some pictures in the next slide, but don't go there yet, though. In terms of design, it's a very long simulated drift, concrete culvert pipes with simulated waste packages, 25 of them, basically end-to-end the whole length. There's a crushed tuff invert. We're doing a whole series of measurements at the inlet throughout the test section at the outlet. Again, intake air, we turn on the heaters, bring in air, and see how the temperatures vary and what the temperature of the air is at the outlet.

We'll talk a little bit about the details. This is some pictures from the field. Again, the concrete culvert pipe, looking down the pipe with the simulated waste packages. This, I believe, is the inlet end and shows some of the scientists putting the insulation on the outside of the pipe.

Phases of the test. The first phase, we simply suck air from the ambient room. We try to control the conditions within the room. And then, we had ambient air for input and it was exhausted at the end. We did a test matrix of six tests and that was completed in December. What you all saw, if you were there on Tuesday, is we're reconfiguring the test. We're almost finished with reconfiguring the test to recirculate the air.

Now, we're going to look at controlling temperature and relative humidity at the inlet and do a test matrix walking through looking at variability of flow rate, temperature, relative humidity, and the whole series of experiments like that. We're also improving some of the sensors in the test again to control the air and humidity at the inlet and do a better job of measuring air temperature and also try to get an idea of the heat flux through the concrete through the boundary. And, really Phase II and Phase III is a combined set of tests looking again at variations on all those variables.

I should also say this was a scale test. It's not a full scale emplacement drift. It's scaled down. The thermal input is scaled down, air flow, and then we conducted a series of six tests anywhere from six to 10 days. We compared it with the preclosure ventilation model, the ANSYS code simulations, and in general, they compared well with the predictions. Predicted air temperature rise within around 20 percent. We're able to predict the measured peak temperatures on the mock waste packages. And, in general, although slightly lower, we were pretty good at predicting the temperature on the inner surface of the concrete pipe.

So, Phase II and Phase III are in the final throws of being prepared to start. They should start in February or the very beginning of March.

Also, at North Las Vegas Facility, we've done a series of column experiments with crushed tuff. Again, this is to generate data for validating the THC predictive models, particularly for the in-drift chemistry models. These were a series of crushed tuff columns and we're looking to characterize processes like how is the permeability altered, what happens to the pH of the water as a function of time and variables such as that.

This is a schematic diagram of what that looks like. This is about a meter high. Some of you all saw this the other day. I think we're in the process of dismantling this right now to try to characterize mineralogy. It's complete. You've got a heat source at the bottom, a cold vent at the top to periodically sample gas, and the way that this works is you heat the bottom and you set up a refluxing condition at the top.

Test 3 again was crushed tuff invert from out at the ESF. And, again, sample gas is a function of time. And then, we can take this apart, characterize the mineralogy and see how the permeability might have been altered, see what the mineralogy has changed to, and also characterize how the permeability might have been altered.

Just a picture of that same test again, the meter high column right here.

Test 3 again was crushed tuff from ESF. We did Tests 1 and 2 with similar samples. We had some difficulties with those first two tests. This third test worked out very well. We did set up a refluxing condition. We had boiling throughout the column except at the very top air space into the cooling cap. We basically had a closed loop heat pipe. We had very little gas loss. It basically reached steady-state geochemical conditions. The pH rose from 9 and stabilized between 10 and 11. Again, we did CO_2 analysis and then we're in the process of dismantling the column to look at mineralogic-petrologic effects.

Right now, there's no intent to do additional column experiments at the Atlas Facility. We feel like we've got enough information right now to, at least, take a first cut at looking at what it means for the in-drift chemistry models. We need to compare the results of this, particularly when you talk about pH evolution, with what we see in the thermal tests in the field. We don't see elevation of pH nearly to that level and we also don't see--effects when you go to a field scale experiment.

Waste package materials--I'm almost finished--I won't dwell on this. Gerry talked a lot yesterday about what we're doing in the waste package materials area; long-term tests at the corrosion test facility, coupons put through completely immersed, looking at vapor corrosion type processes, both general and localized crevice corrosion, stretch corrosion cracking. We're looking at a whole host of materials; titanium which is the drip shield material, Alloy 22 for the outer barrier of the waste package, different geometries, U-bend, looking at different manufactured welds. The test conditions are bounding in several areas, we think; temperature, the ionic strength, and the pH. You know, we're using weight loss techniques, microscopic techniques, particularly look at passive field stability. Again, I won't go into detail. Gerry touched on a lot of that in great detail. He's much more qualified than I to talk through the details.

Waste form, two very high level bullets that we are, in fact, continuing the waste form testing program. It's focused on the drip tests with emphasis on looking at colloid generation off the waste forms, spent fuel and glass. We're continuing to characterize the secondary phases, particularly in spent fuel and how that affects solubility limits, etcetera, in the spent fuel waste form.

So, to wrap up very fast, hopefully not too fast, I touched on a lot of information on what we're doing in the testing program in the ESF, the cross drift, at the Atlas Facility, B4 facility, as well as in the laboratory, and we feel that it continues to address the key processes and the related uncertainties. A lot of the data collected and analyzed that I discussed will be incorporated into the SR, as appropriate.

That was all I had.

ARENDT: Questions, Board?

PARIZEK: Parizek, Board. On Figure 51, I don't know how hot the disk was on that heater experiment. I did watch, but I don't quite know what that means in temperature.

PETERS: I don't remember, Dick, the exact temperature of the disk at the bottom.

PARIZEK: Is it like a waste package simulation--

PETERS: I'll have to ask somebody and get back. I'm not sure exactly the temperature.

PARIZEK: Another question about--well, it has to do with the Shadow Zone and another thing on the angle. The low angle on Page 24 which was Yvonne's experiment, I didn't quite know how far the roof separation is when you finally get to the end of that angle hole.

PETERS: When you get to the back of that hole, probably--it's a really long angle. It's probably a couple meters.

PARIZEK: So, it's more than the drill back type experiments where you only had half a meter?

PETERS: Yes. Yes.

PARIZEK: So, you get a little bit more roof cover there.

PETERS: Right.

PARIZEK: There was another question about Bo's Shadow Zone. I was wondering again about how to get at that. Obviously, in the drift scale heater experiment, that's so dynamic and ongoing, that's not the place to look for his shadow underneath here. But, is it possible that the large lithophysal cavities might provide such a shadow zone? This is a general question maybe for Bo or anyone else because to find his shadow is probably pretty important to the program. How big a lithophysal cavity have you ever found and is that big enough because he was talking about maybe doing a laboratory simulation by building a little model, a sand model or something, and that seems like that would be the less realistic than maybe some field situation, such as a tunnel or a big lithophysal cavity that's been there for millions of years.

PETERS: It's an interesting suggestion. I mean, we've been talking some about how we could test it. Let me come back to lithophysal cavity--

PARIZEK: --but I'm not sure that's appropriate.

PETERS: Yeah, it's tough on scaling. Let me come back to--I mean, Bo mentioned yesterday the possibility of going out for an analog site or something like that. I personally am having a real hard time conceiving of how we can do something--a test in the tunnel where we go excavate something and look for that effect.

PARIZEK: It has to do with really the colloid and colloid migration. Obviously, the Busted Butte experiment is a tough place to quantify migration of colloids. And so, assuming that experiment doesn't produce reliable results, colloid transport in the unsaturated zone seems to be an important problem. Tons of colloids will be produced when the waste form and the waste packages degrade through time. And, that doesn't mean that they'll get transported to the saturated zone and you definitely have colloids in the saturated zone.

PETERS: Right.

PARIZEK: So, how else to get at that? It seems to me if you have like some of these injection experiments between the cross drift, for instance, you should be able to capture water and test to see if, in fact, there are particulates in that water as filtered samples, as one example.

And, the other question was whether you could really pick this up out of the secondary minerals. I raised that question, I guess, a couple of meetings ago through the group that's looking at the secondary minerals business. There, you've got millions of years of history tracking secondary minerals. Well, are there colloid particles in there, other than the silicas--and that sort of thing? So, again, we're looking for some independent or new way to get at this colloid transport question in the unsaturated zone because it could be a fantastic filter for colloids. But, what's the evidence for that and does it matter? I think it does in the modeling and I think I understand more how the colloid data was put into the present models in the unsaturated zone.

PETERS: There's about four or five questions there.

PARIZEK: You may not need to answer them right now, but they're things that--

PETERS: First of all, Busted Butte, problems that we'll soon be having with the field components is the colloid transport seems to be highly dependent on the composition of the injected fluid. So, what appears to be happening is the colloids is never making the drop. They may be actually falling out before they even get into the rock. So, we're doing column experiments with both crushed rock from Busted Butte, as well as we're about to try to get an intact rock to try to do an intact column experiment to try to, at least, get some information on colloid transport in the Busted Butte rocks, in Calico Hills-like rocks.

Now, in the Topopah, everything you say, I don't disagree with. We did not look at colloid in Alcove 1, but in Alcove 8 test, we're looking at possibly increasing the amount of tracers and it's a good suggestion right now because we can certainly seriously consider looking at colloid type transport in the Alcove 8 experiment, you know, and there you're looking at travel through fracture welded tuffs.

In terms of how it's incorporated into the models, I'd have to defer to Bo or someone else about the modeling component of how we're handling colloids right now based on what we understand.

BODVARSSON: This is Bo Bodvarsson. Let me just add a little bit to it. There is actually in the plan to add the colloids component or look into it for Alcove 8/Niche 3 just like Mark mentioned. It's already being planned, number one.

Number two, like I mentioned yesterday, and this is being incorporated into PA, if the Shadow Zone turns out to be a real phenomena that some of us believe, the issue with colloids may become much, much less than it is now because diffusion into the matrix blocks with these tiny poor sizes is not possible for colloids. And, therefore, where you don't have seepage, you may not have any colloidal transport or--to reduce colloidal transport.

The incorporation into the models, like always, we are planning to predict and are predicting the Alcove 8/Niche 3 experiments. We are planning to do the same thing for the colloids if the project decides to put colloids in the Alcove 8/Niche 3 experiments. Predict it and then compare and see how we have to adjust our modeling approaches, as necessary.

BULLEN: Bullen, Board. Bo, before you leave the microphone, I do have a followon question to this viewgraph that's up here. That is that you model 15 percent of the repository with seeps. Are the data that you're getting from Yvonne Tsang's experiments a justification for that 15 percent or where does the 15 percent come from?

BODVARSSON: The 15 percent or so comes from all the seepage data that had been collected, not only in the middle nonlithophysal, but also in the lower lithophysal. The seepage data seem to suggest that the lower lithophysal has a considerably higher seepage threshold than the middle nonlithophysal. So, we take all this information and we do systematic viability and uncertainties are important parameters which is in an AMR--that looks at the seepage model for PA. That is then abstracted by Mike Wilson at

Sandia to conclude that 50 percent will see seeps based on the climate variations, etcetera, etcetera. That's how it goes in the TSPA.

BULLEN: Thank you.

PETERS: Dick, about the colloids and the fracture amounts, you know, we talked about that. Actually, Zell is in the audience, but I asked him about that just before I got up here. Based on what they're looking at with the U-series stuff, you would expect when you analyze a calcite or an opal, you might see elevated thorium concentrations because of possibly, you know, silicate and colloid material. He doesn't see any evidence of that in the chemical signatures, anyway. That's just one data point on the whole issue of can you look in the fracture assembly just for that.

BULLEN: Bullen, Board. We had a very nice tour of the Atlas Facility on Monday and got to see the scaled drift test to try and benchmark the ANSYS code and we understand that the purpose is to benchmark. I guess, one of the questions that arose and we're still trying to grapple with is the scaling factor that you chose. Why quarter scale, maybe why so big, what problems do you run into in the dimensional analysis to try and scale up, you know, take a look at Reynold's number and the flow. Can you respond to that one?

PETERS: Probably not as well as modeler could, but we're grappling with the scaling issues. Why quarter scale?

I mean, there's people in the room including any staffer here that could answer that better than me. But, I won't ask John. John, I won't ask you to answer that question. Part of the scaling decision was, you know, what was logical to put together and put it in the building? Why we didn't do two separate scales, we feel that we can address the scaling in this test with modeling exercises. We're trying to deal with the scaling in modeling space. We've scaled the heat input, the size, and all that to a quarter. We're going to have to deal with the modeling space. I don't think we yet know exactly how we're going to deal with the scaling issues for the dimensionless parameters in any detail yet for that test. We're dealing with it right now.

But, if we talk about additional tests that we're considering for like postclosure, convection type tests, and things like that that we're not considering, as we're considering those, we're seriously thinking about doing two different scales to try to get around some of the problems.

BULLEN: And, we understand that and we also understand that, at least a portion of us understand, that the purpose was essentially to benchmark the ANSYS code. I think one of the comments that you chose or you made was as opposed to predicting performance of the mountain, you're basically trying to benchmark the code so that you can use that to predict it and so you have a basis for it.

PETERS: Right. Right.

BULLEN: Is that not correct?

PETERS: That's correct. If I wasn't clear, that's what I meant.

BULLEN: Okay. And then, as a question about the column test, a very interesting test saw the mineralization of the lower area right above the heater. I guess, the question that I have is you've decided that you have enough data because no--

PETERS: I shouldn't have stated it quite so strongly. I think we need to step back and evaluate that test in the context of what we were trying to get at for validating the in-drift chemistry model and also compare it to what we see in thermal tests in the field. You know, it's a crushed tuff experiment in a column. We saw certain phenomena. I think we need to step back and evaluate that in the context of everything else.

BULLEN: Okay. I guess, the followon question to that is that of Greg Gdowski at Livermore was doing dripping experiments onto metal that had flowed through crushed tuff. Are you going to do the comparison of the mineralization on the surface of the metal to the mineralization that you saw in the bottom of the column and see if you kind of get the same stuff?

PETERS: Good suggestion. I haven't gotten into that detail, but we'll certainly consider that. I think that's a great idea, yeah.

BULLEN: Okay. Thank you.

NELSON: Nelson, Board. Mark, when is the thermal test in the lower lith scheduled for?

PETERS: Right now in the plan, it would start excavation in the next fiscal year.

NELSON: Next fiscal year with results in the middle of 2002?

PETERS: Its current schedule, we would turn on heaters. We would turn heaters in late fiscal year '02. Right now, we envision a nine month heating phase and a six month cooling phase. So, heating phase results, '03 time frame.

NELSON: Okay. Let me ask you one other thing relating to drift degradation and the rock. What are you doing to evaluate both the material degradation that might be associated with temperature changes, some of them fairly quick for rapid quench options? Is the rock sensitive to that and likely to decrepitate during that event? And, maybe also during a heat-up, the stiffness of the rock mass, we haven't heard very much about evaluation of stiffness of the rock mass. There were a couple of plate load tests that were run, not much borehole work in terms of evaluating stiffness, borehole jacks, anything that might give you an idea of that which would give the response of the rock around the tunnel to heat up. Are you planning on doing any borehole work to evaluate rock mass stiffness at that scale?

PETERS: Right now, all we really have in the testing area in that area is what we're getting in the drift scale test from heating up a drift. There's a plate load there. And then, there's similar type measurements envisioned for the cross drift thermal test, two locations.

NELSON: But, those are every expensive and few?

PETERS: Yeah.

NELSON: The idea of getting an idea of how variable the rock mass is from that perspective, are there any plans to look at that, particularly in the lith that really hasn't been tested very much.

PETERS: There's right now no plans to do any kind of borehole jack, base measurements, or any kind of thermal--no.

NELSON: Okay.

PETERS: I guess, we always have to ask ourselves do we really need that? That's something we can talk about maybe online.

NELSON: Right. And, the rock deterioration associated with thermal--

PETERS: Well, a lot of that's an analysis space. You know, we're analyzing all those processes, but you're asking me about testing programs, right?

NELSON: Actually, just evaluating whether the rock is sensitive to thermal changes.

PETERS: Well, we're doing a lot of that analysis. Let me be real clear. I should answer that first. But, in terms of the testing program, we've gotten two thermal tests and that's really the extent of the program.

NELSON: But, nothing working with intact rock pieces just to see--

PETERS: In terms of rock properties? We're looking at possibly looking at thermal conductivity and some other things related to thermal conductivity, but not mechanical--what I'll call more mechanical--thermal/mechanical. We've got a lot of data on that though already from borehole base measurements.

NELSON: In the lith?

PETERS: Well, not as much in the lift, but there's some limited data. I mean, it's documented in--probably pointers to it in the rock properties AMR, but we've got some data from boreholes from the lith.

NELSON: Okay.

PETERS: We can certainly probably let you have a look at that and at least evaluate how much we've got.

SAGÜÉS: Yes, this is really more of a general methodology question. I looked, for example, at parts of the saturated column test and you mention here results from Test #3. How much of an emphasis do you place in most of these sort of bench scale and small scale tests on the reproducibility of the results? As you know very well, they pack the column and maybe it's packed a little bit tighter this time than the other. Are these one-shot tests or are you reproducing them?

PETERS: Well, in the case of these column tests, 1 and 2, there was some difficulties we had with the material that was used to pack the columns. So, it's hard to compare. So, I think the answer specifically to this one is we've got column 3, and if we decide we need to reproduce, we need to do additional test. Let me ask you a question. Are you getting after if we pack it different or if it's--how we pack the column could affect the results?

SAGÜÉS: Well, what I'm saying is like in any experimental setup, there's the question of reproducibility. You may get results that may look just very nice, but if this is not reproduced, then you have the question as to whether those results would come out the same if the experiment is done again. And, this kind of test is already getting to the scale that it is not like a 200 foot long thing that you may replicate. So, how do you address in all these tests the idea of revolution experiments which is one of the most fundamental--of scientific research.

PETERS: I mean, if the individual investigator feels that there's a need to reproduce the experiment, they'll absolutely do it. I mean, it's probably almost--I'd have to answer that case by case. We'd have to walk through every one and address that issue. We've done a lot of column experiments in the past of this nature. I would rather go back and evaluate what this did compared to Greg--you know, Greg has done some dripping stuff, but there's also been a lot of stuff done at Livermore with columns and we need to go back and evaluate to see if we even need to reproduce. I'm not really answering your question right now--case by case.

SAGÜÉS: Case by case basis and--

PETERS: And, trust the scientists, who I consider world class on the program, to make those determinations.

SAGÜÉS: Okay, thank you.

RUNNELLS: Runnells, Board. I have a couple of questions. The first one is a clarification. Could we look at Figure 26? That's a vertical cross-section, I guess, of the rock in the drift?

PETERS: I wasn't trying to portray anything other than just saying that we're using the measurements from the field to build this heterogenous permeability for the calibration/validation modeling of the niche tests. So, that's just heterogenous permeability.

RUNNELLS: It's four orders of magnitude in permeability over distances--those are meters, I guess?

PETERS: Correct.

RUNNELLS: So, over distances of tens of centimeters, perhaps. How do you get that kind of detail, I guess, is my question.

PETERS: Hey, Bo, you're going to have to probably bail me out a little on this. That's Stefan Finsterle's modeling probably and I'm not real clear on how he takes air data.

BODVARSSON: Bo Bodvarsson, Lawrence Berkeley Lab. What we do is the following. We collect air permeability measurements at various scales ranging from one foot intervals and packed intervals in the niches all the way to 10 meters or so in boreholes. A huge amount of air permeability measurements. We take those measurements and we compare the scale effects of these measurements. We calculate correlation lengths. That basically says what is the heterogeneity structure of the median. It depends on how much the permeability varies. The permeability variabilities generally on the order of four orders of magnitude in both middle nonlithophysal and the lower lithophysal. We then based on this measurement construct the permeability frequency diagrams. --what the probability is of having a certain permeability in a randomly oriented space. We then take the permeability diagram and assign randomly to a numerical grid which is shown on the left hand side to generate basically was we have observed at various different scales. So, that's how we think we can replicate the real rock permeability structure in a numerical mode.

RUNNELLS: Okay, good. Thanks, Bo. I think that answers my question. It synthesized on the basis though of--it's a statistical synthesis based upon measurements at different scales?

BODVARSSON: Right, right.

RUNNELLS: Okay, good. Thank you. My question that I wanted to address maybe in a little more detail is Slide 10. This is the ^{36}Cl validation. And, we know one of the problems that is being faced by the two laboratories is agreeing upon a method for leaching the rock to get the chloride concentration from the rock to be right. I guess, the word is right; at least, standardized.

PETERS: That's probably a better word.

RUNNELLS: Yeah. So, my question is how do we know when it's right? I can see two laboratories agree upon a procedure that will yield a standard answer or a mechanistic answer based upon a standardized leaching procedure, but what does correct or right mean in that context? How do we know when we've got the answer that means something with regard to ^{36}Cl?

PETERS: That's the $10,000 question, I think. I think if they were here, the first thing they would say--Mike Caffee and Bob Roback would look at you and say you realize we're the only two people in the world who are looking at this problem of crystalline rocks. Mark would also say to you, you know, I realize I throw all this stuff away when I do my normal--this is the gunk. They're looking where chloride sits in a rock and it's very sensitive to how you leach it. The results, if I'd have had time, if you shake this vigorously, you release a lot more chloride than if you just leave it sitting in the beaker for 48 hours. I don't think we know yet what's right. We've got to go analyze the ^{36}Cl first and see how those systematics look and then those two very bright individuals are going to have to come up with what we think is the right answer for Yucca Mountain tuffs. It's a difficult problem. That's why we're working through it very methodically to try to make sure that we get the right answer.

RUNNELLS: Now, the reason we care whether or not it's right is--I mean, early-on in this study, it was because we wanted to know if there was recent bomb pulse water in the repository. I don't think anybody seriously now would argue that the water is dead, that the water is not moving at some moderately fast rate downward through the rock. I mean, we know that from many, many different directions, many lines of evidence other than ^{36}Cl. So, again, I know you've told me in the past, but tell me again, please, why we care at this point in time what the right answer is for ^{36}Cl?

PETERS: Well, I think we care because--this is Mark

Peters' opinion. We got up in front of you all a year ago or over a year ago with two pretty different looking data sets, both collected by the project. I think we have to figure out why this difference is. Let's just take the Los Alamos data set individually. When Bob Roback looks at the validation samples, the numbers that he's getting are very similar to what they got for all the previous work that Jim did. Livermore's data set is much different, as you know. It's down 5250 times 7^{-15} versus 900. If you take Mark Caffee's Livermore data at face value and you never saw Jim's data, it says the pore water, as you mentioned, there's no bomb pulse. We don't care because we're already accounting for the previous Los Alamos data in the model. So, if Mark's right, we're still conservative. I think it gets at the heart of can you reproduce your results. And, I think what we're going through with the leaching, if we can determine why we're getting the differences, I think that's an important step. I think it's confidence in our ability to reproduce measurements. This is a very difficult measurement though. We picked a tough one to reproduce.

RUNNELLS: Right, I understand that. So, a good part of the reason is a scientific one; we want to understand it and we want to demonstrate we can reproduce something. Now, is another answer that it somehow fits into Bo's model? It's some sort of test validation or it eliminates something from the UZ modeling effort? Is that another part of the reason?

PETERS: Where we started, I think it was looking at is there truly evidence for fast paths? That's where we started, but I think when we came up with the two different data sets, we're also bringing in this--what you call scientific, I call bringing back confidence with everything we've collected over the past three or four--but I don't want to go much further than that because I don't know what--I think we're learning a lot. Once we analyze the ^{36}Cl from the leachate sample, we'll know a lot more about the systematics and then be able to say a lot more about where chloride is coming from in the rock and why and whatnot.

KNOPMAN: Knopman, Board. Could we go to Page 11 where you just have a diagram of the drifts?

PETERS: Yes.

KNOPMAN: Talk to us a little bit more about the bulkheaded areas. Now, between Bulkheads 2 and 3, you say you just walked in there and you saw a lot of moisture, but you don't think it's seepage. That's in the lower nonlith, right?

PETERS: Correct. It's--go ahead, I'll let you finish.

KNOPMAN: Okay. What I'd like you to do is tell us what you saw between Bulkheads 1 and 2 in the lower lith and try to explain a little bit more why you think you're not seeing seepage and you're only seeing condensation and how would you know?

PETERS: Okay. I didn't see it, but I'll tell you what I was told. I didn't go in myself. People in the audience who saw it, you can expand on it if you need to.

The moisture that we observed was concentrated in this area here. So, as you correctly pointed out, it was in the lower nonlith up to the fault, but then when you go across the fault, you go back into upper lith because of the offset. But, it was in that section, there was condensation. There is drip cloths in that entire section. They run basically from here, the TBM is parked right here. There's drip cloths from here and it also goes 20 meters to this side of that bulkhead. My understanding is there was condensation--the drip cloths were wet.

KNOPMAN: And, the drip cloths are on the floor of the drift or are they hanging?

PETERS: They're hanging.

KNOPMAN: Okay. And, they're soaked?

PETERS: Yeah. So, the question about how can we--we're looking at the data right now, as well, to try to say, okay, fine, if they're wet, how can you actually see if there's a drip or not? That's vary valid question. We've got to work though that. But, there was an awful lot of water and actually something I learned just before that Bo pointed out to me is it appears as if there might be--this back end by the TBM might be starting to dry. So, one hypothesis would be, okay, the thermal gradient is dissipating from that. You're still seeing the influence of that, and with time, you're going to see this whole section dry out and not see condensation in the air.

KNOPMAN: But, do you have temperature monitors in there?

PETERS: We've got temperature monitors all throughout it, wind speed monitors, barometric pressure sensors.

KNOPMAN Okay.

PETERS: And, all kinds of things. There is a lot of data that we've just down--that Dave Hudson from the GS just downloaded over the past couple days, but there's a lot of interesting systematics in the wind speeds and in the barometric pressure and the temperatures that we need to correlate with what we've seen.

KNOPMAN: All right. Now, what about between Bulkheads 1 and 2? That's been sealed off? It's no longer sealed off or what's--

PETERS: This is sealed off, but you don't see the same kind of--you don't see nearly, you know, you don't see the condensation in this section nearly as much. There's a little bit up in here, but the majority of this section is pretty much dry.

KNOPMAN: Well, now, how would you take that or do you think you're still not at equilibrium yet to correlate with the 15 percent, the assumption about 15 percent dripping and 85 percent dry, if you have a stretch that long and you're not seeing dripping yet? Does that cause you to reevaluate that assumption about--

PETERS: Meaning that it could be more like 95 percent not dripping or--

KNOPMAN: Maybe, I don't know.

PETERS: Well, right now, I don't think we're ready to say anything based on this to change that assumption. We isolated this heat source. It appears as if we've done a good job at that. Now, we've got to get this to shake out and understand what's going on here and it may just take some time to get rid of that effect. Then, we need to let this thing run for a while, I would say for quite a while, and just monitor it. And, also, work through, okay, if you're seeing condensation from it, how do you actually see a drift? We've got to work through that. We're talking about that, too. But, these are real time hot-off-the-press things that we're working through.

WONG: Jeff Wong, Board. I'm not Dan Bullen, the Board. I want to go back to ^{36}Cl. I just have a small question. The ^{36}Cl question, I see you say it's going to be resolved by the end of the year. Is it going to be resolved by the time that you issue the SR? Because I look at some of the drafts of the SRCR and the issue of fast paths is not an unquantified uncertainty. So, does that indicate that you're going to have a handle on the contribution of fast paths to infiltration?

PETERS: I'm going to clearly evade that question. I'm not going to--there's a lot of uncertainty, a different kind of uncertainty, with the SR date. That's when we're going to have this resolved. In terms of when the SR released, there's folks in the audience who can better address that. In terms of does it translate into uncertain--why aren't we showing it as an uncertainty now, until we get this resolved we still maintain that the model--the previously collected ^{36}Cl data is what we're basing our conceptual models on. Our conceptual models can explain that. So, right now, we're not changing anything based on this discrepancy of the data sets. But, in terms of when the SR is released versus when this is resolved, I can't address that.

WONG: Well, following along with Don Runnells question about knowing when it's going to be right, that contributes to some uncertainty?

PETERS: Yeah. That's a good point.

WONG: Okay, thanks.

PETERS: Other than--okay, yes, I guess. That's a good point.

DIODATO: Okay. One last question. Diodato, Staff.

All right. First of all, thank you for your usual comprehensive intelligent presentation this morning. I have a couple of questions that came up with regard to the modeling aspects of the hydrogeology. First, with the Busted Butte thing on Slide 36, you talked about the importance of representing the rocks and getting the rocks right in the model. So, that's encouraging to geologists to hear the conclusion that getting the rocks right is critical for the modelers. The question would be in regard to this. What scale or feature do you think it's important to represent in terms of the heterogeneity in this particular experiment to get the simulations to accurately reproduce the transport phenomena?

PETERS: What scale at the Busted Butte experiment?

DIODATO: Yeah, yeah?

PETERS: Well, I think, the two faults that are in the back of the block, we're finding we absolutely need to incorporate. It sort of depends on the rock type, as you know, David.

DIODATO: Right, right.

PETERS: I mean, in the case of the fracture vitophere, we just use a DKM type simulation without accounting for every fracture. But, I think we absolutely have to account for the faults. Bo talked about encountering some faults in the UZ model. So, I'm sort of answering your question in a roundabout way.

DIODATO: Oh, okay. Well, I appreciate the answer. The other question was with regard to Slide 9 and this was the drift scale test results with the saturations.

PETERS: Yes.

DIODATO: I first don't understand exactly the differences between--I guess, you have three different ways of measuring the difference for each test method, each observational method? You have radar and you have different things.

PETERS: Right.

DIODATO: And, you get different qualities of fits for the things and my concern is that here it looks like 20 percent on average maybe, you know, would be the error number. And then, we recognize that relative permeability is highly sensitive to saturation and also capillary pressure function is highly sensitive saturation. Both these things are very critical to modeling and model predictions. So, what I'm wondering is if there's some level of acceptance criteria maybe that the project has in mind for when a model produces and acceptable fit to be reliable enough to use in a predictive capacity. Clearly, you're getting there, it seems like, but is there a goal in terms of a model fit for--

PETERS: In generic acceptance criteria, I'm probably not the right guy to address that. But, I'd say generic acceptance criteria, probably not. You'd have to address that model by model, wouldn't you?

DIODATO: Well, or parameter by parameter, yeah.

PETERS: Yeah, that's what I was getting at.

DIODATO: Yeah. But, it's the--

PETERS: So, do I need to re-explain this?

DIODATO: Well, it's the radar. It looks like the radar does pretty well, but they all sample at different scales, I guess, is the other thing; right?

PETERS: Yeah.

DIODATO: --getting of different volumes and--

PETERS: Yeah, right. I mean, the neutron is giving you the skin of the borehole, the radar is giving you on the meter scale, the ERT is even more gross than that.

DIODATO: Right.

PETERS: But, again, remember, I'm talking about very detailed comparisons throughout and it's masking the overall water distribution and I tried to go into that.

DIODATO: Okay. Yeah. I mean, Bo mentioned yesterday that one of the things he would really like to know would be fracture saturations and trying to figure out a way to measure that and it's very problematic. But, you have an example of the difficulties in numerical representation of these saturations, as well.

PETERS: Because he did mention also that's a very difficult thing to measure.

DIODATO: Yes, exactly.

ARENDT: We're about out of time for questions, but we will take one more from Leon.

REITER: Mark, you didn't mention anything about the strain meter in the south ramp. Could you just give a quick summary of what you're doing, why you're doing, is it important, and when you expect results?

PETERS: Yeah. Go back to #3 or the ESF--one more. There you go. Part of the cooperative agreement with DOE and University of California at San Diego is putting in a laser-based strain meter system in the south ramp of the ESF. So, we've got a line of sight, basically laser system set up. It's related to the overall geodetic measurements that we're doing in the region; you know, the Warneke stuff where we've got geodetics stations going up throughout the surface. And, they're just going to be looking at long-term strain rates and comparing that with what we see at the surface. It's part of a long-term program just to look at geodetics in the area to get at the strain rate in the Yucca Mountain area compared to the region.

REITER: When do we expect results?

PETERS: We just finished pouring concrete pads. They're going to install--we'll probably start collecting data within the next couple months, Leon. But, again, that's right now part of the cooperative agreement and planned to be a long-term experiment.

ARENDT: Okay. Thank you very much, Mark.

Our next speaker is Paul Harrington. Paul is Project Engineer, Yucca Mountain Site Characterization Office. He has a degree in applied mechanics and engineering sciences, University of California-San Diego, responsible for overseeing work on repository design.

HARRINGTON: Good morning. This morning, we'll talk three things. Design flexibility; much of this, I think, this Board is familiar with. I've understood that you want to get an update on a briefing that we had given to another organization last month. But, I'll go through that part first, seven pages or so, fairly quickly so that I can spend more time on the accomplishments and next steps. I think, that's probably of more interest to this Board.

Let's go to the next, please? Current status for design evolution, stepwise implementation. We're looking at how to address lower temperature operating modes. We talked quite a bit about that yesterday. We are completing an update to a parametric study on assessing modular construction; design, construct, operate. We'll talk a little more about that.

We're looking at updating design requirements that would be relevant to having a modular approach. If we were to pursue that, how would we convey that from the design organization to the surface/subsurface, etcetera. We've also sent a letter to the National Academy of Science telling them that we do want them to go ahead with the study that they had proposed where we would look at stepwise implementation of design/construct operations.

And, the design evolution, we're finishing off that trade study on the modular alternatives and on some of the below boiling operating modes. We talked about that yesterday. We need to further develop surface work. That will happen really after a site recommendation if we do make a site recommendation. Our work to date and even now continues to be on those things that would be most relevant or drive a site recommendation surface facility. We have a design that we think could work, but probably can make some enhancements to that. We're simply not pursuing that at this time.

CRAIG: Could you define stepwise implementation?

HARRINGTON: Two senses; one in terms of modularization rather than trying to trying build an entire facility and is most relevant to surface, but even applicable to subsurface in one fell swoop. Prior to even starting operations of it, can you do it in steps? Does it make sense to define a series of modules that you can bring online as you need them so that you're not committing the entire capital cost up front, that you're not taking that extended period of time to do the construction prior to being able to start operations. So, in one sense, it's very much a design/construct stepwise approach. We'll build something in modules appropriate to throughput needs.

In another sense, it's taking a more incremental approach to defining and implementing an overall solution rather than taking an approach that commits you to follow a certain design, for example, and instead being able to say, all right, I do need to, especially for licensing purposes, have an understanding of the entire process from beginning through closure. But, rather than having an approach that requires you to follow only that and be unable to react to information that you learned during that process, to have one that's a little more stepwise--I hate to use the word in a definition of it--that would allow you to incrementally look at both what you've done and also your expected continuation to make sure that's really most appropriate in light of what you've learned. So that you do have the ability to reassess decisions that you've made, both for existing things and design approaches, construction and operation approaches, so that if there is sufficient rationale, you would take an alternate course.

So, rather than committing, for example, to having an inability to do any sort of staging in an out front design, simply have the flexibility to say, all right, I'm going to be able to reassess whether or not aging a fuel prior to emplacement makes sense in the future and have the ability and a design solution to be able to revisit that on some stepwise process and see if I need to incorporate it.

ARENDT: Let's continue and take that in the question session. Let's continue the presentation.

HARRINGTON: Okay. This, I think you're familiar with. This is simply showing a progression of features over time generally from warmer solutions to cooler solutions. Without spending a lot of time on this, let's go to the next, please?

Okay. Why would we want a flexible design? Policy decisions, we are pursuing some now. They may change in the future. We need the ability to react to that. Incorporate alternative technical objectives, accommodate new information as we find it. One of our main objectives is a resilient design. Yesterday, I think I termed it as a flexible design. We do need to support the ability to retrieve waste. That's a regulatory requirement. It makes a lot of sense, besides.

In the program, to address stepwise implementation, we had a certain number of uncertainties that are causing us to look at that. Funding constraints, we would not expect to get the level of funding that would be required to build a full-up surface facility in the time scheduled that we have available for us looking at a 2010 receipt date. There are some uncertainties in the schedule itself. We have a number of technical uncertainties that we're dealing with. The stepwise implementation, we think, gives us some flexibility to accommodate funding schedule and other changes, opportunity for learning, etcetera.

The modular study that we're doing is really an update to one that we've done before. We're looking at different approaches across the system that would allow us to begin receipt earlier if that were the case. Also, not required, the level of capital commitment early in the process prior to beginning any sort of operation.

Things within that modular approach. The modular approach to surface facilities, building it in a series of increments rather than one large full-up facility. Looking at the initial subsurface development, we've all along said that we would not expect to build out the entire subsurface set of emplacement drifts prior to starting any emplacement activity. That we would have pursued that in some, if you will, modular approach. We're looking at possibly even separately than the large suite of emplacement drifts having a smaller suite of lesser capacity that could be developed a little more readily than the first 10 drifts or so of the major block. Looking at changing some of the transportation mode parameters, greater use of legal weight truck or heavy haul truck rather than rail in earlier years. Operational capacity. Decoupling surface emplacement; to date, we're really coupled emplacement with receipt, but there may be value to looking at receipt separate from emplacement, particularly if we consider the aging of fuel as an appropriate operational parameter to vary. And, incremental approach to surface storage capacity driven, in part, by the aging. Inputs for design evolution and we'll make that available to the NAS for the stepwise study that they will start.

I want to switch to what we have actually accomplished. I'm not going to go through the testing results that Mark and Gerry yesterday had talked about, but we've been doing some other things. We've done some of these thermal calculations that we did talk about and the natural ventilation calculations.

Separate from that though, when we went to the thinner-walled waste packages using the stainless steel and Alloy-22, that resulted in higher doses on the surface of the waste package. So, we're looking at how that then translates into the transporter effects. Also, because we moved the packages closer together, we don't use the lifting by the skirts anymore. We went to the pallet scheme. So, transporter and emplacement gantry have to change to accommodate that.

We've also changed the turnout on the end of the emplacement drifts, made it longer, more sweeping. That eliminated the shine effect that had been there in the previous design. That then took out the need for the shadow shields that have been in there, simplified some of the material handling issues. We're looking at the ground support. We some time ago removed the concrete liner and went to a rock bolt or steel set issue. Now, we're looking at having that be specific for different types of rock. An earlier consideration was that we would have a one-size-fits-all. Now, we think it would make more efficiency sense to have that be specific to the kind of rock and be responsive to the rock conditions.

Waste package. We're creating things called engineering files. That's really how the design information is being captured to be used as a basis for the design basis documents including the system description documents, the project description documents, the conveyed and site recommendation or other project documents. These things are nearing completion now supporting crit thermal, structural work.

The thing I wanted to mainly convey here is that we're still focusing on four primary waste package designs. The 21 PWR uncanistered, the 44BWR uncanistered, both of them with absorber plates; the Navy long canister, that's a large single canister within a waste package; and the DOE short high-level waste and the SNF codisposal canister. We think that's representative of the family of waste packages that perturbations to those--for example, the longer high-level waste canister can be treated simply as design enhancements or refinements of these four basic cases. But, we think these four are representative of all future waste types and these are the things that we're really focusing work on now. I want to make sure that everybody understands that.

We've also updated the Disposal Crit Topical to incorporate information we developed as a result of some requests for additional information. And, one thing that I'm particularly happy about is we're looking at finally changing the stainless steel closure lid methodology from a full penetration weld to something different. Right now, that thickness is 95mm. It's full pen. There are some throughput issues associated with doing that. It's in a remote area. You cannot access that with welders to do hands-on repair. So, there was some operational issues with that.

We looked at going to a thinner lid or going to a bolder lid or using a sheer-ring. The M&O is still working this. They haven't made a recommendation to us yet, but they appear to be narrowing in on the sheer-ring closure. That does a number of other things for us; one reducing DBE issues. It would cut down the surface facility costs eliminating the need for those heavy welds within that facility and the welders and rework capability associated with it and that would then improve throughput issues.

This is a section through there. This is the stainless steel inner lid and the Alloy-22 outer lids. This had been a full penetration weld right there. There's a sheer-ring now with the sections to it. That's very similar to what the Navy has been using in their canister design for some of the same operational issues we have here. Now, these center circles and the three lids are just grappling areas and that's a fill-in vent port on that inner stainless lid.

In systems, we've updated the project design description, expanded the scope. That, now we'll address in addition to the engineering issues. The PA approach kind of looks fundamental to our approach within the PA. Also, what's fundamental to our understanding of site characteristics and what the operational approach is. It's intended to be a summary level document kind of capturing the design basis and the rationale for that of the facility. We've also updated the individual system description documents for the Quality Level 1 and 2 and a couple of the non-quality level that are of particular interest, such as subsurface ventilation. We've done a preliminary preclosure safety analysis and updated the test & evaluation plan and performance confirmation plan. These are all available for interest.

Within the PDD, one thing I would want to point out is there's a Table 1-1 that we've tried to compile what we think are really the salient features of the design, science, and PA. Given the current state of our baseline definition, it's a little difficult to look at several hundred documents and come away with a concise understanding of what the facility basis is. So, we tried to pull that information into a table that's captured in that PDD.

Within surface, there's been really very little work there. We have updated the engineering files supportive of SR and we have completed a study to look at what we might do to improve the surface facility design concept. Again, that would not be taken further until after a site recommendation were made if it were, but the sorts of things that we looked at in that study were to try and get a better definition of what requirements we really need to put on a surface facility, try and close on a number of the long-going issues like wet versus dry fuel handling, those sorts of things. How can we improve the operability of the facility and commensurately reduce some of the design basis events. This design conceptual layout that came out of that decreased the lifting and handling by a factor of three or four. There were some significant improvements that came from that.

Now, we're going to switch to things we're going to do in the future. The first two, we've really talked about at length in some of the other presentations. The invert design--this came up yesterday. I need to remember the ones that I promised for today. Diffusive barrier, right now we're not crediting the invert for diffusive barrier performance. We think we may be able to come up with some performance within that barrier. So, we're planning on looking at that both from a diffusive barrier and also a mechanical performance perspective. If that invert material is going to have to support repeated traverses of waste emplacement equipment, we need to make sure that it can mechanically support that. If we can also get some barrier performance for diffusivity from it, that would be good.

Right now, the concept that we have with the structural steel framework--and I think this was one of your questions yesterday, what does that look like--really it's granular material. I don't know that we've chosen yet the crushed tuff versus the silica sand or is it something else. In fact, part of what we'll be doing in the--this assessment is to look at something else that gives us better performance. But, there's that as a bed. There's a structural steel, carbon steel, framework that rails and other material would be supported on and then that framework is backfilled in between with additional crushed material. So, there's a potential that degradation of that might create flow paths that would degrade any value that we could get for diffusivity from this thing. So, that will also be part of the reconsideration.

Wastes are treatable. The old design with the skirts have three holes in each end of the waste package. It would be fairly straightforward to insert a grapple hook into one of those holes for off-normal retrieval. Again, normal retrieval is the reverse of the emplacement scheme. Given that we don't have those skirts and therefore don't have the holes, we do have recessed bands around each end of the waste package. We'll have to come up with an implementable scheme for grappling the waste package in the absence of those holes for off-normal retrieval.

Also, look at ground support for longer term functionality, particularly if it looks more and more like we would be considering a 100 year plus preclosure life as probable versus something simply not to preclude with extended maintenance. We need to look and see what we can do to improve that commensurate with not unfairly degrading the host rock to start with.

ARENDT: Paul, you've got about three minutes.

HARRINGTON: Okay. Subsurface design layout, I talked some about that. In the waste package, testing and modeling is really paramount. Separate from that, we'll finish the update of the files and close the inner lid.

With systems, finish the modular, finish the waste acceptance. This is really our effectively contract with the DOE, high-level waste and SNF producers, and it captures what's in the 961 standard contract with the commercial and provide guidance for low-temperature issues.

In summary, we'll accommodate a stepwise approach. What we're doing is focused heavily on how we can reduce uncertainties through achieving lower temperature and other operating parameters and we're continuing to support SR.

ARENDT: Okay. We've got time for a few questions. Alberto?

SAGÜÉS: Yes, I'm interested in the idea of using a ring type of closure as opposed to a welded closure for the inner container.

HARRINGTON: Okay. Can you go back to that graphic, please?

SAGÜÉS: Do I understand that that approach would leave, in principal, say, like at least three different areas in which you could have a path for, say, for example, diffusion from inside the package to the outside once the outer shield would be breached? Is that correct?

HARRINGTON: That is correct, but remember that we're not crediting that stainless steel barrier with any performance now, anyway.

SAGÜÉS: Exactly. And, I think of this as an example of looking at what is happening certainly from a TSPA, say, philosophy. The inner portion of the package is sort of a Cinderella of the design, right, because there's no--but, if you--the main mode of deterioration is possibly solution, that inner thing may buy you a million years worth of ability, right?

HARRINGTON: Okay.

SAGÜÉS: And, just because TSPA doesn't consider it, doesn't mean that its existence could be ignored. How can we look at this?

HARRINGTON: Yeah, okay. I would not preclude potentially taking credit for this if we found that we could for a stainless steel in addition to Alloy-22. The reason being right now this thing shows a pair of fill-it welds worn on either side of the sheer-ring. These are not intended to represent full welds; rather those are at this point expected to be stitch welds, intermittent welds, simply enough to keep the sheer ring in place, but we were not trying to provide an actual leak path barrier. Now, if we did decide that we could make a case to take credit for the stainless barrier, there's no reason we couldn't make those full welds and also weld across the resultant end gaps between those sections. You could conceivably do that. So, at this point, it's not expected to simply because we haven't yet decided that it's defensible to credit stainless, but that doesn't preclude making those welds. It would be a heck of a lot easier to make a pair of fill-it welds like that than it would a two and half inch deep narrow groove full penetration weld. So, even if we decide to credit it, I think there's a better approach than the previous one.

SAGÜÉS: Thank you.

BULLEN: Bullen, Board. Just a couple of quick questions. Right now, the nuclear utilities are putting a lot of fuel in dry storage. By the time the repository opens in 2010, should it happen, there will be a couple of thousand waste packages, at least, that are already in dry storage, some of which are in sizes that are significantly larger than the waste acceptance criteria for Yucca Mountain will allow. I mean, they're making 67 BWR containers and 32 Ps. So, those are pretty big and would not fit into the scheme of things from the thermal loading issues associated with it. Is there any attempt by the DOE and the project to interface with the people that are actually putting fuel in storage right now to try and--I don't want to say influence, but I guess that's the word--try and influence them so that there's actually an allowable interface so that you wouldn't necessarily have to have a couple of thousand packages that--if they do get shifted to Yucca Mountain if they're multi-purpose and some of them are now being licensed for both storage and transport, you're going to end up with a waste stream of a couple of thousand packages to deal with in a low-level waste facility somewhere. So, what steps, what vision, what do you see in the next few years before those 2,000 packages are filled that the DOE can do about this?

MR. HARRINGTON: Until we close on just what our disposal capabilities need to be, it's premature for us to give guidance to the industry to tell them what a disposable canister would look like. That's the thing we're sort of dealing with right now is trying to decide for ourselves exactly what that means. We are meeting with NAI and I think that--

BULLEN: Thank you. I was hoping Lake would jump up. So, Lake, what do you think?

MR. BARRETT: Lake Barrett, DOE. We know about this issue. We've thought about this a lot. When we had the multi-purpose canister which ideally for those that have to go into storage, you put it in the canister once, the canister is for storage, then it goes for transport, then you take that canister and use that as part of the waste package.

In the market based transportation philosophy on our website, in the report we just recently sent to Congress, we again stated that we encouraged the multi-purpose canister type approach, that we could incorporate that into another type of waste package which would be a variation of the four basic types that Paul described. But, it is premature at this time for us to give specific engineering specs to a utility who is building canisters today. So, we cannot do that because we haven't determined the site is suitable, we haven't gone for licensing, and all the other.

So, what we say is let the market decide. We've explained to and have had meetings, you know, and NEI has helped us with this, as well as cask vendors, on where we are in the theory of the evolution of the waste package. And, meetings go on all the time. For the vendors who claim to have multi-purpose canisters and disposable canisters, we hope that that's true, and we will be able to adjust the waste package and engineering requirements later on and we will then credit back to the utility for the cost of offsets. So, that's how we're doing this from a market point of view. We wish them well and we hope they're able to do it, but we're not making it a mandatory thing. Let the market decide. If the utility wants to spend a few more dollars to deal with the long-term criticality, you know, they may get return back.

So, the market is working. Several of the vendors do have what they claim to be multi-purpose canisters that are disposable and we're doing nothing to preclude that in the design. But, we have not done much surface design evolution, at all, because we are concentrating on the four main points of this Board for the scientific suitability of the site. So, we're deferring a lot of these engineering solutions. We know we can engineer this kind of thing.

So, that's our philosophical approach to this now. So, we encourage multi-purpose canisters. We hope that will be the case for where they had to go into storage, but it's not a mandatory government-dictated system.

COHON: Could I just clarify, Dan, just one thing? I just want to clarify something. If it weren't for the Board's four priority areas, would you be working on this instead? Is that what you're saying?

BARRETT: On the multi-purpose canister, our policy is pretty straight in the RFP. So, no, we wouldn't be telling any vendors what to do. What we would be doing would be going more into the license application design and dealing with a lot of these issues. Utilities have asked us tell us, for example, the envelope---you know, how wide can you go, how far can you handle? We've said kind of later. We'll design the surface later. So, as we concentrate more on the SR, as we deal with the SR date, we're continuing to defer that engineering which we think is important engineering. We think it's less important than dealing with the four principal issues that you've discussed. So, we keep deferring this and we don't allow Bechtel SAIC to hire the engineers to go at this.

Things like the sheer-ring, you know, my personal opinion is the first time I saw the sheer-ring was on the plane looking at this. In my opinion, we will never--just like on the 350 cladding temperature, we will not intentionally violate any variance. We're not going to intentionally run temperatures above that cladding and we're going to preserve the integrity of that inner steel liner for exactly the reasons Alberto said. And, if we're going to do it, it's going to be a fill-it weld all the way around and we're not going to tack weld.

But, again, I'm getting into a detail that I don't think is essential to the arguments the site is or is not suitable. Those are the things you must focus on. And, Paul has to fight a rear guard action on a lot of this important engineering that is really deferred until later.

ARENDT: We've got time for two short questions, very short questions. Paul?

CRAIG: Yeah, I want to go back to the question I was asking earlier on. I think you answered it regarding the stepwise implementation, but just for the record, I'd like you to reassure me that stepwise implementation does not have anything to do with the SR and with licensing and should not be interpreted as leading into phase licensing. Is that correct?

MR. HARRINGTON: I'm not going to say that's absolutely correct. The reason I'll say that is as we talked with the NAS, one of the following speakers was from the Nuclear

Regulatory Commission and they asked him the question what do you guys think about a stepwise approach. Part of his answer said we effectively already have one and he cited things like the initial license submittal for a construction authorization and the update for receipt and possess and review for closure as examples of effectively a stepwise approach.

So, I can't say that everyone agrees that what I described here is independent of that. I guess, I would say that they are simply different manifestations of looking periodically at your approach and making sure that the decisions that you do make are appropriate for continuing with and gives you the ability to reassess some that you have made earlier.

Lake, I saw you stand up. Do you want to add to that?

BARRETT: Yeah. Paul, you described that very well. I'll tell you what it is not. Okay? It is not the phase licensing. That word, phase licensing, has different meanings at different times. Ten odd years ago, there was a study started by Admiral Watkins that talked about ways to accelerate Yucca Mountain and one of the concepts then was phase licensing. It was a term. And, that basically was let's do a license for a few hundred tons to install in the repository, put the few hundred tons, and then come back and do the licensing for the 70,000 tons or whatever the case would be. But, we are not doing that.

So, I mean, to answer your question, I would say the phase--I don't know what you meant by phase licensing, but I mean we're going to do a full-up license application kind of like the 70,000 metric tons. It certainly is phased in decision-making and stepwise and when either a site SR or an LA or a CA, you know, there is a continuing test & evaluation program to feed that continuous learning that I will go through. But, it is not this incrementally licensed small amount of waste and continue on with that. That is not what our plans are and that is not our policies.

ARENDT: That's all the time we have. I would suggest that you get Paul or corner him somewhere and ask your questions.

The Longstreet management requests that all the guests, please, be out of your rooms by 11:00 a.m., checkout time. Be out of your rooms by 11:00 a.m., checkout time.

We'll now have a break and we'll be back here at 10:30.

(Whereupon, a brief recess was taken.)

ARENDT: Our next speaker is William Boyle who is the Senior Advisor for Regulatory Policy, Yucca Mountain Site Characterization Office. William as his PhD in civil engineering from the University of California-Berkeley. He's responsible for advice on the implementation of Regulatory related project documents.

BOYLE: Thank you. As mentioned, for those of you who have the package, I'll give an update on uncertainties. I'd like to thank the Board for the opportunity to do this. The management and treatment of uncertainties has been subject of correspondence and has also been a topic at these meetings. So, I'll give an update on ongoing activities.

I'll start with some background and eventually get into some new results that I'm quite sure that most people in this room haven't seen until today when they picked up the package. Now, although I'm the presenter of this work, as Mark Peters had said for his presentation, a lot of the work is actually done by others and I'd like to thank everybody that's been involved with these tasks, but I can't. But, I would like to acknowledge the efforts of Kevin Coppersmith and Karen Jenni and Ralph Rogers and Bob Andrews and Dave Sevougian and Christine Stockman, in particular, that have been fantastic, as has everybody.

Now, to talk about two different tasks; quantified uncertainties review and unquantified uncertainties activities, Lake Barrett actually mentioned both of these yesterday. If you have his statement from yesterday, in the paragraph under uncertainty analyses, Item 1, identifying and describing how uncertainties have been quantified or bounded in the current models, it's that test. And, here's Item #2 in that paragraph; quantifying the uncertainties most significant to performance that have not been captured with the realistic probability distribution. That's that task. It continues to go on to say it's designed to provide insights into the degree of conservatism and in the overall dose estimates. These two activities are also related to the first of the four items that the Board had mentioned yesterday and which they've made available to everybody today on paper.

So, I'll talk a bit about each of these ongoing tasks. For both of them, I'll discuss, in part, the processes used which would be this, but also chose some of the results.

So, quantified uncertainties review. Generally speaking, the purpose was to look at our existing documentation and find out, okay, well, what did we do with respect to uncertainties? This is in the existing AMRs and PMRs, analyses and model results reports, process model reports, and in the total system performance assessment itself. This review was conducted by an independent review team. The review included looking at the treatment and documentation of parameter uncertainty, model and scenario uncertainties, as well, and also there was an attempt to evaluate the transparency and traceability of the treatment.

At the end of the review, we hope to identify lesson learned that will lead to recommendations for future treatments of uncertainty.

The review is not done yet, but we're able to make some observations already. The first is by looking at the documents it was quite apparent that uncertainty was focused on by the authors of the analyses and modeling reports and that's because they were asked to. They were also asked to focus on other things, such as traceability and quality of the data and those sorts of things. So, they were asked to focus on it and they did.

It was also apparent because, although they were asked to focus on it, a prescriptive method was not supplied. They were asked to describe the uncertainties. Because it wasn't prescriptive, we got a variety of approaches. For example, some people when faced with a large uncertainty made an assumption; others went with a conservative value and commonly these are related. Other people did deal with the uncertainty with full probability distribution. Some of the differences, as I've already said, had to do with the nature of the construction, but it's also related to the availability of data, different scientific disciplines, handle uncertainty in different ways in terms of how much do they quantify the uncertainty, and also there were even differences in individual authors.

Another observation we were able to make is the treatment of parameter uncertainty was perhaps the most developed and that's probably to be expected. Your mean and standard deviation, most people understand that and that's related to this. So, parameter distributions and the uncertainty related to them are typically the best handle of the various types of uncertainties. Some examples of where it was handled well were the saturated zone stochastic parameters and the defense high-level waste glass dissolution analyses and modeling reports.

Well, related to that observation is that the discussions regarding the treatment of the model and scenario uncertainty are less transparent and that's because it's inherently a tougher problem in terms of, in particular quantifying the uncertainty and even different conceptual models. So, that's an observation. The recommendations to improve the consistency and clarity of the treatment of uncertainty in the documents is underway.

So, that's the first topic. I'm switching topics now and now I'm switching over to the second topic, the unquantified uncertainties or UU activity. For any of you that ever had soil mechanics, I've always associated this with unconsolidated and undrained, but here it has a new meaning. So, what do we mean by this and I'll try and demonstrate it with this figure here and this is one of these terminology issues. Eventually, I'll draw on this one.

This is a made up distribution right here just for the purposes of illustration, but what it shows is for probability density, this Y axis, just think of it as--well, the number of times we measured something and the something we measured was whatever this was expressed in millimeters per year. Now, that blue curve is actually the data points--you can think of them as lying on the line, but most people associate such plots with these bar graphs I think most people see at some point in their school career. It's like these are the measurements we made, how many of them we made, and that's the curve that fits the observations. If we use the full distribution, then we have a quantified uncertainty that when the TSPA people, Bob Andrews and his people, go through their Monte Carlo simulation, they come in and they'll sample from this distribution. That means they can get values that range from .001 up to .1. And, if they do use the distribution, we have a quantified uncertainty. However, in various ways in the project for various reasons, we didn't always use the full distributions, but instead as shown in this figure, we used a bounding estimate, in which case TSPA wouldn't sample from this distribution; they would always use that value right there. So, we've eliminated the uncertainty in this case. It's no longer an uncertain number. We're always using .1. That's one way in which we unquantify the uncertainty. In this case with full distribution, you can quantify it, but picking a certain value, you've unquantified it, and as this unquantified uncertainty propagates through TSPA, it also makes the uncertainty related to the TSPA less quantified.

There were other things we did. It wasn't just selecting single values. We also perhaps in some cases shifted this entire curve. Imagine, if you will, that it just moved over. Assuming in this direction is more conservative. Or perhaps in other cases, we replaced it with a different distribution like a uniform distribution. For whatever reason. All I want to bring up here is there were a number of ways in which we unquantified the uncertainties.

So, now, this task was to look at the significance of having done that, both conservatisms and optimisms, as well, and evaluate that significance and eventually drug up insights and guidance. That first step to this activity was to look at the inputs to it. It's like, well, where were we doing this in our TSPA, in our documents, and in our models? This first bullet refers to a conservatism review and it really wasn't a conservatism review. It was a review by an independent group that looked at the analysis and modeling reports and the process model reports to try and determine areas in our models that were conservative or optimistic and it was a qualitative assessment, but they went through and they documented that. So, we have that result.

We also had the initial results from the review I just mentioned this morning where people went back and read through all the AMRs and PMRs and were documenting areas where the treatment had been to unquantify the uncertainty. We also started with discussions with the TSPA group and the initial insights from SR Rev.00. So, we had a candidate list of items for which the uncertainty had been unquantified.

Here is what we did with that candidate list or are doing with it and some of these steps have yet to take place. In Step 1, we took that much larger list based on those three inputs and tried to identify key unquantified uncertainties. Now, although we have a large list, the purpose of this task wasn't to do an exhaustive study today of all the unquantified uncertainties. We wanted to pick a smaller subset just to see what insights we could gain from looking at that smaller subset, but that smaller subset wasn't going to be a random pick. We wanted to deliberately look at those that might have an effect on--based upon, you know, insights that we'd already had. Also, to make the list, it had to be something that was unquantified. If it was already quantified, we weren't going to put it on the list. So, we developed a working list.

From that based upon the reviews and the meetings that I've described before, we also went and talked to the principal investigators, people like Al Eddebbarh, Bo Bodvarsson, and some of the others you heard speak yesterday and asked them, okay, where are the unquantified uncertainties in your models and could we represent them some other way? So, we developed our working list after talking with principal investigators and I'll show you the list in a bit. We then went out to try and replace, if you will, that single value that I showed on that chart before, the Point 1, with a distribution. So, we had meetings with the technical groups and this is ongoing. We have some of the new representations already, but this task is still ongoing.

The next step is to take those new representations, plug them into TSPA, and see, well, what does it do? How does it change the result, in what way? We have some results from that and I'll show that today, but that's still ongoing, as well. Eventually, we will produce an interim integrated report. I believe, Steve Brocoum mentioned yesterday that there's a change request in the work that's not finalized yet. So, I can't give you a date for this, but let's say, summer; late spring, early summer, mid-summer, sometime there, it will be settled in the change request.

The next step is based upon these analyses. Develop recommendations for uncertainty treatment and the license application, document it in a final report later this year, and the final step would be to manage uncertainty treatment in the future.

Here is the working list. Some of these, you heard about or saw yesterday. Al Eddebbarh had a page that listed these, slightly different terminology, but there's a one to one correspondence for the saturated zone. Bo mentioned the unsaturated zone yesterday quite specifically. He had a slide on the drift shadow zone. So, this was our initial working list. It's not all the unquantified uncertainties, as I said before.

Now, we also had other input from the Nuclear Waste Technical Review Board in a letter in December. It was mentioned that possible additions to the list would come to us and we've received it and we've looked at those items in your list. We're taking care of them in the following ways. Based upon discussions with TSPA and the people working on the uncertainties task, we believe that some of the items in your list, the Board's list, are already covered in TSPA 00 somewhere. Other items are covered in TSPA Rev.01. Other items are already covered or at least the phenomena are already covered in this list as part of the unquantified uncertainties activity. Other items, we'll probably add to this list. That leaves a subset of items that didn't fall in the four I've just mentioned and we've yet to determine what to do with those. Should we add them to this list or should we defer it to a later date? We'll get back to you and we'll let you know what happened to them all.

Now, I'm going to talk about four of them today and it's probably easier if I use your pen here. I'm going to talk about neptunium solubility, engineered barriers. Neptunium and thorium, that will be a topic all by itself. Then, there's three on waste package dealing specifically with the welds. Uncertainty in the weld stress state, geometry of defects, and the aging effects. So, I have those as examples and I'll get to those in just a bit.

Now, I'll say at this point all these examples, they're not with the natural system. There's Richard. I know he sleeps better when we look at the natural system and you can see there's plenty on here that deal with the natural system. We just haven't gotten to them yet in terms of the examples. For example, the reason neptunium is up here is, well, that's the first one we started work on. So, it was the first out of the box. The waste package ones, however, we decided to look at those because in looking into the insights of TSPA Rev.00, we know that these items are correlated with the TSPA results, more so than other items. So, we decided to look at those first. But, we will get to the natural issues in due course.

So, how are we doing it? We've got the list and now what do we do with the list? We want to quantify the uncertainties by having the technical investigators provide representative estimates of models and parameters. And, we do that by meeting with them and setting the ground rules, if you will, on how to give us their best estimate. It includes probability training and it leads to a iterative series of interviews sometimes with calculations, modeling analyses by the principal investigators in those five topical areas that was on the previous slide. The investigators are free and encouraged to use their knowledge of project-specific data, literature data, any data they know of in order to come up with their estimates. We didn't put any constraints on them in terms of give us a number of an estimate such that you're comfortable defending in a Court of law or anything like that. We just asked give us your best estimates with the uncertainty.

The goal is to get these distributions. We always have representatives of total system performance assessment there because we don't want somebody to give us something that's so complicated we can't possibly do the analysis within a reasonable amount of time. The TSPA people know what's possible and whatever the principal investigators give us, we always ask them, well, why did you give us that and you better be prepared to document it. We don't want people to just give us, you know, like a sensitivity study; just make up numbers for the sake of seeing what the effect is. We want reasonably good estimates.

So, after we have the list, after we get the new distributions, what do we do next? Well, we need to evaluate the implication of these now quantified uncertainties. That is we plug them into the TSPA and we run it through the TSPA model and see how do the results change. All the various ways that the uncertainty and sensitivity analyses were conducted for Rev.00, we could potentially use those same methods for using it looking at this new version of TSPA, if you will.

And, in particular, in addition to those, we're considering all these various types of ways of analyzing the results looking at the results, if you will. Like, for example, we could take all those new distributions, put them all at once in TSPA and look at, well, how does that affect the result? And, I can't show you that today because we're not done yet.

Another thing we could do is put in the new distributions one at a time and see how it affects TSPA and that we have done and I will show you some results today. We'll also do this today. We can take this new result and compare it to the old results. We can also look at the contribution of input uncertainty to both total dose uncertainty and we'll do that, but also uncertainty in an individual radionuclide, neptunium does, and we'll do that today. We can also look at how does the new distribution affect time to a specific dose rate; for example, 1 mrem/yr. We'll do that today. This is an easy enough one that you get out of comparing at the old results; look at how does the new distributions affect the time and magnitude of the peak dose rate?

Something that Bob showed yesterday that I won't be able to show with the new results today is we can look at the new results in a different way and, for example, examine residence time in a particular barrier system, how did the new distribution affect that, or how did new distribution affect cumulative release at subsystem boundaries? And, also, Bob Andrews talked about this yesterday. I won't be able to show any of the results with the new distributions, but eventually we can do that, as well.

Here are the examples that I drew the arrows for on that long list. I'll talk about neptunium solubility, three different new distributions, if you will, or three different representations for welding effects on the waste package, again effects on the welds, defect geometry, and the weld stress state following mitigation. Bob actually presented results yesterday. It was on Page 39 of his talk. You can go back and look at it and see Rev.00 results versus results calculated using a different distribution for the transport pathway from the waste package to the invert.

Now, yesterday, Dr. Knopman mentioned that she wasn't a materials expert and neither am I. I'm not a materials expert, I'm not an expert in neptunium solubility either. And, there are impenetrable terms here from time to time. So, what I'm going to try and do is I've dealt with these terms longer than most people in the audience. So, I'm going to try and put it more in layman's language, some of the things that are being talked about. So, I know that there's probably experts in the audience who know this better than I and by expressing this in layman's terms, I don't need to gloss over something that's important. I'm just trying to communicate to the broader audience here.

Here's the first example I'll talk about, neptunium solubility. With the principal investigators, whenever we meet with them, one of the first things we do is we always go through, well, how is it represented now? What do you have now before we get to what might you do differently. TSPA-SR Rev.00 for dissolved neptunium concentration is based upon conservative assumptions that use bounding chemistry, pure phase materials, and the neptunium solubility is a function of how acid the water is, pH, and the amount of CO_2, carbon dioxide gas, present in the system. That's shown, in part, by this graph. We don't show the effect of how much CO_2 is present, but we do show the relationship of the function of how acid the water is, pH.

So, this is a plot of how much neptunium dissolves in water as a function of pH and what we use now is this line right here, that one, the Np_2O_5 line. That's it. So, if we know the pH, let's say it's 6, we go up, we find where it intersects that line, we come over, and we get a fixed certain value. That's what we use in the calculations now, setting aside the CO_2 issue for the time being. But, as you can see, the circles and triangles, those are actually test data, and you can see--yes. This third bullet, that line does not explain large spread in measurements of neptunium concentration. Another way of saying it is although this line certainly bounds all the data, you wouldn't necessarily call it a good fit, if you will. It's a bound, but not a fit. These other lines shown here are one possible model to perhaps better fit the data. And, also, shown on this slide are two standard deviations from the mean of these test results and they plot as the straight lines there. Okay. And, these measurements are by Argonne National Lab for those who don't know ANL.

All right. Well, what did we get from the principal investigators is an alternative distribution. There again is what's being used now. This is the same plot, neptunium solubility in water versus pH. Here is the new representation and we're not done with it yet. I'll jump down to the third bullet. If you remember on the previous slide--I'll put it up myself on the overhead--that the initial model I showed did show a pH dependency. That is these lines occur with pH. Whereas, this model doesn't show that yet, but this third bullet right here. We're still examining that. What that means is in our next iteration, these straight lines may end up being bent up just as these are, but when I show you the calculations in a bit, it's based upon this representation. It's a triangular distribution. So, now, what happens is again at a pH of 6 what we do now is we go up, we intersect that line, and we come over and we read the certain value. What was done for the calculations, I'll show you, is this triangular distribution where there's the peak, it now becomes part of the Monte Carlo simulation. On each realization, that distribution is sampled and the neptunium solubility is obtained from that distribution.

Now, I mentioned we always ask the PIs, well, what's the basis for this new distribution? Well, Np_2O_5 is a pure material, but we knew these other things. That neptunium is predicted to be incorporated into uranyl crystals. To a non-expert, what this means is neptunium and uranium are sufficiently alike such that neptunium can occur in uranium crystals. It won't necessarily be excluded. Along those lines, neptunium has been observed in the dehydrated schoepite, a uranium mineral in Argonne National Lab laboratory tests. We also know that neptunium and uranium dissolve roughly similarly in spent fuel drip and batch tests. So, what we're getting at is that neptunium and uranium are sufficiently alike that, as we create this schoepite in dissolution, we might be binding up neptunium in it which would then explain why the solubility and water is less. It's because it's bound up in that solid instead. So, that's what we ended up with is a new distribution.

What did it do to the results? Okay. The top slide, as you can see, is TSPA-SR Rev.00, not total dose, just neptunium. In this case, we're just going to examine it. You can see the colors here. For those of you that have black and white, in general, the topmost curve will be the--even I can't read it--yeah, 95th percentile median, 5th percentile, in that order. Every once in a while, the mean will cross the 95th percentile in some of these plots. So, that's what we had with the conservative estimate with the solubility fixed as soon as we knew pH. Here's the new results based upon the new distribution of neptunium solubility with a triangular distribution.

The old results, Bob Andrews tells me, it's waste package degradation parameters control things up to about here and then you see there's a spread in the neptunium dose after that and that's controlled more by the natural system parameters. Down here again, we have the waste package parameters controlling things up to this point and then a spread. But, what's more interesting is let's consider what should qualitatively happen by replacing a single value, as we had here, with the distribution down here, bearing in mind that the distribution is in all cases lower than this line that we had before. You might expect that by going with lower solubilities, the dose is going to drop. So, we can check the peaks and, sure enough, the peak here is greater than the peak here. So, we did have a drop as you might expect.

The other thing, by switching from a single value to a distribution, we should get a spread in the results. And, particularly, if you look at the time of peak dose, we cover many more orders of magnitude. Our results are wider than right here. Now, this large spread out here at 100,000 years may actually occur for this situation, as well, but it may occur later in time. So, we don't know yet. But, even at this time of peak dose, we do see an increased spread, a more uncertain result by switching from a single value to a distribution.

Now, I'm switching to the waste package. There's three items to look at here; aging, defect geometry, and stress state. I have aging and defect geometry here. For all three, I'll go through how it's represented now, the new representation, and ultimately I'll get to the new calculations.

Now, what this aging is to a non-expert, it's the heat of annealing and the heat of welding do things to the metallic crystals. They set up the possibility such that with time changes will take place that would lead to increased corrosion of the weld. That's what the aging is. And, we've represented it in the present model and I believe Bob Andrews mentioned this or Gerry Gordon or they both did--we increased the general corrosion rate for welds anywhere from one times to two and a half times the general corrosion rate. All welds, they get this enhancement factor. That's how we were taking the case of aging and that was based upon measured ratios of passive current densities of aged and unaged samples. As a non-expert, it's based upon some measurements. It's based upon data.

The new assessment, we're switching from a general enhancement applied to all welds to a situation where we're going to a low-probability, but high consequence representation and it would be only out of every 10,000 weld packages will have increased corrosion, but instead of one time to two and a half times, it's going to be a thousand times. So, we're switching from all of them get some multiplier to only one in 10,000 get a multiplier and it's 1,000. The 1,000 times is consistent with recent measurements by the Center for Nuclear Waste Regulatory Analyses. We've got more work to do. This is a very important bullet. For example, the basis for the one in 10,000, that's not firmly defendable yet. So, we have more work to do on this one.

Defect geometry. There's cracks in the weld. There's a possibility that these cracks are oriented radially, leading to through-wall propagation. It would be easier if I had--oh, no, I'll try and use this. Do the cracks go this way and eventually propagate through the wall or are they this way and propagate through the wall? Those are both radial cracks. Or are they parallel to the surface of this in which case they're circumferential and don't propagate through. In the present model, we assume that all defects are radial. That is they all have the propensity to go from wall to wall through. Now, I'm not a waste package, I'm not a materials expert. It certainly can't be any worse than this. We have 100 percent of them. So, it may be the real number or it may be a bound, but it certainly can't get any worse than that.

How did we present it in the new assessment, the unquantified uncertainties assessment? Instead of 100 percent, just one percent will be radial. Here's the basis. It's based upon a statistical analysis of literature geometry for carbon steel and analyses of the potential for those non-intersection cracks to propagate radially. Now, what's important to note here is we're switching one certain number for another. We had a certain number before, 100 percent, and we have a certain number now, one percent. And, again, this bullet is very important. What we haven't done yet is added uncertainty about the one percent.

So, what might we expect from this result? And, I have to go back to the aging, too. By switching one number for another, given that this one is conservative, these results should tend to lead to lower doses, but not necessarily any change in uncertainty. This one, by switching from a general multiplier on all to a situation where we have low-probability, but high consequence, this really should lead to a spread in the results or increased uncertainty, if you will.

The last of the three for waste package welds is the stress state in the weld region following mitigation by laser peening and I think Gerry Gordon talked about this. But, again, to a non-expert, what happens in the weld region, it creates tensile stresses. The material wants to pull apart. So, you can use laser peening or induction annealing to induce a compressive stress, such that the cracks won't open. That's what they're doing. For those that live in southern Nevada in a relatively new home, this is exactly what they do with our concrete slab foundations. You know, they don't want the concrete foundation to fail in tension; so they use the big steel cable to add compressive stress and that's what they're doing here by different methods.

So, how do we represent it in the present model? It's an uncertainty and I'll try and draw it here. I'll try and draw both of them. Again, this is the number of occurrences and this is stress state. It's a triangular distribution, if you will, where this is the yield stress, YS, and that spread here to here--that's not the--this is 30 percent of the yield stress and this is also 30 percent. So, that's the triangular distribution represented currently. What we went to in the unquantified uncertainty assessment is the experts told us, no, a more realistic representation is 15 percent. So, what this should do, we didn't change the central tendency, this value, the middle one. So, it may not have much of an effect on the mean peak dose, but we now have the uncertainty. So, perhaps, we should see a narrowing of the uncertainty.

Now, I have to put this one up again. Now, this is Rev.00 results. You don't see any of the new information. I have to leave it up here because I'm going to keep comparing back to it. TSPA Rev.00 waste package uncertainty model, right. This is what's in Rev.00 now and I'll leave that there for now. Both sides are the same. This has the original representations, the plus or minus 30 percent, the 100 percent, radial cracks, and the general enhancement for the aging. These are the results you get.

Now is when we start to compare them. These results over here, what they show is what happens to the results if we enter the new stress state and defect geometry representations keeping everything else the same as it was over here. And, as you can see, a narrower spread in the package failure distribution. This is a fraction of waste package has failed. Compare; here's the new results. They're only this wide, whereas the Rev.00 results are this wide. That's because the first waste package failures occur later in time which here they occurred roughly 10,000 years, and here, it's after 20,000 years. But, there isn't much change in the maximum dose in the 100,000 year time frame. That is this line here, the red dose rate is about at the same height over here.

For the stress state, remember, we kept the mean value the same and narrowed the uncertainty. So, we get narrower results, but we keep the value the same. And, with the defect in geometry, we should expect--we went from a conservative case to a more representative one, and if we were correct in our estimate of conservatism, something ought to get better on this side. And, with the case of defect geometry, it wasn't really the dose, but it was in the failure time of the waste packages.

Now, over here on the right, it shows the effects of changing how we represented the aging. And, again, all we changed here is the aging. That is we put the stress state and the defects back the way they were in this model. And, I had said before, the one thing we ought to expect in this representation by switching from a general enhancement to all weld packages to only one in 10,000, but it's a thousand times greater, there's a much wider spread in the results, much earlier failures for a few packages. See, now, we have them as early as 2,000 years based upon the fractional representation of packages that are failed. We have a bigger spread in the waste package failure rate. We also have a bigger spread in the dose.

But, we have a lower--compare this dose here to this one here. All these scales are the same. So, you can flip back and forth. There's a significant improvement because we're not adding that general enhancement to all welds. We are only catastrophically, if you will, a thousand times increased the corrosion for one in 10,000 of them. So, this drops as shown over here, but the uncertainty spreads.

BULLEN: Bullen, Board. Before you leave that one, it's one in 1,000 weld patches.

BOYLE: Right.

BULLEN: How many patches on a can? So, how many containers does this effect?

BOYLE: That detail, I don't know, and I'd have to ask one of the waste package people if any are here in the audience. Or a TSPA person that may know that. If Bob knows?

ANDREWS: Yeah, Dan, repeat the question? There's 40 patches around the weld.

BULLEN: So, there's 40 weld patches per can?

ANDREWS: Per can.

BOYLE: Does that answer--

BULLEN: Yeah, that answers the question. We can figure from there. Thanks.

ANDREWS: Yeah.

BOYLE: All right. This is all three of them all at once. And, the side I just showed you on the aging had results over here, had waste package failures over here, and had dose over here, but when you add the aging, stress states, and defect geometry all at once because this is a non-linear system, some of the effects cancel each other out. We still have the issue with the aging, but the improved representation of the stress states and defect geometry counterbalance that. So that when you take all of them together, we have this new improved performance and do not-- even for the 100 realizations we had here, we didn't have any early waste package failures. So, when you take all three of them together, we end up in total with later first failures compared to the base case here. This number is larger. And, because of the fewer failures, we get lower dose. Compare this at roughly 10^0 to this at 10^2. So, roughly, two orders of magnitude just by looking at these three items.

I had mentioned early-on that we were going to look at different ways of looking at these results. I'll put this one back up. One of the ways I suggested we can look at the results to try and gain insight is pick a does, if you will, and what we picked was .01 mrem/yr and 1 mrem/yr. So, there is the 1 mrem/yr and there's the .01. For both of them, slice the results along that horizontal line which is what we have over here in these two plots for the .01 and in this case for the 1 mrem/yr. And, what we're plotting is the time at which the does rate exceeds .01 and the time at which the dose rate exceeds 1 mrem/yr. These are cumulative probability plots.

And, there's a lots of observations. This first bullet just tells how we created the plots. These colored curves are called cumulative distribution functions or CDFs. They indicate that some realizations never exceed the given does rate, 1 mrem, and only 10 percent of these realizations --it's this little green line down here hits right at about the 10 percent line--only 10 percent of them exceed it. Both the high dose, the 1 mrem and the low dose CDFs indicate later, but only slightly lower for the defect stress state model. Defects and stress state model is the red one. It's the second one for those that have it in black and white.

Later, but only slightly lower. Here's are Rev.00 results and they really don't change that much, but they are slightly later.

The low dose rate CDF, that's this plot, indicates much earlier, but lower doses for the aging model. That's this blue one with the long tail. That's the one that had the earlier failures which I showed you before. These are the same results as before, just presented a different way. The high dose rate CDF indicates both later and lower doses for the aging model and it's this blue one right here, the second from the right. Both the high and low dose rates CDFs indicate later and much lower doses for the combinations of all three models, the green one. Okay. That's the end of the results.

And, this schedule and planned products. I had mentioned before in the steps of developing the new assessments, we were going to have an interim integrated report. I indicated sometime in the summer and here would be the contents of that report. Now, although these two are listed as separate bullets, an assessment of unquantified uncertainties and a final integrated report, this will actually probably be part of that in the change request that's coming over.

What's important to note here is there will be an evaluation of key unquantified uncertainties for the lower temperature operating mode. We deal with that already with the principal investigators when we meet with them. The first thing we always ask them is is there a temperature effect. Set aside whether you have a hot or a cold design. A hot design goes through various temperature and we always have the first cut, is there a temperature effect? Some things, there are, and some things, there aren't. We also ask the investigators if we had cold design, would you expect differences? Like Al Eddebbarh for the saturated zone and distribution of porosities and permeabilities, no. Others, yes. But, we're eventually going to go back to all the PIs for the low temperature design and ask them once again, given this low temperature design, give us a new assessment if a new one is warranted. And, ultimately, in the final integrated report, we'll have guidance based upon all these results that we're getting now.

My final slide, I just wanted to--I believe you've seen this before presented by Abe Van Luik and perhaps others. I just wanted to bring out that this entire unquantified uncertainties task and also the quantified uncertainties task are related to these four bubbles, if you will. We're analyzing our uncertainties, we're assessing them, we're trying to communicate them. You know, I saw different ways of plotting them. And, ultimately, through the guidance we would give, we would lead to management of uncertainties. So, I just wanted to bring out that these activities are completely in keeping with this uncertainty strategy that's been presented a number of times before.

So, feel free to ask questions.

ARENDT: Thank you very much. We've got about 15 minutes for questions. Jerry Cohon?

COHON: Thank you very much, Bill, for the excellent presentation. I think you did a great job. Someone who could actually explain some of this stuff to those of us who need the explanation. Thank you also for the excellent progress. I really think this shows tremendous progress since when you started several months ago. I think it's very encouraging.

I have lots of questions and I'm going to triage them, and if time allows, I'd like to come back later on to pick up the rest. I'm going to start with the most important ones.

I'd like to start with Slide 7, Step 4. Step 4, license application. What about SR?

BOYLE: Sure. And, if we get insights now, you know, we're not going to wait. This will, as far as I know, be before SR. There will be recommendations for people to use from here on out. So, it's got to be one or the other. As we learn, we'll make the recommendations as we go. Some of them would probably certainly affect SR.

COHON: I feel strongly when it's a Board position, as well, that the site recommendation process, if you get to it and you get all the way into the meat of it--that is the President recommends a site, Nevada objects, Congress has to act--I think you're going to need to present summary estimates of uncertainty. And, I think also--I'm assuming, therefore--maybe this is a bad assumption--that the uncertainty work you showed us and the uncertainty work to come will be important, essential, for you to produce that kind of summary uncertainty assessment.

BOYLE: Right. And, you know, along those lines, I know that, traditionally, we as a project have always showed horsetail diagrams. And, it's not clear to me, at all, that that's warranted for decision-makers.

COHON: I'm just going to your point. Step 4 focuses on LA.

BOYLE: Right.

COHON: But, you're going to need it do it for SR. Whether this work you're showing us gets incorporated in SR--

BOYLE: Yeah, yeah.

COHON: Okay. Slide 14. If I read the graph on Slide 13 properly, the ranges on Slide 14 don't even include one of the data points--the only data point that was the basis for the old estimate.

BOYLE: Sure, right. That one right there.

COHON: Right. What's the rationale--

BOYLE: Right. And, as I indicated the principal investigators, this is a work-in-progress and we're not done yet because we have to--oh, it's actually on this slide. The pH dependency is still being evaluated.

COHON: Yeah, I know, but I'm saying even if we accepted the hypothesis that it's flat and--

BOYLE: Right.

COHON: Not even to include the only data point that--

BOYLE: Right.

COHON: Because then the line before it strikes me as--

BOYLE: Yeah, and I wasn't present for those discussions, but I did point out these two standard deviation lines before. They don't correspond to these two lines, but they also fall below that point. So, some people who wanted to do the analysis saying plus or minus two standard deviation, that point wasn't captured, as well. But, it will be interesting to see how they do capture--the experts, you know. Will they bend the curves up as this model shows?

COHON: 16. This has more to do with looking back and trying to understand what has been in TSPA rather than where we're going in the future. Now, looking at the first item with regard to aging, would whoever provided the factor of one to two and a half before have characterized that as conservative? Did we think that that was a conservatism?

BOYLE: Yeah. You see, there's some uncertainty there based upon what I read here. You know, it's not a fixed value. So, they're sampling from a distribution. And, I'd have to ask one of the experts. I'm assuming they believe it's conservative; otherwise, I can't explain why they would--

COHON: Yeah, okay. Let's not put words in their mouth. This is a wonderful demonstration though of what happens--

BOYLE: Yes.

COHON: Okay. I just wanted to underscore that. Seeing package failure and dose much earlier, only one or two of the many hundreds of results, but nevertheless, a very nice demonstration.

My last one of this round, 21. I want to challenge you on this. For the purpose of helping me to understand better, you said when we put all the various things together, they tend to cancel each other out and this--

BOYLE: In this example.

COHON: In this example, I know. And, I glad we just only have these three phenomena to worry about. And, you said because the system is non-linear and there's a lot going on. Now, that only makes sense to me if you can explain or someone can explain physically how those three phenomena cancel each other out. I mean, how--if aging could have produced an early failure by itself, what's the physical explanation for how aging, defect geometry, and stress states will--

BOYLE: As a non-expert, they're obviously related somehow such that the other two can cancel it out or, for all I know, these are 100 additional runs and it was just a statistical fluke.

COHON: See, and that's the other possibility. As Bob tells us, you use the same--do you use the same sampling, Bob?

ANDREWS: This is Bob Andrews. We have no statistical flukes and correct the record on that one. There is an explanation when you have the--remember we combined in the nominal case plus or minus 30 percent on the stress state at those welds, at the annealed welds. At the tails of that distribution, that stress state lid gives very little--maybe it's a few millimeters, five or so millimeters, of compressive zone. So, the amount of compressive zone becomes less at the tail of that distribution. For that case, if you happen to sample a higher aging multiplication factor, a rate of degradation, if you will, at that point, it will go through relatively quickly. And, as you saw, there was one realization where it did go through quickly. If you take that 30 percent and bring it down to 15 percent which is this curve, you have no probability, if you will, hitting the tails of that distribution. So, your distribution of amount of compressive zone, instead of being broad, probably going from the order of maybe five millimeters to 15 millimeters, has now become pretty narrow, between seven and 11 millimeters. And, that extra couple of millimeters gave you a lot more time for that one realization.

COHON: My materials experts are nodding their heads. I'm sorry, I just remembered on more really important question and it goes to overall approach. You've very nicely laid this out and you showed how you were going to take the various new treatments or the new quantifications, unquantified uncertainties, and do what you showed us in a long list of different kinds of sensitivity runs. I'd like you to tell us what happens under the two possible situations. One, you do all of those and you find that this new treatment, the new quantification, really shows no impact on results so far as you can tell versus the other situation where it seems to be really quite sensitive. Is there a next step? Does the new quantification stay in TSPA or do you just leave it there in the case where it didn't really have an impact; in the case where it does have an impact, does that imply you're going to go back to the PIs and analyze it further or try to refine it even more, the quantification?

BOYLE: And, that relates to those recommendations. I think we still have to work through that. It would seem to me if there are things that really are sensitive that we should consider putting them in, particularly now that we've done the work. We've maintained all along that we knew they might have an effect, but we're going conservative for any number of reasons. But, now, if we have it, they've done it, they've got it plugged into the TSPA, maybe we should consider using it in the future whether it--particularly for the sensitive ones, maybe even for the nonsensitive ones. It's a more representative model and perhaps easier to explain. But, we'll deal with that in the recommendation.

COHON: Okay. And, for every quantified/unquantified uncertainty, there's going to be a recommendation?

BOYLE: I don't believe we've considered that yet, but we could go through them one by one by one and also perhaps have like, for example--and I'm not saying this will happen--if we had a global recommendation, quantify them all.

COHON: Right, okay. Thanks.

CRAIG: Yeah, I enjoyed that presentation a lot, Bill. My question has to do with uncertainty and the uncertainty distributions. Now, you used in this presentation uniform distributions, triangular distributions, log-normal distributions, and I think linear normal distributions. The results that you get depend a lot--for example, you use a log-normal distribution and the actual distribution is linear normal, you're going to have a heavy bias toward low values of which may either be beneficial or damaging to the case you're trying to make depending on the process involved. What was the process that you used in order to decide what kind of a distribution function to use in each case?

BOYLE: And, I hope that that's captured in the first task I talked about today; how do people treat uncertainties? And, what Professor Craig is asking is how do they determine which curve to use to fit the data? We owe that explanation. And, there's actually quantitative ways to get at this which I know and I forget which one applies to continuous distribution, such as the uniform distribution, or discrete distribution, such as the Poisson distribution. There are statistical tests that people use where you can get a numeric estimation of, well, does the beta distribution fit better, log-normal, negative exponential, whatever you want. That is a way to get at that. You know, the various experts, I'm sure, did it various ways, but I'll say this. I think few of them actually went that step of using a statistical quantified measure of testing which distribution is better, although some did. I am aware of some that have done that.

CRAIG: Well, there's certainly a strong tendency if you have a large spread, orders of magnitude spread, to assume that graphing things on the log paper is the right thing to do. And, this carries you, if you don't think carefully about it, into a log-normal distribution with the kind of bias potential that I just mentioned.

BOYLE: Right, right. Yeah, and I'll make an observation to people. When you have logs on one of the axis or both of the axis, small changes in the log rhythm can produce tremendous changes in the results. So, you have to think through this carefully. I'd like to think that the investigators generally have.

KNOPMAN: Knopman, Board. Bill, perhaps, you could talk us through how you would deal with model uncertainty?

BOYLE: And, that's a tougher one. The one I always come back to and it's a simplistic way, if you will, and I know we've done this, in part, on the project in places; the single heater test, for example, where we had different models, and the equivalent continuum model and the dual permeability, the DKM, model. And, we had temperatures and the two different models both calculate temperatures. We can again do statistical tests and they have similar to the plot that Mark Peters showed today for saturations on mean square error, root mean square error. You know, each model makes a prediction. You compare them to the measurements. You get a statistical estimate of, well, is one better than the other. That's one way I know. When you already have two models, two or more models, and you have data to compare them to, there are ways that are at least a help. I'm not saying they're definitive, but they can give some insight.

But, what's a bigger issue is how about you have completely different conceptual models? How would you measure or estimate that one is better than another. If you can get it to the point of getting it into a numerical code, such that you can make predictions and compare it to data, we're back to where I was. But, that's what I always come up with.

KNOPMAN: So, I mean, there's the model discrimination question, but I think even if you have a preferred model, there's still model uncertainty apart from what you then express in individual parameter uncertainty.

BOYLE: Oh, yes.

KNOPMAN: And, I'm just wondering how in TSPA you would go about--what sort of analysis you would do to be able to generate some distributions and dose rates as a consequence of model uncertainty when you've decided on a preferred model. Even when you've decided.

BOYLE: Right. And, I'll try and put this in terms of the way I usually think of it. We're not going to have the measurements to compare to, you know, out to 100,000 years. So, we can't use the tool that I just suggested. You're essentially asking how believable are the results? And, you know, perhaps, I ought to leave that to Bob, but the way it is--the entire TSPA is built of parts, and if you can at least look at the parts and get some feeling for the parts, and know that they were put together appropriately, you get some measure of confidence. But, it's hard to state quantitatively that--you know, we get the horsetail numbers and you can give means and percentiles, but that doesn't necessarily really address your question. That's just turning a crank; how good was the initial model to begin with? And, I would like to compare it to measurements which, if this ever goes ahead for certain sub-aspects, we will be able to through performance confirmation, but not out to 100,000 years.

KNOPMAN: I mean, take something like seepage. Seepage is not--it's an output of a model; it's not a parameter. But, it becomes an input to TSPA and goes in in some lookup table or something. So, seepage numbers can have a distribution that somehow reflect your confidence in the model.

BOYLE: And, that has to be--right. And, I would submit that the best way to get at that, the uncertainty, is by looking at tests. LBL has conducted a lot of tests and we can compare. At least for the conditions of the test, here's what that same model predicted for the test and here's what we actually measured in the test and how well did that model do? Then, begs the question of, well, okay, it did well in predicting the test, but how well will it do in predicting an actual--you know, for all those years in a repository under those different conditions. But, I think that's what people frequently do. They'll say how well does it compare against this, and if it does well here and my real problem is somewhat similar or reasonably similar to the test, I have a belief that it will do reasonably well there.

KNOPMAN: See, I think there's still an element. There's a time element that you don't capture when you're doing just these individual parameters that I think does come up in the model when you're thinking in terms of model uncertainty. Your material is going to be changing as a consequence of time, heat, or whatever. You know, you're just in a different realm; you don't know. Therefore, there should always be this increasing uncertainty over time.

BOYLE: Right. And, I agree; no matter how big a test we conduct or have conducted, they're not at the volumetric scale nor the time scale of a repository. We do have large and long tests that we can compare to and what we hope to do is in a sense look at the model, make sure that it captures the physical processes correctly, and then go forth and make the much longer predictions. We always have the performance confirmation that will allow us to continue, to check it for longer times. But, it is an issue.

ARENDT: That concludes this presentation. Thank you very much for an excellent job.

COHON: Thank you, John. We turn now to the public comment period and it seems we have a problem, as we did yesterday. That is too many people and too little time. Let's see what we can do.

First, let me start by confirming names of those who signed up or want to speak at this public comment period. Steve Frishman, Judy Treichel, Kalynda Tilges--apologies for mispronunciations--Merlynn Rose, Jonathan Deyarmond, Piper Weinberg, and Sally Devlin. Everybody whose name I read, do you all want to speak during this time period knowing that there's another public comment period at the end of the day today?

(No audible response.)

COHON: No one is changing their mind. Okay. Did I miss anybody?

(No audible response.)

COHON: Okay. I'm going to have to ask each of you to limit your comments to five minutes with apologies, but if you don't get it all in, you're welcome to speak again in the afternoon public comment period.

Steve Frishman? Steve is from the Nevada Nuclear Waste Project Office.

FRISHMAN: Good morning. I'm Steve Frishman with the Nevada Nuclear Waste Project Office. As usual, I'll give you some maybe fairly broad observations and a few comments about what has been presented over the last day and a half.

First, I think I want to compliment you on the questions that you posed for yesterday's meeting. I think you're right on the mark with the questions. I'm not sure that the answers were quite as good as the questions. In relation to one of them, in particular, that having to do with importance of barriers, there is more information out there than what was presented to you yesterday. In a Yucca Mountain Project/NRC technical exchange last week, there was a presentation on importance of barriers and there were graphics presented there that went directly to answering the question that you posed. What was presented yesterday is getting more and more obscure all the time in trying to look at importance of barriers. And, someone raised the question of why in the degraded case, why just one patch? It's totally arbitrary. What it does is it fogs the answer to the question of what does neutralization of the waste package do to performance? And, you want to see a direct answer to that and last week we saw one and yesterday, we didn't.

We also saw last week a plot of what if you neutralize all engineered barriers and you could look at that and you could look at the difference between neutralization of all engineered barriers and just the waste container and you could glean some additional important information about relative contributions within the subset of engineered barriers.

So, I'm just pointing out to you that there's other information that is presented in other meetings that are important to you and I know that you have staff people and sometimes members at some of those meetings and you probably ought to be watching more carefully for what presentations are made in other forums that would be important to getting directly to the questions that you want answered.

And, the question itself is a very important one because, as you assert in your question, there is an enormous reliance on the waste container and, in addition, on the engineered barriers. This comes back to the question that I've discussed with you a number of times before about whether given--regardless of the graphics, if you understand what the consequences are, whether this is really geologic disposal or isolation as we like to think we used to know it because just in the matter of the last day and a half, things are continuing to change.

As an aside, I for one am really glad that we don't have an SRCR out there because, first of all, it doesn't reflect what was being discussed. It doesn't reflect current thinking. And, second, it's pretty clear and I agree with at least the sense that I get out of reading your last letter that the project is not ready for a site recommendation and I think the very impressive list of unquantified uncertainties in the last presentation is probably a pretty good example of why the project is not ready if you dig into every one of those elements on that list.

So, it's important, I think, as I've observed before, a real primary facet of the Board's responsibility is to give the Congress and the public some real insight into the site recommendation when it comes and we know that eventually it's going to come. I only urge you to be even more diligent in asking the kinds of questions that you're asking and then trying to evaluate just is the project or program ready for a site recommendation, and within your charge under the Waste Policy Act, I think you don't have a very broad interpretation to make. People are counting on your expertise to sort out what is presented to you and presented to the public. And, also, sort out whether, in fact, if the Secretary makes a site recommendation, there is a firm, reliable, and technical basis for that recommendation. And, as long as the large list of questions is unanswered at the time of site recommendation, then I think you only have one choice and that's to say do more work.

COHON: Steve?

FRISHMAN: Yes?

COHON: I'm sorry, time is going to be up in about 20 seconds. Maybe you can come back later?

FRISHMAN: Well, I can either do that or just leave you with that startling message.

COHON: Okay. Well, you're welcome to come back later, too. Thank you. Judy Treichel? And, Judy, and all subsequent speakers, rather than my butting in like that, I will raise my hand when you have one minute left. Okay?

TREICHEL: Yeah, well, do something because I don't want to watch my watch.

COHON: No, don't watch your watch. I'll watch mine.

TREICHEL: Okay. For a while, I've been very concerned about this whole process and maybe it's just that I go to too many meetings. But, it gets to the point where you almost think you're going crazy and that could very well be to many meetings because you hear so many loony things and conflicting things. For just a minute, I'd like to have you take off your professor hats and put on a regular person hat, probably an Armagosa Valley resident had, and just start to take a look at this.

One of the things I did was I just started writing down what it was that was bothering me so I could figure it out and I decided that the title of this piece that I may somebody finish would be <u>Alice Does Virtual Reality</u> or the <u>Yucca Mountain Project Goes Through the Looking Glass</u>. If you remember the story of <u>Alice In Wonderland</u>, she came upon a bunch of things that weren't understandable. And, that's exactly what this process is doing. We've been asking the question all along what could you find that would disqualify the site? Well, I finally now--they used to be able to tell you because you used to be disqualifiers. Then, there was a lot of humming and hawing and whatever. Now, the question is very quick. It's nothing. We're still in site characterization. We're still trying to figure this thing out, according to DOE, but the answer is there's nothing that could be found that would disqualify the site.

So, there, you have it and the two things that a repository has to do is it has to keep radiation away from people and it has to do that for the dangerous lifetime of the radiation. That's it, period. And, we've all known that; everybody in this room has known that. It's known internationally as other countries are looking for repository sites. We were told when Nevada was singled out that that's the two things that Yucca Mountain itself, the block of rock, would have to do. And, we could be assured that it would do that because there were rules in place and it would have to be able to show compliance with those rules. Well, you know, everybody here knows that as more was learned about the site, the rules went away. The rules still aren't back. But, in presentation after presentation, you see that they are complying with proposed rules; rules that aren't even there.

Those proposals were given some real harsh treatment in large meetings like this where those seats were filled with members of the public who stood up and told NRC we do not want any part of--performance based, decision-making. There's no history. You don't know what the real risks are. You haven't got any performance you can look at. They told DOE when Part 60 turned into Part 963 that, no, we don't want to take away disqualifying and qualifying conditions. There has to be a pass/fail on this thing. There has to be something that would put it down. There were loads and loads of comments.

We've never seen the finals. We've never seen why the public was blown off. So, you know, the constant monitor that we hear is science will decide. This is sound science. This is not sound science. This is adventures in math. With all of the graphs that you see, Graph #15 from the last presentation with the horsetails, those aren't just horsetails. That's what I told you. Don't be professors. That's stuff is really fun. I would be delighted if Yucca Mountain, Nye County, this area were turned into the University of Geologic Nuclear Waste Disposal and it would be wonderful and you could listen to these presentations and you could do the studies. There's generations worth of studies. There's PhD programs out here waiting to happen. But, this is not the place where you build something. Whether it's phased in, whether it's modular, whether it's go for it all at once, we're not ready to do that. And, those horsetails aren't just an academic experiment. Those are doses to individuals. Those are damage to the biosphere.

This is an Alice In Wonderland situation that we're looking at here. As you all know, you see those graphs and it shows 100,000 years, 200,000 years, and a peak dose out there. Well, then, you just establish a line where you cut it off at 10,000 years, you make your first package go bad at 11,000 years, and we're home free.

So, just in closing, my daughter who used to work for me is a wonderful graphic person and I'm not as good and I couldn't make it in the beautiful color.

COHON: Thank you, Judge. Kalynda Tilges.

TILGES: I guess I'm going to have to come off to the side or I'm not going to be able to see over the top. You might all go to sleep on me.

My name is Kalynda Tilges. I'm the coordinator for Citizen Alert. Sitting here the past couple of days, I've come up with some questions and comments. The first one is a question for Lake Barrett. But, before he answers, I'd like to get through all of this.

This project seems to be changing so much. It's not the same as was originally presented in the draft EIS. I'm really curious as to what this project is supposed to be. Your predecessor, Lake, Dr. Itkin, has three different views of the mountain from what I've seen at a Technical Review Board meeting at Pahrump. He presented it to the technical

Review Board as a flexible repository design because, well, we don't know what we're going to come up with. At a presentation of Congress, it was permanent deep geologic disposal. At a meeting that myself and a member of our board and other members had with you and Dr. Itkin at the Forrestal Building in D.C., Dr. Itkin told us it was going to be a flexible design, more like--retrievable storage because we don't really want to close the mountain. Fifty years or so from now, we're probably going to need to go back in and get that stuff because of energy crisis.

So, I'm kind of curious as to what your view of this project is now. Is it the same, is it different, and I know you want to answer that, but I'd really like to get through all of my stuff first. That will give you time to think.

Number two, this is to the DOE. If you're so uncertain about so many things, how can you be certain that you're going to be certain by the time the SR comes out? It seems to me that there shouldn't be an uncertainties in a project of this magnitude for the site to be recommended. There's been approximately 15 years of study on this project and with so much still to go, how can DOE push so hard? The tests need to be completed and all data in before the site recommendation goes.

Talking about the waste packages, we're relying so heavily on the engineered barriers and the waste packages. My thought is that while you're still uncertain about everything, this project needs to be put off until the certainties are covered. Maybe a good way of testing these packages to make sure they really work other than computer modeling with more uncertainties is to repackage the stuff that's out at the sites and these marvelous new waste packages that you've shown up on the boards and leave them out there for 100 years or so and see if they really work. Okay? That would be a good way to test it. Don't test it in my home.

Along with that, I have to say that I would have much more confidence in the Department of Energy if they had the guts to go to Congress and say we need more time. We should also be studying other sites because real sound science, you lose all look and thought of sound science when you only pick one site for study. I realize that was Congress; that wasn't the DOE. But, you should have the guts to go tell them that this is not sound science. We need to study other sites. And, also, maybe going and telling them this just won't work like the truth. Also, with the Department of Energy changing its own siting guidelines and the Nuclear Regulatory Commission changing its licensing rules to approve to get this all fast tracked through, it appears that all of these meetings, all of these studies, it's just a formality.

I have to say that this is being done to an unwilling public. The majority of Nevadans oppose this project. No one gave you our permission. No one asked you to come out there. No one asked for this project. You weren't given permission. The majority of Nevadans don't want it or you out here doing it. I say that because of polling results and I also say that as a representative of the thousands of members across the State of Nevada that Citizen Alert has. I also say that for myself and my family. Just for the record, we don't want you out here. We don't want your project and you do not have our permission.

Thank you.

COHON: Thank you. It's not necessary, Lake, but if you care to respond to the first question, you're more than welcome to.

BARRETT: Barrett, DOE. Let me try very quickly and then I'll be here all day and we can maybe talk more on it later, if you'd like. Basically, it's all the same thing with the exception of the energy thing at the very end. We don't know--this will be a hundred year plus operation. We don't know or pretend to know what we're going to be 100 years from now. We believe with the science and technology we have now, we can design a flexible facility that can adapt new things as we learn them in a learning organization and we don't want to preclude hot, cold, or different designs at this point. We must demonstrate to the regulator and to this Board that we do have a fundamental sound science way to go forward that can responsibly deal with the material that we have already made and are making today in our energy war.

So, basically, they're all the same kind of things, just different times and different meetings. It's the same fundamental design that is not just we know exactly the design and this is it and we're not going to change it. We need to be a learning organization and adapt as we learn new things. And, there are always uncertainties in anything we do. There's an uncertainty this building could fall down type of thing. So, there's always uncertainties in any endeavor.

COHON: Thank you, Lake. Next is Merlynn Rose.

ROSE: My name is Merlynn Rose and I work for Shundahai Network as an office manager. I come here today as a concerned citizen from Nevada. I was born and raised here in 1968 and I've been here all my life. I'm raising a family here as a single mother. Yucca Mountain scares me to death. Okay? There's a lot of things about it that me, as a common member of the public not having scientific knowledge, I don't need to have the science to tell me that that's not right. That you're going to stick something in a mountain that could blow up. It doesn't tell me it's right that there's a water table underneath that mountain that could somebody rise up into that mountain and take that radioactive water from those casks back into it and distribute it through our water tables to the people who are living here.

We just moved out to Pahrump, Nevada to help work with the people in Pahrump who are very concerned about Yucca Mountain happening because this is their lives. We have people all over this state that are extremely concerned about this. You're not only talking about that, but you're talking about transportation of this waste. If you put that mountain through, that transportation and that waste is going to happen.

We are humans, human. We are born to make errors. As scientists, you know that we are not perfect. So, what is to say that somebody doesn't make one human error that caused thousands and thousands of people their lives; one error. I'm up here to ask you people as humans, not scientists, as humans, to really look into your hearts to say, you know, this is not right. All of what we read about now, there's still stuff that says that this stuff is not going to be good for our lives. It's not going to be good to bring this into our homes. It's not good to bring this radiation into--which is Western Shoshone Nation which is, under the Treaty of Ruby Valley in 1863. You don't have their permission. It is their land.

That is all I have to say for me. I brought a letter from my son. His name is Jonathan Deyarmond. He's six years old. He's in first grade and he knows what's going on here. I have a letter from him which has a comment that says, "Don't put the waste in the mountain because it will get into the water. The mountain will explode and everybody will die." And, he's got a picture of the mountain erupting. This isn't something that I told him about. This is something that he knows because he sees what's happening around today. I want this submitted into the public comment because this is our future. This is only one six-year-old child, but this is our future. And, we're messing with at least seven generations of people here, you know. This isn't just about us in this room. This isn't about our scientific studies that are on paper. This is about our lives. This isn't about figures.

You know what I've heard a lot around here today and yesterday is a lot of I don't know and maybe. And, I don't know, but I want to live. Okay?

COHON: Thank you. Next is Piper Weinberg.

WEINBERG: Hi. My name is Piper Weinberg. I'm also working with Shundahai Network. A lot of the things that Merlynn had to say, I have to agree with. But, first of all, I'd like to say that I really support all this research going into figuring out a way to hopefully properly contain all this radioactive nuclear waste. We know that it's in containers all across the United States right now and it is an enigma, it's a problem, but it's unclear if the solution is to bring it from already contaminated sites to one central site in Nevada. The question is why would we bring it to Yucca Mountain? This land, as Merlynn has said, is according to the 1863 Treaty of Ruby Valley, Western Shoshone land. The DOE has maps that show the Western Shoshone and habitation of this land. As Corbin Harney has said, Western Shoshone people do know that this is their land and many are opposed to the Yucca Mountain siting. So, how does that influence the decision to bring 77,000 metric tons of nuclear radioactive spent fuel to this particular location?

Another question; why Yucca Mountain, why did you choose a land where there are threatened species? The Desert Tortoise is around. There are five different other species that are classified as sensitive by the BLM; two bat species, a lizard species, Allen beetle. There are other problems with Yucca Mountain. There are around 33 earthquake faults. The past few days, we've been talking about how water will move differently through faults. It's still unclear. These are uncertainties that we're aware of. It's not only an earthquake zone, but the water is moving through the mountain. We're still, as we say, unclear why this particular site is chosen and why we can't look at other sites or even contain the waste at already contaminated sites.

If we do truck the waste or train the waste across the country, we're going to expand the scope and the scale of how the waste will affect people. The waste will be going through 43 states. That's another thing to think about. Why would we want to do that? And, as young Jonathan Deyarmond said, a six-year-old, if you're concentrating so much nuclear waste in one particular area, there is a potential for that to change the geology of the mountain. Even if we're looking at plans to not concentrate the waste in one site, but to have it in other sites within the mountain, again that's another uncertainty. We don't know how that's going to affect the geology of the mountain, how it's going to affect the water table, the water movement through it.

One last thing is that Yucca Mountain is already theoretically full. All the space that has been designated for nuclear storage is already claimed. It's already full of waste and we're still continuing to create that waste. So, why are we looking at this particular mountain and why are we continuing to create this problem again and again? We're going to have to go through these sitings. We're going to have to go through all this research of how to contain this waste so it's not affecting people as drastically over and over for decades if we don't stop creating it as we speak.

Thank you.

COHON: Thank you. Sally Devlin?

DEVLIN: Thank you, Mr. Chairman, members of the Board. I'm Sally Devlin, the public. And, I brought with me a letter that's almost a year old from Senator Bryan. This is regarding rural health. Our brilliant Congress passed a law that says you cannot get any rural health unless you're within 300 miles of the hospital. And so, of course, that eliminates Pahrump. When I spoke to him about this and I spoke to Congressman Gibbons and other people, they said they forgot that you in Pahrump can be locked in for as long as three days with forest fires, floods, and a few dozen things. We were locked out again last month.

So, I do want everybody to know why I am asking Mr. Hess at Bechtel for 50 million because we're eliminated from the Government and all kinds of things. I'm sitting here looking at 200 people or so and I'm saying, please, nobody get sick. Don't ever die because we have no medical here, whatsoever, in Amargosa.

The other thing I have to say to you is--and it's really pretty scary. If you can't stand the seats anymore and you want to commit suicide or something, do not do it in this building. Go outside on the grass so you don't make a mess. Thank you. I hear somebody's got a funny bone. But, understand, you are completely at God's mercy or whatever you want to call it at this time.

I do want to make some comments that are not quite so funny to Mark Peters and that is you provided a wonderful program on the waste packages and the drip shields and all this wonderful modeling and degradation of the models and the design and so on. But, the thing that bothers me the most is you did mention the colloids, but you forgot my bugs. Now, for those that don't know my bugs, Sally bugs are microbic invasion. And, four years ago, they were picked up and we have been exploring them ever since. The bugs will be transported from the nuclear sites into the canisters and can make a big bloody mess all over the place. So, I want to hear more about my bugs and I'm sure the science will be looking into.

Bill, I loved your presentation and I love the oxymoron, quantified uncertainty. That was a new one and I congratulate you and I will quote it to my Toastmaster's Club. I love all these new words. They're absolutely marvelous. But, I do have to object to the projected numbers that you're using. I also see you're not talking about my bugs, but what is--because my bugs love the nickel and remember that. That was the first article I gave to the Board about three or four years ago. So, be very careful of that. I'm going to be watching.

But, we do want more information. I am always disturbed and Dr. Craig gave me a book and it went out 800,000 years. You're starting out at 1,000 years. I am terribly sorry, but I will not play with you because if I bought a coffee pot, I'd get a warranty. And, I want from President Bush a disclaimer signed in his own handwriting saying this is safe from Day 1 when they put the stuff on the road or pack it up or whatever they do. And, I think the public has a right to this disclaimer. So, when you go back to Washington, you ask George, please, that Sally wants a disclaimer.

So, this, I will leave you with because this is very serious stuff and I am very disturbed with the numbers. I don't think projecting out a million years or 100,000 years solves the problem that we have today. I know it's all very new and I congratulate you and I hope that for the next 25 years, you have a lot of fun learning because I'll be right here standing and saying we need a hospital and, please, go outside on the lawn when you want to commit suicide.

Thank you.

COHON: Thank you, Sally. My thanks to all for their comments during this public comment period. Recall we have another one at the end of the meeting this afternoon.

We will now take a break for lunch. We'll reconvene at 1:25. Thank you.

(Whereupon, a luncheon recess was taken.)

A F T E R N O O N   S E S S I O N

COHON: Welcome back to the afternoon session of our meeting. We're going to start the afternoon with a session on decision-making in a learning environment. We actually have a bonus. That is the agenda has called just for Russ Dyer, but we actually have four of these leaders of the program on a panel to deal with this. Lake Barrett, Bill Boyle, and Bob Andrews have all presented before in this meeting. They need no introduction and Russ doesn't either because everybody knows Russ.

So, with that, gentlemen, take it away.

DYER: Thank you, Dr. Cohon. Let me preface this by starting out and saying that this is more of an introduction and an invitation to a dialogue than a strict presentation here. What I intend to go through is some of the status and some of the history and talk about some examples of how the program has changed in response to various things over time. And, I also want to talk about some of the things that are set before us and our proposed approach to that.

Let me start off by kind of stating the obvious. During site characterization, DOE must make some decisions about he repository and they may be in the form of assumptions, they may be design decisions that could constrain future actions or decisions. Now, are any of them irrevocable? We'll talk about that a little bit. But, as Paul Harrington pointed out so aptly, the whole philosophy behind the program is that of a phased program or one that moves from one stage to the next stage in incremental steps. There's never one huge, giant leap that gets you from the beginning to the very end. So, there are a series of staged decisions or steps that one goes through.

Now, it is a reality that over time the ongoing scientific investigations will provide information. That information may bring into question certain understandings or states of knowledge. There may be new tests that get brought online to test hypothesis that are developed or to test out new ideas. That provides the opportunity to bring this new information into the decision process and I won't say revisit, but you do in a way revisit the impact of some previous decisions. There are some that you can mitigate or change by changing some features of the program. There are some that you address in other ways. DOE and I hope to show you this; that we have not only the ability and the intent, but we also have a track record of responding and adapting to new information and incorporating it into the decision process.

Steve made a point of this yesterday, Steve

Brocoum. The test & evaluation program of which performance confirmation is a subset is a long duration activity. As long as there is a program, whether it be in the site characterization, in the operations, in the monitoring, or even in postclosure, there will be some kind of a program that will bring information into the program back in before the decision-makers and it's going to increase our understanding of the behavior of the natural and the engineered systems in comparison to our previous state of knowledge or our predictions.

We'll evaluate this new information for its effect on system and subsystem performance including design and I'll talk about some other things that one looks at when one considers new information. We have the ability and the opportunity to revisit some of the design and operating decisions and modify them based on the feedback and evaluation in light of this new information.

One thing that seems to be--it may be a semantic disconnect--is some people have a perception that the narrow performance confirmation program is a program that would merely confirm that dials on instruments haven't moved too much and that the system is still standing. In my view, there needs to be part of a long running program that is robust enough to challenge the validity of the models that lie at the basis of our understanding of how the system works. That will change with time.

The bottom line here, the last thing on the slide is that decisions can be revisited and I'll show you some examples of some of this.

1990 was a seminal year for the program and the project. A couple of things happened in that year. In late 1988, the site characterization plan came out and then we had before us the task of how to really start implementing a program that was laid out in the site characterization plan. At about the same time in 1990, a seminal product came out; the 1990 National Academy of Sciences Re-Thinking High-Level Waste Report. I put a few quotes on here, but some of the things that the NAS laid out in their report got incorporated into the program at a very early stage pretty much as a philosophy of how we do business. I'll show you some of that.

But, some of the things that were fundamental to the NAS's proposed approach for the program was an evolutionary program that took advantage of the state of knowledge at a point in time and made decisions, moved forward to the next stage, if you will. The whole concept of a stepwise or stage approach is pretty fundamental to the NAS's idea. The idea of revisiting or what I'll call a robust testing program is also embodied in the NAS report.

Let me go to the next slide which is Figure 5 in your diagrams. Now, what we struggled with in the early days was how to put in place a program that was iterative in nature and that would, in fact, not just go out and collect data points. At one point in time, there was an idea that one would just go out and gather data, assimilate the data, evaluate it, and make a conclusion. It early-on became obvious that that was not a practical or a very realistic approach to this. Even a simple test, any simple test, has a conceptual model behind it. If you look in the data of the tables and you read, let's say, a permeability for a certain hydrostratigraphic unit and you read a single value in there, it's not clear just from that data point whether that is truly an isotropic homogeneous media or if we just used an isotropic homogeneous approximation to it. You've got to read more. You've got to read the actual report that talks about how the test was constructed, what the test was designed to accomplish, how the data was put in, how it was evaluated to really get a sense of what that number means.

We talked earlier and I'm going to come back to this in a minute because in the iterative cycle early-on we've put a lot of emphasis on the test planning. What is a test going to do? Is it going to go out and just gather data or is it going to be a test that is robust enough to differentiate between alternate conceptual models. We had a multitude of alternate conceptual models laid out in the site characterization plan and we had tests that would help us resolve whether one of the alternate models should become a preferred model. Very few tests, in and of themselves, are actually definitive, but you can put together a suite of tests that give you a much better confidence that your preferred conceptual model should, in fact, be preferred. If you get new information that is inconsistent with the model, then you need to revisit your whole framework which starts with a discussion of the conceptual model, itself.

There's a lot of detail in selecting the test, fielding the test, and so forth, getting the test results out, and then evaluating the test results. And, you need some context to evaluate the test results in. Let me use for an example some of the surface mapping that we did early-on. That was a fairly discrete activity. We laid out a geographic area for which we needed information. It was reasonably clear when that effort was done that that was a test that didn't need to be revisited for a while. There are other tests, certainly, evaluating whether the test has actually been completed, whether the information that's been acquired is sufficient for a particular purpose needed an evaluation of some kind.

There were a couple of main users at that time for the information coming out of the site characterization plan. One was feeds into design because we were and we still are, I think, well-ahead of our understanding of the natural system as opposed to development of the engineered part of a repository system. The other is how important is this information? In the early days of performance assessment, the 1991--performed its assessment, there were a lot of assumptions that were stated just so that we could get started on that. You can think of it as essentially starting out with a set of assumptions with a TBV column to be verified. Over time as we've actually got measurements, have actually run the test, those assumptions have either been validated or they've been replaced by another way of thinking of things. We've been through, I guess, four or five iterations of TSPA and each one of those has caused us to go back and reevaluate what is really important about our understanding of the behavior of a repository system.

There is a loop in here on this slide. There's an interactive loop right there which potentially takes us back to the very beginning. If our state of understanding is inadequate to support a particular decision, a judgment, at a point in time, do we need to construct another test to look at some other aspect of either the natural or engineered system or do we need to change our framework or change our strategy? So, this was one of the big decision boxes here. Early-on, we somewhat naively perhaps were thinking that TSPA would be a tool that could help us with most of these decisions.

Now, let me go to the next slide and this is where we may have quite a bit of dialogue. Over time, the idea of the TSPA, total system performance assessment, pyramid has evolved. I think it may have evolved beyond this diagram. This kind of a construct may be confusing us more than it is helping us now. But, the idea was that we would take all knowledge down at the base of the pyramid and that includes information garnered within the program, that information from the literature done by other people in the technical community, work in natural analogs, everything that is known would lie at the base of this pyramid. But, you need some organizational scheme to make sense out of that. We've tried a couple of organizational schemes over time. There is all knowledge and then there are some reports that we put together. And then, to kind of summarize the probably tens of thousands of individual reports that we have on the project, we came up recently with the concept of the AMRs, the analyses and modeling reports, and then building up from that to the process and modeling reports, and then at the very top of the pyramid is TSPA.

Now, TSPA has always had limitations. We've known that. It's a tool that is designed to evaluate regulatory compliance. As the regulatory construct has changed over time, the focus of TSPA has also changed. There are other tools that allow you to evaluate other aspects of performance that lie below. I guess, one of my aversions to the TSPA pyramid is it gives an image of everything feeding up to the very top. And, you have the high priest of TSPA here who is the oracle who brings forth a pronouncement of what is happening. But, there are tools down at the process models, process model report level or down at the AMR level, that provide you insight into how specific elements within the overall construct operate. Now, the challenge is to make sure that the most important parts of that understanding are rolled up into TSPA and properly accounted for in TSPA. That's why we have had these iterations over time. As we get new information, new knowledge, we have refined the model, and I expect that will go on for decades, if not centuries, that our understanding will increase.

As I said, the TSPA pyramid has been used. We can argue about whether--or argue is not a good word. We can have a dialogue about whether it's an accurate representation of how we actually execute our technical program. One of the things in your handout, I'm not going to put back on the slide, but if you'll notice, there's some arrows in the side which indicate feedback which are consistent with that original kind of block flow diagram that I showed. As we get information that challenges the adequacy of a particular model at whatever level, what do we need to do to review our technical basis and to gain greater confidence in our technical basis?

It's fair to say that we're currently considering alternate representations of how one takes the vast body of knowledge that exists in the project and in the world and summarize it or bring it together into a fairly cogent argument that incorporates all the important things and gives you an evaluation about the performance of the system. Now, there are going to be a need for different tools for different things. Regulatory compliance may need one tool. Other venues may need other tools.

This is another representation which is the same story told a different way. At the very bottom down here, we have data collection which in itself is a non-trivial exercise that I hope you appreciated from Mark. Let me just concentrate first on the series of blocks that run up this way. You'll see a series of technical reviews that are interspersed with each stage in here. We look at the major stages, data collection and then a technical review, analysis & modeling and a technical review, abstraction modeling and a technical review. But, the reviews that are conducted at each stage along here are different. For instance, if you're reviewing the adequacy of the data collection for a particular test, what you're looking for is have the procedures been followed, was the test plan adequate, did the test meet the objectives that were laid out in the test plan? Fairly fundamental questions that you would ask. Is the test complete or is this an incomplete set of knowledge that we expect to expand on with time? That information is fed into a higher level understanding, the analysis & modeling report, which may take these very process level understandings of how some compliments of the system work and try to make sense in a larger scale, make a larger system view of them. And, there is a degree of abstraction that's involved here. The technical review at that stage is to make sure that this first level abstraction is consistent with our understanding that the data is honored, if you will, in the abstraction and that we haven't overlooked, say, alternate explanations, alternate conceptual models that might just as well be a way of representing or handling or treating the information that we have.

And, you can go on up the pyramid or in this case up this pile of boxes and get to more and more abstracted concepts of how this system behaves. At the very top, of course, is the TSPA calculations and there's a technical review after that. We've touched on many of the things in discussion of TSPA, discussion of treatment of uncertainties. Those are very legitimate questions that bring us back to the question of how robust and how adequate is the TSPA results that we have. How adequate are they for the intended purpose?

Now, not everything, not all the knowledge that we have in the project rolls up into TSPA. There are other, I'll call them, documents that reside off to the side, on the left hand side here. The site description document. I think we're in the second or third revision of the site description document which has been a summary of our understanding of what the characteristics and processes observed of the physical system are. Those are again drawn and consistent with the data collection and some of it may have some of the analysis and modeling kinds of results in it, but it's another way of capturing that information.

The design itself and the concept of operations is another place where another description of our understanding of the system resides. And then, the process model reports up at the upper left hand corner here, are a higher level, kind of a system level description broken down right now into our nine major system elements that we've broken the system down into.

Now, what we've shown in blue are primarily internal reviews, but there are also inputs and reviews from external sources, also. Those go on--some of them go on in, more or less, a periodic cycle. Some are, as a particular report or document is finished, we may ask for an external peer review. We have formal panel reviews of the Nuclear

Regulatory Commission, of yourselves, the NWTRB. They would figure in as some of the places where we get outside advice and counsel as to the adequacy of some of these products. And, of course, we use outside experts to help us in the formulation of tests and the interpretation of some of the test results.

As I said, the review criteria to each stage in this box, each one of the review boxes, has a different purpose, a different focus. I'm not going to go through these. I talked about some of them earlier. If we need to, we can go through some of that in the question and answer or, I hope, a dialogue period afterwards.

The whole concept of evolution and continuous improvement is built into the basis of the way the program runs because we had a recognition early-on that we were going to continue to get information over time. The whole concept of flexibility, as Paul Harrington talked about, that's one of our precepts for design because it's presumptuous to think that we know everything now that we will ever know. The feedback and reevaluation is built in and I'm going to give you some examples in a little while. There's a point, though, that I'm going to make with some examples and that is that as we make the decisions, the decisions that are made are appropo to a certain stage or phase of the project. So, is the information adequate for this particular stage or do we need to modify something or do something else?

Now, let me jump off of the slides here and talk about some examples and talk about management of the program. This is an exercise in risk management. Every project is. When a potential changes comes in, there's a couple of questions that come to the forefront. One is how urgent is this issue and second is how important is this issue? They are not necessarily the same thing. Let me give you some examples.

Something that is, what I would say, urgent and important is there an immediate public or worker safety and health issue involved? It may not be with the testing program, at all. It may be with the environment underground that the workers are in and it may need immediate attention on the order of hours. Is there a concern that has been raised that if true would suggest that there was a potential fault or failure mode in our safety case? Our safety case has evolved with time as our understanding and our approach has changed. If so, how immediate is it and how important is it? That needs an immediate evaluation. Then, depending on how the evaluation, what the sense of importance and urgency, that pretty much informs us of how the response should be framed. Is it a potential improvement in the safety case?

Is there something--and the example I'm going to use is Bo's Shadow Zone which appears to be a good idea. It if proves out, it may have some implications for credit that can be taken in the performance of the natural system. But, the next filter that you need to put on it is a risk benefit analysis. Now, if I do this, what will it cost me and what will I gain from it? If the cost in dollars or schedule are low and there is a benefit, then obviously it's worth pursuing. But, very seldom does something come absolutely free. It's true that there ain't no such thing as a free lunch.

What is the potential impact and how does one balance a new idea or a new concept against the other things that are already judged to be important that are going on in the project? And, we have different tools for dealing with that depending on what the situation is. One of the calls and it is often a judgment call is if our understanding of some aspect of the system is judged to be adequate--that is for the intended purpose--yet another approach may be right--that is get us closer to something that's a closer approximation to reality--what is the risk benefit involved here? How much do you gain from pursuing a program that increases your understanding, but it has a cost to it, versus, let's say, we have an approximation that is--and let me use the neptunium case, for example: I think by most standards, a very conservative bounding estimate. Now, is it worth our while to put effort into a large program to really-

-let's say that we only had a few data points and one was high and a few were low. We have a pretty good case with neptunium. If you're confident that you truly have a conservative bounding estimate for something, is it worth the while to--and worth the project resources--to pursue trying to make that better in the realistic sense or is this conservative bounding approximation adequate for the purpose at that time?

Those are decision we face every day, all the time. Let me give you some examples of some decisions that we have looked at recently within the last few years and some of the things that kind of played into it. The first one I'd talk about is ^{36}Cl. Whenever the initial ^{36}Cl data came out, it caused some major perturbations in the program. We re-looked at the engineered barrier philosophy. We looked at the adequacy of our hydrologic models, the UZ models. Prior to that time, there was some thought that maybe we might be able to use an equivalent continuum model. We've now gone to a dual continuum model that Bo talked about. So, there were changes that were made in the program because we thought this was something that was very important and that had potential impact on the basis of the safety case.

Now, there is another aspect to it because the scientific community said, you know, we've got a lot of other conceptual models out there and a lot of the evidence doesn't seem to be consistent with what we're seeing here. Can we get a second opinion? And, that was pretty much what the driver was for the ^{36}Cl validation study. We continued using the information that we had from the original ^{36}Cl and that's what you see the current models and approach based on. But, we needed to get a better feeling ourselves of where we really stand as far as our confidence in our scientific models.

Now, as a prudent manager, I'll also tell you that there was a cost consideration that was involved. Whenever we changed our approach in the waste package design, our engineered barrier cost went up by billions of dollars. Now, is that a necessary expense or is it worthwhile looking at this and making sure before we commit those dollars that they're really needed for this situation?

The other example I'll give you is that of the Richard's barrier which we talked about, oh, I guess two or three years ago. We were talking about the possibility of putting a Richard's barrier in the invert over the waste packages after the waste packages had been installed and instead of a straight backfill, constructing a Richard's barrier using a fine granular material on the bottom--I'm sorry, a coarse granular material on the bottom and a finer granular material on top. And, theoretically, it seemed like it had some very, very powerful implications in performance space. So, we did some proof of concept tests and found that, yes, the concept of a Richard's barrier does work, but other considerations led us not to take that concept into the current design concept. Guaranteeing that you could maintain that division between fine and coarse grained materials over very long times was difficult to do and to justify.

Let me take the last thing and that's Bo's Shadow Zone that he talked about yesterday and what kind of considerations need to go into that that will kind of dictate things that we will look at as we make decisions in the future as to whether to pursue that and bring it into our conceptual model and eventually into the construct in the TSPA.

First, is it a reasonable hypothesis? That's being debated in the scientific community now. If it is a reasonable hypothesis, what kinds of tests might we have that could--I hesitate to use the term "validate"--but give us confidence in the adequacy of this particular model? Then, what will it take to field those tests? How long will it take? What kind of information might we get out? Is there any suite of tests that can give us a better confidence in the validity or non-validity of this particular concept and bring that forward, understand what it would cost us in the way of time, people, dollars, and when that information might feed into a decision process?

So, those are all things that weave in and out of our decision process. The point I'd like to make is that this is an interactive process. It has been iterative since as long as I've been on the project. It will be iterative in the future because we will continually get new information over time. We've got to accommodate that new information within the construct of the project.

I think that's what I said in the conclusions, Page 11. With that, what I'd like to do is start a dialogue here.

COHON: And, I'm sure you have. I see many hands go up. Just let me do a time check real quickly. I expect that we're going to have a very interesting discussion right now and I don't want to cut that short because not only will it be interesting, but it's very important.

Bill, my guess is that your safety strategy presentation is going to be really quite brief, like 15 minutes, tops?

BOYLE: Sure, tops.

COHON: So, I think we're on pretty firm ground if we let this go for a good 20 minutes to a half an hour. I think we'll still be okay. All right? Norm?

CHRISTENSEN: Christensen, Board. First, I'm going to make maybe three observations and then invite you or others to comment on them and they really have to do with maybe general observations about learning cultures and then the specific situation that we're in with regard to Yucca Mountain and SR and licensing.

One of them has to do with what I guess I'll call the political constraints; one of them dealing with perspectives and the other one maybe with sort of human nature. Maybe I'll start with the human nature one. I reside in a world that prides itself as being the archetype of a learning culture and yet universities are populated with a lot of liberals who are from the most conservative institutions I know. It is to say that I think that the process of change becomes very difficult because we are human and that's just an observation that I think it is a fundamental one and difficult.

The political one has to do with thinking about this as a continuous process of learning in an environment that has some discontinuities in it and probably some of them involve SR and some statutory decisions that will be watersheds. That is a point at which rolls for the Secretary of Energy, the President, the Congress, the State of Nevada represent discrete decision points that are set points in time and are not within the control, necessarily, of DOE and yet in many ways will punctuate this process, and in effect, may well constrain the kinds of questions that can be asked at various points in the decision tree before and after that and how a learning process might work. I'd like you to maybe comment on that.

And then, the final thing I would offer in terms of perspective is that at one level, I think, given the complexity of the process that you're all dealing with and the inevitability, as the Board has commented on and you have commented on of uncertainty, that all of this makes perfect sense. From another perspective, it also presents critics and others with a constantly moving target and one is playing off between that problem of the moving target versus the area of uncertainty. They call it a moment when in the middle of the controversies over acid rain when Bill Ruckelshaus commented to one rather well-known scientist "what do you mean you don't know how many acid lakes there are", there's sort of the ability to accept uncertainty on the part of the politicians and for that matter the public.

It's a little bit rambling, but maybe with those thoughts, if you would like to comment on any part of that, I'd appreciate your thoughts.

DYER: Well, let me start, Dr. Christensen, and I'm looking to these three guys that are up here to give me help. You're absolutely right. Change is difficult by human nature. It involves pain in some form. But, we know that the most successful individuals and the most successful organizations are those that can cope with change. Recognizing that it's going to happen, essentially using it as a given and expecting change rather than fighting it, to me, is a realistic outlook. There are some individuals that have more difficulty accommodating change than others. I think it's fair to say that anybody who has worked on the project for over 10 years has a high threshold of change toleration.

Your next comment about the--

COHON: Russ, I'm sorry. This is Cohon. I'm sorry to interrupt. But, normally, a very important point that I don't think you got. You were talking about change as something that happens to you. Norm is talking about change that you cause to have happen internally. Those are two very different things.

DYER: There are two contexts. I mean, there are external forces that drive change. We have certainly responded to that and every year it's anybody's guess what the budget is going to be. I'll use that as an external change influence. There's also change that is generated internally. I mean, the ideal if for us to recognize and anticipate the need for change internally and respond to that before an external stimulus causes the response. Did I--

COHON: Yeah, thanks.

DYER: And, you're absolutely right about the changing nature of the program is frustrating. It's frustrating to people internal and external to the program because our understanding is dynamic and it is changing and the ideas and concepts are changing as that understanding changes. But, there are precedents for that.

You specifically mentioned the site recommendation. As I said, there is a staged or stepwise approach and that's one construct that one can look at this endeavor in. One way to look at it. I'm not going to say it's right or wrong is that the site recommendation decision becomes essentially the national investment decision as to whether to go ahead with the next phase which is not construction operations of a repository, but a licensing process. There is going to be new information that will be gathered in that stage or phase of the program. The idea that the repository design that you heard Paul talk about would necessarily be the design that would be in place actually necessarily constructed, that may or may not prove out.

Let me take an example from the regulated nuclear utility industry. One has an operating power plant. It's up and running. There is a safety basis for that power plant. As new information is gained, it may be necessary or preferable to change that safety basis. The change may be directed internally. It may be a way that the operator has determined that they can make the plant operate safer, more efficiently, and cheaper in which case they can make a proposal to the Nuclear Regulatory Commission. It's never a unilateral action. Or it may be generated externally; some information comes out about behavior of some piece of material that's relevant to the assumptions in the safety case that you had before you. And, we're going to have to face up to the fact that there's going to be a continuing change in our understanding over decades, if not centuries.

CHRISTENSEN: Let me ask just one specific thing here and it relates to that point. It's on your Slide #5. It's that kind of decision tree diagram where you come to a decision box on that where if the answer is no, you return to Phase 1 or site disqualification.

DYER: Right.

CHRISTENSEN: And, I guess, the question--and I think this really gets to the core of concerns that may exist in a variety of constituencies--is to what extent does that question change or the loop change once you pass SR? Is that question the same? And, this is not just a question for DOE, but, in fact, it is a public policy question. The extent to which the public, the decision-makers, can conscience that question in exactly the same form following that watershed moment. And, that's sort of what I was getting at is that the decision process, I think, has the discontinuities built into it that make the process less than continuous.

DYER: That's a very good point. Let me take a shot at it. I see Lake jumping in his seat here. In my mind, I see this process continuing because one must always challenge the basis of the safety case, no matter what point you are in the system. And, if new information comes up that suggests that you have a fatally flawed safety case, there may be some point at which the decision would be to terminate the activity. If you'll remember, built into the original Nuclear Waste Policy Act was the requirement for retrieval. So, I think that that kind of an option and a potential decision was envisioned by the people that framed the policy.

COHON: Lake, you want to speak to this?

BARRETT: I think Russ said it very well. I mean, in a nuclear safety culture, you're constantly questioning whether your stewardship eye for doing the right thing for the public--because we are public servants, all of us--keeping an eye on the short, medium, and long-term, including the very long-term for many generations. So, we constantly watch it. Just like if you're operating a plane, you're responsible as a plane operator to know when to shut the plane down based on information that comes in and you have to constantly be evaluating all your data, reading all your instruments in the long-term and short-term. It turns out this effort in a repository has a relatively long time constant to it. But, the same principles, I believe, apply; to be doing the right thing based on what you know and do it in the appropriate time scale and build in flexibilities to adjust to these.

COHON: As was pointed out earlier this morning, there are multiple NRC licenses, licensing decisions after the SR decision, any one of which could terminate the project if not granted.

Dan?

BULLEN: Bullen, Board. I couldn't have asked for a better lead-in from my colleague, Norm Christensen. With this diagram and then if you could move to diagram 8, please, I guess I would have a little bit of confusion because in my spare time before I came to this meeting I looked at your performance confirmation plan. And so, maybe in the light of constructive conversation here, I'd like to point out a couple of things. It's a very interesting read, by the way, because it talks about the identified performance confirmation testing and monitoring activities that you're going to do and Appendix G has 24 different things that are delineated there and there's a real nice table here that you go down.

But, I'll come to one for an example because they're all laid out in the same format and it's ventilation monitoring. Okay? It's just something that we would take a look at if you had an operating license and you were going. What I don't see here in the layout of all 24 of these is that each of them is divided into the purpose and then a description and then the parameters that are addressed. And, for ventilation monitoring, we've got dry bulb temperature, wet bulb temperature, air pressure, relative humidity, radioactive gas content, the one that might be very important, and then oxygen and carbon dioxide concentrations.

Then, we go to test interfaces and constraints which is where I thought I would see the tieback into where does the data go? And, what I don't see in any of these is what am I going to use this data for and how will I evaluate it? So, maybe, in Rev.03--I think this is Rev.02 of the performance confirmation plan--you might want to say, okay, we're going to take this data and we're going to use it in this parameter. But, I'm not sure that after you get to that point it feeds back into performance assessment. It may feed into some other evaluation that you have to do. The last one is period of performance or schedule, how long you have to do these tests.

The thing that I see as a disconnect are, one, does it necessarily have to feed into performance assessment or does it feed into something else? The question I have is that, say, for example, I do see radioactive gas in the off-gas, then where's the enunciator that says, hey, this is really a problem? Where does it say I have to have an exit strategy that says I've got to go and repackage or find that package and repackage it now or do I do a TSPA that says, you know, if I fail one every 20 years, I don't exceed the regulatory limit. It's not a very good sale point for the general public, but I don't exceed that limit and so it's not a problem.

So, what I'm looking for here or I'm looking for there is what's the exit strategy or what's the response strategy, I guess is a better way to put it? When you take a look at the performance confirmation tests, if I see something that doesn't necessarily just add to the data that I already have, how, as a learning organization, am I going to respond to that? And, I guess, that's what I look for is that as I looked here, I thought, okay, there's got to be something that says I saw radioactive gas, I better do something. And, I didn't find that. So, maybe, in the next iteration, you'd like to address something like that. In my spare time, I read too much of your stuff. So, you know, maybe you should be careful what you write.

DYER: Good observations. Now, let me ask a followup question of you. Have you read the test and evaluation plan?

BULLEN: I've read parts of it. In fact, I was at your office on Monday trying to get the most recent version. So, maybe, I'm missing some components there.

DYER: Well, you know, the performance confirmation plan is a subset of the test evaluation plan. I looked at the flow chart, the decision chart, out of the test and evaluation plan, the current version, which goes to two foldout pages about like this which I chose not to use here. There is a systematic approach to it laid out. I wouldn't claim that it currently addresses all the considerations that we have to address eventually.

BULLEN: Bullen, Board. And, I didn't mean to imply that you had to have all this done by November. This is just as we're talking about a learning organization, the things that you would see if you looked at the plans to try and say, okay, well, you're gathering this data, you want to use it to learn, you want to see where it's going to fit. The followon question is, well, if the data are bad or indicate something bad, then what do I do? And, that's what I didn't see in the performance confirmation plan. I'd be happy to look at the test & evaluation plan later and we'll look at that, but I was just trying to see that there's the process within the organization to take the data that you have and then to do something positive with it or constructive with it; not just put it on the shelf which I know you're not going to do.

BARRETT: We haven't started to develop that anywhere near to where that will need to be when we approach the operating phase. I mean, it's like limiting conditions of operation. The basic nuclear culture in place at reactors, you know, 50 or 59 questions--safety questions. Are you outside the safety gate, the safety envelope? All of that yet needs to be developed and we know we have to do that.

BULLEN: Okay. Thank you.

KNOPMAN: I, too, would like to follow up a little bit on some of the points that Norm raised. There are two different distinct points and let me just start with one.

To me, your learning environment, your description of it, makes a lot of sense, but you have an external world you have to function in. I think it's really worth talking more about the public process that goes on, the public understanding, the public's ability to keep up with an organization that is geared to change, as well it should, as information changes and circumstances change. You have imposed on you some external requirements for public process and public communication devised 30 years ago in a very different world, the EIS process primarily. It seems to me we're thinking about how you begin to shape your interactions with the public, rather than taking back part also of what's externally imposed as the public process and simply living with it. That is, you've got some flexibility here to operate in a different way as far as the public is concerned to help them help the public and decision-makers understand this changing process.

Has this come up? Do you have thoughts about how you start altering your public processes so that you don't have this number of people here who are walking around with their three-inch thick EIS and realizing a certain critical aspect it out of date. You know, there are areas that's not changed that much. It's the only document they've got in hand. They don't have the SRCR, but they're trying to follow this process. What's the program--what's a learning organization's response to this kind of--

DYER: There's a level of frustration there that I'm sure is echoed in much of the public because in the EIS process, we're almost forced to take a couple of bounding end member approaches and put a great deal of effort into analyzing what those end member approaches are and objectively trying to figure out what the impacts, pro and con, of each of those are. The assumption is that reality will be somewhere in the middle. That is a lack of specificity that is troubling to many, internal and external both.

It's is, at best, a difficult situation. I think one of the better approaches that we have was in some of the EIS meetings; having a panel of people available to hold a discussion, available as resources that anybody could ask questions to as to exactly what things mean here and what it might trend to in the future. That was one approach that we took that I thought was pretty well-received.

KNOPMAN: Just another angle on this. Norm raised the question of discontinuities in the decision-making and learning environment with site recommendation being the one looming ahead. And, I think there's a pretty substantial difference, I guess, in the kind of learning organization that you're describing, that you're aspiring to, or that you are on your way to being in a compliance mentality or culture which the regulatory process puts you in. There's a real difference because I think compliance doesn't necessarily mean a continuous improvement model that you have. Compliance means good enough to hit the compliance target. And, it's not just the nuclear. This is true with the Environmental Protection Agency and pollution control. There's a major debate going on in that area of how compliance, per se, can sometimes hold back innovation and change. I think what can happen when you go into a compliance regulatory proceeding or multiple proceedings because this is going to perhaps happen in multiple steps that you only have to do enough, you know, to get to the next hurdle so that you miss that bigger picture and a broader view of public objectives, put it that way, which is why I think we view the site recommendation as different because it's not in that compliance arena.

How do you propose to deal with that because from a strictly nuclear culture perspective you hit your compliance targets, I would think, but correct me?

DYER: Well, let me start and then I know Lake is very anxious to jump in. I guess I don't see the compliance environment as being a static environment. There's an expectation of continuous improvement on behalf of the entity being regulated that you would continuously try to improve your safety case. Your task is not just strict compliance, but it is continuing improvement of your charge for insuring safety and health of the public and the workers. I mean, that's part of what you're being licensed to do. You need to make sure that you're doing the right thing to bring that forward. I do not see that as a static environment.

COHON: One example, by the way, Russ--this is not challenging, at all, what you just said, but recognizing that compliance in an organizational sense is easier to deal with than continuous improvement. In a way, you're created your own compliance situation for SR. It's called TSPA. The language you all use is a wonderful demonstration. The examples you gave us, can I get credit for it in TSPA, that's driving--that's a very powerful factor, not the only one, but a very powerful factor in your thinking. And, I'm not faulting you for that because, I mean, you've got to organize this project somehow. You've got to have a basis for making all of the individual decisions you have to make, but it can also be constraining. Everything is defined in terms of what does it do to dose number in TSPA? So, Alberto pointed out a very nice one in the Harrington presentation. You know what I'm talking about, the sleeve that replaced--the ring that replaced the weld. And, we know you'll look at that, but that's a nice example of how TSPA can create your own kind of like quasi-compliance culture.

BARRETT: Just a couple of comments on Debra's thing. I mean, we basically as a group try to work these imbalances issues. The regulatory legal aspects is something we must live with. There's three basic points we came down to in a small group; technically sufficient decisions, they need to be legally defensible, and they need to be, you know, fair to the public. And, in fairness to the public, the key thing was try to communicate what's going on, take great pains on the opening transparency to website, and to try to get information out. We were very disappointed we couldn't get out the information in the SRCR and we're anxious to try to get that out as soon as we can. Never mind the decision, just the information part of it. And, we tried with the overview to do that and that didn't work out so well.

But, back to the compliance world and we do live in a compliance world; that's necessary, but way insufficient. Admiral Rickover's Naval nuclear culture are on a continual improvement, excellence, and I think the nuclear utilities have increased their capacity factors by embracing the principles of quality assurance questioning. Ken Hess in his presentation talked about some of those principles that Bechtel had constantly questioning the safety and improvements that you can make and it could work here, as well. It's that same cultural aspect we are working on and we're not done yet. But, you're never done organizationally as you constantly improve and balance these competing goods as we go forward. And, I think, that's what we're trying to do.

COHON: We're going to move on, but did you want one last comment?

KNOPMAN: Well, just the final one. I mean, the application here is what do you do about certain testing, for example, at the site going on now that appears to have marginal value? You're not sure how valuable it may be in terms of making TSPA--getting a better case from TSPA. But, in a long-term sense for performance confirmation or for just baseline ambient studies that you would want to draw in 20 or 30 years hence, you know, they're important, but you've got to make these near-term decisions. This is how it affects you, I think, in day-to-day--I'm not suggesting this is easy and that there is an easy way through it, but it's good to know and acknowledge that you've got competing interests there.

COHON: Paul is giving me this look that says this is a really important question that you're going to have to ask.

CRAIG: No, no, it's not an important question. I don't have any questions.

COHON: Okay.

CRAIG: But, I do have the microphone. I think Norm framed matters exceedingly well by pointing out the tension between continuous learning versus the constantly moving target. I've been watching this now for some years. I think the constantly moving target turns out to be really a major problem. Judy Treichel in her comments this morning brought that up in a very clear way. She says no matter what happens, there's going to be a tech fix. Right? A learning organization is geared to do that sort of thing. And, if they're doing their job properly it's going to be very easy to perceive the organization as finding a fix to anything. And, it seems to me that the program has hurt itself by failing to specify better some tests. What is good enough?

Now, the Board has taken the position correctly in my view that in order to go forward, you need one plan that's good enough that does the job. Then, after that, you modify it and you improve it. But, there's a threshold test. You need something that's okay. The problem that we now have is that while you may, in fact, have a design that's okay, it's very, very difficult for any of us to be convinced of that. That's part of the problem that you're having in dealing with the Board.

The documentation you give us is so voluminous that people like me are simply unable to comprehend it at a level adequate to be comfortable with it. Now, I have these two CD-ROMs which I'm told when you print out produce a stack like this and that doesn't include the AMRs and the PMRs. And, trying to hold the concept in my head is really tough.

There's a wonderful paper by a fellow named Miller written back decades ago called "The Magic Number 7, Plus or a Minus 2" which I call your attention. Human beings--and it turns out ravens also--are able to hold something like seven separate concepts in their head at one time and after that you have to clump. You count the money. You're going to see three or four pennies, but if you put out 10, you have to count and put it in piles. This is well-known in Las Vegas. So, we have limited capability to comprehend concepts and it's the nature of the beast and we have limited energy and limited time.

At the present time, there does not exist a document which communicates the key ideas to me in a fashion that I find comprehensive. Every time I start to look through something, I have questions and I go into another document and I have more questions, and I get caught up in the minutiae and I get caught up in trying to figure out what's important and what's not important. This is intimately related to this tension between the continuous learning and the continuous moving target. We need a target.

It seems to me that that issue is also intimately tied up with the question that you're presently wrestling with which is what does the Board really want? What will it take to satisfy the Board? This is an issue that I think we should explore and this isn't the time to do it, but I do believe that I'm framing the question in a fashion which gets to the meat of it.

COHON: Yeah, and let me save you the trouble of pointing out to the Board is not what would satisfy the Board for what is good enough. So, I mean, there's that dilemma.

If you don't feel a burning need to respond, I don't think there was a question there, we can move on. Is that okay? Well, go ahead, Russ?

DYER: Let me explore one point because Paul brought out a huge challenge and that is the communications issue. There is an enormous body of information there and we've attempted several different ways of trying to make that available by itself and in various summary forms, none of which have been terribly successful. A CD-ROM just means you have a whole lot of information on a little bitty thing. But, one thing that we may be able to get to with the CD-ROM and the electronic information management approach is a hypertext-linked kind of approach where one could start at a relatively high summary document and then just to satisfy you, you could pull the string down as far as you wanted to go into the underlying documents. That's some time off. I'm not sure that would satisfy the needs of anybody.

CRAIG: That wouldn't do the job. My conception of the job required some really hard thinking to pull out what's important and separate it from what's not important. Hyperlinks to 10,000 pages of documentation is a different task; it may be a valuable task, but it's a different task.

DYER: Well, the top level document, whatever it is, would have to be a document that met that need in my view. It doesn't exist now.

COHON: Very good. Russ, thank you for stimulating such an interesting and useful conversation.

We're going to move now to the repository safety strategy and this will start with a presentation by Bill Boyle and I appreciate your willingness to make this as brief as possible today.

BOYLE: Okay, thank you. I'll start by saying that this talk is related to the previous discussion in any number of ways. It's a roundtable discussion about repository safety strategy and path forward. The repository safety strategy has been evolving in our current environment, has been for years evolving in a learning environment. It's primarily a communications tool which we need those. There's no regulatory requirement for such a document. I view this talk in some ways as a continuation of the previous one, and when we're done with this, I think discussion on either topic is entirely appropriate.

Okay. The repository safety strategy, which I'm pretty sure Dr. Wong brought with him today and I don't know who else did, it's in Rev.04, Interim Change Notice 1, which just by the fact that it's in the Rev.04 shows that it has evolved with time which relates to the moving target aspect, as well. When I got involved with the possible path forward on the repository safety strategy, I asked, well, let's look at the history of it. And, over the past four or five years, it actually gets revised about once a year which is quite a moving target, but I'll return to that in a bit.

Now, this presentation is going to--I'll briefly put forth some of the discussions that have occurred within the Department and our management and operating contractor on, well, what really ought to be in such a document. I'll let you know right now the reason it's a roundtable discussion is there certainly is not unanimous agreement on what should be in and what should be considered and that sort of thing. So, we really are looking for reactions and comments from yourself and anybody in the audience or anybody else.

But, our near-term goal is to update the repository safety strategy for the development of a safety case consistent with that strategy, to support a decision, whether or not to recommend approval of Yucca Mountain.

Now this, I've already briefly touched on this. There's three different definitions on here, if you will; safety strategy, safety assessment, and safety case. If you out and do a word search of the Nuclear Waste Policy Act, NRC regulations, EPA, and DOE regulations on this, you won't find these terms. They're all ours to do what we wish with to some extent. So, here's our proposal for what should be in a safety strategy, a general approach for the application of multiple lines of evidence, and logical arguments to conduct a safety assessment and present a safety case.

Now, even though the following two terms, safety assessment and safety case, aren't in U.S. law or regulation, they are terms that are used by the international community and I think we're certainly not in contradiction with these terms right now, but this NEA document is certainly not a regulation that we must adhere to or anything else. I believe we're in agreement in principle. We may have differences here and there, but in general, this is what we would hope to capture in our repository in our safety strategy, a general approach to apply lines of evidence and argument, to conduct the safety assessment, and present a safety case. And, I think the fact that these terms are not in regulation or in a statute, in part, leads to different interpretations of them. I think it's good that the NEA has provided, at least, for safety assessment and safety case, something to work from.

Now, the following pages are some points that come out of a lot of the recent discussions within the program and project about, well, how should the repository safety strategy evolve. One of them is separate the safety strategy in the safety case. Rev.04, I see in one that Dr. Bullen and Dr. Wong have is more than 100 pages and there's a lot of good material in it, but it starts--it's probably to the point where it certainly can't serve as is as the high-level communication tool that Dr. Craig had mentioned.

One change we're considering is to keep the strategy as a concise description of the general approach and primarily to use it as a tool to facilitate communication between the DOE and stakeholders. Another goal not listed here is to make it a robust enough strategy so that it would become less of a moving target, such that it wouldn't be subject to revision on almost an annual basis.

Another thing we've considered is time the safety case more directly to regulatory requirements. This is in some ways, in part, you know, a business approach as we do have certain milestones, site recommendations, license application which naturally lend themselves to being vehicles in which to document, well, here's what we know now. So, we may use those major documents as a means to update our safety case.

On the next page, Page 5, these guiding program principles, they will be incorporated in the repository safety strategy, as well. I mean, they were talked about yesterday and today that we will use continuous learning, informed decision-making, and responsible stewardship. That will be part of our strategy or we're considering it as part of our strategy.

Now, some of the elements of the safety strategy and I'll get to those in a bit will evolve with time. Another way of putting that is some of these multiple lines of evidence, the mix, you know, how much we rely upon one item rather than another, that will change with time as we learn more.

This next bullet on Page 6, I think a lot of people are in agreement with, and it's already reflected in Rev.04 in contrast to the earliest versions of what was a repository safety strategy, is it has to address preclosure and postclosure. If you go back to the original waste containment and isolation strategy, you focus more on postclosure. But, there's a belief that strategy should address both.

Now, the safety case itself which is the documentation of why we think what we think should be based on conclusions from both direct and indirect lines of evidence. We'll use the regulatory requirements and expectations to guide our analyses and testing. We'll incorporate the direct evidence that pertain most directly to repository system at Yucca Mountain in our safety case. Some of the examples of indirect evidence we'll rely upon include analogs and independent expert review. As Russ just mentioned a little bit ago, that expert review includes yourselves, it includes people like Gary Dublianski for the State, it includes our initial internal reviews, ourselves; there's a wide range. When we say independent expert review, it encompasses a lot of people. The safety case will also base conclusions on results from the test & evaluation program.

Now, Page 7, the multiple lines of evidence and argument, this is one of the points that was brought up by the Board on the four items that were of interest to them. And, this listing here is not exhaustive, it's not complete, it's just a starting point. The way I like to think of these items, they're like a toolbox of things that we can use in order to make progression in our understanding of Yucca Mountain and anything that we might put there. This includes multiple controls and barriers. We can rely upon those that we should have them. Also, a line of evidence that a safe facility might be appropriate is the ability to meet the applicable standards. It's not the be all and end all, but to be able to show that, yes, I'm in compliance with the standards. That is a line of evidence. We can supplement it with our understanding of natural system attributes, you know, perhaps independently of some regulatory requirement. We also can use as a line of evidence the fact that we have a robust and flexible design. That as changes were to come forth, we could respond appropriately. We also can rely upon numerical and process and performance modeling. In the TSPA, if you will, proposed closure is the performance modeling, but we can rely upon modeling at subsystems and look at that result independently of TSPA. We can rely upon analogs. I've already mentioned the independent expert review and also the test & evaluation. So, again, these are just like a sample starting point of the types of tools we have available to us at any time to go out and gather more information or evidence in order to further develop a safety case for Yucca Mountain.

Now, this is the last slide I have to show. I'll perhaps draw some others. This is a graphical representation for evolving elements of a repository safety strategy. This graphic here, it's not our preferred choice; it's just an example and I'll make reference to others shortly. It was interesting in the discussions within the project in the program on how to proceed with the repository safety strategy that very frequently the graphical representation of the strategy generated a lot of comment. Perhaps some of the comments were substantive, perhaps others were more different ways to cut a pizza. Ultimately, the strategy was the same; it's just that some people like to present it different ways.

Russ has actually in his talk already shown two different ways in which to capture elements of a repository safety strategy. One is the famous pyramid he showed. He showed the other one on Page 8 that was later shown again. Another group of people preferred a hub and spoke representation with an integrating tool as the hub with independent spokes of information, models, and testing coming into it. Other people prefer, if you will, either the stovepipe model or I like to think of it as the Greek Temple model in which you have some top pyramid into which things flow, but they're independent pillars or columns or stovepipes. So, there's all these different graphical representations for how should we consider our repository safety strategy. There wasn't agreement even on the graphics or sometimes the substance.

Now, with that as a background to stimulate some of the discussion upon multiple and independent lines of evidence, I have some specific questions or examples that I will put forth and I'd like for people to comment on it at will.

Is Bo still in the room? There he is. He's such a good example and I think it's the Berkeley accent. Take, for example, the UZ model that LBL has that Bo's responsible for and it's fully three-dimensional and if we use it to do thermohydrologic calculations and Bo has influence over that model, why, I know that Bo in his career has worked on geothermal systems in Kenya, here in Nevada, southern California, northern California, and other countries. Now, his experience on those geothermal fields doesn't appear explicitly in the model for Yucca Mountain, but you have to believe that his experience in those other places conditioned how he put the model together in the first place. He knows what's worked elsewhere, he knows what will work here.

So, the question I would ask is if we went out and used this in an analog one of these other places, Kenya or northern Nevada, is that really independent given that he probably took it into account some way when he developed the model?

Now, I'll go on to another example and this has to do with the multiple and independent and again I'll use Bo as an example and somebody else for the Berkeley accent, Tom Buschek from Lawrence Livermore National Lab. Now, Berkeley can do three-dimensional thermohydrologic calculations for Yucca Mountain, and if appropriate set up, they can get at things like relative humidity, flow in the rock, temperatures, all these sorts of things using tuff. Well, so can Tom Buschek using nuff. And, he gets that a different way. He uses the mix, the multi-scale model, which is a mixture of one-dimensional, two-dimensional, three-dimensional calculation that he superposes to get at an answer for the same things that LBL can get at if they wish using a fully three-dimensional representation. Now, given that nuff and tuff are actually brother and sister or cousins or whatever, they have the same theoretical basis, but they're being implemented differently by different organizations, does that count as multiple and independent or are they all one and the same.

COHON: Same data sets?

BOYLE: Yeah, let's say--

COHON: They're using the same data sets?

BOYLE: Sure. So, those are some of the examples because the Board's fourth point was multiple lines of evidence and derived independently of performance assessment, but these are like specific examples that raise issue of, okay, well, what do we mean? What's multiple and what's independent.

So, there, those are the questions and I'd love to hear comments from people.

COHON: Good questions. I don't see any hands flying up. By the way, we will welcome comments and questions from the audience. What I would ask you to do is just step up to the microphone, and when I'm ready I will call on you. Okay. So, step up there, but don't talk until I call on you.

KNOPMAN: If you have two models using the same governing equations, maybe only different in the way they're diskertized (sic) and dimensionalized or whatever, same data set, I'd say they're not independent. Now, they're multiple because there are two of them. But, they're not independent in their derivation of a particular result on, let's say, temperature distribution in the near-field environment. Now, a model of heat flow at Yucca Mountain compared to a model of heat flow in another location, different data sets, same model, that arguably, I think, even though it's the same modeler, could be argued as independent in the sense that you've got two separate data sets and a model that can explain the field or just the observed field data, plausibly. So, at least, that would be candidate for me for independence. I don't know. That's the way I would sort through those things. We really have to look at what's different in the way you're getting the result. Are you coming at it in a different way?

CHRISTENSEN: I just wanted to add to Debra's comment and say that it seems to me that to some extent that bubble of independence is very much conditioned by the specific question you're trying to answer. And, in one case, one could make an argument. The use of the same model, even though you're looking at different sides for particular questions, wouldn't be independent and other situations would. So, it's very question dependent, it seems to me as to whether you really have--the question of independence is really dependent on the kind of question that you're asking.

BULLEN: Bullen, Board. Actually, you have moved into a realm where I'm really happy that the Board doesn't have to render an opinion and that's in the validation and verification of these kinds of models because you have to come up with independent data sets and one that you expect to know the answer to and walk through the steps of the model itself looking at the data and how it's evolved and what kind of results you get and get the result you expect and then apply it to the case where you want to see the answer. And, in our case, we don't have to do the NRC validation and verification of software or models or data sets; fortunately for us because that's an extremely difficult task in a lot of work and all you guys have to do it and we would be happy to watch you do it and I'm sure we will.

But, in the case of what the Board expects and I think if you asked all 10 of us that are here, we'd probably have a different representation of what multiple independent lines of evidence might be. And, in my case, I don't go all the way to the modeling aspect. I look for things like we mentioned, natural analogs, and we look at waste package material analogs even though we don't have corrosion resistant materials that have been around for hundreds of years. We look at other things that have been there and try and figure out why. An example is iron based materials like the Dehli Pillar (phonetic) that have been there for about 1500 years in an arid environment. And, you think, okay, wow, arid environment and the metal lasts. So, those are the kind of natural analogs that I draw upon for multiple independent lines of evidence.

Now, directly applicable to Yucca Mountain, nope, because it's not corrosion resistant material, but builds a sense of confidence in the fact that you know what you're doing and that we understand the processes that you've taken to get to the point that you've reached, I think, is yes. So, I dodge the question because I'm glad we don't have to do it, but in the case of multiple independent lines of evidence, things that make us understand the rationale for why you did it and why you expect it to perform that way and an example outside of Yucca Mountain to verify that your thought processes are correct.

BOYLE: Now, this is my own personal observation from many years dealing with principal investigators is I believe that many of those multiple lines of evidence exist. Take, for example, the corrosion in the Dehli Pillar, I would guess that Pasu and Gerry Gordon are aware of it. They probably don't document it. So, I think some of the multiple lines of evidence issue is a communications issue that I am convinced that the engineers and scientists on this project are aware of multiple lines of evidence, but perhaps we have not been that good at putting them forth.

RUNNELLS: Runnells, Board. I had the good or bad fortune of being stuck in an airport for about five hours on the way down here because the airplane wouldn't fly. I dug through the stuff in my briefcase and I retrieved the repository safety strategy which is the first time I've read it. I must say that it may not be the high-level document that Paul was describing, but to me it was an enormous help in getting an overview and pulling a lot of stuff together. But, in particular, the incorporation of a good discussion of natural analogs, not just a description of those analogs, but for the first time that I've seen, at least a qualitative tie to modeling, a qualitative description of how the natural analogs tie in with thermohydrologic modeling in a qualitative sense, igneous dikes and what effects do we see around igneous dikes and that sort of thing. To me, it's a good demonstration of independent multiple lines of evidence, but nicely tied together to show how they support each other. Now, I don't know which version of the repository safety strategy I have, but I look forward to the next version because I think this one is well-balanced and provides a very, very nice overview, including the multiple independent lines of evidence in the cases of the natural analogs.

CRAIG: I'm going to give a take on this. It doesn't strictly fit in with multiple independent line of reasoning, but it does express my particular way of looking at many of these issues. There's a wonderful book written by a former physicist, John Hart, called Consider a Spherical Cow, which I commend to your attention which begins by approximating a cow as a sphere if I recall correctly. Good enough for certain purposes. As a person with physics background, I like to do back of envelope calculations. One of the things that I routinely try to do is to take some pieces of your complicated documents and build a simple little model that will roughly represent the physical processes, and if all goes well, confirm to me that the results of these makes sense. With these numerical codes, I must say I am not persuaded by the fact that you've gone through a QA process. This may all happen, but it just isn't good enough to convince me to take on faith that everything is okay.

On the other hand, if I can do a simple model, frequently a one-dimensional model that gives roughly the right answer, this gives me an enormous feeling of comfort and it may actually be good enough in many of these circumstances because for much of the work that we're talking about, we're not talking about 10 percent accuracy, we've got orders of magnitude flexibility here. So, the one-dimensional kinds of arguments may just be dandy.

Now, whether you choose to consider that kind of thinking to be high-level thinking or low-level thinking, we can talk about, but in any event that's the type of thinking, one type of thinking that I and many of the people I deal with finds compelling; some way to do an independent validation of the model or a rough test of the models to convince yourself that they make sense.

NELSON: Nelson, Board. I started making doodles about what it was that I might think would be a reasonable approach to defining and I found many definitions, many approaches. I think that there isn't an one definition. A lot of it is the case that you almost make as to why it should be considered one of multiple independent lines supporting the traits that you have made to include whatever you have included in your model. For some reasons, you may have an empirical model and you don't have theoretical basis. So, you may actually develop or borrow or assume or observe a theoretical development that may support your empirical observations and vice-versa. To a certain extent, I mean, if you can make the case that those would be multiple or independent lines of evidence. I think, in some cases, you've got a process which you think is very complex and you try to model it complex in the wish to do something simple to understand whether it's really sane or at least going in the right direction. Does it make sense on some gut level? But, in some cases, you actually have to break apart a model into simple parts and you wish to know if you recombine it, would you be having some complexity appropriately there in which case there's some natural environments, some geologic environments which are appropriately complex and you can make some observations about. That would possibly be supporting as a multiple independent line of evidence.

But, one of the best uses in my mind when we say this is the issue of time because one of the big things that are very difficult to validate anywhere in this project is the issue of time. If you can define did you like the analog in the natural system, that's going to be, to me, one of the things I'd be out looking for because time is going to be one of the questions that is the hardest part to validate. And, that's where the geology will help. So, I don't see any just one definition for a line of evidence. There's many.

Is that, at all, helpful to you, Bill?

BOYLE: Yes.

COHON: Go ahead, identify yourself, please?

HANAUER: This is Steve Hanauer, DOE. Could I get projected Russ Dyer's Viewgraph 8, please?

COHON: I'll tell you what. While we're waiting for that because he's going to have to switch to the other, why don't we call on Dan Bullen? As soon as it's up though, we'll come back to you.

BULLEN: Bullen, Board. Just a little followup on Paul Craig's comment. Jeff Long and I were walking around the project office on Monday much with the assistance of Claudia Newberry, by the way, and we appreciate that. But, we did swing by Mark Nutt's office. Mark made a presentation to us about 18 months ago of the simplified TSPA which, I understand, is a deliverable coming in day after tomorrow or something like that which we would eagerly anticipate receiving primarily because it allows us to do symbol, back of the envelope kind of changes where within the realm of reliability of what I think is probably the TSPA-SR. Is that what this one is written for? That we can essentially use the CD and the software to move all the dials to the left and move all the dials to the right and see if it behaves the way we'd expect it to behave. If, for example, it rains 100 times more on the mountain, do you expect to see more release, less release, faster waste package failure, slower waste package failure; those kinds of things that allow us to see if we can see if we can get the feel for the performance.

Now, this isn't a multiple line of evidence. This is just sort of a confidence building exercise. Claudia assures us that as soon as it's delivered, we can have a copy of it. But, I would basically encourage all of my fellow

Board members to also get a copy and to become familiar with it. Then, the next question is that we have for Bob Andrews is how relevant is this to what you currently have for TSPA-SR and specifically if we do want to look at a colder repository design, would those capabilities exist, and if not, why not and how fast and all those kinds of things? So, Bob, could you maybe comment just for a second on--is this too simple for us to learn anything from? I recognize it as a valuable tool.

ANDREWS: No, I think using even a more simple TSPA than the one we have now which has a lot of things in there does gain a lot of insights. You know, a lot of the sensitivity studies that Bill was even talking about earlier this morning and that we talked about yesterday give you an insight. You can look at subsystem performance and essentially what's gone into that simplified TSPA is essentially the subsystem performance of the individual barriers. I believe, although I haven't seen it most recently--it's been about a month--you can, as you say, turn the knob and see what it does to the performance and gain your own conceptual insights into that. I do not know personally if this plan should do that for the cooler operating mode design. There's always a little time lag between getting the information in and developing the more simplified representation of that.

BULLEN: Not that I'd ever want to influence you, but it would be very helpful if that did get done.

NELSON: But, some of us don't need to have our confidence built any more.

HANAUER: I'd like to suggest a connection which may not have been obvious. I use Russ' Slide 8 as the bare bones on which to build this thing. If you want to do anything that you haven't done before or if you want to see into the future, the only thing you can base it on is the data and information that you have today if you have to decide today. If you want to do anything different from what you've already done, you must organize this data and you use models to do that to answer questions which are not answer directly by the tests you've already done.

Now, the question is what do you do with this body of information which I will loosely describe as data and models. Here is depicted in the blue column, one way to organize these data. You abstract it and you do a TSPA and integrated calculation which has many virtues. It puts things together. It enables sensitivity studies, important studies, and so on. However, it also has some vulnerabilities that we all know about. I'd like to suggest it is not the only way to organize these data and these models.

In fact, we've already had some from previous discussion, a description of not one column, but in fact two.

They use overlapping data sets of all this information. and to some extent overlapping analysis and models. But, in fact, the one branch of the one that Bob describes and that is in TSPA-SR is based on all Q data and has many conservative things in it, in order to do what was the goals of that particular TSPA. Now, Bill has described to us another TSPA which is being done using more realistic models which may depend on data which aren't in the Q archives, alterative models, alternative view of the value. And so, in fact, we are going to have one of these days, not one of these blue columns, but two. This is very valuable because it will enable us not only to get a more realistic view of the uncertainties and maybe some insights what to do about them, but it will also give us a view for the first time. It will give us an estimate of the conservatism in what I will describe as Bob Andrews' TSPA; of course, the other one is, too, but he hasn't got it yet.

Now, I'd like to suggest that there are some other things that you can and should do. And, all based on these data, I've arbitrarily said everything we know is in these data, and they are arbitrarily based on these or other models because you have to have models to organize the data. So, the question is to what extent are these independent lines? So, we had some examples by Bill Boyle.

Let me suggest another example or two. One is Dr.

Bullen's suggestion about validation. You can't do validation using TSPA. It predicts what's going to happen 10,000 years in the future and it takes 10,000 years to validate it. I'm sorry that's not helpful. You have to validate the model some other way.

Another example is Dr. Craig's simplified calculations, extremely useful. You can't see into these TSPAs on the scales that we now use. It's too hard to unravel them and see what influences what. So, you can use these data and these analyses and these models or some other models if you want to do simplified calculations and get yourself another line of evidence.

Why am I making this speech? Because in this context, the question of dependence and independence, I would suggest to you, is a little like angels on the head of a pin. The degree of dependence and the degree of independence of one of these things or another is a continuum with the shrine of independence on one end and the total dependence on the other end, neither of which will be achieved.

So, yes, we really need independent lines of evidence, but don't ask for what you can't have. We've got the data that we have and the theoretical understanding that we have and out of these, we need to fashion these independent lines of evidence.

COHON: Thank you. I think we all agree that a 10,000 year old coin made out of Alloy-22 would be an independent line of evidence, if you could only find one.

Actually, what the Board said, very carefully chosen, was multiple lines of evidence--I guess, we used that phrase--derived independently of TSPA. I don't think we disagree about that and your point is well-taken.

Abe, for the last comment.

VAN LUIK: Thank you. Abe Van Luik, DOE. I'm really pleased that there was one other person in the universe that thought that the repository safety strategy was a very good overview and description of our technical program. I thought it was, too.

My day job is working for Russ Dyer. With DOE's written permission, I also serve as the chairman of an expert group that's run by the Nuclear Energy Agency. It's called the Integration Group for the Safety Case. We asked the group which is 14 to 18 countries depending on where the meeting is held and three international agencies, both regulators and implementors, we asked them what do you do to build multiple lines of evidence into your safety case? And, they all raised their hand and said, oh, we do a lot of things. So, we formulated a questionnaire, sent it to all these groups, and what came back for each one is a much shorter list than what Bill just showed which showed that we have an expectation, but not a clear vision in the international community even of how to meet that expectation.

So, I just wanted you to know that the discussion that has been going on here, I will take that from the transcript and share it with my committee because I think some of the clarifying comments have really been helpful and will provide insight into encouraging others to think along the lines that you have outlined. So, this is speaking with my NEA hat on, rather than my DOE hat; although the comment on the RSS was definitely from my DOE hat.

Thank you.

COHON: Thank you, Abe. That was a wonderful way to close this session unless any of our panelists want to put in a last word.

(No audible response.)

COHON: My thanks to all of you. That was very useful and valuable.

We will now take a break for 15 minutes until 3:30.

(Whereupon, a brief recess was taken.)

COHON: We continue now with a presentation on the Nye County Scientific Program and we're pleased to welcome back to the Board Tom Buqo from Nye County.

BUQO: Thanks for having us back. I'm Tom Buqo. I'm a consultant to Nye County.

A few weeks ago, I had a discussion with Dr. Diodato and he said that the Board was interested in hearing about three things. First of all, an update on our early warning drilling program; second of all, status report on Nye County's water right filing and so I'll be giving a briefing on that; and then, the third item was Nye County. We're closing out on EWDP. We're in the third phase of a three phase program there and DOE has asked us for a proposal for additional work. So, I'll be going over what the proposed level of that is. That is preliminary and I'll be touching on that as we go onward.

In terms of our early warning drilling program, we're going to do a very brief overview. Phase I again, six wells at six sites. We completed wells in the alluvial aquifer, the volcanic aquifer, and the paleospring deposits at two sites. We got water samples from everything. We've had two rounds of water samples, regular sample and analyses done since then. We conducted three aquifer tests in EWDP wells. We also conducted a couple of tests in wells in the Amargosa Desert and we've done routine water level monitoring ever since.

In Phase II, we continued on with the effort. We completed 11 wells again in the alluvial, volcanic, and paleospring deposits. Our most important well was at NC-EWDP-2DB where we got down and penetrated the carbonate aquifer. Just even as we're speaking, they're up there now developing one of our wells at 7SC which is a deeper well in the paleospring deposits and we'll be doing some aquifer testing there in the next few weeks. Things are going pretty well and I don't want to belabor this. I've made some previous presentations on our findings on that, but I want to get into what we're going to be doing for Phase III.

Phase III, our primary priorities are, number one, get back into 2DB, clean it out, and get our geophysical logs completed, our samples out, and our testing done. Testing there, we've got to be careful because we've got to be able to coordinate that with the alluvial testing complex. We're only 6,000 feet away from it. So, we don't want to go in and start impacting the test. So, it may be delayed, somewhat. But, that's our plan there. We'll go in and we'll get our chemical samples out, our logs out, we'll go in and packer test off the carbonates. Immediately above the carbonates is this tremendous loss circulation zone. So, we want to packer test that also. When we do our initial test, we're going to get a composite transmissivity for the whole thing. We don't want to mislead people and think there's this tremendous carbonate aquifer under there. So, we want to go in and packer test the individual zones because our feeling is we've got a really transmissive loss circulation zone sitting in the lower most tertiary. Underneath that in the carbonates, it's probably doing to be pretty tight. One thing on this program though is we continue to get surprised as to what I think is probably going to happen. A lot of times, we find out something quite different. So, preconceived notions are nice, but that's all they are.

Our second priority is the alluvial testing complex. Current plans call for two wells. We show here at 1500 feet. That's in a flux. Dr. Chu just came up and said, well, I don't know, it may only be 1,000 feet. We don't know, but we'll be putting in two intermediate holes that to support that facility and that will be for the cross-hole testing. We'll also be probably putting in a couple of piezometers to help define the heads in the alluvium only so that we know whether or not we've got good gradient there. We're trying to precisely locate the wells for the actual testing and we need better control on the gradients out there. We'll be doing a standard 48-hour aquifer test in there, we'll be collecting our samples, and then it will be turned over to the tracer complex for continued work there.

Our next priority is to get up on the Test Site and drill at Site #22S which is immediately adjacent to Forty Mile Wash. In discussions with everyone, it has come back with a consensus that that's the most important hole that we could drill in Phase III. I've heard that the pre-ops permit that we need to drill on the Test Site is through the system now and we should be getting notification any day now and we're excited about the opportunity to go onto the Test Site and do the same types of work there that we've done off of the Test Site.

The plan there is to put in one piezometer to 800 feet, plus or minus. What that does by putting in a piezometer first is allows us to collect enough information to design the subsequent well that we put in. We've found that if we go and we try to put in that well without any information, it's hard to have those materials on hand. So, we get into delays. By going in and putting in the piezometer first, we now know where the water table is, where the first competent formation is, and how we need to design the actual well that goes in. We're looking at a well to 2,000 feet, plus or minus, at that location.

I'll kind of jump ahead a little bit. We also think that that would be an excellent place for another alluvial testing complex. Nye County has always maintained if one ATC is good, then three of them must be three times as good because we're looking at a very variable system and the results you get at any one location are just that, the results for that one location. We need to have as much information and as much data from as many locations as possible.

Once that work is completed, then we will go and look at our other sites. Right now, our priorities are to go to Site 15D where we expect to find the hottest water that we've found yet, to go over to 12D and put in our test well and conduct our test across the Highway 95 fault using piezometers here for observations on this side and using our existing EWDP wells here as observation points across the fault. We want to see if future pumping in the Amargosa Desert, whether it's for farming or municipal or industrial purposes, is going to draw water across that fault or bring water up from the carbonates on that fault. We need to know what that degree of communication is.

So, with that, I'd like to kind of shift gears now and talk about Nye County's water right filings. Go over the who, what, where, when, why, and the significance of that. The who is the Nye County Board of Commissioners. That's who filed for these water rights. They were filed last February. It's been almost a year. The water rights survey has been completed. We filed 10 water rights applications in total. They're for municipal use. The points of diversion are located as shown in the areas immediately in the vicinity of Yucca Mountain and the Nevada Test Site. In fact, some of the applications, the ones in Mercury Valley and Frenchman Flat, are located on the Nevada Test Site. Two of the points of diversion are located under our existing rights-of-way for early warning drilling program wells. Two points of diversion are located on BLM land and six are located on the Test Site, one right on top of Army Well 1.

Although none of the points of diversion are located within the Amargosa Desert Hydrographic Basin as defined by the Division of Water Resources, many of them are located within the order of designation. The order of designation for Amargosa Desert extends beyond the boundaries of Amargosa Desert. As a consequence, our proposed place to use at present is in the Amargosa Desert. Well, here's a map to show the locations upside down and backwards. We show the Test Site boundary on the map so you can orient yourself and also the intersection at Lathrop Wells.

Well, why did Nye County do this? Well, the first answer is Nye County needs the water. Our projections of growth in Nye County and southern Nye County and the county as a whole indicate that by the year 2050, the population of the county will be about 162,000 people. Most of that growth will occur in the Pahrump area if current trends continue. We don't see any reason why they won't. So, we'll have 150,000 people living in Pahrump. Right now, there's 30,000 people living in Pahrump. They're pumping just under 30,000 acre feet a year. The perennial yield is 19,000. The safe field is 26,000. We are in an overdraft situation in Pahrump Valley.

Amargosa Valley represents a total wild card. We cannot predict what the population of Amargosa Valley is going to be in 50 years. It could be 5,000 people, it could be 50,000 people. We do know one thing. It's incumbent upon Nye County to see to it that the resources are available to meet future growth in the county.

The second major reason is protection from speculators. There's been a history of speculation in this basin. An outfit called Amargosa Resources, Inc. tried, but failed to come in and do massive water right appropriations with the idea of shipping them west to sell them to the first outfit with a dollar. The result of that was a lot of time and effort spent by a lot of organizations fighting and supporting it and so on, but the sad fact is a tremendous amount of people lost water rights as a result of that action. There's another outfit now, Vidler Water, that has gone in and done blanket water right applications over all of Lincoln County, Nevada. They're down in Mesquite Valley or Sandy Valley and Clark County. So, the county is concerned that if they don't take action, some speculator will come in under the cover of dark, file these applications, and try to turn a profit by shipping that water to somebody else. So, that was the second reason. So, the county has laid claim to the largest unappropriated block of water left in southern Nevada.

It's also protection from inter-basin transfers to go to Las Vegas. The Las Vegas Valley water district came in in 1989 and filed applications in three rural counties including Nye County. It was all in the northern end of Nye County. They didn't file around the Test Site. So, we figured, well, here's the opportunity. We've got to protect this water. We better get it and file on it before a speculator does or the district. Subsequently, in discussions with them, we feel it might even lead to a partnership with Las Vegas on this.

The other area that needs resolution and the filings are geared at is the resolution of Federal land use and land management policies and their impacts on the water resources of Nye County. Now, our water right filings were not protested by any individuals or groups within the State of Nevada. They were protested only by Federal agencies. Those Federal agencies were DOE/NTSO, Nevada Test Site Operations. DOE/YMP filed a separate protest. The National Park Service for Death Valley National Park filed a protest and the U.S. Fish and Wildlife Service. So, we've got the Federal Government against Nye County is what this looks like.

The state engineer's ruling may lead to State and Federal Court challenges. We don't know. He'll rule one way or the other. He may grant us part of them, all of them, none of them. We don't know. But, we have the feeling that some people may not be satisfied with his ruling and it may end up in Court. We think that as a result of it, this action will finally bring the issue of Federal lands versus State water to a head. It is not the Federal Government's water. It's not Nye County's water. The water belongs to the State of Nevada and you simply go get a permit that allows you to go in and place it to a beneficial use. The water still belongs to the State.

Okay. We expect more State actions and Federal actions. We expect more land withdrawals to occur. Every time an acre of land is withdrawn for a Federal reservation, whether it's for Nellis, the NTS, Yucca Mountain, a national park, whatever, that's an acre of land where we can't go drill. It's an acre of land that's got an implied water right with it that's taken out of the balance of what's left over for everybody else. We want to get in and claim this water before they reserve the entire west. We have to because we'll turn around 20 years from now and find we don't have the resources if we don't take action now.

We also feel that this has got some far-reaching consequences beyond Nye County and the Nevada borders. Well, that's nice, Tom, but what's that got to do with Yucca Mountain? Well, here's what I think the significance is with respect to Yucca Mountain. You all are aware of FEP, features, events, and processes, in a saturated zone flow and transport. Well, they got a FEP for water management activities. The screening decision on whether or not to evaluate that said, well, what we'll do is we'll include the existing water management strategies, but we will exclude any changes to those strategies. My initial reaction to that is what do you mean you're going to exclude change? Well, I asked what's the reason for this and the response was regulatory guidance. In going through the EIS, I came to conclude that this is regulatory guidance. So, I dug into this. They cite that the National Academy of Sciences, National Research Council, and the TSPA analyses followed the recommended approach using as default societal conditions as they existed, not as they are today and not as they're going to be 50 years down the road. As a consequence, the TSPA is based on the assumption that populations would remain at their present location and population densities would remain at their current levels. We believe that that is taken out of context, that that's not what that document says, at all. What the document is talking about is a population-based risk standard. You can't predict how many people are going to be there in 1,000 years. The TSPA uses this discussion as the rationale for ignoring the present population, the short-term future growth in the area, and water resource management strategies which are indeed very predictable. They are not speculative.

Actions that have been taken. We have increased water use in Amargosa Desert by the residents who live here and the farmers who live here. Nye County has made their water right applications. Las Vegas Valley Water District has massive water right filings east of the Nevada Test Site. There's been increased water use on the NTS for mission related and private actions and we're seeing more of that. They want to put a solar facility and wind facilities in southern Nevada. They look at the Test Site, great place, but their going to want to use water to do that. Any water used for any Federal purpose is water that is now not available for non-Federal purposes. So, these actions are not speculative; these actions are quite real.

COHON: Tom, I know you're going to change topics here.

BUQO: You're right.

COHON: And, I don't mean to get you to put too fine a point on this, but if the water rights are granted, what implications do you think that would have for Yucca Mountain and the way it's being analyzed?

BUQO: Well, at some point, those water rights would be developed and put to a beneficial use. Now, you're pulling out 33,000 acre feet a year from areas where previously there was no development, at all. In the immediate vicinity of Yucca Mountain, you're going to have municipal water supplies being drawn within the 20 kilometer boundary that could cause the change in hydraulic gradients, travel times, that sort of thing. When the TSPA looks at a static situation of no growth, we say wait a minute, it is growing. You have to go in and look at--and we believe the assumption should be that every drop of water that's legally available is going to be put to a beneficial use by the year 2050; that that is a reasonable assumption.

COHON: So, for it to have an impact, though, you would still need to see a change in what you understand to be the TSPA methodology?

BUQO: Yes.

COHON: In other words, they would still have to take growth into account?

BUQO: That's right.

COHON: Okay. Thanks for the clarification.

BUQO: Okay. Let's shift gears now. As I said, DOE has said that they would like to entertain a grant proposal from Nye County. Originally, they wanted a two year extension on the EWDP. Then, they came back and said, well, instead of doing that, why don't you do a five year grant proposal? This is all preliminary. Nothing has been submitted to DOE. We're still in the thinking stages. We have a workshop, Nye County workshop, scheduled for mid-next month that we're going to go through and discuss it among ourselves and finalize what the proposal will be. But, as it sits now, we've identified nine work elements.

Number one is continued date collection at ONC#1, just like we've been doing for the last five years. We would continue work at the ATC. At the suggestion of AC&W, we would go in and archive water samples. We'd go in and pull 10 samples out of each EWDP well, we'd get them over to the sample management facility for DOE to archive for future generations. In case a new technique is developed, they'll come in and have a sample available. You can't sample water from 50 years ago unless you plan now to have that water available. We would go in and do a workshop and figure out what to do for annual chemistry monitoring. Let's face it; there's no repository, there's no wastes, there's no contamination, what's to monitor? And, I talked to Zell Peterman and said, hey, Zell, do you want 250 sulphate analyses over the next five years? No, he doesn't. So, we need to have a workshop. We need to monitor some things, but we don't need to go in and monitor for the entire universe right now. So, we'll work that out.

Water level monitoring, we see that as a necessary element. We want to go in and we've collected literally tons of samples from our EWDP wells. We haven't had time to go through and sort them, clean them, analyze them, do chemistry on them, and that sort of thing. So, we have a working element in there. We've got one on regional groundwater studies.

The two in red are the ones that I want to concentrate on today. We've also got a couple of more that we're talking about. One is some unsaturated zone studies. One is a horizontal drilling program. We're looking at various options. If anybody has any suggestions, we would love to hear it.

So, I'd just like to concentrate now on the EWDP and the surface geophysics. I was asked a question 18 months ago that was a very valid question. If cost were no object, where would you go, what would you do, and why? At the time, I think I mumbled something about, well, I'd wait to see what the results of the first few phases of drilling are. I mean, actually, my response was I don't know. Well, we've had 18 months now. We've been working with the data. We have a much better idea of what it's telling us and what we would like. Re-envision would be the thing to do. Currently, we'll be looking at something like proposing an additional 45 wells; 25 shallow wells, 15 deep intermediate wells, and 5 deep wells.

Surface geophysics, in lieu of doing a whole bunch of wells--we could say, oh, we should do 100 wells. We feel that doing some more surface geophysics would allow us to reduce the number of wells which would be a big cost reduction and it would also allow us to put our wells in smarter locations. Use the geophysics to go out and screen the areas. So, we're looking at three geophysical methods.

Seismic reflection, that would be between our existing deep boreholes. The idea there is to give us an idea of the basement configuration or the paleozoic basement configuration and to look for specific reflectors within the valley fill sediments that would be targets for monitoring.

We've been looking and working with Doug Duncan with the USGS on the square-array direct current resistivity method. We have been struggling with this. There's a transition, as you go from the volcanic rocks in the pilot Yucca Mountain to a volcaniclastic environment elsewhere. Where is that transition? In talking to the GS with this method, we should be able to see where fracture flow predominates in the volcanics and where it goes to force flow and that should be our transition zone. It may be a little more complicated because we don't think that transition zone is like this. We think that transition zone is going to be like this with different units coming out further depending on how far the flow. But, we think it's got potential and it would be, at least, worth checking out.

And, I'm no geophysicist, but the way it was described to me is they run a very long resistivity line to give us the depth. They're getting down to 1,000 feet now in Arizona in the Flagstaff area. Once they get that done, they rotate it 15 degrees and then they rotate it 15 degrees and they keep shooting it. And then, you plot it up on basically a diagram, and if it plots up as a circle, it's porous flow; if it plots up as an ellipse, it's fracture flow. And, the orientation of the ellipse on the diagram tells you the orientation of the fractures. We would follow that up then with a couple of holes in each area to verify the results of it. If so, that could be a very powerful predictive tool.

Where would we do these techniques? Well, the zone of alluvial uncertainty, and I'll show a figure that shows what's been defined as a zone of alluvial uncertainty. I think that some of this work should be done along the site-scale numerical model boundaries because we've got some real concerns about that and I'll get into that a little bit. Of course, for the drilling, it would be based on the results of the first three EWDP phases in the geophysical survey. Final well sites would be selected in consultation to everybody that wants to talk to us about it. Nye County has never said they have a lock on good ideas. We hear a lot of good ideas from a lot of people, we check them out, we follow them up. Just because somebody gives us input doesn't mean we're going to use it, but we're going to consider it. And, if it's good input, then we'll move it. 19D is a good location, consensus was we should move that well site, so we did. So, I think that part of it is very important. So, we would do it in consultation with the NRC, NWTRB, ACNW, UNLV, USGS, and, yes, the State of Nevada. We would be seeking input from them, too.

One thing that would be different on additional phases of EWDP is road building would not be a binding constraint. It has been in the past because of the costs of road building and the permitting of road building. But, in looking at where this key area is of alluvial uncertainty, there are no roads. So, that means we'd have to go into a roadless area and start to put in a road.

With respect to the surface geophysics, again we would want to concentrate on the zone of alluvial uncertainty and the model boundaries. We'd want to go across some of these inferred compartment boundaries. I'll talk about that very briefly in a minute. We'd really want to key in on this volcanic rock sediment transition zone. Where do we go from that volcanic rock fracture flow environment into the valley fill forced flow environment? We want to do some work across the Highway 95 and Bare Mountain fault zones to nail those in; where they are and what their attitude is. Then, of course, some tie lines between EWDP deep boreholes and wells so we can reduce the number of wells that would be necessary.

Oh, I'm sorry, I didn't cite a reference on this. It's from a DOE document I pulled off the internet and I don't remember the specific reference. But, the yellow line here shows the area of alluvial uncertainty. We'll we've been working in the area for a while now and we feel that

that's a pretty limited area of uncertainty. If you look on the big map, you see a bunch of data down here and a bunch of date down here and a fairly small area of uncertainty. We think the area of uncertainty is actually a lot more for this area. It's both alluvial and consolidated rock. Over here, it's primarily consolidated rock, but there are areas where we'd like to know something about the alluvium.

Okay. Again, in terms of the why, Nye County sponsored the low-altitude aeromagnetic work and additional gravity stations being done that were done by the USGS. Based on that, we have better definition than ever before about the depths of the paleozoic and about magnetic features that are probably related to structures in the Amargosa Desert and the areas up on the Test Site. One of the key features that we see from the magnetic delineations are these three east-west trending lineations. We're fascinated by those because the first thing is all of our current EWDP wells except one are below that. So, we haven't gone in. We need to know if these are exerting some sort of control on groundwater flow. If you look at the cross-sections that go south through Yucca Mountain, they show that those volcanics are staggered down as they're going. We're going through the process now of evaluating what's happening to our aquifers as they're being down-faulted.

To further complicate things, we've got these guys going across here, these structures. Well, we believe those are related to Fridrick's detachment model that over here in this trailing edge basin, we think that's comparable to what we're seeing in EWDP land. In fact, that model helps us understand why we see a particular volcanic unit in one well, and 6,000 feet away in another well, we don't see that unit. We think it's because of the tilting of the bed like this and this diagram--or not even a diagram. I guess this sketch shows between 2 and 19 how that happens. These are not flat-lying units out here. They've been torn up, they've been faulted, folded, twisted, thrusted, and so on. So, the reason we think we need more wells and it would be good from a characterization and understanding point of view is we're still trying to define those pathways so that Nye County can monitor them.

We think those flow pathways requires an understanding of the style of deposition. How did that rock get there? Did it flow there? Did it fall there? Was it volcaniclastic and got transported there? Was it deposited in lacustrine or alluvial environment? What's specific within those? What specific depositional environment? We think we're seeing deltaic environments, we're seeing fluvials, we're seeing colluvial. We need to know because each one would have a different set of transport processes.

There's been a tremendous amount of postdepositional deformation. Once these rocks came down, that was not the end of the story. Like I said, there's been a tremendous amount of structural deformation out there and we need to have an understanding of that. When it's this complex, we need to know which compartments are going to be bringing flow down from the repository area because that's where we need to monitor.

We need to know what those aquifer properties are. We've gone out and we've done some tests, but some tests are, you know, half a dozen tests in two years or eight tests in two years. We'd like to bring that up to where we have some test results that we can do some statistics on and do some distributions and nail these parameters down so we know not only what they are at a specific location, but what the range is likely to be in areas where we can't go drill.

Then, finally, we need better definition of hydraulic gradients, not only those horizontal gradients, but those vertical gradients. In some areas, we're finding that the vertical gradients are much greater than the horizontal.

NELSON: Point of clarification. You have a symbol there that's approximately four kilometers. What does that refer to?

BUQO: Depth down to the brittle ductal transition, I think. Jamie, are you here?

(No response.)

BUQO: No, Jamie didn't make it. Sorry. I think that's a depth down to this right in here.

Okay. I mentioned flow across the site-scale model boundaries. We were honored to be asked to go attend the NRC/DOE technical exchange on the saturated zone flow and transport workshop in Albuquerque. At that time, they presented this table that talked about here's a comparison between the fluxes in the regional scale model and those in the site-scale model. I believe the PI got up and said and they match. And, we said, well, wait a minute, they don't--well, we didn't then. I mean, we wouldn't.

But, we got back and we scratched our heads and looked at it and said, well, these don't match. We have areas here where if it's a negative number, it's flow into the site-scale model. So, this is flow into the site-scale model. And, if it's a positive number, it's flow out of the site-scale model. So, here's a flux line that says it's coming in. Here's a flux line that says it's going out. It says the water is going this way. It's going one way or the other. So, we need to find out.

And, like the gentleman said earlier today, gee, I sure would like to have some measurements to check this again. This is not 1,000 years in the future; this is something we can go in along those site-scale model boundaries today, punch down a couple of shallow holes, and see which way that water is flowing. To us, that's one of the ways you can reduce the uncertainty. As we look at this and we start calculating the percentage of errors across any given one, it sure gives us a lot of uncertainty about their uncertainty.

With that, I'd like to--I mean, it was brief, it's a lot to fill out, but we'll throw it open for questions now.

COHON: Yeah, very good. You've done a lot of work.

Questions from the Board?

BULLEN: Bullen, Board. Actually, it's a very impressive amount of work and I'm really pleased that Nye County is contributing as much as they are. I guess, the question I have is how does your data feed into the DOE and how does DOE give you feedback on your prioritization of the limited resources that we know everybody has?

BUQO: Okay. We have a procedure. When our data comes in from the field, the first thing is it has to go through Nye County's review process because we've found with more and more importance that we have to put that metadata (phonetic) on that data before it's released to anybody. We got our hands slapped earlier for the collegial transfer of data and we learned the significance of that. So, now, it's a major effort.

Rena, could you stand up, please? That's Rena Downing. She works for Nye County. She's a geologist. When we collect data, it goes to her. She does not release that data in tabular form, in letter form, report form, on the internet, or anything until she's satisfied that the metadata accurately describes it. At that point, DOE gets it. It goes on the internet for anybody that wants it.

The last time on the first phase, we did that data package and we thought that was great, but, man, that was cumbersome. Let me tell you, it was tough putting that thing out. We didn't concentrate on metadata. So, we put out data that we later got calls about. Well, what about this, what about this? So, we said we're not doing that anymore. We're going to clear that data and we're going to be satisfied with it and then get it out.

We have routine conversations with DOE, not only the formal level, but also the informal level which is really good. And, discussions with some of your folks and some of the other organizations about the data, particularly what does it mean? Are we looking at the right things? What should we be doing? I mean, like I say, Nye County is never going to have a lock on good ideas. There's a lot of them out there and we listen.

I was fortunate enough to give a poster presentation in Beatty at the GSA. Ike Weinigrad (phonetic) and Will Carr It was just a delight to sit down and pick these guys brains for about three hours. They wanted to go gamble and I was, no, stay here and talk about this. So, while the data transfer has become more formalized, the verbal interactions is still very informal and it's pick up the phone and, well, what do you think about so-and-so? I think that's a valuable part of the process.

BULLEN: How about your feedback from them on the prioritization of the work that you're going to do?

BUQO: Well, do you got any feedback? We haven't heard any objections. I mean, part of it is--based upon this Board and the NRC has made it clear that they want data closer to Yucca Mountain. They want to see 22S put on. We're not in a position to come along and kick somebody and say, come on, give us our permit. We just stand back and wait until the process runs its course and now we're ready to go, it looks like. So, we'll be getting out there and doing it.

But, our priorities are what's good for science and getting the answers out. We would have loved to have been up on that Test Site two years ago drilling, but we can't. But, now that we're going to get access, that becomes a top priority. One of the keys of our program is we're not so schedule-driven that we can't kind of drag our feet and slow down waiting for the results of the thing. We can wait until we get the results before we proceed onward. The other one is we can accommodate change very quickly. If you go in and you drill a hole and you find out something that says we've got to go over here now, we're able to accommodate and change our priorities to go to the next best location.

PARIZEK: Parizek, Board. Tom, again, thank you for your presentation. I'm looking at the page that gives the yellow triangles which is the Phase III drilling and the holes that have not yet been put in. You went through a listing of priorities on 22S and some others which are already blue squares. But, will you this year start on some of the other golden triangle sites for drilling or is that still in the more distant future?

BUQO: That's still in the more distant future. We've got to make a decision. Once we get done with 22S, we have the option of going up and doing 10 and 20, but we've got budget and schedule constraints and priorities. When we list our other sites, these are ones that were originally planned. We've got conductors in the ground. We could go drill at those at any time. But, as it sits now, it will depend largely on what results we get out of here. If we get some surprises out of here, then we may sit down and talk to everybody and say do we need to hold off on that and get up here right away and collect some more data.

PARIZEK: It still is a big hole of where the yellow is just south of the footprint of the repository down to Route 95 where you have quite a clustering.

BUQO: Sure.

PARIZEK: From a Yucca Mountain perspective, that's pretty critical data gap in there.

Then, on the page which talks about 25 additional holes or more than that, rather, because it's 25 are shallow, 15 medium, and 5 deep, you don't have a map as to where these might be? I mean, obviously, someone was thinking--well, you came up with the numbering. You must have been thinking where the other gaps in your information base are. But, do you have some kind of preliminary sense of where these would be?

BUQO: Yes, I do. Well, from my perspective again, we're going to be having a workshop to nail down so we're--

PARIZEK: That's the one in a month. Now, a month meaning this month or in March?

BUQO: February 15.

PARIZEK: February 15, okay.

BUQO: Is when Nye County will hold their internal workshop so we can put dots on the map.

PARIZEK: You're looking for input from everybody, but it sounds like Nye County is a closed shop?

BUQO: It's a closed shop. We would appreciate input from anybody prior to that workshop or shortly thereafter because we'll be coming in with a proposal. My thinking, but then is just me, is that we've got a lot of compartmentalization. That we need to be able to take a look at this compartment and this big compartment. This one may not be as much of a concern. And, we would want to go in and put in, at least, one deep well here to see what it is. But, by deep, my thinking is we don't need to go to the paleozoics in every well. The further we go to the north, the deeper the paleozoics get. So, the cost goes up. The information value of that, you know, what's it worth because our feeling is any contamination that's going to go downward, it's going to take a quick lateral or a horizontal pathway. It's not going to go down to the carbonates and then come popping up someplace else. We've got an upward head. So, our money is better spent on really doing a good job within these compartments. So, that means we're going to want to put in a test well on each side to test across those boundaries. We're going to want to put in enough shallow wells that we can see what's the attitude of the upper volcanic package in that area. Is it sitting in there tilting like that like Fridrick's model would suggest or has it got some of this motion to it, too?

PARIZEK: So, late this month, some of those dots you'll start to show on a map and the logic behind the sites you're picking?

BUQO: Yeah. Yeah. And, it would be funny because you'd be surprised. 25 shallow wells sounds like a lot, but when you start coming over and investigating some of the model boundaries with some of them, two on each side of the model boundary just to give you gradient and the transmissivity, then six places over here, and all of sudden, 25 is not that many. I'd love to say 100 wells, but in reality, you know, how many can Nye County do in a year?

PARIZEK: But, now some of those would also depend upon the geophysical surveys you all are supposed to do because that's a new addition to your work plan?

BUQO: Absolutely. Yeah.

PARIZEK: So, there will be payoff from that work.

BUQO: Sure. We've got currently the one seismic line that runs down through here and it's very important because it's got control to the south with the Felderhoff Wells. It fits in with the Fridrick model and it looks like the seismics is actually a pretty good indicator that we can nail down that tertiary paleozoic contact and the nature of that contact. When you look at the seismics that was published by Broker in his work--and I don't have a viewgraph of that, I'm afraid--but you can see these exact features in that seismic profile that says at least along that profile, that's a pretty darn good model.

PARIZEK: Okay. And, one other point. On the uncertainty zones, there was a yellow, not exactly a box, but that was from the TSPA-SR report and that's getting narrowed down, will be narrowed down further, but I'm glad to see that you have now this funny shaped box to the left of it and then you have the one to the right. How many holes do you put in that very big one on the right versus how many you might put in the box to the left of the yellow? That's obviously a value judgment again from a Yucca Mountain perspective.

BUQO: Okay. From our perspective or I should say from my perspective, we, of course, would concentrate in here in the shallow environment. When you look at the model, the model boundary on the east is the driving force behind the water coming into that model. The biggest single thing is down here through Rock Valley that just takes a little shortcut through the southeastern end of the model. It's a flow-through and that kind of distorts the values. If you add up all the values and compare the regional versus the site-scale, there's only a four percent error. But, you say, oh, this is lovely. This is a beautiful fit. If you delete Rock Valley, just that one flux line, now it's a 14 percent error. Then, you start looking at the individual flux lines going up here and the directions are different and the errors start going way up. So, we feel it's worth some wells. It's not worth a ton of investigation, but it's worth going on and nailing down what is that gradient across there. It's one thing to go in with a groundwater modeling and use a general head boundary to try to simulate it, but like the guy said, I'd rather have some measurements.

PARIZEK: In other words, you want the model to give you the water that you want to get in your allocations?

BUQO: Yes.

PARIZEK: Roughly a little higher. Is that that 19,000 acre feet that we were hearing about a while ago?

BUQO: We'll put every drop of it to a beneficial use.

PARIZEK: I mean, so a good model has multiple values, the least of which is going to be this allocation?

BUQO: Sure. Sure. We've always said Nye County would love to see a well-calibrated, validated groundwater model that we could use for water resources planning efforts.

PARIZEK: Including a transient one?

BUQO: Yes. Oh, yeah, we'd love to be able to plunk wells in at our points of diversion and see is it going to lower the water table under Yucca Mountain, how much is it going to induce flow from there?

PARIZEK: And, I guess, I was glad to see that none of these wells are actually extraction wells. I mean, if you're going to have that many wells with so much water, you'd be in the water business. But, I guess, you have another place where the extraction wells might be located someday.

BUQO: Yeah, and those are all subject to change depending on what we find out and the State Engineer has to do a little thing called a ruling first. If he says it's only 16,000 acre feet a year, then we'd say forget it, it's not worth it.

KNOPMAN: Knopman, Board. Just following up on some of Dick's questions, were you here yesterday, Tom, to see Al Eddebbarh's presentation in particle tracking?

BUQO: No, I'm afraid I wasn't.

KNOPMAN: Al's model showed a fairly tight flow path coming first southeast and then south from the repository footprint. It just looks like a lot of the area that you've described as still some uncertainty there. It doesn't come into play in terms of the potential flow paths down from the repository area. I appreciate what you said about trying to better define the boundary conditions on the east side. But, why not put a few wells in along that predicted flow path or at least more than one? You've got the 22S, but something further upstream from that. It seems to me that would be a very good chance to see whether that's--and even do some possible tracer studies.

BUQO: Oh, and I agree. Two things on that. One is we've got 19D which is on the--I don't want to use the word "plume", but the flow path, their predicted plume--and we've got 22S that is near it. To get on that flow path, we need to drill west of Forty Mile Wash which means we'd have to get the roads built and get over on that other side. With respect to, well, we have this predicted flow path and I don't mean to be glib, but that flow path is predicted on the basis of a bunch of conditional axiomatic models that say if all of the above is the answer is correct, then this is the answer. And, it's in lieu of data.

Our whole program is about getting data to feed into a model so we get a more accurate thing. I've investigated hazardous waste sites across the United States and I've seen some real good and some real bad contaminant transport models. I've seen models that you could not use. What my experience has been is where you have data and you have a good conceptual model, then they can do a pretty good transport model. We're not sure about the conceptual model and the data, we know, is lacking over a huge area. So, it kind of calls into question. I know it's the best that we've got and it says we need to investigate and I agree. And, we would put some wells in right on that flow path. But, we'd want to go a little beyond that, too.

COHON: And, you would welcome DOE's input on the location of the next wells?

BUQO: Oh, absolutely. We can't waste $500,000 putting a well in a stupid location.

COHON: Right, I just wanted to confirm that.

BUQO: We have to go get the biggest bang for the buck and we want to do monitoring. We want to put the wells where the contaminants are likely to be. What good does it do to put a monitoring well off the flow path? It's going to come up clean and that's not protecting public health and safety. That's doing a disservice.

COHON: Well, thank you very much for your presentation.

BUQO: Thank you.

COHON: We turn now to John Kessler from EPRI. John, as most in this audience know, is a long time expert on performance assessment and he's going to update us on EPRI's efforts in PA.

KESSLER: I appreciate the invitation from the Board to speak to you today about our most recent performance assessments and a few related issues to that.

What I'd like to go through today with you are the purpose of our TSPAs, the scope of where we're going to Phase 5 or really the fifth iteration of our performance assessment that's described in our November 2000 report which I had hoped by now you would have copies of. It looks like it's taking a while to get out of our publications department. I'll give you the lightening tour of model components and assumptions, base case results. Then, I'll switch gears a little bit and talk about a barrier identification exercise we went through in the report, as well, just to say what barriers are there and semi-quantitatively how important might those barriers be. We also do a quick review of DOE and EPRI conservatisms and optimisms to try to give you a little bit of insight as to why our model looks the way it does in comparison to DOE's. And, I'll wrap up with a few words on performance confirmation which we feel is pretty important to site recommendation, as well as license application phase.

So, the broad brush purpose of why is EPRI also doing performance assessments for Yucca Mountain? Really, what we're after is an independent assessment of the technical issues. Specifically, our utilities in sort of a broad brush way of looking at it saying--they want to be able to decide and plan. So, they're asking EPRI to say, well, you know, what do we think is really going on? What do we really think are the important technical issues? So, we also provide some input on regulatory and legislative issues as it's appropriate based on the results from our technical analyses. And, certainly, we want to provide insight to outside review bodies, such as you, ACNW, and others.

So, what I'm about to show you is based on really one scenario which is the normal release scenario that you're all familiar with; container degradation followed by waste dissolution, contaminant transport, on into the biosphere. We did not consider these three broad classes of scenarios. Colloid-aided transport, we've taken a look at what DOE and M&O have done there and we're satisfied it's pretty conservative. We've also seen that even with their conservative analyses that colloid-aided transport contributions to dose are marginal, at best. So, we felt at least for now it wasn't a huge optimism on our part and to leave that out. We have not considered volcanism quantitatively yet. We're very satisfied that the consequence scenario that DOE is running along now for the volcanism is pretty conservative. We might want to look at that ourselves later on this year and evaluate what we think might be a more reasonable set of scenarios from a quantitative standpoint. We also have not looked at human intrusion.

Our model components, the code itself, IMARC, Integrated Multiple Assumptions and Release Code, it's mostly a logic tree format as opposed to Monte Carlo. Right now, we do have bits of Monte Carlo in there in the sense that our container failure time are Monte Carlo simulations and those are really provided as a lookup table than to the rest of the logic tree format within IMARC. We have 54 branches that we look at in the logic tree format. In terms of the IMARC shell, we've got the usual things; time steps, mostly global inputs, lookup tables, things like that that we use. We have really just two submodel links. One is the source term model and then the UZ/SZ transport model that links directly into the IMARC shell.

So, the logic tree part of IMARC is shown here. We really just look at four major sensitivities. One is infiltration where we look at three alternatives to the infiltration with these probabilities on them. I'll get into a bit of details about what those numbers are. Focused flow factor, none, meaning that basically water percolates straight down. There's no focusing of the waste as it comes down to the repository horizon. Strong focusing is that there's a lot of local channeling into certain parts of the repository versus others. I'll describe that again in a little bit more detail in a few minutes. Solubility and alteration time, we assume that they're correlated. That is that general radionuclide solubility and the alteration time for the spent fuel matrix are correlated so that there can be high solubility, fast alteration time, moderate or low and slow wit those probabilities. Retardation here is in the UZ/SZ mostly in terms of Kd's. We look at three alternatives of those.

The net infiltration portion of our model was developed by Stuart Childs at Kennedy/Jenks. It's based on three climate states that Austin Long at University of Arizona developed. What we have for our climate states are three. Ours is also fairly simplified. We've got a greenhouse scenario we start with. Austin believes that we're about to enter a greenhouse scenario which has a lot of analogies to DOE's--what's the second climate state?

SPEAKER: Monsoon.

KESSLER: Monsoon, thank you. The monsoon scenario. We put it in the first thousand years rather than in the second time period. Austin believes we'll return to interglacial which is roughly what we're at now between 1,000 and 2,000 years post-emplacement. And, after that, we're stuck with a full glacial maximum beyond 2,000 years. So, the net infiltration values for those in millimeters per year are listed here after Stuart has gone through his model.

The focused flow factor conceptual model was developed by Ben Ross based a lot on the March 2000 AMR of Mike Wilson's, the "Abstraction of Drip Seepage". So, we have two end members that we looked at. The zero focusing where basically the percolation rate at the repository horizon equals the net infiltration rate repository-wide. Then, we look at a focusing factor of 22 which basically means that 4.5 percent of the repository or 100 over 22 get 22 times the area-average infiltration rate. What that means is that the other 95.5 percent of repository is dry, no dripping.

Attempting to make some tracks here, I'll jump to really some of the basic results from the drip shield/waste package combined failure distribution model. Each is described separately in the chapter written largely by Dave Shoesmith with input from John Missari on both the modeling, as well as some of the details, and for example, weld flaws, things like that. So, what we have here is they looked at, you know, do we have all of the drip shields emplaced correctly at the beginning? They said, well, probably on the average, we may have something like 14 failed an emplacement. You'll have to read the report on how they came up with that number. But, something like 14 may not have been emplaced correctly right at the beginning. We can have general corrosion, hydrogen-induced cracking. They do carry along that particular mechanism. So, what we see is that they tried to look at the temperature versus time at both the center of the repository and at the edge of the repository and they see that they're really insensitive to the temperature distributions there in terms of failure times. And, what really is offsetting it is the 14 failed at emplacement.

In terms of a few details about the container degradation, they believe that aqueous corrosion starts also at temperatures up to about 120, localized corrosion above 100C, and stress corrosion cracking, they believe, is only a viable mechanism on the outer weld. Temperatures have cooled off enough by the time you get to the inner weld, they feel that it's not a viable mechanism.

We also take some credit for cladding. We have two different models for whether we've got active dripping on the cladding or whether we have basically human error corrosion.

We make the, I still think, conservative assumption that 2.4 percent of the initial cladding has failed at emplacement. I believe that compares to something like 8 percent in the DOE model. We have general corrosion that's not specifically driven by the fluoride drill mechanism; that is we don't concentrate all the fluoride on one particular part of one rod. We've assume that localized corrosion is unlikely. So, we have roughly for the cladding failure something like on the order of 10,000 years for the lifetime of the cladding.

Our source term model is a compartment model where the compartments are in the boxes here. You can see that the double arrows imply diffusive transfer between these compartments. You'll notice we do carry along corrosion products. We wanted to test whether sorption on corrosion products, generally the iron hydroxides were potentially important. We also carried along the invert and do consider diffusion and potential sorption in the invert and we also allow diffusion either into the matrix or into the fracture which is another difference between us. We assume also conservatively, that those compartments are well-connected, very much like Bob Andrews showed you yesterday. I thought that was a great talk on describing the details of how things go through and the kinds of assumptions we're forced to make given the lack of detail and what the pathways may really look like. We have advection directly into a local flowing fracture and we assume that 100 percent of the waste form in the failed cladding is assumed exposed. So, we still have a lot of conservatisms in our model.

Our UZ/SZ flow and transport model was developed by Frank Schwartz at Ohio State and Ed Sudicky at the University of Waterloo. Again, trying to get through all of this in a half an hour, I'm really dropping details here. But, our UZ model is a 1-D dual permeability continuum model. We have a few simplified vertical columns. You'll be interested to see what we get for results given the fact that we've simplified a lot of the UZ transport here. Our saturated zone model is a 3-D dual porosity/dual permeability model. Our saturated zone thickness in the model is 200 meters and vertical dispersion for us is an issue we aren't treating--we aren't mixing things into a well at the end. So, we do care about concentrations in the saturated zone.

Biosphere dose conversion factors conceptual model was developed by Graham Smith and company at QuantiSci. Again, it's a compartment model. Why I'm showing you this very busy viewgraph is really to point out what we think is a useful way of making parts of performance assessment a bit transparent. This is an interaction matrix where the leading diagonal elements on this matrix really are features where we can actually move radionuclides from one compartment to the next. The off-diagonal elements are really events or processes that link or are able to transfer things. For example, up here, we may have for the 6.8 the transfer between the surface soil and the flora, uptake, rain splash, things like that, and we can show you exactly what's in our model, what's getting transferred where, and then show you the equations for that. A compartment model like that is something that helps make life transparent. So, if you're looking for a simplified model, perhaps a compartment model using some sort of interaction matrix approach would be one potential technique of making things a lot more transparent. Anyway, the thick arrows then are the exposure pathways to the critical group.

Okay. Skipping right to the base case results, I apologize for those of you that do have the paper copies that I didn't quite do this right. But, bottom line is the total dose is the thickest curve here. Our peak dose which is out on the several hundred thousand year time frame is less than 1 mrem/yr. So, we're roughly two orders of magnitude or more below the M&O model. In terms of dominant radionuclides, yes, we do see technetium and iodine coming out a little bit ahead of the others down here. The dominant radionuclides out here for us are thorium-229, U-233, then neptunium-237, followed by selenium-79. You will see that we did not make the change in the half life of selenium-79. We actually have it up too high in the report. This is a corrected failure.

Differences in terms of dominant radionuclide, we think, have a lot to do with our critical group consumptions and dose conversion factors.

Another difference that really shows off the dose conversion factors is this is for the drinking water pathway only. For drinking water, we're down in the hundred of a millirem for peak dose. And, we show that selenium is really knocked down in terms of its contribution to the drinking water pathway.

Shifting gears a bit, we looked at barrier importance analysis. Really, it's probably better to say here barrier identification. What are some potential barriers here? We wanted to assign a value to the various components in the Yucca Mountain system. Our motivation also is defense-in-depth. Really, we're asking the question are all the eggs in the one basket or two baskets, as some have suggested in the current DOE approach. That the container does everything and the natural system really isn't doing much. We wanted to also provide insight on important features, events, and processes. To do all that, we used what we call the hazard index approach which is really a variant of the full neutralization approach that you've heard a lot about in the past two days. Except we really fully neutralize as opposed to what you've seen. We go all the way. We eliminate very single barrier completely at the beginning. It's really used to try to identify what barriers really might be there for you as you're trying to carry along barriers. We add the potential barriers in then one by one and then the amount that this hazard index which we just link along with a theoretical dose rate is reduced and indicates the potential importance of that particular barrier.

This is a theoretical exercise. Do not believe the numbers. Do not take them out of context. It is an exercise to try to understand what barriers might be there. Okay. You've been warned. We make the assumption that all 70,000 metric tons of spent fuel are dissolved in $.6m^3$ water and one poor individual drinks it all in one year. That lovely theoretical exercise has no physical meaning and gives you a hazard index of something like 10^{17}. Why are we starting so unrealistically. Has Kessler lost his head yet again? All FEPs can be evaluated quantitatively this way. We try to pull in as many FEPs as we can or features, events, and processes. We want to include things like basic engineering decisions. The repository layout does have some influence on what you get for a final dose. We wanted to make sure we got a chance of somehow including that in the analysis.

So, we looked at 13 really classes of features, events, and processes or FEPs here that we add one by one that broadly represents some potential barriers here. First of all, on the average, only four percent of the repository is wet; that is active dripping into roughly four percent of the repository. That compares to the 13 percent or 15 percent number I think you heard from DOE or M&O. We also say that our moderate alteration time for the waste form is something like 3,000 years. There's another potential barrier. We'll add that in next. Then, we throw in the solubilities. Up to this point, everything is infinitely soluble. Then, we put in some realistic solubilities here. Then, we'll throw in the cladding barrier, then we'll throw in the container barrier, then we'll throw in drip shields, then finally we'll add dilution in the unsaturated zone. So, that up here, these are basically point sources concentrating all 70,000 metric tons in one place. Now, we get in the dilution in the unsaturated zone which takes into account things like the fact that the waste is spread out and not all in one point. Next, we'll add in sorption in the engineered barrier systems. That could be the container corrosion products, sorption in the invert. Then, we'll move out to the accessible environment assuming it's at 5km so we can pick up flow and transport through the UZ and the first 5km in the saturated zone. Then, we'll turn on the retardation mechanism, the sorption in that piece of the UZ. We'll move the accessible environment next out to the front of the alluvium continuing to add on bits of the system or visional barriers. Then, we'll pick up the alluvium by moving it out to 20km. For all of this, the analysis is just for the drinking water pathway. Then, we'll pick up the dose from all the pathways and bring the dose back up a bit.

Again, sorry this didn't print out right. But, we've got the hazard index curve here for all radionuclides and there is the 13 that we're going to add in succession. So, here's our 10^{17} up here. So, here's where we start. We're actually trying to get down to dose rates somewhere in that region if the name of the game is compliance with something like a 10^{1} dose limit. Okay. So, we add four percent of the repository wet. We assume that--this is not a model, but we have no contribution from the dry zones. In our analyses, we satisfied ourselves that diffusion from zones where there isn't any dripping really doesn't add much. So, this brings things down to four percent of the first value. When you're adding the 3,000 year alteration time, you're bringing things down by roughly a factor of 3,000 because you're spreading that release now over 3,000 years.

Next, we add in moderate solubility. This is for 22 of the dominant radionuclides contributing to dose and you see we bring the dose down by another couple orders of magnitude. Then, we add in cladding which brings things down by roughly another order of magnitude or so. So, cladding does seem to be an important backup barrier to the other parts of the EDS until finally all the cladding has failed and you come back up to that other solubility line.

Then, we show the containers failing over time. Yes, indeed, containers are important. It delays things a lot because we're assuming we don't really have much in the way of container failure until well after 10,000 years. But, eventually, all those containers fail and you come up roughly to the same line you were at before. We add in the drip shields. Again, it shows a little bit less performance, but remember part of this quantitative relative importance is the order in which we added these barriers. If we added them in a different order, we would get somewhat of a different result.

Then, we go through and we add in dilution in the unsaturated zone. We're now going away from a point source and actually taking credit for the fact that the waste is spread out over the entire repository footprint. That brings things down a whole lot. If we add in EDS sorption, we've got a lot of credit here for EDS sorption. This is a barrier that right now DOE has neglected. Maybe, we're being optimistic here. We don't think we're being optimistic, but again we find that it's an important barrier for all time.

We move now and add in the unsaturated zone and saturated zone which is accessible environment at 5km here and again we get really mostly dilution here that shows up on this, as well as a little delay. Now, we add in the retardation which we're down to here and again we get more delay and a slight lowering of the peak dose. Then, we move out--you know, there's another 15km or so of alluvium--of the fractured tuff to help us. So, we're down to this curve. If we move the accessible environment out to 20km which is here, you find the alluvium doesn't help us very much, but a little bit. And then, the dose from all pathways brings us back up because all the rest were for drinking water and we increased the dose by roughly an order of magnitude when we pick up all the non-drinking water pathways in our model.

So, what do we find? We looked at hazard reduction factors. How much does each one of those barriers reduce that theoretical dose down to something that finally does make physical sense when you put in every single feature that's there? So, hazard reduction is shown here, roughly at the time at which the peak is is shown here. This is a rough guess as to, you know, is that particular barrier more an engineered barrier or is it more a natural barrier or is it just some combination of the two that's just sort of one of each? And, when you go through all these hazard reductions, you get a total hazard reduction on the order of 10^{18} or so and where is it coming from? Well, the hazard reduction from engineered features is something like five to 14 orders of magnitude depending on how you want to split these up. The hazard reduction due to natural features is something like five to 14 orders of magnitude. So, those are the two numbers I want to leave you with which is the idea that, well, from this sort of very cursory type of experiment, we're satisfied that, no, the eggs aren't all in one basket. There's plenty of hazard reduction coming from both engineered and natural features. This last line here is that the hazard reduction are actually an increase due to all pathways and is roughly that order of magnitude when you consider the non-drinking water pathways at least in our model.

Okay. Switching gears a little bit, we have a chapter where we looked at some incomplete survey of the conservatisms and optimisms from the DOE models and also the EPRI models. I'm just going to point out a few of the ones we saw in the DOE models. I really liked what I've heard in the past couple of days in terms of looking at uncertainties analysis. That's all very useful. The source term diffusion model, I thought Bob did a great job of explaining where the potential conservatisms are in that model. We agreed. Both of us are conservative on that model. The EPRI model is as conservative in many of the same areas that we saw the DOE model. We have a few other conservatisms that we--a few of the M&O conservatisms we backed off from that have to do with we do allow diffusion into the matrix and we do consider how far it is to the nearest flowing fracture depending on what kind of a zone you're in, wet or dry.

Volcanism consequences, we think, are quite conservative in the DOE model. The unsaturated zone transport, I'll talk about just one particular aspect of that; that's the FEHM particle tracker. We're about to put out a white paper on that finally that basically reviews some of the work that was in the AMRs where we basically said we agree that it looks like the FEHM particle tracker that the project is using now is conservative, and if it was fixed, we think that the travel time estimates through the UZ would increase by maybe even two orders of magnitude. I've also heard privately that they're aware of that, that they're going to fix it, it's just a matter of when it gets fixed.

The saturated zone transport, I'll talk about on the next few viewgraphs in terms of the conservatism that we saw there. It's a different aspect in terms of conservatisms than you heard from Al yesterday. Optimisms, yes, there are some in their model. One is, well, do they have 70 percent of the heat removed by ventilation? Maybe, maybe not. I've got this as a question mark as to whether that's an optimism or not. Maybe, they're optimistic in thinking 70 percent of the heat can be removed. We don't know really what that means in terms of performance.

Again, it seems as if you can't win on how you're going to try to be conservative on your choice between temperature and relative humidity. If you try to be conservative on temperature, you tend to drive relative humidity estimates down and then you're being optimistic about that and vice-versa. Again, the total importance to performance is a little bit less clear. But, the bottom line is that we're satisfied that looking at what conservatisms we saw versus optimisms that DOE's current assessment overall is conservative.

Okay. This is getting back to that saturated zone. conservatism which is the concept of the flowing interval that Al mentioned a little bit in his talk yesterday. What they've done is they've gone down boreholes and they've put down flow meters and they've packed off intervals and roughly what they find is that in some intervals you get flow and in other intervals you don't, which makes sense. What they've done though is they've said, well, gee, because we can't tell you in this flowing interval whether it's one fracture that's contributing to flow or maybe a group of fractures. We'll just have to be conservative about it and say it's just one fracture that contributes to flow. So, what that means is that they've conservatively assumed a lot less fracture matrix interaction than if perhaps there are several fractures in these flowing intervals that are contributing to flow.

Well, Frank and Ed both believe that reality is more like there's going to be several fractures in these flowing intervals that contribute to flow. We agree that the distance between the flowing intervals is something like the 20 meters that I believe the M&O is using, but within these flowing intervals, we think the typical fracture spacings is less than a meter. That has a big, big impact in the amount of fracture and matrix interaction and the velocities which even a conservative tracer will go through the saturated zone.

Just to give you one example of a sensitivity we've done based on our assumption that we have roughly one meter or less fracture spacings within those flowing intervals, this is a matrix retardation sensitivity for neptunium-237 for the drinking water pathway. What you see is that basically for the low retardation which is near zero, we still have--this is roughly at 10,000 years travel time through the saturated zone. So, we have much, much slower travel times based on this conceptual model and these assumptions about flowing intervals. And, if you get some sorption for something like neptunium-237, we can really start delaying the arrival of neptunium-237 through the saturated zone and Mark's contrast to the M&O model which I think is fairly insensitive to the Kd's for neptunium.

So, what does this mean in terms of what's important and what isn't? Well, what we see is we have basically no impact on dose if we eliminate the alluvium. What we're saying is that our tentative conclusion is because DOE has been conservative about the importance of the fractured coarse media part of the flow path, they're now having to wind up relying a lot more on the alluvium. Since we've taken more credit for that part of the saturated zone, we're finding that the importance of the alluvium just isn't strong.

I'm attempting in one viewgraph to answer a couple of very big Board questions here. Boiling it down, we hear the Board asking one question. Is it really necessary to assess all the uncertainties? I think you've heard some pretty good answers over the past day and a half which run something along the lines of no from Bill Boyle. We would agree it's no. Many of the parameters we treat as fixed are truly unimportant to performance. And, therefore, it's really not worth the effort to look into those.

Other more important fixed parameters could, during SR analysis, be investigated using expert judgment. I liked Bill's list a lot that he showed you. We tend to agree those are very good ones to look at in their UU analyses. We very strongly support that effort. We recognize that it's going to be using expert judgment. Non-Q information is okay information for site recommendation decision-makers.

Conservative versus best estimate kind of analyses, as Bill talked about, to provide some insight into the potential degree of conservatism, we think is really valuable not only during site recommendation, but also we think this is something that should be presented to NRC during licensing. So, we encourage the kind of M&O effort led by Coppersmith, but as presented by Bill Boyle to you earlier today.

Next big TRB question. Is TSPA an appropriate decision-making tool? We say yes. We think it's a comprehensive and quantitative measure of the degree of public health protection. We don't know what other kind of measure there is that gives you some sort of insight onto the degree of public health protection that's so direct.

TSPA is now based on many years of experience, multiple practitioners arriving roughly with consistent results which provides some level of confidence that TSPA has some value. Most of the TSPA submodels are based on solid data. There's years of R&D incorporated directly or indirectly in a lot of the submodels you're seeing. We also think that there already are multiple lines of evidence built right into TSPA. Many of the submodels are already employing natural analog information either directly or indirectly. We encourage that, as well as the qualitative development of natural analog information that should continue. Performance confirmation period that you heard a bit about will further bolster the TSPA results.

New topic, performance confirmation and other long-term R&D activities. We think the performance confirmation and these other long-term R&D activities defining what they are is important to SR, not just to LA. We think that it's going to help provide clarity when managing many of the important uncertainties. We've heard a lot about managing uncertainties, performance confirmation, and related long-term R&D tests. We think it's an important building block in managing those. It's an opportunity to improve understanding and bolster the safety case and we think that SR decision-makers can use long-term R&D plans along with current knowledge to make an informed decision. There are still uncertainties out there. A good, well-developed performance confirmation and long-term R&D plan will help provide some people like you, hopefully, with an idea that, yeah, we really think they'll get there if there is a well-defined understood, long-term R&D plan for the 50 year time period or so.

To that end, we've embarked on a two-year program to clarify the role of performance confirmation in both SR and in LA. Again, two months ago, we issued an interim report on performance confirmation where we reviewed the performance confirmation issues. What is it that really constitutes an appropriate performance confirmation activity?

Well, it has to be able to truly confirm long-term performance. It has to have clearly defined goals and stopping criteria so that you know you're going to get there. You have some kind of confidence that this is a meaningful test that can really be done and really get you information that you need.

As part of this interim report, we reviewed what was the current DOE performance confirmation plan that we had at the time which was the May 2000 version. We believe it's generally sound, but needs improvement. I think Dan Bullen made some comments earlier along those lines. But, we don't necessarily object to the 20 odd tests that they have thrown in the appendix. The why for those tests are pretty weak. They've got eight steps in terms of how you do a performance confirmation activity. We like those eight steps. They need more elucidation and then the particular tests that they pick have to be really tied into the criteria.

Other long-term R&D could provide the bases for model improvements. For example, I think that Debra Knopman talked about, well, maybe we can with some of these other options get away from, say, the drip shield. Okay. I've got some long-term R&D tests that would help you establish the basis for dropping it. Are there other long-term R&D tests you could do that maybe would help you change your final thermal loading prior to closure? Maybe you'll need to start, for whatever reason due to uncertainties, with a lower thermal loading and you can do a large enough scale thermal test and maybe by closure time you can go up to a higher thermal loading.

So, what we're planning to do this year is do an external review of recommendations for appropriate performance confirmation and other important long-term R&D activities. The idea is to establish some sort of consensus on what are the appropriate kinds of performance confirmation and R&D activities that are useful. We'd also like to bottom out details of one or two performance confirmation activities with some sort of more detailed test plan. For example, show how supporting models can take about 50 year data and extrapolate it to 10,000 years. That's really a tall order. That's what we're talking about in performance confirmation. Can it be done? We'd like to provide some sort of demonstration as to how you link all this together. You've got to define error bars that are meaningful for 50 years that again could be extrapolated to 10,000 plus years.

We're certainly going to choose container degradation or some aspects of it as part of the example. We may also investigate if our budget can handle a larger scale thermal testing is another potential long-term R&D plan. We're going to try to get it done the middle of this year.

So, a quick list of conclusions here. We believe that DOE's current TSPA is conservative. We think the repository performance is bolstered by a diverse range of multiple barriers. We think that the efforts to quantify uncertainties should be risk informed. That is just don't go for all of them, go for the big ones. We do like Bill's list. TSPA is an appropriate tool for repository decision-making and that performance confirmation should play an important role in repository decision-making.

COHON: Thank you very much, John. Questions?

BULLEN: Bullen, Board. Actually, maybe we should invite John back a little more often so he doesn't have to put quite so much information in a 50 minute talk. I wanted to go back to your slide on the hazard index or hazard indices and ask a question about the order in which you put them together.

KESSLER: Okay.

BULLEN: If you put them in one at a time, I mean, starting with the 10^{17} dose and you put them in and, say, put an engineered barrier in and it drops by a factor of 10^{14} or whatever and then take it back out and put another one, instead of doing it sequentially, can you get a handle on sort of the absolute magnitude?

KESSLER: We do that, too. Of course, I would have loved to have shown you more viewgraphs. I was afraid I wasn't going to have time. We did do Gerry's complete elimination of all the EBS barriers. Bare fuels sitting on the invert. And, we went through and did a dose assessment of that and found, well, the timing of the dose peak moved way, way up. The peak was still less than 1 mrem/yr. So, we're finding that we get mostly a delay from the EBS barriers, some reduction, but it's mostly the timing of the peak that we found in our model that was affected.

BULLEN: Just a last quick question about the performance confirmation plans and your review of it by the middle of this year. That will result in another report that will basically come up with EPRI's statement or suggestions for how the performance confirmation will be improved. How will we get that information, I guess is the question.

KESSLER: Well, it's coming out as EPRI reports the middle of this year that hopefully will get to you quicker than we haven't gotten the reports I've talked about today.

BULLEN: Thank you.

SAGÜÉS: Okay, very good. Can we look at the transparency that has the cladding curves?

KESSLER: Yes. Now, don't ask me something I can't answer since Dave's not here.

SAGÜÉS: No, it's actually--

KESSLER: Which number is that, Alberto?

SAGÜÉS: 10. I don't know if that's the right number on the lower left hand corner.

KESSLER: I'm getting there. I've got it.

SAGÜÉS: Very good. This is really more of a probably general question on a little bit of a philosophical issue. But, we're talking about a barrier, right, maybe, half a millimeter, .7 millimeter thickness around it, and what we're doing here is we are basically asking ourselves, as grown people and engineers and scientists, do--I don't know if that's the right words to believe or at least to have faith or to pretend that the body of this thing made out of a material for which we have very limited experience--and most of that experience is in the temperature regime which is somewhat higher--would it be nice to sort of believe or consider that the mean life of this in a wet environment is going to be, what, some 20--the median life some 20,000 or 30,000 years for the red curve? Is that something that I, as a metallurgist or as a scientist, am I ready to really seriously consider this without something other than extrapolating knowledge that we have acquired in a very short time and without having a well-defined base of basic knowledge to guarantee that? I think that this may be asking too much for an engineer to really take seriously. So, this is beyond just--we can all say, okay. We can go to reactors. We have measured corrosion rates. We have sliced some of these things. Yes, sure enough, if you get the corrosion rates that's measured and you get a calculator and you extrapolate, that's what comes up. But, is that something sort of rational to do or are we just simply engaging in a pretend kind of exercise?

KESSLER: I would hope it's not just plugging numbers into a calculator. Again, Dave Shoesmith should be answering this question. But, from what I understand of this approach, to try to answer your question, it's not just blind faith on extrapolating from some numbers. Dave has certainly based it on what data are available. Granted, they're short times compared to what we're talking about here, but I know he's also considering what mechanisms are there, does he expect them to be robust or not in his estimation of the long-term behavior and corrosion rates of these. So, he has considered those things.

Obviously, none of us can answer--again, like Jerry was asking for the 10,000 year old Alloy-22 coin, there's not a 10,000 year old zircaloy coin out there either that we know of. All we can do is base it on what we understand about the mechanisms, the rate at which those mechanisms may work, what our understanding is about the environmental insults that might go on under these kinds of environments and use some judgment along with the data to come up with what we think, what we hope is a reasonable approach to extrapolating these things into the long-term.

SAGÜÉS: Right. Do you know what bothers me about this is such a thing, a tenuous little thing, even if it were made out of gold, I would have a little bit of trouble really believing that. Or, maybe, suppose you have a thin layer of gold; well, I guess, you also could get a cut like that. There could be all kinds of things. We don't know whether there will be a dimensional stability of--inside that. Maybe, there's some kind of a swelling mechanism that says we'll need to depart a little bit. I just want to express this concern because again we're being asked to look at this not just from a point of view of some observations in the laboratory, but also trying to look at this from the point of view of just plain common sense. And, I'm having trouble. Not with you, of course; this also applies to the project. This is something that I think needs some thinking beyond manipulating the variable numbers. I just wanted to express that concern.

KESSLER: Of course, I have to agree with you. It only makes logical sense that you need to think about what you're doing when you extrapolate whatever data we would have. It's all going to be short-term compared to these numbers out to these time frames. I can say this is not the first time we have seen the results of what--what the assumption is is very low corrosion rates. I think Gerry Gordon had it in his talk yesterday about Alloy-22. It's the same thing. Data are out there that show the corrosion rates are very low. So, what you have to assume is that something about this environment increases those corrosion rates over what's seen in the lab and that there's some other mechanism that we haven't thought about and again we can't really address, you know, what we don't know.

SAGÜÉS: Right. I think to compound this at the same time that we're assigning these astonishing qualities to this very thin piece of metal, we are totally throwing away two inches of stainless steel that is around it, aren't we?

KESSLER: Again, it's this approach to what mechanisms do you think you know well enough and what mechanisms can you rule out? What Dave has done, as I understand the project has done, is that Alloy-22, as well as zircaloy, have a lot of mechanisms that can rapidly fail things that we both feel strongly can be ruled out. Stainless steel is not such a material, that there are pitting things and other issues where you can't rule them out in these kinds of environments, and therefore, we would be proceeding at a lot more risk if we started taking credit for stainless steel. I think, on a fundamental basis, that's the reason why we take credit for some things and not others.

PARIZEK: Parizek, Board. John, I was looking at the hazard reduction factors table and I get down tot he engineered features and natural features and they seem to be tied, 10^{5-14}, 10^{5-14}. Obviously, I'm feeling even better. But, is that 10^5, 10^{-14} or is it 10^5, 10^{14}?

KESSLER: Yes. It's anywhere between 10^5 and 10^{14} depending on how you want to divide these things up. I've got a lot of boths here, okay, that are heavy hitters. I've got them both here that's a heavy hitter. I've got some things that are mostly engineer. Okay? Now, this is a semi-quantitative, emphasis on the "semi" here. We're asking a simple qualitative question. Do we have all the eggs in one basket? Are there some natural barriers here? Okay? So, all of these broad classes of barriers that are combinations of natural and engineered FEPs, you can't really separate them out. So, this is my perhaps poor attempt at attempting to provide some semi-quantitative understanding of are we putting all the eggs in one basket? So, don't push it further than that.

PARIZEK: It serves that purpose. I mean, is that good enough for Government work to be that many orders of magnitude difference?

KESSLER: Okay. There's two different ways of doing things. I consider this a barrier identification exercise. Is there a potential barrier here? Now, barrier defense in terms of, what, licensing space or whatever, maybe that's more of the one off--the full neutralization of a single barrier at a time that you want to use more of the quantitative information of. So, you know, it's a somewhat different purpose that we're just trying to find out are there some barriers that are buried behind some perhaps bigger barriers here and that we wanted to see whether they existed. We're trying to identify, you know--we're looking for multiple barriers that might be out there and we thought this was one way of potentially identifying them.

PARIZEK: Right, I appreciate that. A correction, now. As far as the flowing interval diagram on Page 23, again, the way the tests are performed here, you're citing all of the yield in that interval to a single fracture when, in fact, you're saying they could be made up of a number of little fractures in the interval.

KESSLER: Right.

PARIZEK: And, if you do that, then you go down and think you're at 25 and say, well, jeez, it doesn't make any difference whether you have alluvium or not; we're going to get a hell of a lot of benefit out of the rocks. And, again, as Bo has said many times, there's billions and billions of fractures and so you could really get lost in terms of where the radionuclides could go and get lost in that rock which is really what is being said here. Right? That you really could get a lot of benefit out of these rocks?

KESSLER: It's conceivable.

PARIZEK: All right. Getting back to the comments earlier about bang for the buck. What additional testing might you do to get some major benefit?

KESSLER: Okay. There were pack in the intervals here that were of a certain distance. It's conceivable maybe in the ATC region or back here where you have fractured tuff that you might want to go for smaller intervals. Somehow, try to assess do I really have all the flow out of a single fracture or, in general, are there groups of fractures contributing? If there were groups, then you could make this; otherwise, you've confirmed that, yeah, you've got the right model, that it's a single fracture, and that you have to go with the way the M&O is going. All I'm suggesting is the amount of effort involved in coming up with that improved understanding of what these flowing intervals look like could have a potentially large benefit to your safety case.

PARIZEK: --in terms of their opinion--

KESSLER: Well, again, it's sort of in the Bill Boyle area which is an expert judgment. Okay? They are interpreting the data differently than the project has interpreted it. The project has chosen to interpret it conservatively for whatever good reasons they may have. All we can tell you is the project says that we four along that interval. We conservatively assume, therefore, it comes out of a single fracture because we don't--we can't tell you for sure it doesn't. Ed's and Frank's expert judgment is we think it will come out of a group of fractures, and when you go through the analysis, here's the potential implications of that different approach.

PARIZEK: I mean, that's such a huge benefit that spending some effort on that sort of test seems highly justified.

KESSLER: That's what we would conclude, too.

NELSON: I, first of all, want to publicly apologize for making catty remarks at the expense of my good, brilliant friend and highly confident Board member, Dan Bullen.

But, really, what I want to ask you, John, is one of the largest hazard reductions is associated with EBS sorption. I mean, that's three orders of magnitude, generally, what you're talking about here. That's roughly the difference between the peak load that you get and the peak load that the project gets.

KESSLER: That's one of the areas where we think it's going to make a difference.

NELSON: Right. So, can you tell me in brief what it is that you're assuming about EBS sorption that the project is not assuming?

KESSLER: When the container corrodes, there's corrosion products. We assume that they're there. We assume that they're likely to be in the way of the flow pathways. We assume that they will, as they can, sorb certain radionuclides. The project is saying, well, we don't really know where they're going to be and we don't really know the form; all legitimate, conservative assumptions. Again, we are applying our expert opinion which is that we think that they're going to be there and we think they're going to stay in the way. We think they can contribute.

In addition, we've also considered the invert. Okay? They haven't taken any credit for the invert. We assume that the invert is there. We assume that there's going to be flow and diffusion through the invert and we assume that some credit can be taken for that. That's what you see.

NELSON: And, you have a more detailed model in the report that may come out sometime?

KESSLER: Yes. I'm sorry, I have no idea why you don't have it. I'm sorry, Priscilla.

NELSON: That's okay.

KESSLER: Yes, there are more details.

COHON: Thank you very much, John.

KESSLER: Thank you.

COHON: We're now going to turn to the public comment period. Two people have signed up; Charles Hilfenhaus and Sally Devlin. Is there anybody else who I've missed? Jerry Szymanski, that's right. I'm sorry, Jerry. Anybody else?

(No audible response.)

COHON: Okay. We will do them in that order. When I call your name, you can talk from that microphone, the one I'm holding if you want to stand up front here and do it, or you can sit down like I'm going to do and do it. It's all up to you.

Charles Hilfenhaus. Please, repronounce your name so it's proper. I'm sorry if I messed it up.

HILFENHAUS: Thank you. There's been quite a lot of very interesting and detailed scientific presentations today. However, I want to comment on the fact that the decision to site at Yucca Mountain was not made on the basis of science and technology. It was made as a result of the Nuclear Waste Policy Act of 1987 and was purely a political decision. Some of us in Nevada still refer to that bill as the Screw Nevada bill. The final decision on siting at Yucca Mountain will probably be made this year and will also be a political decision, not made by anyone in this room, but made by the members of Congress who will be voting upon it.

The level of scientific analysis, such as it was in 1987, proceeded with a logic, more or less, like this. If we can't put nuclear waste on the Nuclear Weapons Test Site, where in the hell on earth can we put it? There is a certain brutal truth behind that because in studying the issue over the years, one of the facts that come to light is the total radionuclide loading of Yucca Mountain is estimated to be somewhere of a nature of 140 million curies. The total radionuclide loading already under the Nevada Test Site in unconstrained caverns as a result of underground nuclear testing is estimated to be of the order of 270 million curies, roughly twice as much. Therefore, for those of you who have been doing analysis of the waste migration modes are really I believe wasting your time because I do not believe that by the time any radionuclides escape from Yucca Mountain they will be detectable within the background of existing radionuclides that will be flowing from Yucca Flats and Pahute Mesa and other connected aquifers.

There's a second question related to the thermal loading that I want to address. It's really obvious that the cause of thermal loading is the continued decay and other radioactive processes going on within the spent fuel. About 25 years ago, I was working at a nuclear power plant when the steam generator cladding required recladding of the tubes in the steam generator because of radiation-induced metal embrittlement. I've not seen much addressed on that particular issue in terms of the modes of the containers. I've heard of chemical corrosion and water effects, but I haven't heard of the addition of radiation effects on the materials on the containers, particularly how that might affect it over extended period of time, since it's obvious we have no data that is really within 50 years old to extrapolate from.

The third question, I guess, the same one that I've tried to get an answer on, since there is radiation that is inducing the thermal loading, is there any thermal neutron component within that radiation, and if so, what effect would that have on the total environment inside of Yucca Mountain?

Thank you.

COHON: Thank you. Is there anybody who cares to respond to any of those questions at this time?

(No audible response.)

COHON: Thank you, Mr. Hilfenhaus. Sally Devlin? Would you like to sit or stand?

DEVLIN: I'll sit next to you every time. Thank you.

COHON: Thank you.

SPEAKER: Even though Abe is here?

DEVLIN: Even though Abe's here, yes, of course. And, Russ is here. Anyway, this is Sally Devlin, the public again, and I don't see any of our officials here to say a sincere welcome and a thank you all for coming. So, I will have that honor. And, again, it's so nice to see everybody. I hope one of these days we'll see you all in Pahrump again and that it won't be another three years. And, I promise not to make cookies, but I really, you know, have to leave you with one of my usuals. I watch a great deal of television. One of the things I've found from NASA is they are giving a $10 million prize to anyone who can create a spaceship that will carry four people in it 100 miles up twice around the earth. Now, I think that's a lovely price and I really think, emulating them, that I would like DOE. And, everybody is supposed to take this back to Washington because I know none of these agencies talk to one another and suggest it to DOE that they give a $25 million prize to anyone who can make all the radionuclides, the waste, all for both repositories and the DOD stuff, go away.

With that, I will leave you laughing. Good night. And, thank you again for coming.

COHON: Thank you, Ms. Devlin. Jerry Szymanski?

SZYMANSKI: I'm Jerry Szymanski. That's S-Z-Y-M-A-N-S-K-I. It was a very informative meeting. I learned two things. The first one, the DOE is--either was or is becoming a learning organization. Well, that's a very (inaudible) development. The second point which I have learned is that DOE performance assessment is conservative. Well, I probably don't have any problems with this performance assessment. It's a very nice piece of work, the program is. We've got the wrong mountain.

It became a tradition for me about January meetings to provide the Board with some material. And, maybe a minute of introduction. Last year, I had provided the Board with a document which I had to read then with the purpose of seeking Board's assistance in making sure that site recommendation report would not go to the President, the Congress, and the Secretary short of having the results (inaudible) project and I think Board was quite instrumental initiating this project. Well, we had the meeting at Carson City and Deputy Attorney General Harry Swenson thought it would be appropriate to ask a question. And, the question was, well, what about if UNLV findings would be such? We were talking about something completely different about the nature of the mountain. I think Dr. Van Luik attempted to answer this question. The answer was very peculiar to me. I couldn't understand it. And, if I can phrase it correctly, there are indications that UNLV project will be inconclusive. Well, I said to myself if it is inconclusive, what do we do with this?

Well, fortunately--and you remember when I decided to speak we had some bunch of arguments which were totally irrelevant and wrong--but, anyhow, we have a first view of the UNLV findings (inaudible) 2000 and they are impressive. The work cannot be questioned. The results meet with the highest standards for science I can imagine. They were derived in adversarial setting to test certain results and the result is startling. That is the probability for occurrence of a hot flooding event has to be somewhere between--now, I'm taking your interview date at the face value--has to be somewhere between 1-3 and 10^{-6}. But, USGS has a remedy which is fixing. I imagine it's a part of learning process. Well, they explained this thing, the mountain was cooling for a long time.

In order for them to proceed with this scenario, they have to assume an unheard yet process whereby the magnetic bodies in a crust which produce ash flows measured in terms of the hundred cubic kilometers cool conductively. Now, this is scientific nonsense. The chances of defending that position in my judgment are zero. So, that takes us to a situation whereby we have a probability on one hand and on the other now we have to deal with the hot water with unknown quantity, unknown volume, to compute what is releases. That, I submit is mission impossible if you want to do this with any degree of precision and reliability. Brought to mind here is that (inaudible) cannot be licensed--we know that now, the facts are there--as a permanent repository.

Whatever we do now with this problem, the facts are it cannot be licensed. In support of this statement, as the tradition dictates, I provided an assessment whereby I hope Board will take a look, and by means of this report, I am seeking Board's assistance. That is make sure that when site recommendation report goes to the President and the Secretary and the Congress, that UNLV data will be there and here will be in that report an analysis of potential regulatory problems. I would imagine the conclusion would be very similar. It cannot be licensed as a permanent repository.

Well, short of that, what will happen? I think the President (inaudible) he will not read this document which is already written. That report does not have the words "operating" there. So, he will sign it. What will follow from that would be national decision which is an interim storage facility at the Nevada Test Site which in my judgment, personal judgment, would be a very logical decision provided that the mountain can be used as a permanent disposal facility. Well, what about if we come to the conclusion they want? There are two choices. Either we will transport it in and out or we transport it in and leave it on the surface. That, I submit, is irresponsible.

With that, thank you very much for giving me an opportunity to speak.

BULLEN: Bullen, Board. A quick question, Dr. Szymanski. I missed the number. The hot flooding--

COHON: Hang on, Dan. Dan, hang on one second.

BULLEN: I'm sorry. Bullen, Board. Dr. Szymanski, I missed the number. The hot flooding probability that you cited from the UNLV work was 1 to 3 times 10^{-6} per year?

SZYMANSKI: That's correct. It's the annual probability. Now, what we think and you will see the reasons for it, scientific reasons, that actual probability is about two orders of magnitudes higher. In other words, we are speaking at once about every 10,000 years. That's our review.

BULLEN: Okay.

SZYMANSKI: It's not necessary to go as far because we can debate this issue and so on and you will see that--

BULLEN: And, that number is in your report?

SZYMANSKI: And, there's a reasoning where that business hinges on (inaudible) how we can find out. However, my analysis of unlicenseability is on a basis of the facts with which I disagree in interpretation of them. And, that probability is enough (inaudible) orders of magnitude greater than the volcanism. And, the consequences are probably infinitely bigger than volcanism.

Thank you.

BULLEN: Thank you.

COHON: Thank you, Dr, Szymanski. Any other comments from the public?

(No audible response.)

COHON: Let me conclude the meeting with the following remarks and they're really just remarks of gratitude for all that participated.

First, to all of our speakers, I think this was a very high quality meeting in terms of the presentations. I want to thank especially those speakers from the DOE and the contractors who responded to the five specific questions that the Board posed in advance. We're well-aware of how much effort went into your preparation to respond to those questions. We found it very valuable and we hope that you did, too. My thanks to all of the other speakers. I think you all did a very, very good job.

My thanks also to those who organized the meeting. To Dan Metlay, our staff member, who is the lead person in pulling together the content of the meeting. To Linda Hiatt and Linda Coultry for their usual wonderful efforts in organizing everything and getting us here and getting our materials here and home, we hope.

Our thanks to the people of Amargosa Valley for their hospitality and thanks to you all for your participation.

We are adjourned.

(Whereupon, the meeting was adjourned.)