UNITED STATES OF AMERICA

NUCLEAR WASTE TECHNICAL REVIEW BOARD

***

COMBINED HG&G/SG&G PANELS:

MEETING ON THERMAL MANAGEMENT

FOR A HIGH-LEVEL REPOSITORY

***

The Dupont Plaza Hotel
Embassy Hall A
1600 New Hampshire Avenue, N.W.
Washington, D.C.

Friday, November 18, 1994

The above-entitled meeting commenced, pursuant to notice, at 8:30 a.m.

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P R O C E E D I N G S

WELCOME/INTRODUCTION/OVERVIEW

DR. LANGMUIR: Please be seated. I'd like to welcome you all to the second day of the HG&G and SG&G, our favorite acronyms, for this Board panel's meeting on thermal management for a high level waste repository.

Yesterday we heard about the DOE's program approach to thermal management strategy. Perhaps the one word that best summarizes this strategy is flexibility. This is good to hear, that this flexibility has been now brought into the program.

We also heard about thermal management options concerning waste acceptance, storage and transportation. Again, the options and flexibility in the system thermal management strategy is well articulated and emphasized.

In the afternoon, we heard about plans to evaluate a couple of processes, and how Yucca Mountain would respond to thermal loads, whether high or low, and the advantages or disadvantages of the various strategies. Perhaps the one single feature that stands out is that the geologic-hydrologic heterogeneity of the mountain is still not fully characterized, or sufficiently characterized, and has, therefore, not been fully reflected in the modeling.

So, thermal testing, thermal loading system and performance studies will be necessary for understanding the merits of the various thermal management strategies. And this is what we're going to hear about this morning.

I thought I'd mention something that came to me yesterday, though, that I thought you might appreciate. I discovered that this whole program is built on the back of envelopes and I -- all the best thoughts and the greatest thoughts people are having are on envelopes, and I felt that we could probably speed this whole program up if we -- and I've set up a company, I need investors for this -- we're going to make envelopes that have two backs to them and provide them at the beginning of each of these meetings in a looseleaf binder. So, I'm looking for investors.

One other trivia. I learned some new acronyms yesterday. I found out that 3-M is not a company in Minnesota. It's a manmade material, and that a "groaner" is a geologic repository operations area near the north ramp.

We'll start out this morning with a presentation by Steve Saterlie. His title is terminal loading systems study update.

Steve.

DR. SATERLIE: Thank you.

Okay. Well, yesterday you heard a little bit about -- you heard several people talk about the fact that we're starting to put together test programs, and one of the things that we're trying to do in this system study is to provide some analysis that would help guide us in those test programs as to what particular parameters, what range of parameters we need to do measurements over.

And, so, with that in mind, that's where this system study, the last number of months and continuing on into this year, has been working.

I'm going to give you a bit of an update of the work that's in progress right now, talk to you a little bit about the study objective, and I'm going to give you some examples. I'm not going to provide you with the entire set of calculations, that would take too long, although there is a report out, an interim report right now, which I don't know if you've got a copy of it -- if you haven't, we can certainly send you a copy.

REVIEW OF ONGOING ANALYSES

THERMAL LOADING SYSTEM STUDY UPDATE

[Slide.]

DR. SATERLIE: Waste stream variability, we'll talk a little bit about that. You heard about a little bit of it from Buz Gibson yesterday, and I'll show you some calculations in the potential repository.

I'll talk to you about effective depth variations, some diffusion, diffusive gas flux calculations, and then summarize.

[Slide.]

DR. SATERLIE: I'm going to skip the next two. You've seen those before. I mainly wanted to identify that what we were concentrating on was the -- doing the analysis to make some recommendations for testing.

[Slide.]

DR. SATERLIE: So, the objectives of the study were originally, when started, were twofold -- provide recommendations to the design of testing to do the sensitivity analysis, look at the range over which parameters may affect waste isolation, and then, if possible, make some recommendations to further narrow the range of thermal loading.

[Slide.]

DR. SATERLIE: Out of a number of things that we've looked at, we've seen some areas where there have been changes as a function of thermal loading that might affect waste isolation. These probably come as no surprise to you; you've heard speculations in the past and things like that. But both permeability and diffusive gas flux, the depth of the repository, and we'll talk more in detail about that, some thermo-mechanical effects, and I won't show you those calculations, but I'll summarize the results.

Waste stream variability, and then I'm going to give you a quick summary of some performance-confirmation monitoring estimates that we did.

[Slide.]

DR. SATERLIE: Okay. One of the things that Buz Gibson talked about was this waste stream variability the other day. And he talked about the fact that the fuel that we've been looking at, both the youngest fuel first and the oldest fuel first. As you recall, the '93 study was based on the youngest fuel first, which was a bit hotter and in that respect we felt somewhat conservative in running the calculations. These are the average values of the age of burnup of those fuels.

The program approach right now and the analysis conducted to date is changing the reference to the oldest fuel first reference. And so we've been shifting the calculations and starting to run some calculations with oldest fuel first.

What you're going to see today and part of the problem was that some of these calculations are very complicated and it's very difficult to make changes and rerun a whole set of calculations, so you're going to see some apples and oranges today. There's going to be some calculations run with the youngest fuel first and there's going to be some calculations run with the oldest fuel first, but we're moving toward making a lot of -- redoing the calculations with oldest fuel first, so bear that in mind.

And this is the average ages in burnups of those fuels.

What I'm going to show you first is some calculations. You saw Buz Gibson's calculations yesterday, just the heat output of the waste packages. What I'm going to try to do is show you some calculations that were done in the potential repository using a heat conduction model, and the actual temperatures that are predicted in the rock and the variability in those temperatures.

And so, this one because of the case that had been run, a very large case, we were only able to strip out certain sets of drifts and replace them with --

What was done, we took three years worth of fuel and we identified one of those years had the highest variability in fuel that was received, the waste stream. And so, we stripped out three drifts in this calculation, reran the calculations, then overlaid them. This was done by Sandia, at our request.

And -- but because we had to do that, we had to use the youngest fuel first and some of those heat outputs I'll show you in a minute, where we approach really high values.

Buz showed you yesterday the oldest fuel first. There is now some limits placed on the canisters and that requires, the case we're looking at -- later on in the calculations, that about 600 of the large capacity waste packages would have to be derated.

[Slide.]

DR. SATERLIE: These look like this. I hope that -- I don't know if you have black and white or color, but --

The green values are the oldest fuel first, and you can see that those are cut off around 14. It's actually a little bit higher because this is the actual selection at the utilities, and the red is the '93, you can see that in some cases you get the fairly high fuel heat outputs.

[Slide.]

DR. SATERLIE: Now, the calculations that were done were done with a conduction model. Let me put two up here.

[Slide.]

DR. SATERLIE: What I've got here is really -- let's see. I think it's best to put them this way.

I don't think that those are going to come out quite as good as I hoped.

These were done at about 78 MTU per acre.

No, that one's not going to come out very good at all.

And I've done -- selected two times here; at 23 years, and at 70 years, and what you've got here is about 12 drifts. There's three years worth of fuel went into about

12 drifts.

Here's the distance scale. And what we found, the yellow -- thank you -- the yellow gives you kind of the average that was calculated. This doesn't mean zero temperature. This was the average that was calculated for that 78 MTU per acre. The temperature calculations were done and pulled off at about five meters into the rock above each drift. And that's where these temperatures are selected.

And what it shows here, though, is that in some places now we have fairly -- this is actually is an increase in temperature, even though it says minus 70 versus a decrease in temperature from the average value that was calculated.

So, what this means, red near a green is that you have roughly about 100 degrees C difference over a distance that's about 20 to 50 some meters.

DR. VERINK: These are in meters?

DR. SATERLIE: These are in meters, yes. Sorry I didn't label that. And those are in meters --

DR. CANTLON: Is that the vertical access?

DR. SATERLIE: This is a plan view, yeah.

No, no, I'm sorry, this is a longer drift.

DR. CANTLON: Plan view.

DR. SATERLIE: Plan view, yes.

And you see that at 70 years this is dropped off quite a bit and averaged out, but there's still some spots that are hotter and cooler than others, and these do last for a sufficient period of time.

Here we're looking at about 50 degrees difference, for the most part, between those same areas. So the point of that is we've got some calculations -- we need to look at this a bit further, but there's a significant amount of variation that can occur.

Now, this is somewhat unfair for two reasons. First of all, as I told you, we selected just the youngest fuel first without any limits placed on it. And this was also randomly selected from those period of years. Hopefully, we would be a little bit smarter at selecting the fuels and placing them -- thermally manage them, as we say. But that's certainly a concern that we have to face and it shows the range of temperatures that we need to be concerned with.

[Slide.]

DR. SATERLIE: Another thing that we looked at was the fact that the repository is not at a constant depth.

[Slide.]

DR. SATERLIE: If we go to the next chart here, this is a side view taken -- this is not the surface of the mountain. What this is is 200 meters below the surface.

It's a contour that was run, that the folks ran to show the limits that they wanted to place on the depth of burial. So the mountain surface actually is up here, but it follows these contours 200 meters higher.

This happens to be one view. This is the primary area and this is another lower block emplacement area that I think you've heard about a number of times. Here is an estimate of the water table. Of course, in a different direction, this can have different distances, as well.

[Slide.]

DR. SATERLIE: What we did is we selected certain points in there and found that the depth of over-burden varies from about 200 meters to about 430 meters over that range. We ran some calculations. Tom Buscheck helped me run those calculations. But what we did was we simulated the -- as if it was all buried at 200 meters and then we simulated as if it was all buried at 430 meters, using the VTOUGH code, axi-symmetric uniform heat distribution.

Along those same lines, the depth of the water table varied from about 110 to 359 meters.

DR. BUSCHECK: It was for YFF.

DR. SATERLIE: It was for YFF.

DR. BUSCHECK: So we could compare it to our past calculations. These calculations were done for YFF so we could compare it to our previous calculations.

DR. SATERLIE: Okay. I'm sorry. So make that change, if you would, please, on there.

[Slide.]

DR. SATERLIE: What I'm going to do is just show you an example of a couple of the cases that were run here. There were a number of cases run. Here are the predictions at fairly high thermal loading, 110 MTU per acre, at 1,000 years. What we wanted to do was see how the temperature and the time history of that temperature and the liquid saturation changed.

So what I've plotted is depth below the ground surface. You see the repository, potential repository drawn in here at 200 meters, and the potential repository here drawn at 430 meters.

What these different calculations are are the -- this is for the center of the repository. This is for part-way out, about -- is that 50 percent out, 75 percent out, and then almost 85 to 90 percent out in the radial ring towards the edge.

You see the -- in this case, I think there are boundary conditions similar to what was used. So we restrict the ground temperature to about -- what was it -- 13 to 20 degrees. I can't remember what it was.

DR. BUSCHECK: It was 13 to 31 at the water table.

DR. SATERLIE: Right, 31 at the water table.

DR. BUSCHECK: Except we adjusted it for the other -- we have a constant geothermal gradient. It's not quite the same at the water table. We have a constant geothermal gradient. So it's slightly different on the left and on the right, but we have a consistent geothermal gradient in those two models.

DR. SATERLIE: You'll see in this case we have high temperatures. Here is the boiling front and it goes considerable distances. What was found was that the time that it stays above a certain temperature increases as you go deeper into the rock, which probably comes as no surprise. It actually increases by a factor of three.

[Slide.]

DR. SATERLIE: Here are the saturation profiles, the liquid saturation profiles. If you recall, about 68 percent is the value that occurs at the repository horizon. Actually, no, that's a little bit different because what was done was the liquid saturation gradient with height was taken and the values at 200 meters were used as the starting point. The value at 430 meters was used. So that differs a little bit from what we originally considered, about 68 percent at about 340 meters.

So that's all laid out in the report. The point is that you see we get these saturation regions building up. Now, as Tom Buscheck said yesterday, we're starting to look at changing these boundary conditions, the relative humidity at the surface, and we need to look further at what the effects of those are going to do for us.

[Slide.]

DR. SATERLIE: We did some calculations with diffusive gas flux. The gas diffusion coefficient is given in terms of the pressure and temperature and a binary water vapor and air diffusion parameter. We looked at a couple of values of beta. One value which can vary somewhat has been estimated for tuff to be about 10-to-the-minus-2. This reflects the tortuosity, the porosity, and the gas saturation of the rock.

However, there is some other literature out there for soils that for porous media, beta, this binary diffusion coefficient may be one, or even a bit higher. So what we did was we selected these two values -- actually, this value changes a little bit as the temperature increases -- selected those and ran calculations.

[Slide.]

DR. SATERLIE: Now, I don't think it's necessary to concentrate too much on the actual values here, the magnitudes that were done, but the important thing to note is that this was done for a low thermal loading case, 24 MTU per acre. What we found here is this is the value taken for the first estimate for tuff the 10-to-the-minus-2. This is the much higher value beta.

What this shows is that we get a significant amount of dry-out into the rock at this higher value of beta. Right now this is the degree of uncertainty that exists in this parameter. So it goes from getting almost no dry-out at these low thermal loads to getting a significant amount of dry-out. That's the kind of parameter and the kind of measurements that we have to make and try to determine how much dry-out is going to exist in those rocks. That will have to be done through measurements.

[Slide.]

DR. SATERLIE: All right. Let me try and summarize the results that we've had. As I indicated, the fuel variability has been examined. We're changing to oldest fuel first. However, using the youngest fuel first and the conditions we did, we find areas that are excursions of plus or minus 50 degrees Centigrade within a 20 to 50 meter space.

I didn't show you any calculations of thermomechanical effects, but we did have a number of thermomechanical calculations that were done. We did it for a couple of thermal loads, 83 and 111 MTU per acre. What we found was there was a stability criteria that we looked at for the rock and at the highest thermal load, we exceeded that stability criteria.

We didn't exceed it at 83, but there was some question of rock movement and it appeared that some tunnel support would be required between these values. We want to have the subsurface people take a longer look at that and tell us, indeed, what kind of tunnel support would be required at each of those thermal loads. Of course, then we'd factor that into a cost ultimately.

In the monitoring issue, there has been a question of performance confirmation, what kind of instrumentation are we going to be using or need to use in the drift. What kind of measurements are we going to be able to make? Are we going to have to put these instruments in the drift and leave them there and what does that entail as far as the type of environment it would be subjected to, or can we use a robot to send these instruments in there and make occasional measurements?

So we went through a fairly extensive examination of that and determined, at least on a first order sense, what measurements we felt needed to be made in situ in the drift over a long period of time and what others could be made on a routine basis that you could send the instruments in.

I don't know if you recall -- from the '93 study, we had made a very, as they say, a back-of-the-envelope calculation of what we felt the temperature limit that we could put instrumentation at, and that was about 160 degrees Centigrade. Well, a lot of others said, "Gee, you can use robotic equipment, you could put the instruments off." So in doing that, in looking at that, what we found, though, was for those few measurements that did have to be in situ, that at this point in time, the best we could do was, we felt -- and we talked to a lot of instrumentation companies about this -- that 200 degrees C might be possible.

I say might because these instruments are currently not off-the-shelf, but they felt that there were some changes that they could do to do their instruments that would put them at 200 degrees C.

So right now that leaves us with a range from, I'd say, between 160 and 200 degrees C that may be possible. The point of it is it's a study that now can be taken by some of the subsurface people and some of the instrumentation people and can be used as a starting point to go further.

[Slide.]

DR. SATERLIE: Bulk permeabilities, we've talked a little bit about that in the past and I didn't show you any data. The current range of uncertainty in those parameters for the Topapah Springs, the TSw2 level, is about .1 to 10 Darcy, about two orders of magnitude. At the 1 Darcy or above level, you get a significant increase in the gas phase convection.

Diffusive gas flux sensitivities depend on the connectivity of the pores and fractures, and I showed you some examples of that. The current range of uncertainty is about two orders of magnitude and that can make a big difference in how much drying you actually get in the rock.

I showed you some examples of repository depth sensitivity. The repository varies from about 200 to 430 meters in depth from the surface. The liquid build-up and temperatures were found to depend on this. For example, going from 200 meters depth to 430 meters depth essentially triples the time that a repository stays above a given point. For example, if you wanted to take it at 50 degrees C or 80 degrees C, it would triple the time that it would stay there at the deeper depth.

[Slide.]

DR. SATERLIE: All right. We've got a number of further parametric studies planned or underway. We want to complete the waste stream variability and we want to look at the oldest fuel first now, which I think will be a lot more moderate, do some drift-scale hydrothermal calculations, and we want to couple these with ventilation calculations. We want to really investigate some of these thermal management issues that have been talked about in the last day.

We've got some work in progress right now to look at the spatial variations in thermal conductivity in the rock and what that effect has. We've got some further hydrothermal calculations we want to look at.

[Slide.]

DR. SATERLIE: On the thermomechanical, as I said, some of the things we want to have looked at are possibly have subsurface give us some input about what types of tunnel support might be needed. Those kind of interactions need to go on so that they can be looking at their designs, as well. We need to look further at dual porosity effects, the fracture-pore interaction and how much of the flow really is in the fractures, and then eventually develop recommendations for testing by the end of the year.

I don't know how successful we'll be with some of those. Some of those calculations are very difficult.

[Slide.]

DR. SATERLIE: Essentially, where we are is we've done the scoping calculations, as I told you yesterday. You saw this chart yesterday. Where we're at right now is to try to provide some recommendations for the test program so that we can better define those test programs. Then once the data comes in, we will start making recommendations as to the final thermal loading.

Thank you very much.

DR. LANGMUIR: Thank you, Steve. We are a little behind schedule. So let's hold questions and we will have an opportunity, perhaps before lunch and certainly at the roundtable, for questions for Steve Saterlie.

Our next presentation is total systems performance assessment, an update. Bob Andrews was sick with food poisoning, I'm told. So Srikanta Mishra of INTERA will be giving the presentation for him.

TOTAL SYSTEMS PERFORMANCE ASSESSMENT UPDATES

[Slide.]

DR. MISHRA: Good morning. I am giving Bob's presentation of his viewgraphs. So occasionally I hope I will be allowed to trip and miss a few subtle or maybe not so subtle points that he wanted to make.

What I want to do in this presentation is to talk a little bit about the updates to the total system performance assessment calculations that the M&O's Performance Assessment Department has carried out. In January of this year, the Board heard detailed presentations about the TSPAs carried both by the M&O and the Sandia National Labs and since then we have added some other calculations.

The intent is to provide a performance assessment perspective through the discussion going on over the last day and today on the general issue of thermal management.

[Slide.]

DR. MISHRA: I will begin by reviewing some of the key elements of the isolation arguments and the potential effects of alternate thermal loads on these and summarize the fundamental thermo-hydrologic assumptions affecting the TSPA-1993 results. Basically, the intent of these two bullets is to talk a little bit about how thermal loading affects the general issue of performance, whether it be with a generic discussion on isolation argument, or whether it's the specific discussion on performance measures as embedded in the total system performance assessment calculations.

Then I will move on to talk a little bit about the sensitivity, the original sensitivity calculations. Moving on to the additional sensitivity analysis, the total system performance assessment results, and also discuss some extra calculations that we did using drift-scale thermo-hydrologic analysis. Both of these, again, answer the question as to what is the sensitivity of performance to thermal loading in a specific sense. Then we use these results to gain some insights and present some preliminary plans for the TSPA in 1995.

[Slide.]

DR. MISHRA: This is a modified form of the key components of the waste isolation argument that was presented to the Board by Jean Younker last month. Essentially, the argument is that we anticipate a dry environment within the engineered barriers. We expect to have robust engineered materials for those packages that are expected to be contacted by liquid water and/or humid air. We anticipate slow dissolution of the waste matrix due to limited availability of water and low solubility of radionuclides.

We also anticipate slow release of radionuclides through the engineered barrier, once again due go the limited availability of water and because of the low permeability of the matrix in the geosphere, a slow release of radionuclides and migration to the accessible environment.

[Slide.]

DR. MISHRA: So now let's talk a little bit about which of these components of the waste isolation arguments are impacted by thermal loading. Essentially, the impact comes because the emplacement of heat-generating waste in the repository impacts the environment in the vicinity of the waste packages in the drift. So the thermo-hydrologic regime within the drifts is a very important aspect in the performance of the waste packages and ultimately of the engineered barrier system.

The degradation of the waste packages, in that the initiation of the aqueous corrosion and its rate, will be impacted by the thermo-hydrologic regime. The rate of waste form dissolution will also be impacted by the thermo-hydrologic regime and the release and the migration of radionuclides in terms of radionuclide solubilities in liquid water and the advective and diffusive fluxes are also impacted by the thermo-hydrologic regime.

And by the thermo-hydrologic regime, I basically refer to the temperature conditions, the humidity conditions and the saturation conditions as they exist in the vicinity of the waste packages and in the drift.

[Slide.]

DR. MISHRA: Let me digress here a little bit and put up this chart, which is not in your handouts. But I thought it might be interesting to talk a little bit here about -- to go back one step and maybe talk about the overall scope of performance assessment. This is a bubble diagram that you have all seen before in a variety of forms. The only difference here is that I have tried to highlight in red those bubbles or those components of the waste disposal system that essentially are impacted by the thermal loading process. What is shown in these different colored text is that where the appropriate regulations start to kick in.

For example, waste package degradation would lead to an evaluation of substantially complete containment, in combination with waste form alteration, waste package release and engineered barrier release and gives rise to the release from the engineered barrier system. So this is the other regulatory requirement -- gaseous and aqueous release to the accessible environment, those come in here and here, and finally we get to the dose standards.

As Scott Sinnock tried to point out, in any evaluation of the waste disposal system, it is important to have in mind what is the appropriate performance measure. I would like to submit that from the perspective of the total system performance assessment, these are the performance measures because these are directly tied to the regulatory requirements. In our analysis, we will always be focusing on how the system behaves with respect to these performance measures.

[Slide.]

DR. MISHRA: So having identified that the thermo-hydrologic regime does, in fact, or is likely to, impact the performance of the waste package system and ultimately the engineered barrier system. Let's talk a little bit about how these have been implemented in the total system performance assessment process.

In the first iteration of total system performance assessment done for Yucca Mountain, which was TSPA-1991, the internal pulse was implemented in a very simplistic way. We just assumed that for the first 300 years, I think it was

300 years, that it -- basically, what it did was it caused a delay in the contact of water with the waste packages. What we tried to do in TSPA-1993 is to modify it in a way that we could identify the exact thermal dependencies and perhaps begin to implement these thermal dependencies, albeit in a simplistic format. So this is essentially a recalculation of what was done.

We started by developing a panel-scale thermo-hydrological model to predict the temperatures and saturations in the linear field. But, of course, there's a disconnect there in that we're trying to match information obtained from one scale and apply it at a different scale. What that leads to is it leads to an under-prediction in the expected waste package temperatures and as a consequence, an over-prediction would be expected in drift scale saturations.

I will talk about how we have improved upon this representation. This is what was done in the original version of TSPA-1993. Once we had the temperatures and the saturations, then we used either a temperature criterion or a liquid saturation criterion to initiate corrosion. These criteria are as follows. We assumed that aqueous corrosion is initiated either when the temperature of the waste packages drops below the boiling point of water, which is 96 degrees C, or when the saturation of the environment right next to the waste packages is above the residual saturation of that medium, the assumption there being that once the saturation goes above the residual saturation, you have a continuous liquid fill which is allowed to contact the waste packages and, hence, initiate aqueous corrosion.

Once again, what this does is, particularly for the liquid saturation criterion, it provides very early initiation of aqueous corrosion, and that's a very conservative assumption. The corrosion rates were also considered to be temperature-dependent. What this does is once you start to initiate the corrosion process based on the saturation criterion at a very early time, you get very high corrosion rates, and I'm saying that's a very conservative assumption.

[Slide.]

DR. MISHRA: In terms of the corrosion rate for mild steel, we looked at two different models. One was developed by the M&O, David Stahl, and one came from Lawrence Livermore, Allen Lamont. Both of these had different functional forms, but both of these did lead to very high corrosion rates for the assumed saturation and corrosion initiation criteria.

[Slide.]

DR. MISHRA: For the corrosion of the corrosion-resistant material, whether we use the deterministic model or whether we use the stochastic model, it also led to the same thing. The other conservative assumption was that the degradation of cladding was assumed to be congruent with the corrosion-resistant material.

So essentially what we had in terms of the degradation of the waste packages, we had several thermal dependencies, but these were, once again, a very preliminary attempt at implementing the thermal dependencies, most of which turned out to be quite conservative.

[Slide.]

DR. MISHRA: Moving on to the waste package EBS release, I talked about waste package and cladding failure distribution. The next assumption was that the entire waste package surface was assumed to be degraded at the time of the failure. The failure is conservatively defined by the penetration, by the time at which the first pit penetration takes place. We also assumed that the entire waste form surface could be exposed and contacted by liquid water at the time of the failure.

What this does is, once again, it just enhances the failure process or accelerates the failure process and the release process.

[Slide.]

DR. MISHRA: Some more assumptions that we used in the TSPA-1993 implementation, if you will, relate to the dissolution rates and the solubility limits for the radionuclides in terms of their temperature dependencies and how we calculated the diffusion coefficient to the waste package and the backfill based on the average saturations that we obtained from the scale model. Once again, there is a disconnect here in terms of getting information from one scale and assuming that it holds at another scale. So what we're doing is we're neglecting the capillary differences between the rock and the backfill material here.

So that is essentially a recalculation of what assumptions and implementations of thermal dependencies we had in the TSPA-1993.

[Slide.]

DR. MISHRA: Moving on to additional calculations that we have done based on the comments that we received or based on things that we identified we should have done, but we didn't do. This is just a list. This is all in your package. I'm not going to go into any of the details. I'm basically going to show some results that brings in the TSPA-1993 results and also some of these additional calculations.

[Slide.]

DR. MISHRA: So as an alternative to the CCDFs that a lot of people love to hate, here is a barograph that shows the waste package failure distributions as a function of the thermal load, the waste package thickness, and the corrosion initiation criteria for a variety of thermal load conditions here, going from the 28.5 kilowatt per acre case to the 114 kilowatt per acre case. And notice that we have added a new case here, the 87 kilowatt per acre case, in addition to the three that were evaluated as part of TSPA-1993.

Two waste package outer barrier thicknesses, ten centimeters and 20 centimeters, and, once again, we have the two corrosion initiation criteria and the saturation criteria. Saturation going above residual saturation implies the initiation of aqueous corrosion as opposed to temperature in that when the temperature falls below the boiling point of water, you start to have aqueous corrosion.

These opened and closed triangles and the lines joining them basically delineate the duration of the waste package failure, when waste package failure begins and when it ends. As you can see, for the 28.5 case, it basically made no difference, where it made some difference in the 57 through the 114 case.

The temperature criterion was always a better performer than the saturation criterion because the saturation criterion was a more conservative one.

As you can see here, in terms of the temperature criterion, the 114 kilowatt per acre case performed almost as well as the 28.5 case and the 87 kilowatt per acre case and the 57 kilowatt per acre case are actually the worst performers.

[Slide.]

DR. MISHRA: This table presents some expected values of the releases as normalized to Table 1 of 40 CFR 191 at 10,000 and at 100,000 years. I don't want to go into the details. The only thing that I want to point out is that you can see some difference going from thermal load to thermal load and from case to case in the 10,000 year time period. But, essentially, if you are comparing performance to a 100,000 year standard, then essentially there's no difference because the waste packages that are being currently designed are not expected to contribute significantly if the standard is the 100,000 year duration.

[Slide.]

DR. MISHRA: So to summarize what we have learned so far, basically we've learned that the waste package is dependent on the model for corrosion initiation and, in particular, we find that the saturation dependence criterion that we used causes the early failures.

We have also looked at the spatial variability in the overall thermo-hydrologic performance by looking at the response of the center of a waste emplacement panel as opposed to the edge of the panel. We do find that there are some effects in terms of corrosion initiation and the rates and that affects the distribution of failures. The corrosion model also affects the distribution of failures would not dry significantly and extend.

Diffusive releases from the waste package and the EBS generally dominate the advective releases. And this perhaps is an important conclusion, in line with what Scott Sinnock was trying to point out yesterday, that no significant waste package cumulative release differences occur during the 100,000 years. There are some differences in the 10,000 year time period, but essentially performance is not a discriminator between the various thermal loading options, at least based on the preliminary models that we have so far.

[Slide.]

DR. MISHRA: As I pointed out in the beginning, what we did was we used a panel scaling model to predict the thermo-hydrologic environment in the drift and that was -- that led to the under-prediction of waste package surface temperatures and an over-prediction of the saturation. In order to improve upon that work we have done, we have performed some drift scale thermo-hydrologic calculations and have carried out some corresponding assessments of the total system performance.

So what we do is that directly at the scale for drift, we have a waste package emplaced in the drift, which is essentially a two-dimensional model going from the ground surface to a depth of 1,000 meters below the water table, and it has some fine distribution in the vicinity of the drift and the waste packages to account for processes that might be going on there.

So we can directly calculate the temperature, the liquid saturation, the flux and the relative humidity in the vicinity of the waste package. This was essentially done for the systems study group. So we looked at two different thermal loads, 25 and 87 kilowatts per acre. We looked at two different backfill alternatives. We either had no backfill or we had a gravel backfill. We looked at two different waste package emplacement configurations, a square spacing and a rectangular spacing, and three corrosion allowance thicknesses, the ten, 20 and 45 centimeter ones.

The corrosion initiation criterion that we used this time around had a new switch. Corrosion was assumed to initiate either when the relative humidity in the vicinity of the waste packages was about 70 percent or when the temperature fell below 96 degrees. So we dropped the saturation criterion. The saturation criterion was used to look at the release from the waste packages in that the water was allowed to be mobile when the saturation exceeded the residual saturation of water in the backfill.

So in terms of mobility, because it was believed that it would be appropriate to use the saturation criterion, but not in terms of corrosion initiation, we went back to the temperature and introduced a new one, which is relative humidity.

For all of these designs, we looked at the sensitivity to the release from the waste package to the accessible environment and also to the doses.

[Slide.]

DR. MISHRA: Some more details about what this drift scale thermo-hydrologic model approach and assumptions were. As I said, it's a 2-D vertical section. It goes from the surface to 1,000 meters below the water table. The refined mesh in the vicinity of the drift, we did this calculation using two different codes, the code TOUGH-2 developed at Lawrence Livermore labs and the finite element called FEHM developed at Los Alamos, just so we would get some idea as to whether the calculations that we were performing produced robust results or not.

We calculated temperatures, saturations and fluxes directly from the model results. Humidity was done as a function of temperature and capillary pressure using the Kelvin relationship. We looked at sensitivity to the flux going from zero to .2 millimeters per year. What we did not do was we did not vary the stratigraphy, as Stahl did his

his calculations. We did not look at the uncertainty to rock properties, even though we know that the hydrologic properties at Yucca Mountain are significantly variable along and across bore holes.

We did not look at variations of backfill properties. We just assumed that the backfill to be emplaced had a set of properties. Corrosion initiation, as I said before, was either based on a humidity criterion or a temperature criterion.

[Slide.]

DR. MISHRA: Some more details about the implementation of the thermal dependencies. In addition to the humidity switch, we did not change the corrosion models. So this is the same model that David Stahl provided us to use in TSPA-1993. It just has the corrosion rate dependent on temperature and on time. We also assumed that the cladding -- we took no credit for the cladding and assumed that it failed when the corrosion allowance barrier -- well, the corrosion -- basically, we're not taking any credit for cladding.

We also assumed that the entire waste form surface was exposed to water instantaneously, but we would evaluate some sensitivity to the working criterion. Dissolution rates and solubilities are the same as in THPA-1993. Diffusion coefficients, we used this particular rate.

[Slide.]

DR. MISHRA: Here are some results of some representative drift scale thermo-hydrologic calculations, waste package failure distributions and cumulative releases.

[Slide.]

DR. MISHRA: I would like to show one graph to indicate what is happening here. This is the case of the 87 kilowatt per acre case. Initially, you see a temperature increase and the temperature dropping, and this is the time, 100 years, at which the backfill is in place. Actually, the temperature increases and then it keeps coming down. So the humidity drops and increases once again. That's not correctly represented here. There should be one or two additional points up there.

The solid squares show the temperature response as a function of time. The liquid saturation and the backfill is essentially constant at some near residual value, which is the straight line shown here at the very bottom. The stars show the relative humidity behavior. This is just to show what our typical thermo-hydrologic drift scale response is.

[Slide.]

DR. MISHRA: I will go back to my barographs and here, once again, I am trying to summarize the waste package failure distributions using the drift scale thermo-hydrology calculations, showing sensitivity to thermal load, to backfill, to waste package emplacement geometry and to the corrosion initiation criteria for the ten centimeter outer barrier case and for the .1 millimeter percolation flux case. What we see here is that using the relative humidity as a switch, but not using corrosion rates dependent on relative humidity causes the system to fail much earlier, as you can see here, particularly for the high thermal load case.

For the low thermal load case, the relative humidity corrosion initiation criterion leads to better performance.

[Slide.]

DR. MISHRA: For the 20 centimeter outer barrier case, we, once again, get a similar kind of behavior. Notice that the waste package failures are, indeed, quite spread in time if we look at the temperature criterion. But for the relative humidity criterion, particularly for the higher thermal load case, we get very early failures. I will come back to that point in a little bit.

[Slide.]

DR. MISHRA: This is a table that tries to summarize the expected values of the neptunium and technetium releases as normalized to Table 1. Here you see that in the 10,000 year -- these are releases from the waste packages. During the 10,000-year period, there are basically very small releases. During the 100,000-year period, you have higher releases and depending upon the saturation initiation criterion, you get more release from the 87 kilowatt per acre case or from the 25 kilowatt per acre case.

[Slide.]

DR. MISHRA: As I'm nearing the end of my presentation, I thought I would just put up a couple of CCDFs. This is a CCDF of the total normalized waste package release for the 10,000-year period. Once again, there is not much of a difference between the various cases. This is just to show that depending upon the emplacement geometry and the release initiation criteria, you might have some difference in the CCDFs.

[Slide.]

DR. MISHRA: There's a mistake in the labeling here. This should be the 100,000-year release. Basically, you see that all of the CCDFs have been shifted to the right, as you expect. Once again, there is not much of a difference between the 25 kilowatt per acre case or the 87 kilowatt per acre case.

[Slide.]

D3R. MISHRA: To summarize the TSPA results based on a drift scale thermo-hydrologic model and its implementation, what we have seen is that the humidity initiated corrosion can lead to earlier failures at higher thermal loads. This occurs primarily because we are not accounting for humidity effects on the corrosion rates. We still use the corrosion model which predicts that the rate of corrosion is essentially dependent on temperature and to some extent on time and we just used humidity as a switch to initiate the corrosion process.

When we have a backfill, we generally get higher failure times, meaning that the failure is -- the onset of failure is delayed. We had the same situation when we used rectangular spacing for the waste package emplacement as opposed to square spacing. As expected, the thicker outer barrier gives rise to a delay in the expected failures, even though we have a conservative treatment of cladding.

Once again, one of our key messages here is that we do have differences -- we do have some differences in the waste package and the accessible environment release, but these are -- first of all, these are not significantly different and, secondly, the significance of this difference is strongly affected by the assumptions that we have in the waste package in the EBS model.

[Slide.]

DR. MISHRA: So having said that, work on -- or our plans for the upcoming iteration of TSPA, which is TSPA-

1995, is expected to be complete by the end of FY-95. We want to continue using our drift scale thermo-hydrologic model with alternate backfill characteristics, with alternate thermal loads, look at the effect of hydrogen-80 uncertainty in the hydraulic properties, continue to predict humidity and use humidity as a criterion for the initiation of corrosion.

Indirect prediction of spatial variability in the sense that we know there is going to be some difference between the effect of -- between the responses of the packages which are placed in the center of the repository as opposed to those packages that are placed at the edges of the repository. We have some ideas as to how we might capture that difference using some scaling rules.

We want to use a revised model for the initiation of aqueous corrosion and particularly one of the models that we want to evaluate is the new version of the corrosion model that's been proposed by David Stahl, which has a relative humidity dependence in addition to the temperature and time dependence.

In order to improve the representation of the waste form alteration and the release from the EBS, we want to use the drift scale liquid saturations as a means of predicting how the waste form surface is exposed to water gradually in time and, also, what percentage of the waste packages in the EBS have access to diffusive pathways.

Skip the next couple of slides which talk about issues to be addressed in TSPA-1995 because they recur in this slide here where I talk about key information needs for TSPA-1995.

[Slide.]

DR. MISHRA: We are looking for more information in the area of unsaturated zone hydrology so we have a better understanding of what is the infiltration and percolation rate in the vicinity of the repository and what about the variability and uncertainty.

Till now, all of the analyses that we have done, whether at the drift scale or whether at the far field scale, have basically assumed that the fracture matrix system at Yucca Mountain can be treated as a equivalent continuum and as a means of evaluating its robustness, we want to look at the issue of fracture matrix coupling, particularly as it affects the return flow of condensate from high thermal load cases and also in terms of matrix diffusion and the process of radionuclide transport in the geosphere after the radionuclides are released from the EBS.

The issue of bulk rock characteristic curves in the Topapah Springs 2 horizon and also in the other alternatives, the uncertainties are important because they do affect what temperature and saturation conditions prevail in the rock units which overlie the repository horizon, which overlie and underlie the repository horizon and, hence, affect the thermo-hydrologic conditions in the drift.

In terms of the waste package engineered barrier system, information needs are backfill and invert thermo-hydrologic properties, the criterion for corrosion initiation, and what is the uncertainty in the corrosion models and parameters for the corrosion allowance, for the corrosion resistance, for the cladding.

Effective diffusion coefficients and low liquid saturations from materials which are representative of the backfill, of the invert that is used within the drift, and the spent fuel dissolution model for the expected thermo-hydrologic conditions.

[Slide.]

DR. MISHRA: So in conclusion, we have performed several sensitivity analyses to supplement the original total system performance assessment for 1993. It basically confirms the conclusions that we made in that for the simplified assumptions that we have right now, performance is not a strong discriminator between various thermal loading options at long times. If you're looking at a 100,000-year release or if you're looking at peak doses over the million year period, you might see some difference in the performance over the 10,000-year period, but, once again, those differences are strongly dependent on the assumptions that we have with respect to waste package degradation and the EBS release.

And the importance of these has also been pointed out by the preliminary drift scale thermo-hydrologic analysis that we did to improve the representation of the near field thermo-hydrology in these calculations that I just talked about. We are using it to provide a framework for our TSPA-1995 analysis and focusing on the key components of the waste isolation argument.

With that, let me conclude this presentation. If you have any questions, I will try to answer them as much as I can. Thank you.

DR. LANGMUIR: Thank you, Dr. Mishra. I'll start it out. We have some time for questions. I'm a little unhappy in the sense that my feeling is to get something licensed, you have to be able to explain it in a way that is at least somewhat intuitively logical or what you'd expect to happen might happen. I don't see that here.

I was very surprised that the performances don't seem to be sensitive to thermal load. That isn't intuitively obvious why that would be so. I would expect that -- and I didn't see refluxion, by the way. Another issue that comes up here is you emphasize, and I think my sense is rightly, that there's a lot of assumptions involved in these models. How do those assumptions impact your conclusions? How uncertain are the conclusions that result?

For example, I didn't hear the mention of refluxion as a process that was incorporated in your corrosion calculations or predictions.

Again, as I said before, I'm very surprised that we don't see any notable differences in corrosion or failure, regardless of load, because that, to me, is not intuitively obvious that that would be the case. Can you comment on that?

[Slide.]

DR. MISHRA: That's a tough one for me. I think it is probably intuitive that when you look at performance over very long time periods, if you're looking at million-year doses, for example, if you're looking at 100,000-year release, for example, the various corrosion initiation models will not make a significant difference, because the waste packages are not intended -- at least that's my understanding -- are not intended to provide protection over that period of time.

When you look at the 10,000-year performance, however, and this is what I have tried to point out, you see some difference. Whether it says that the high thermal load is better or the low thermal load is better, I think is much less important than the fact that the assumptions that go into the model are still very, very, very uncertain right now.

For example, what confidence do we have in the corrosion model? What confidence do we have in predictions of temperatures and saturations and humidities in the vicinity of the waste packages. As Tom has pointed out, we're in the process of evolving towards a much more robust understanding of the thermo-hydrologic environment in the presence of the drift, in the vicinity of the drift. As David Stahl and other scientists at Lawrence Livermore have been working, they have been trying to come up with a corrosion model that takes into account all of the key environmental variables in the vicinity of the drift.

So I think those uncertainties are embedded in these analyses in a very preliminary way. Right now we cannot discriminate between the performance of various options, at least not in a very definitive sense. That's my feeling.

I think in the 10,000-year time period, all of our conclusions are more suggestive than definitive and I would say that over longer periods of time, maybe the reverse is the case.

DR. LANGMUIR: Thank you.

DR. PALCIAUSKAS: Could you put up slide number 20?

[Slide.]

DR. PALCIAUSKAS: I think this slide illustrates quite well at least my confusion or uncertainty. I notice that the biggest difference in this slide, and what stands out clearly, is your assumptions concerning the corrosion initiation criteria. In other words, that seems to be the most important factor determining the outcomes of the computations. Simply said, it's going to be very difficult to draw a conclusion concerning performance unless we know something about the corrosion rates.

DR. MISHRA: Right. And as I pointed out, the corrosion initiation criterion we had here, and as David Stahl would likely say, is very simplistic in that we use humidity just as a switch and we do not have a humidity dependence on the corrosion rates. It's a model which is still being evolved, so to speak, within our waste package group and we hope to use a model like that in the TSPA-1995 so that we have a better handle on how corrosion is initiated and at what rate it proceeds.

DR. PALCIAUSKAS: So in other words, we really can't draw any conclusions concerning performance until we have something firmer in the way of corrosion models.

DR. MISHRA: In a sense, that's very true.

DR. STAHL: Dave Stahl, M&O, B&W Fuel Company. In the last few months, we have made some corrections to the relative humidity switch. Unfortunately, Bob Andrews has not been able to input into this calculation and certainly that would make dramatic differences in the early time failures that are shown for the high temperature-high thermal load case. So that's one thing that has to be done.

In addition, as he has noted, Srikanta has noted, there is a great deal of conservatism in the failure of the corrosion-resistant barrier and of the zircalloy cladding. Hopefully, with a little more assistance here, we'll be able to provide some more input into that in the next six months to a year or so.

Also, I know that Lawrence Livermore will be doing some analysis in regard to the subsystem performance, of whether we meet the subsystem requirements. Certainly, as indicated here, we may not using those assumptions. We need to bring a little bit more reality into the analysis.

One thing on a negative note, however, we do not have any factor in there for microbiological corrosion. That is something that we'll be starting to evaluate this year and certainly that could perhaps give us some early failures certainly for the lower thermal loads that we hadn't included.

DR. LANGMUIR: What about refluxing? Is this part of your analysis?

DR. STAHL: I'm sorry.

DR. LANGMUIR: Refluxing. The idea that you're going to have large volumes of water that are just going round and round perhaps at intermediate temperatures.

DR. STAHL: Yes. Well, the analysis assumes that you do have corrosion when you have a water film on the surface. Absence of the water film, you don't have corrosion. So you have to look at what the surface character is, as we discussed yesterday, depending on what conditions will lead to the formation of the film and sustain that film.

DR. MISHRA: If I might address the question of refluxing. The way to incorporate refluxing is to have a thermo-hydrologic model that couples the near field with the far field. So when you predict the temperatures and the saturations and the fluxes in the vicinity of the waste packages, you have implicitly taken into account the fact that you might be having convective cells in the system because of the emplacement of heat-generating wastes.

What we have here is a 2D slice that goes through the repository, goes from the surface to the water table and a 1,000 meters below it and we have an enhanced resolution of the region around the drift.

So what this does is in a way, when we predict the temperatures and the saturation conditions in the vicinity of the waste packages within the drift, we take into account the fact that the type of thermal effects that are taking place in the region above the repository and below the repository. There is dry-out above the repository and there is also re-wetting above the repository.

So in a way, the issue of refluxing and what fluxes might engender are already implicit in this, with one caveat. This is still an equivalent engineering model that still does not address the question of fracture matrix which might be activated under those conditions. I think that's something for which we look to Tom and perhaps to Karsten to give us some more insights.

In a way, we are at the downstream end of information such as material degradation and thermo-hydrologic behavior. Because we have limited resources, we sort of have to take that information as much as we can and try to input it into whatever simplistic models that we have.

[Slide.]

DR. MISHRA: As I showed you here in my bubble diagram, in a way, what we're doing is we have a very large envelope and we just take each little box and put inputs and get outputs and try to sum them up and that's what results in the CCDFs.

DR. LANGMUIR: Any more questions from the Board? Pat Domenico.

DR. DOMENICO: Do the releases start once failure is initiated and progressively increase as canister failure progresses or is there some diffusion or rate control limitation on the releases once they're exposed, once the material is exposed?

DR. MISHRA: There is a diffusion mechanism. Once the canister fails, then it's a question of water coming in contact with the waste form and then it's either solubility, limit controlled, or alteration rate controlled. So there is some diffusion modeling that goes on with respect to the waste form.

DR. DOMENICO: So the release may not increase progressively with canister failure. It may not.

DR. MISHRA: Right. It may not.

DR. DOMENICO: So it's the initiation that starts things.

DR. MISHRA: It's the initiation that is important.

DR. DOMENICO: So the time it completes failure doesn't enter too much into the actual releases.

DR. MISHRA: It does affect it to some extent and I think it's some non-linear coupling. I don't think I can answer the question very well.

DR. LANGMUIR: Dennis Price.

DR. PRICE: Is it your assumption that -- the couple to thermal is nuclear radiation and irradiation, the intensity of it? Is it your assumption that irradiation does not cause any chemistry changes along the path to release or have you looked at the effects of a long-term intense irradiation to the chemistries between the source and the release?

DR. MISHRA: No. I don't think it's part of our model. It is not.

DR. PRICE: Is it of interest or should it be?

DR. MISHRA: I can't answer that question. This is -- I should defer that question to Bob, but I'll take note of it.

DR. LANGMUIR: Question from Dan Bullen. Go ahead, Dave.

DR. STAHL: David Stahl, M&O. In regard to the radiolytic effects, certainly for the corrosion allowance material, the barrier is thick enough to prevent that from happening. But if you do have failure of the corrosion allowance barrier and then create water films, for example, on the corrosion-resistant barriers, then radiolosys can decrease the pH and perhaps give you earlier failures.

That's something we need to look at and have not done yet.

DR. PRICE: And that is with respect to the package itself. How about the geochemistry?

DR. STAHL: They've done a little bit of examination of that issue. Ray Stout is one that's been looking at that. Perhaps Bill Halsey can comment on that part of it.

DR. HALSEY: Bill Halsey, Lawrence Livermore. To answer your question, Dennis, yes, it is of interest. We're interested in whether some of the surface processes can be affected by radiation effects. We're looking at some of those in transport processes, along with the geochemistry.

Conclusions on that are somewhat down the line. We aren't going to have those available to put into these kinds of models. The maturity of the models that are used for transport in the TSPA at this time couldn't use those results if we had them. The complexity is not consistent with the transport models we're using currently. I think that's someplace that we hope to be in the future.

I wonder here if I could comment on the corrosion issue, also, that Don brought up. You have to step back a little bit and realize that this TSPA was really the first one that tried to include thermally-dependent effects. The waste packages until this analysis were just given an arbitrary failure distribution with real mechanistic processes included.

What we put in in this round is just the very first and simplest portions of that. Srikanta went through a lot of the things that were not included, a lot of the assumptions that were made. When you add all of those up in some of the most extreme cases, you had a relative humidity or a local saturation switch in the rock. When the local saturation of the rock reaches a certain point five meters out into the rock, when it reached, I believe, 7 percent saturation five meters into the rock, using some of the corrosion models that were used, you then had corrosion rates that were conservatively estimated for samples in a brine pressure cooker at above-boiling temperatures in aggressive conditions.

This is not just conservative. This is fairly well decoupled from realism. Now, if we had all of the information necessary to make those connections, once again, the models do not yet have the complexity to deal with all of those processes. But we have to start somewhere and putting in some of these temperature-dependent processes lead us to the sensitivity analyses that Srikanta was showing to tell us where we need better information, and we're hoping to get that in the future.

That, once again, gets to the conclusion you made that we can't really draw thermal loading conclusions from these, but we can start to get thermal-dependent sensitivity indicators to tell us where to go next.

DR. MISHRA: I would just add one thing to that.

I think all of what Bill said is very, very true with respect to the early time period, when we're looking at 10,000-year standards. But if we're looking at standards -- and as we all know, we have no standards right now. We have some standards and the key standards with respect to release and perhaps with respect to dose are not yet known.

Should they turn out to be standards with respect to a longer time period, I think some of the conclusions that we have might well be true, because the current design sort of expects a 10,000-year standard. All of our design is based on a 10,000-standard.

DR. LANGMUIR: I think we have to go on. Thank you very much.

DR. BULLEN: Can I have one quick question?

DR. LANGMUIR: We're over time here. If it's really short.

DR. BULLEN: I can hold off.

DR. LANGMUIR: We'll have time later in the day, certainly. The next presentation is status of the Thermo-hydrologic Review Evaluation Team and our presenter is Ardyth Simmons.

STATUS OF THE THERMOHYDROLOGIC REVIEW EVALUATION TEAM

[Slide.]

DR. SIMMONS: I was asked to speak about a review team, an internal review team at the project that is looking at our thermohydrologic models and testing program. The purpose for the initiation of this review team was to develop a project approach to modeling and testing thermohydrologic processes.

At the present time, there are numerous groups, many of which you've heard about in this meeting, some of which you haven't heard about or have only been referred to indirectly. We felt it was important to assess all of what had been done to date -- there are voluminous reports that have been written on these subjects -- and to take a look at the models and their applications in the field and in situ experiments, particularly because we are in the process of planning our ESF experiments.

We're going to use this information to plan an external peer review prior to finalizing our plans for the Exploratory Studies Facility, heater tests.

[Slide.]

DR. SIMMONS: The objective of the external peer review now, the one that we would be preparing for, will be to evaluate the project's approach to understanding hydrothermal conditions at Yucca Mountain. This includes consideration of performance assessment, of the thermal loading, of the test design and sufficiency, and of model sufficiency.

[Slide.]

DR. SIMMONS: The scope of the external peer review will be to examine the sufficiency of the laboratory and field experiments to the understanding of thermohydrologic processes. To look at the sufficiency of the models and the modeling approaches to predicting performance with respect to; coupled process modeling, thermohydrologic process models, and the implications of these for the thermal loading decision. And then look at the sufficiency of our approaches to; understanding the viability of the approach for making the thermal loading decision, the compatibility of the observations from the data and the models, and the appropriate range of alternative conceptual models that are being used.

[Slide.]

DR. SIMMONS: So we have established what I just went over as the purpose for the external review, peer review. Then what this internal review team is doing is to write a white paper that will describe the project's approach to modeling and testing the thermohydrologic issues. This white paper will be used to develop a project focal point with respect to these issues that will be then given to the external peer review.

The review team membership includes DOE, the M&O, Lawrence Berkeley, Los Alamos, Lawrence Livermore, and Sandia membership. We have constituents from the end users of the information, of the thermohydrologic information, performance assessment membership, and we'll be having design input, as well.

The review team will disband, however, at the completion of the external peer review.

[Slide.]

DR. SIMMONS: The next page in your handout gives you an outline of the white paper. I'm not going to go over this in detail. Generally, we will establish the background and then go over our current understanding of ambient conditions and what the thermohydrologic conditions will be. We will compare the alternate representations used in the thermohydrologic analyses and then we'll deal with the existing uncertainties and our approaches to resolving them. And then finally the technical issues that need to be considered, that we think need to be considered by the peer review.

[Slide.]

DR. SIMMONS: Right now, as I said, the review team is writing this white paper that will develop the project focal point. We're having monthly meetings to continue to develop it. We would like to have it completed by January of '95. However, we may not be able to meet our goal because neither the review team effort nor the external peer review are funded in '95. So we're doing this essentially on a volunteer basis.

The reason why we're doing this is what I stated earlier. We feel that it's really critical to have this review conducted prior to the finalization of our plans for testing in the exploratory studies facility, because we want to make sure that that testing goes along in the best possible path and design.

So I just wanted to let you know what was happening with that. Are there any questions?

DR. LANGMUIR: Ardyth, I'm assuming you're starting with the SCP, which has a tremendous recipe of options, things that one could do. I gather you -- I presume you've come a long way down from that to focus yourself on activities that in the current situation are perhaps quite different than what was being considered at that time.

What are you emphasizing right now? What do you think is going to come out in the white paper in terms of major activities that you propose to do? I would guess you have some idea right now about that.

DR. SIMMONS: We haven't gotten to a point yet in the review team to be making recommendations for what the future emphasis should be. What we're presently concentrating on is synthesizing the results of the previous tests and modeling exercises. Then, of course, we hope to be able to provide information that will assist in the best possible design and testing program in the ESF.

I would say that that testing program would probably be somewhat different from what was laid out in the SCP. But we're not at a mature enough stage right now to be able to state what those differences would be. That is what we hope to do this year.

DR. LANGMUIR: There's a lot of hydrologic test work proposed for the ESF, as well. Are you integrating what you are going to do or proposing to integrate it with their tests? Certainly that's the best way to go if this integration is possible.

DR. SIMMONS: Yes. That is definitely part of the plan.

DR. LANGMUIR: Is your program the only one looking to do this preliminary review approach to things before you go into the ESF? Are any other groups considering an external analysis and peer review of proposed ESF work?

DR. SIMMONS: Not in external peer review. However, there is an effort in the project to look at all of the tests that would be conducted in the exploratory studies facility and to try to get the best consolidation of those tests and a clear understanding of what the objectives of each of those would be. We're working with the test planning package that someone showed in a presentation earlier today. That is going to be more comprehensive than what the scope of the peer review that I just described to you will be.

However, that effort to look at all of the ESF tests is going to be an internal effort.

DR. CORDING: What is the schedule on the external peer review? When would they complete their recommendations and when would you have that integrated into your program?

DR. SIMMONS: Our goal is to have that totally finished by the end of this fiscal year. That would include not only the recommendations made by the external peer review, but also our responses to those recommendations and how we would implement them in our testing and modeling program.

But some of the schedule is dependent on whether we are able to pick up supplementary funding this year to complete that work. John, really a peer review takes a minimum of six to nine months to do, based on the ones that we've conducted in the past.

DR. LANGMUIR: Dennis Price.

DR. PRICE: Could you explain the term "external" and you might face north in your answer. I'm just kidding. Because I think Dr. North, if he were still on the Board, would be asking if any of the members of the review team have less than some direct connection with DOE.

DR. SIMMONS: Yes. I'm actually glad that you asked that question because I didn't bring that out in my presentation. But we have a formal peer review procedure that we use in the project for conducting external peer reviews. The intent of that is to convene a group that is totally external to the project, that has not been involved in any of the previous work that's done or in any consulting on the project in the past or present.

The Department of Energy selects who the Chairman for that peer review would be based on their credentials in the scientific or engineering community. But then the Chairman of that review selects the membership for the review team. We have a documented process by which we include all of the background information on these individuals and we record all of the interactions that they have with our participants in formalized meetings and basically a large record is made of all the interactions.

At the end of their review session, generally, a series of several meetings, they produce a set of recommendations for us, which we then have to respond to and tell them how we're going to implement those into the program.

This has been done a couple of times in the past. I think the most recent one an unsaturated hydrology peer review that Claudia Newberry was responsible for.

DR. PRICE: There, nevertheless, is a direct chain link with the DOE among the membership. In other words, it's not an independent peer review. The term "external" and independent --

DR. LANGMUIR: Ardyth is speaking of a panel that Allan Freeze chaired on the unsaturated zone hydrology, which was, from what I could see, quite independent. There were no connects that I was aware at all in that group.

DR. SIMMONS: Yes. That is the purpose. If I somehow confused it, I didn't mean to.

DR. PRICE: No. You probably didn't confuse it. I probably listened wrong.

DR. SIMMONS: No. They are totally independent from the project.

DR. LANGMUIR: Pat Domenico.

DR. DOMENICO: Ardyth, is the purpose of the white paper to offer guidance to the external review or to just bring them up to speed?

DR. SIMMONS: It's to save them some work, actually. We've done a bibliographic search and there are hundreds of papers that have been written on all the topics that we've been talking about. So what this white paper will do is synthesize that information and we'll be giving it to the peer review to kind of give them a head start.

Now, if they choose to read all of those, that's up to them. But we felt that it was important to have a project summary and focus of all the work that had been done. That's not to say that there would be a consensus on everything, but just a bringing together of all the work that had been done.

DR. DOMENICO: When you start to gather your team, who has no experience with this project, where would you suggest you start? That's a joke. You don't have to answer it.

DR. SIMMONS: I know what you're saying, but, actually, we've got a list of a fair number of people from the hydrologic and testing community.

DR. LANGMUIR: Thank you, Ardyth. We have a few minutes before the scheduled break. I had to cut off Dan Bullin, who was going to ask the previous speaker a question, if Dan is still around. We'll wait till later. Randy Bassett, since there's some time before the hour here.

DR. BASSETT: I'd like to ask a question of the previous speaker, as well. I guess I would say I would encourage you as fast as possible to set the equivalent continuum model aside and begin to look at the idea of more focused flow. I think the questions that you have on one of your last slides, number 23 or 24, I think, where you said you wanted to look at fracture maintenance interaction.

I think we're there. I think we need to know that soon. I think we need to model that process as soon as possible. I think it has impact on a wide variety of issues. Corrosion rates also fit into that whole scenario. For example, we see at our particular field site fracture flow as being a very significant method of liquid transport, the Apache Leap site there in Arizona, where we have tunnels that are penetrating fracture unsaturated tuff that are similar to Yucca Mountain.

The question is how do you simulate the re-entry of water back into this dry zone. Is it focused? How does it focus? How does the temperature change when these zones, of course, wet and drain? Then I think this leads into the corrosion issue. As water re-enters into the drift, can you maintain liquid water and, in fact, liquid water that has significant mass transfer from liquid water to vaporization to condensation on canisters or facilities, corrosion, dripping and movement of mass from the surface down to the bottom. And in that whole process, do brines, in fact, actually form?

Just a simple analogy. In the tuffs that we see in the west, we see concentration factors of 50 to 75 for chloride from rain water down to the water that we see in the tuff itself. This is in ambient conditions. So you begin with chloride values of half-a-milligram per liter.

You have very significant chloride values just in the unsaturated zone.

So I wouldn't rule out corrosion that's driven by relatively high concentrations of brines. It's certainly possible, especially if the pond evaporates, refluxes. I'm not saying that your analysis to date is inadequate, but I'm saying I really wish there could be a shift to look at this other process ASAP.

DR. MISHRA: Could I have some time to respond to that?

DR. LANGMUIR: In fairness, yes, certainly, before the next question.

DR. FARRELL: J.J. Farrell, M&O. My question is why should we take our eye off the ball of getting a reasonable equivalent continuum model in drift and mountain scale response before we get into the, let's say, endless search for the ideal crack?

DR. MISHRA: I think I'll try not to answer the philosophical question. Going back to Randy Bassett's concern with respect to the equivalent engineering model, I think it's well taken, as I pointed out in my presentation. The equivalent continuum model is perhaps the simplest abstraction that one can use to represent flow and transport in a fracture environment.

Now, the reason, to some extent, why performance assessment is using an equivalent engineering model is because everybody else is using an equivalent continuum model. The reference hydrologic model off of Yucca Mountain being developed by LBL and USGS is an equivalent continuum model. Much of the hydrothermal analysis that has been done since 1986 or even prior to that have been based on equivalent continuum model assumptions.

It is not the charter of performance assessment to provide an adequate representation of the fracture matrix system. We just take whatever is available and use that as a basis, so that there is a consistent link. But what we are trying to do, within the limitations of our analysis, is to see what are the limits of the applicability of these models even with the simple one-dimensional or two-dimensional representations that we have.

And if, for example, the hydrologic modeling off the ambient state at Yucca Mountain comes up with some non-equilibrium flow representation, then we would obviously be using it. So I think the point is very well taken, but I would submit that performance assessment is looking for other groups within the project to provide us that lead and that understanding.

I'm not saying that we will not use it. We will certainly use it when we have it.

DR. LANGMUIR: Why couldn't you have used the

WEEPS model representations already provided in TSPA-93 by Sandia on its performance assessment program?

DR. MISHRA: In a way, the WEEPS model is not a conservative representation of a fracture matrix system. It just assumes that you are driving all the flow into the fractures. In a way, it's a double-porosity type of a representation which says that there is no matrix imbibition or there is no matrix contribution to the flow. And when you have WEEPS, the problem is that only a finite number of these WEEPS will intersect the waste packages.

So in a way, the performance of the WEEPS turns out to be better than the performance of the equivalent continuum model. In either case, there is no -- to my knowledge, there has been no validation of the WEEPS model based on the hydrologic data from the site.

DR. LANGMUIR: We've succeeded in using up all the time to the break and here's Dan standing here and hasn't asked his question yet. Let's let him ask his question.

DR. BULLEN: I'm sorry. I snuck out into the hall to ask the question clandestinely. But in your -- I know this is not a performance assessment workshop, but I do have a couple of questions about the results that you've presented for neptunium and technetium.

The first question is does your neptunium model of PA -- specifically, your figure of 22 -- include daughter in-growth from americium and plutonium-241?

DR. MISHRA: I take the Fifth on that, because Bob should be here to answer that question.

DR. BULLEN: So the answer is yes, from the audience.

DR. MISHRA: Okay. Thank you.

DR. BULLEN: That's good. Then the next question that I have is actually with respect to do you ever flood the repository. Is the repository under water in your model?

DR. MISHRA: No.

DR. BULLEN: Then by what transfer mechanism do you get the waste from the package to the wall for no backfill environment? I look at the results and I see backfill and I can understand the transport pathway and I understand that we can fail containers. But after we fail the containers, what mechanism do you get that waste out of the container, to the floor, to the wall? How do you get it out?

DR. LANGMUIR: Magic.

DR. MISHRA: Yes.

DR. BULLEN: It just raises some significant questions when you talk about the cumulative release from the packages on your Figure 22.

DR. MISHRA: I think that's a very valid question.

When I say no backfill, I don't really mean no backfill. There is always a backfill invert on which the waste package is resting, which provides at least a diffusive pathway for release from the waste package into the edge of the drift.

DR. BULLEN: This is a surface diffusive pathway.

DR. MISHRA: Yes.

DR. BULLEN: But if you do surface diffusion calculations just along a surface and you're not assuming flow, do you know how long it takes that radionuclide from the waste package to get out of the clad, out of the package, along the package surface and back to the rock? Have you ever done that number?

DR. MISHRA: Not along the package. We assume that when the package fails, it just -- all of its surface is directly in contact with the invert.

DR. BULLEN: Okay. I think that's a little over-conservative.

DR. LANGMUIR: I think we have to take our break and try and stay somewhat on schedule. Let's reconvene after the break at 10:40.

[Recess.]

DR. LANGMUIR: Please take your seats. Our next speaker is Bill Halsey of Lawrence Livermore. His presentation is on underground heater tests. I gather it's a team effort with John Pott, and they'll introduce each other as it proceeds.

IN SITU TESTING REQUIREMENTS

UNDERGROUND HEATER TESTS

[Slide.]

DR. HALSEY: I'm going to be talking about underground heater tests primarily for the hydrothermal and then the thermal geochemical and just the mechanical portions of that that tie into the hydrothermal. Then John Pott from Sandia is going to talk about the primarily thermal/mechanical, with a coupling to the others.

This is not my area of expertise. I'm filling in for Dale Wilder, who had to leave last night and knew that he wasn't going to be able to be here. So if I don't have all of the answers, don't tell anybody.

[Slide.]

DR. HALSEY: These are the people that actually are doing the work at Livermore. Dale Wilder is a Technical Area Leader. He spoke yesterday about many of the processes that we're interested in. Wunan Lin is the Task Leader for the ESF testing. Then we have the geomechanics, hydrology, geochemistry and the manmade materials tasks that are all involved.

[Slide.]

DR. HALSEY: Just briefly, this is a summary of some of the in situ coupled process tests, the program for these that Ardyth discussed yesterday. Primarily, I'm going to be talking about the bottom two here. That tells you where we are in the program.

[Slide.]

DR. HALSEY: This, from Mike Voegele's presentation yesterday, shows the long plan of information, what level we expect to have at different points. I was just going to point out the areal power density we want to have bounded by 2001, decided by 2008. We have to have thermal information to go to the source term bounding here in the EBS thermal, going from a concept at the site suitability to a bounded description in 2001.

That's going to lead us to some early aggressive thermal tests to get information in that timeframe and then some longer tests to resolve some of these issues further on out. So it applies to the improvements in knowledge here and also in the subsystem analyses up here under the natural barrier and some of the material interactions issues for the repository design.

[Slide.]

DR. HALSEY: This is a summary of what I'm going to -- of how we got where we are at the moment. The program approach begins with the assumption that we will defer as much of the testing as is reasonable to the post-license application timeframe. Then you look at each of the major milestones of the previous chart and try and determine what are the necessary pieces of information to get past that point.

At 2001, we're requesting the NRC to allow us to begin construction of an underground facility and that facility will be consistent with one or more thermal loading designs, construction methods, operational concepts, and performance strategies. There are a couple of things that we need to show about the thermal response at that point. One, that it's safe to construct it, and that's the thermal/mechanical that will follow, but we also must show reason to believe that the thermal response is consistent with the post-closure performance strategy.

That's where the bounded hydrothermal models of the previous chart come from. We don't have to have all the final answers, but we need to show reason to believe that the hydrothermal response and the coupled processes will be consistent with the performance strategy. It's the early thermal testing that provides that technical basis.

[Slide.]

DR. HALSEY: Dale Wilder yesterday went through the discussion of the hypothesis testing, which shows both low and high thermal loads, what are some of the important hypotheses. I'm not going to go through those again. He discussed them to some extent.

[Slide.]

DR. HALSEY: However, I'm going to go back to this and that's where the information comes in from the test program. We're talking about these two columns, the early in situ and the main in situ tests. You can see that both for the lower and the higher thermal loading regimes, we're looking for a substantial step forward from the large block test to the early in situ in the level of knowledge. This is the timeframe where we get information up to this point for the site suitability and then this is what we're trying to get in terms of improvements in knowledge for the license application in 2001.

Then we've got another seven years or so to upgrade to these more complete levels of information.

[Slide.]

DR. HALSEY: This is another tie to Dale's presentation where he showed the whole process of knowledge flow from trying to conceptualize real world processes through experiments, the field testing, developing of the mechanistic models, applying these. Dale has major reports, the near field report and the altered zone reports, and then he just touched upon the rest of this. I'm going to be talking about some of these tests and then how they flow on through the system.

They have to be abstracted into the subsystem performance. There are many subsystems at the repository. Some of those have specific requirements and then those all get rolled together and abstracted into the total system performance, which we heard about earlier this morning.

[Slide.]

DR. HALSEY: To begin with, to gain the type of information on coupled thermal/mechanical, geochemical and hydrologic processes and how they affect the performance of the repository, if you didn't have a time constraint, how would you develop the tests. This is an example.

First, you would go out and do a prototype where you'd probably want to heat for a while, cool down, do some analysis, and then you would take the results of that to plan a more detailed and thorough test, set that up.

You'd probably have at least five-year heating duration. You'd have to let it cool for quite a while because you're putting in a lot of heat. And then examine -- look at the results. The problem is if you add this all up, I think it's around 17 years. It's a good way to do it.

[Slide.]

DR. HALSEY: By my calculation, we're right over here at this end of the chart. Where we need data inputs are here and here. We don't have 17 years. So we have to come up with a different approach. A different approach was what Dale was describing as a series of accelerated tests in parallel. You start setting up for the longer duration tests and you scale these and locate them on the calendar so that they can, indeed, feed information through the processes that are necessary into the major milestones.

So you need information coming out of these to support the parallel development of the subsystem models and mechanistic models and the analyses that will support the licensing arguments.

[Slide.]

DR. HALSEY: So then you face a challenge. Because of the time limitations, the heater tests have to be accelerated. The question is how much can you accelerate without distorting the coupling between processes and changing the phenomenon. If you change the fundamental mechanisms, then you have trouble using your data to represent reality.

[Slide.]

DR. HALSEY: What are some of the criteria that you end up looking at? Well, a short list is things like, and not in a particular priority; the velocity of the dry-out front, the spatial extent and duration of condensate generation, what are the peak rock temperatures that you're going to reach, what is the time rate of change of the temperature at different locations within the rock.

And attached to this, you have the thermal gradients that are generated. And how much rock are you drying out, how much water are you mobilizing, et cetera.

[Slide.]

DR. HALSEY: Your design for the test ends up being a compromise between the realities of the schedule and how much can you accelerate some of these processes. I'll go through, I think, in the order that they're in your package, not necessarily the order that I was thinking about them. I put it together wrong.

I'd like to just comment on the evolution of test plans. People had said yesterday all of this is currently evolving. That's true. If we go back about five years, we had done some fairly thorough testing for the EBS -- or test planning for the EBS field tests and we ended up with a large-scale, long-term heater test using up to 11 parallel drifts, with access above and below. So it's a 3D arrangement of drifts for heaters and instrumentation, running at least seven to ten years.

One of the comments yesterday on the list of tests that Ardyth showed was there seemed to be a lot of mechanical tests and things like that, but not that many that apply to the coupled processes in the EBS. That's because the tests for the coupled processes in the EBS is a big one. Probably in scale and cost, approaches everything else on that list, physical scale and time scale. I'll show just a quick graph of it later.

Why is it so big? Because we are looking for processes that occur over fairly large spatial scales. Why is it so long? Because looking at things over large spatial scales requires a long time.

A few years ago, we had schedule pressures which resulted in splitting off an accelerated test to run about three years in parallel with the larger and longer test. We reexamined this in the program approach and said what do we need when and, indeed, the long test we believe can be deferred. The results of that probably aren't needed until we have to finalize the thermal loading and ask for a license to operate and emplace waste. But for the reasons that I discussed earlier, we do need some testing. And without having time to do test planning, the question was what do you need.

Based on conversations with a few of the people, the answer was it looks like we need a two-year heater duration aimed at a 200 Centigrade rock temperature peak. We've been looking to see if that's adequate since then. I believe that we're going to have a call from the Test Coordination Office later to see can we, indeed, get this kind of time scale test in and get some results prior to the licensing. At the moment, the answer is yes.

I'd also like to say that in looking at some of these processes, at the moment, there is current interest, but not yet detailed plans for additional small tests; single drifts, bore holes, that can be fielded in multiple locations to assess some of the heterogeneity in the different rocks and the hydrothermal response of the different rocks.

There are some indications that at the lower thermal loading regime, some of the processes may be more sensitive to the rock heterogeneities. So this may be of increased importance with the current philosophy of aiming first at the lower thermal loading regime. Those tests we don't have detailed plans for, although there are people here who can discuss it, if the Board wishes.

[Slide.]

DR. HALSEY: As I said, the test is a big one. Here is a sketch of the 11-drift test. This is about two acres or a little bit bigger. It runs three panels to look at different processes and different scales. Above and below, there are access drifts for instrumentation. So you can examine processes above and below.

I need to point out that in a test like this where you're looking for things like buoyant gas processes, buoyant gas convection processes, you really want to seal all of these so that you don't have gas flowing around in this and develop a lot of convection cells. So it's not an easy place to get around and you need to seal and close all of this off. It's not a good place to do a lot of other tests because you don't want to perturb this for a time period of like five years or more.

I'm not the right person to talk to the details of a test like that.

[Slide.]

DR. HALSEY: The smaller test looks like a portion of that and here is a three-drift representation. This is half-an-acre, little less than a half-an-acre, I believe, what Tom tells me, with heaters placed into drifts. Again, you're going to want these sealed off. The question of how much can you accelerate this test leads into a parametric study of what size heaters are you going to use, how fast are you going to heat versus how much time are you going to take, and what conditions are you going to reach, how far apart are you going to place these versus what spatial scale are you going to examine processes over.

If you put them too close together, you don't see a lot of the larger spatial scale processes. One of the reasons that this is as large as it is, I'm told, is to look at the possibility of ponding condensate up above. If you're too small, you may not see that kind of process in the time scales that we're talking about. If you make it too big, your time scales get too large.

[Slide.]

DR. HALSEY: Just a couple of examples. If you're trying to avoid heating the rock above 200 Centigrade and you put in, on that layout, 6.3 kilowatt heaters, you then start calculating temperatures of the drift walls and the midpoint of the pillar. And you see that the midpoint of the pillar comes up in about a year to the boiling point and sits and what you want to have is coalescence of the boiling between the pillars so that you are building a dry-out front, which has some spatial variability.

You may get condensation and then you can see where you reach the 200 degree limit on some of these. Right here, it looks like you can heat for about three-and-a-half years. But what information can you gain in shorter times, such as the two years that we think we've aiming for in the accelerated test.

[Slide.]

DR. HALSEY: For that particular layout, where do you get information? The sampling regime. If this is the center of that three-drift test, so then you have a rock pillar, you have another drift with some more heaters in it. Here is the temperature profile as a function of time and then it's a zone out here in the rock that we're interested for many of the coupled processes.

Yes, we're over-driving the walls. We have high heating rates. We have high temperatures. But out here we have conditions which are reasonably similar in kinetic rates and in properties to the repository at substantially extended times. I will show that in a minute.

You have very high liquid saturation levels. You may get ponding of water or liquid saturation. You have an extended time and distance. You have a number of meters over a period of a number of years, where you may have the possibility of water refluxing, water migration. So you can look at some of the geochemical processes.

So after your test, you're going to have to go a number of meters into the rock and then look at what's happened in there. This is also where you want to put a lot of your instrumentation to look at things.

[Slide.]

DR. HALSEY: As an example of the acceleration problem, the rate of advance of the dry-out front for a test like that, here at 6.3 kilowatt. At the rock surface, what is the rate of advance of the -- near in, what is the rate of the advance of the dry-out front. It's pretty fast. You're going many meters per year.

For the accelerated test, you're pushing it pretty hard. For the longer-term test, where you have four to seven years, it's not as accelerated, but it's still many times the heating rate for the repository.

But I'd like to point out that further out into -- one of the reasons this is so accelerated is these drifts are smaller than the repository emplacement drifts. So you've got a smaller drift. You're coupling the heat into the rock and you're getting a very high gradient and you're driving it hard. That portion of the rock is going to see substantially different kinetic conditions than the repository. But if you go out into the rock a number of meters, five to ten meters, then these rates are more like a few times that of a typical repository and may be much more representative.

So these are the kinds of things that you have to balance and trade off when you're trying to design these tests.

[Slide.]

DR. HALSEY: Here is one of Tom's calculations showing what that test looks like at one year and two years, where the red shows the boiling zone and you are, indeed, creating a -- coalescing the boiling zones and you may get some ponding of water up here. You start to get some asymmetry. You can start generating buoyant gas processes, if they're there, measure a lot of different things.

[Slide.]

DR. HALSEY: This is, again, one of his calculations. Dale Wilder yesterday showed some of the difference between a conduction-dominated thermal transfer and a convection-dominated thermal transfer in the repository. They look very similar to this. If you start to see this difference, this is now at different bulk permeabilities, you start to see this asymmetry and this flattening of the temperature curve.

You have the conduction-dominated process here, the convection-dominated process here, and you can start to measure that. The chart that Dale showed yesterday looked very similar to this, but it was at 100 years. This is what the accelerated test looked like at two years and four years. You can start to measure this. And this is something that in the repository takes 50 to 100 years to observe, the examples of what you get a number of meters into the rock in one of these heavily-driven tests.

Now I am just going to touch briefly on where you use this information. As I said before the break, we don't have the models to use all of this data yet. We're still developing the mechanistic models to understand all of these. We heard the discussion for the hydrology, should we be using equivalent continuum or fracture models or do we need to be using combinations of those, where it's appropriate, and those are evolving.

We have subsystem models. We have total system performance models. And as I said, right at the moment, they can't handle those complexities either. They are evolving. And each step is an improvement, but, please, just because there's been an improvement, don't think that we've gotten to the final answers yet.

[Slide.]

DR. HALSEY: This is my little portion of the total system pyramid, the PA pyramid that I think many of you have seen it many too many times. This is just the EBS and the near field portion of it and then the pyramid goes on up to the total system. There are many other parts of this pyramid, unsaturated zone, processes of transport, the saturated zone. But this is our little portion of it. And this is where we had geomechanics, geohydrology, geochemistry in the near field environment.

We have the waste forms, the containers and the other materials that we're putting in there, all the cements, the organics, things like that. And we have to put those all together. These are a couple of processes that Ardyth talked about yesterday. We need to understand how those get put together.

We get some test data, we start putting those together, then we have to combine these processes, abstract them so that they're simple enough to actually use. If you just start hooking detailed mechanistic models together, you rapidly get to something that you can't use. It won't run on a computer and it's too big to write down. And turn them into an EBS near field subsystem model.

This can provide a source term for the TSPA and, also, it can be used for; test analysis, system design, and also for compliance with the subsystem performance requirements, which, as Dave mentioned, we're going to try and do for the first time in this program later this year.

[Slide.]

DR. HALSEY: That subsystem model that we use at Livermore, a number of them are on the program. Ours is the Yucca Mountain integrating model. You can see this is a bubble diagram similar to what Srikanta showed. You have all these different parts. This is just our portion of it and it ends up with a release rate. And you can see we have places in here for the near field chemistry and the hydrology, the flow descriptions. And this is temperature-dependent, all driven by the thermal load.

We worry about the container failures, the flow of water from the rock to the container, from the container out into the near field environment.

I'd like to just point out that in TSPA-93, we used some portion of those that have a little star in them. You'll notice that we don't have a whole lot in a couple of these.

[Slide.]

DR. HALSEY: Getting to the point and reiterating the point that I made before the break, recent total system analysis for TSPA-93 was a substantial improvement for the program in terms of thermal effects.

We went from the previous analysis, which had one waste package, one thermal loading implied, but no explicit thermal processes; a container with an arbitrary failure history that was just written and there it is, and everything was assumed isothermal after 1,000 years, the waste form and the near field were assumed isothermal, to a lot of improvements. We started putting in some mechanistic corrosion processes. We started to put in temperature dependencies for the oxidation, the aqueous corrosion; some temperature dependence into the waste form performance. We started to put in some hydrothermal modeling, as Srikanta has said. We were using the results of equivalent continuum models in the repository scale and -- in the mountain scale, repository scale, and drift scale to try and look at what water fluxes might be.

There's a few things we didn't do and I will get to those in a minute. We put some temperature dependence into the near field. We tried to look at some of the dry-out effects and reflux effects. So when we got temperature-dependent flow, there were some things we couldn't do. We couldn't turn those into temperature flow-dependent water contact on the waste package. So the corrosion models had very simple switches. They're either on or off.

When they're on, it's as if you were sitting under water. They're aqueous processes. When they're off, they're not occurring. That's not realistic. There's a transition through these. We need to be able to couple those together and get a continuum transition between non-aqueous to aqueous processes.

A few things that we -- I put this up primarily -- I wanted to show that we had made improvements, but then in terms of the interests of this meeting, a lot of things were not included. We did not have hydrothermally driven water contact, as I just said. We didn't have any of the near field geochemistry detail. We did not have extended dry-out effects. We did go to a higher thermal loading, but the resaturation was assumed to occur as the boiling front returned.

So it immediately re-wetted after the temperature field dropped below boiling. So this is not an extended dry 114 kilowatt per acre. It's one where you have rapid fracture flow immediately following the boiling isotherm. No manmade materials. When you look at the design for these drifts, you've got all the cement, grout, all these steel rails, all this material added, backfill, organics. We didn't have anything -- after the waste packages failed, we didn't have anything between the waste form and the rock. That's why you have the question just before the break -- how does water get from the -- or radionuclides get from the waste form out into the rock when you don't have a flooded repository.

We ignored everything in between. Again, I will repeat what I said before. The results of these analyses are wrong, probably, but we don't really know. We don't believe the numbers that come out so much as the dependencies. You can go back and do the sensitivity studies that the previous speakers were showing and start to see what is important, where do these things feed in. At the same time, we're starting to build these models.

If we had all the data and we had mechanistic models, we couldn't incorporate them yet. If we had the data, but not the -- we don't have the mechanistic models. All three have to be developed in parallel, the system models, the data, and the mechanistic understanding. I think we're in the process of doing that.

[Slide.]

DR. HALSEY: I'll summarize and then get out of the way here. The in situ EBS and near field test planning is evolving with the program approach. We believe that we can get some accelerated tests that will provide the basis for the thermal strategies in the license application for construction and that there is time to do the longer-term tests for understanding the thermal processes more completely, to establish a thermal loading and a performance strategy for the operational licensing.

The test plans are being evolved on the basis of hypothesis testing, looking at the entire regime of thermal loading, from the lower to the higher end. And these tests will provide the basis for the coupled process models which will then go into the performance model. We need to describe both the near field natural system and we need to include the materials that are used in the EBS, and these are then coupled to model development and application that go into design decisions and analyses and eventually into the licensing documentation.

That's all I've got.

DR. LANGMUIR: Thank you, Bill. I think maybe it's most efficient to go on to John Pott and ask for questions for both talks at the end of this, since we've taken 30 minutes already.

[Slide.]

DR. POTT: I'd like to continue talking about thermal tests, thermal underground tests, and whereas what Bill talked about had as its emphasis hydrology, these tests emphasize more the mechanical effects, but both of them look at coupled thermal/mechanical and hydrological processes.

With the program approach, we needed to re-look at the suite of tests that were defined in the SCP and see which of these tests we could defer and which tests needed to be done in the near term in order to meet the information needs that were coming up, such a site suitability and the initial license application.

What I'm going to concentrate on in this talk are the initial tests, the ones that really will address license application. These tests have also been evolving over time for a number of other reasons, such as the changes in the ESF, going from two shafts to ramps, going from drill and blast construction to TBM, and changes in going from a -- thinking about waste emplacement in bore holes to waste emplacement in large MPCs in drifts.

[Slide.]

DR. POTT: As an overview of what I'd like to talk about, the first thing is the objectives of what we have done. Second, I'm going to talk about the requirements and the information needs that are driving our current test program. I'm going then to talk about the proposed revised test program, sort of where we stand today, and then present some conclusions.

[Slide.]

DR. POTT: Our objective was to develop a revised test program. First of all, we wanted to really address the information that would be needed to collect, when it was needed by, and at a sufficient level of confidence. We needed to do this in a timeframe that would provide the information when it was needed. Thirdly, so the objective of this talk and what we've done so far, we've really just focused in on the pre-closure issues. We still have some needs to do some long-term tests that, however, can be deferred, that will address post-closure issues.

[Slide.]

DR. POTT: I will first start off talking about thermal load decision tree. What a thermal loading decision tree is is sort of a systematic way to try to reach -- to help us reach a decision on thermal loading. What it is is a series of questions that we step through and we try to raise all the questions that need to be answered in order to make a thermal loading decision.

Well, in order to answer these questions, we come up with what we've called conditionals. These conditionals are calls for additional information that we need in order to answer the question. This thermal loading decision tree then tells us what information we need and it also sort of gives us the manner of in what order should we collect it.

Once we've collected information that can answer this question, then we know downstream what additional information we need to collect. This then is one of the drivers of the test because it -- of our test. It raises the information needs that we will have to answer in our thermal test.

This excerpt, for example, of the decision tree tells us in order to answer the question about stability of drift emplacement, which obviously has some mechanical emphasis, we need additional confidence in our thermal/mechanical models. Also, we need to understand -- to develop our models that join the rock. Behavior. We need to predict the thermal -- we need to know, rather, what the thermal/mechanical properties of the rock mass are.

We need to also know something about the thermal chemistry on the effect of the mechanical response and also the effect of the silica phase inversions which cause significant thermal/mechanical effects.

[Slide.]

DR. POTT: I'm going to go through the next viewgraph a little quickly. We have a few more excerpts from the thermal loading tree that also raise additional conditionals that have to be answered. These have to do more with things -- hydrological questions.

[Slide.]

DR. POTT: The silica phase transformation which I mentioned also raises additional questions.

[Slide.]

DR. POTT: And also backfill is something that the thermal loading decision tree has worked on and that's something we've looked at.

[Slide.]

DR. POTT: Another source of our information needs really comes from what I call our customers, the people who are going to actually use this information that we want to generate. One of our principal customers for these tests is the designers. To design, for their initial license application, they'll need to have information for their Title I design. And through talking with the people who are responsible for the repository design, asking them what it is that they need to know that they think our tests can provide, we came up with a list that includes things like the properties of the rock mass, the thermal properties, the mechanical properties, the thermal expansion, properties of the fractures.

The strength of the rock obviously is something they're interested in. Material interactions, that has to do with ground support, how does the ground support interact with the ground at elevated temperature. Then, also, model validation is another need that they would need for their Title I design.

[Slide.]

DR. POTT: Another customer for our tests is Pre-

Closure Performance Assessment. We did the same thing. We've talked to them and are, in fact, talking to them now about what their needs are. They have their list of similar items. They need, again, to know the properties of the rock mass, including thermal/mechanical strength. They have interests in stability and particularly were interested in stability of intersections of drifts and, also, the effect of temperatures on the properties.

[Slide.]

DR. POTT: Another customer for our tests is Waste Package. The kind of things that they indicated that they need would be rock mass thermal properties, again, information on the near field environment, and also some indication of drift stability under thermal loads. If the drifts are collapsing on the waste package, that obviously has some impact on them.

[Slide.]

DR. POTT: Another customer or the last customer I will talk about is Post-Closure Performance Assessment. What they need for the initial license application are some bounding estimates. They won't need as complete information. But in order to do that, they'll need to know some rock mass thermal properties. They need to be able to predict the temperatures around the drifts.

They're also interested in the temperature effects on the thermal/mechanical properties of the rock mass. They also have an interest in what's happening to hydrological properties at the elevated temperatures. And they have some need to look at coupling, the thermomechanical-hydrological coupling and to sort of test out different hypotheses.

[Slide.]

DR. POTT: What you can do is sort of make a summary of this and here what I have listed are the various customers that we've identified who would use our information and then the various information needs that we have. You can see there is a bit of overlap among the customers. Particularly, for example, rock mass thermal properties is something that all the customers want because they all need to predict the temperatures around the drifts. You can't do that unless you know what are the thermal properties of the rock mass.

[Slide.]

DR. POTT: So one driver for our tests is what information do we need to collect. We have constraints on us in order to collect that information. One, obviously, is time. It takes a certain amount of time to run these tests. But, also, there are time limits on when that information is needed. If we collect this information too late, it doesn't do anybody any good.

There are constraints on location. The targets for these tests predominantly are the TSw-2 unit, but also the TSw-1, because certain parts of the repository may actually hit units that look like TSw-1. Another constraint on our tests are construction methods. We're limited on what kind of construction methods will be available to us on one side. On the other side, we need to construct things for certain -- to answer certain of the information needs, we need to construct things to look like a repository, and I'll get into that a little bit.

[Slide.]

DR. POTT: Well, with those constraints and those information needs, put those together and we came up with a proposed test program to meet the short-term needs. The first three are a little bolder than the last. They're the ones I'm going to talk about. Those are the ones that involve temperatures. The last, the plate loading test, a standard test, is more just mechanical test. I'm going to talk about each of these three tests in order, the first being the axisymmetric heater test.

[Slide.]

DR. POTT: But before I do that, let me take a step back. I came up with the information needs and I have to show now that the suite of tests that we've come up with meet our information needs. If you look at it in a little different way, here's the same information needs we had before, but now I'm going to list each of the three types of tests that we propose. And you see that even though no test can meet all the data needs, between the three tests, we can meet them all.

[Slide.]

DR. POTT: The first test we're just calling the axisymmetric heater test. The purposes behind these tests is to look at the thermal properties of both the TSw-1 and the TSw-2 units because both of these units may be encountered. And TSw-2, certainly we need to know -- to understand that. TSw-1, we need to know whether we have to disqualify an area or not when we run into that type of unit.

Another objective of this test is to provide a means to do model validation. We also will look in this test at changes in permeability associated with temperature. We'll look at the drying front associated with this. We'll look at the fracture flow. This axisymmetric heater test we're going to look at in both horizontal and vertical configurations. I showed you a test, so I guess you don't know what that means yet. It says ideal geometry.

The reason for this test is really what I call very simple geometry and that's the strength of this test. We can't accomplish everything we want with it, but its simple geometry allows us to get a lot of information fairly simply.

[Slide.]

DR. POTT: And what the axisymmetric heater test simply is is just a central heater which will then give us an axisymmetric geometry, at least if we -- in a vertical configuration. In addition to monitoring the power, we will also monitor such things as temperatures, displacements, moisture content, and things like that.

[Slide.]

DR. POTT: As I mentioned, we can consider both vertical and horizontal emplacement. This is a plan view, looking down. One idea would be to go ahead and, off an existing drift, build a U-shaped drift around a pillar and then horizontally or perhaps at a slight angle to the horizontal to measure any moisture flow downward, insert a central heater.

And the advantage of a horizontal over a vertical is then we can get more area -- it allows us to access the rock mass surrounding the heater for more size. So these are sort of complementary tests. The vertical one is symmetric with respect to gravity. Here gravity will have an influence, but on the other hand, we can get more instrumentation around it.

[Slide.]

DR. POTT: I'm going to reverse these a little bit. The second test, which is sort of complementary to the first test, is a heated block test. I want to show you the figure first and then discuss it. A heated block test is similar to what was done in G-tunnel. What we do is we isolate a block of rock approximately two meters by two meters by three meters tall by cutting slots. We then can insert flatjacks through which we can impose mechanical load, control mechanical boundary conditions.

You notice that we have the joints here at an angle with respect to the flatjacks so that we can actually impose shear along the joints. Then we have two lines of heaters on opposing sides of the block and this allows us then to also, in addition to controlling the mechanical boundaries, impose thermal changes to the block, temperature changes.

[Slide.]

DR. POTT: To go back now, the heated block. The advantages of this one are now we have the controlled boundary conditions. We can begin to look at fracture properties. We can look -- we have a better chance now to look at such things as rock mass deformation and strength. This gives us a different configuration, but it allows us again to look at validating models that look at combined thermal/mechanical hydrological effects. So a complementary type of model validation. And it also allows us to look at thermal expansion of the rock mass.

[Slide.]

DR. POTT: Some additional -- no. That's all I want to say about that. Let me do the same thing. I want to reverse some pictures here and talk about the third type of test, the thermal stress test. Now, as opposed to the simple geometries and configurations of the first two tests, this one now emphasizes looking more like what a repository drift would look like. The shape of an invert is to be decided because that will follow whatever design most likely to be used for a repository.

The idea here is to heat the roof -- at least the roof of a drift, taking the roof because if we have rock mass failure, we can detect it by heating the roof, not heating the whole thing, because of our time constraints. And we can do various things about trying to change the gradients by changing the amount of heat throughout the heater to match more what it looks like in a repository.

So we heat up the roof of a drift and then we, again, instrument it to try to measure things like displacement and -- let's see. I guess that's all that's shown here is displacement. Thermocouples. So what we try to do here is displacement stresses, temperatures and moisture.

[Slide.]

DR. POTT: Now, to go back. The uses for the thermal stress test is it -- as opposed to the other two tests, it allows a demonstration of the rock mass behavior on the emplacement room scale. So now we've moved up to the scale, the actual scale of a waste emplacement room. It can simulate the in-drift emplacement problem. The others were more geared toward, for example, obtaining properties. This test is not so good at obtaining properties, but it is good at actually modeling the true waste emplacement problem.

This one we can allow a thermal overdrive and actually determine not only are the drifts stable, but how high do you have to go before they become unstable. We can do this on a fairly short time scale, based on our previous calculations for this kind of test. This also allows us for the first time to look at the interaction between the rock and the ground support, which was raised as an issue.

[Slide.]

DR. POTT: Some of the other additional things we get out of this test are: the geochemical effects of manmade materials which would be used in a drift; near field environment, again, because we're looking more like an emplacement drift; room scale model validation. The other two also had advantages in the simple geometry, simple boundary conditions advanced for validating a model.

But here, by having a realistic type room, there are advantages to that. Also, we get to look at thermal/mechanical effects on rock mass permeability.

[Slide.]

DR. POTT: The summary and conclusions. The test program is being modified to meet the needs of the program approach and the way it's being done is sort of through an iterative approach, where we've proposed a set of tests and told people what we think we can do for them. They're letting us know are we on the right track, are we actually meeting the needs of our customers.

Even though what I've talked about are the first three types of tests, the only tests I talked about, there will be additional later tests, similar to what Bill talked about, that will be needed to support later licensing decisions. We just having been emphasizing that on the mechanical side. I shouldn't say mechanical side. On the tests that emphasize the mechanical part of the coupling.

[Slide.]

DR. POTT: Finally, as part of the summary, what the proposed thermal/mechanical tests will provide. First of all, they're to provide the information that's required for the performance assessment, for site suitability, but predominantly initial license application is where these are headed. These tests can be fielded within the time window that we have to provide the information on license application. The tests are fairly simple and they're flexible enough to fit within the construction and operational constraints.

This is important because these things are in a state of flux. There is no definite word on, for example, what kind of excavation equipment will be available and when, exactly how much of the ESF will be excavated or even what unit we'll be in. These kinds of tests are flexible and so that they can accommodate the current situation.

These tests, however, are traceable and are consistent with the SCP performance allocation process. In other words, when the SCP laid out what the data needs are, these are still consistent with that. They will directly feed the thermal load decision process.

That's all I have.

DR. LANGMUIR: Thank you, John. We're going to restructure things just a little bit here in order to give people a chance to ask John and Bill Halsey questions, since we're a little bit behind a this point. We do have time this afternoon. We're going to move the final presentation by Bill Boyle till the afternoon at the beginning of the session and continue now with questions for John Pott and Bill Halsey. And since John is up there, let's start with questions for John. I believe that Ed Cording has one.

DR. CORDING: Thank you. Some of the questions I think will be both for John and Bill together perhaps, because I think that certainly we're looking at coupled processes and there's a lot of coordination between the two.

I was wondering -- I guess one thing for John is in looking at the low thermal -- if a low thermal loading is selected as an initial approach, is there a possibility that looking at higher thermal load conditions where significant stresses are being applied to the rock surface and more effective drift stability, the drifts are being more affected by the stress conditions than the thermal conditions.

If that's the case that we're looking at the low initially, is it possible that the -- looking at these drift stability questions would be something that could be delayed till after licensing? At least a full look at that sort of condition.

DR. POTT: I think in a sense we have deferred --what we have in the plan still is similar to what Bill described, our repository drift where we would heat the entire surrounding rock around the repository. So we have deferred some of that.

The thermal stress test -- well, again, based on my conversation with what I call my customer, Design, they did not see that we could defer that. They wanted to know something about rock mass strength. So I guess my answer to that is in talking to the people who are our customers are, they still are interested. It's still an open question to them.

DR. SATERLIE: Steve Saterlie, M&O. Let me see if I can amplify on what John was saying a little bit. I think there's really two issues there that are interconnected that we have to worry about. First of all, we do and could have a big package which can produce fairly high localized temperatures that we have to worry about.

Secondly, from the standpoint of constructing the drifts and designing this repository, they're going to have to worry about and know where it fails. And that failure knowledge has to be, I think, part of and incorporated in the license application because that may indicate how much -- how many rock bolts or the kinds of tunnel support that may or may not be required.

I believe it does need to be -- those tests do need to be conducted during the timeframe we're talking about.

DR. CORDING: Another question on the descriptions that Bill gave of the heater tests for the thermal hydraulic testing, where showing that the walls are being over-driven two levels, which would certainly be at or perhaps above what one would obtain with the -- in round one of the emplacement drifts.

I was wondering if that isn't what your plans were for integrating the thermal/mechanical effects with the thermal hydraulic in those same tests; for example, even using -- looking at lining conditions and other things in that suite of tests.

DR. POTT: You're talking about the drift tests that Bill Halsey --

DR. CORDING: That's right.

DR. POTT: Yes. I'm not sure I can answer the question.

DR. CORDING: The question is basically can you get your information along with the thermal hydraulic information from those tests.

DR. POTT: Well, we've been looking at that issue quite a bit. I think the -- and I'm not sure we have a definite answer. It tends to be that we can get the information we need from his tests, but he cannot get it from our tests. Cool-down is more important to him than it is to us.

DR. CORDING: Bill?

DR. HALSEY: We have tried over the years to see what can be done to combine some of these tests and where they can work together. There are some areas where we're using synergism, but we do have some different requirements for the tests. As John said, they're interested in the mechanical response of the rock that we over-drive. However, we want to make sure that that rock doesn't fail because we don't want it falling apart and messing up the geometry of our tests.

So if we expect it to fail, we're apt to bolt it all in place, which makes it hard for them to get their information. Also, as I pointed out, our tests, we want to have the gas phase flow pretty well sealed off and they need access to go in and apply mechanical loads and things like that.

DR. POTT: The configurations look similar and it's sometimes deceptive to think that, therefore, you can just go ahead and do everything with one test. It's not clear that that's true. We've been meeting for years trying to do that.

DR. CORDING: I certainly see the importance of understanding thermal/mechanical behavior. There's going to be a tremendous demand for space and time underground in the projects before 2001 and I think it's going to -- I think that there's going to be a lot of choices having to be made in trying to combine things to the point that they can be accomplished.

Perhaps some of these others where you're going a little bit further with it are things that may have to be delayed a bit.

DR. HALSEY: As John said, we've looked at this in the past and we haven't had time to do it for this latest rescoping in response to the program approach.

DR. CORDING: I think it would be interesting to see what possibilities there are. I'm sure that there are things that could be done that would benefit both groups, to have the same tests.

DR. POTT: I think Bill and I would both agree with that.

DR. HALSEY: We're interested in some of their tests which have shorter durations going in when they're all done, so you can see them tearing the rock apart and seeing what's happened to the chemistries and things like that.

DR. CORDING: Sure.

DR. HALSEY: You saw some of that reflected in John's presentation. They are looking at some of those processes.

DR. CORDING: One other question. In terms of the configuration of the perimeter of the tunnel, would you prefer to have a TBM-type mine surface or is drill and blast acceptable? What are your feelings on that?

DR. POTT: For the heated block test, that's --

DR. CORDING: I'm thinking more --

DR. POTT: -- the axi-symmetric heater.

DR. CORDING: I'm thinking more of when you're really trying to simulate a tunnel surface.

DR. POTT: The closer we can get to a real repository, obviously, the more you reduce the uncertainties. And it's not clear that we'll get a TBM. However, there may be some sort of compromise, such as drill and blast, and maybe machine the last bit or something like that.

DR. CORDING: Your feelings would be somewhat the same on that, Bill?

DR. HALSEY: Yes. The closer you can get to repository conditions, the better, but we'll take what we can get and we can probably -- if it's drill and blast, then we can use the surface or modify it.

DR. LANGMUIR: Pat Domenico.

DR. DOMENICO: Either Bill or John can address this. I'm looking at a slide here on the heated block test and that would be really the only information you'd have before going into licensing. It says here "controlled boundary conditions, fracture properties, rock mass deformation and strength, thermal expansion of rock mass." And then it says "model validation, coupled thermal/mechanical/hydrologic effects" for the heated block.

Just basically what are you going to measure hydrologically speaking and what do you anticipate to get out of the heated block test? I understand the need for all these mechanical properties. I'm just curious as to --

DR. CORDING: You're talking about the Fran Ridge block test.

DR. DOMENICO: That's correct, the heated block test.

DR. POTT: Okay. First of all, I think our tests will be -- our tests are geared, the three that I described, not the big drift scale, are designed to be completed in time to feed license application. We talked about complementary tests. So if you have a slide that shows tests, it doesn't show maybe all the Sandia tests.

DR. DOMENICO: I see. All right. Fine.

DR. POTT: There was a slide earlier that had, I think, all of them. Both of us are talking about tests that couple thermal, mechanical, hydrological and chemical. It's just that his tend to emphasize hydrological. Ours tend to emphasize mechanical.

DR. LANGMUIR: I think the question is for Bill Halsey really here.

DR. DOMENICO: I think the question might have been for Bill, then.

DR. HALSEY: Up to licensing, we intend to have the Fran Ridge large block test and some of the accelerated in situ tests from the ESF. That's what I showed the difference between what we expect to get from a large block test and the accelerated in situ test is primarily larger scale.

We will look at these processes at Fran Ridge on a few meters scale, but with well controlled boundary conditions and with three-dimensional characterization. What we get in the accelerated aggressive in situ test is a scale over a substantially larger size, many tens of meters, where we can generate condensate zones which may be able to drive fracture flow, look at buoyant convection gas flow over tens of meters.

DR. DOMENICO: My understanding is that prior to the license application, you will not have access to the ESF.

DR. HALSEY: I think that that's -- Bill Boyle's talk is how are we planning to get there between now and then.

MR. BOYLE: I will address that this afternoon. But we'll have access before then.

DR. DOMENICO: You will have access.

MR. BOYLE: If the TBM works reasonably well, we'll have access.

DR. DOMENICO: I have one for Bill having to do with the coupled process question. Obviously, one of the bigger issues we have is what is the linking here that's going to alter the permeabilities and porosities when you have these heat effects. My sense from Dale's talk yesterday and perhaps from yours as well is that you're not going to see those in the accelerated tests or at least you won't see them happening the way they might happen in the repository. It's too quick to see -- you might see precipitation, you'll not see the solution on the time scales of the accelerated four-year tests.

Is that pretty much how you'd read it for the block?

DR. HALSEY: No. I think we will see the processes. We may not see them to the time extent. They'll have somewhat different kinetics. But some of the regions that I was indicating in the viewgraph where we want to get some of that coupled process data, the geochemical data, is regions where you may have substantial water flow over a period of a number of years and then you can go in and see what has happened.

That's a slightly larger scale extension of some of the information that Dale talked about yesterday that will come from the large block test. We'll do it on a little larger scale underground. There are many meters of rock there that will see conditions that represent a significant time period in the repository. It certainly is not the same as we'll see in confirmation testing.

DR. PALCIAUSKAS: Vic Palciauskas. I'd just like to ask a question on your response. You're going to be basically sampling, at least thermally, about 10 or 20 meters away from the drift in your seven-year experiment. That's a lot of volume.

How are you going to make all those measurements, geochemical samplings over such a large volume? You're going to have to be very selective. Can you give me some indication about that?

DR. HALSEY: No, I can't, because I haven't designed those tests. Tom or Ardyth or someone who is familiar with the test layout and the instrumentation.

DR. LANGMUIR: Is this part of Bill's presentation this afternoon or not?

MR. BOYLE: No.

DR. PALCIAUSKAS: I mean, verifying convection in the surrounding region is not going to be exactly a simple thing to monitor, I think.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. I'm not the precise person to answer it. Wunan Lin has looked at this in great depth. However, we do intend to get a lot of vertical profile sampling in terms of the temperature profile. That profile, as Bill and Dale showed, could be highly diagnostic of how important a point convection is. We intend to get as good lateral definition as we can.

I think to think that one can run a smaller test for the sake of getting more dense sampling, you have to also consider the competing problem of heterogeneity. We're trying to conduct these tests large enough so that the relevant scale of heterogeneity is reflected somehow in the test.

Also, we found that we could not go to a small test because of edge cooling effects. We had to over-drive the temperature at the center just to offset the effective edge cooling. That's another competing problem. We also wanted to have enough of a target for ponding, if that's going to occur above the boiling region.

So I don't know if we'll ever get an idea amount of sampling. We're going to try to get through our analyses and looking at different types of things. Where the hypotheses break down, we're going to try to sample enough to see whether those hypotheses can be validated to the extent possible.

DR. LANGMUIR: One of the issues would be the permeability changes that would occur because of coupled effects. How can you measure that in the underground materials you've been testing? Using a pneumatic approach or what?

DR. BUSCHECK: Certainly with a pneumatic approach we could be measuring even during the tests and the perturbation when you're injecting gas during a test that's perturbing about four-tenths of an acre I think could be shown to be relatively small.

There are other tests that we can deploy. George Danko has remotely deployed thermal probes which can actually locally, in a very fine detail assess the importance of convection on heat flow. So there will be other independent means to locally look at conduction versus convection heat flow.

DR. HALSEY: If you remember the very large test, we had access drifts above and below with a great deal of instrumentation, cross-drift instrumentation above, below and between the drifts. We're trying to figure out just what can be done when you just have three drifts and you don't have access above and below. But you can do slant instrumentation holes above and below and do pneumatic tests, temperature tests, humidity tests, try and look for some of the fractures that are dynamic.

But that's one that I certainly don't know all the answers to yet.

DR. LANGMUIR: I had one last one for John Pott. I was concerned that the list he showed us was really the kind of a large shopping list I remember seeing in the SCPCA, the SCP site characterization activity. The acronyms get to be a problem after a while here.

Anyway, looking at the overall rock system as he is, I saw some familiar words. Tridymite and cristobalite have come up and concerns expressed throughout the flowcharts of what they might do to the rock properties. My intuitive sense is we're looking at secondary mineral phases and small fractures which represent a small volume of the total rock and the rock is beginning with cristobalite. The stuff you're going to start with is cristobalite. It's not going to be -- it may be also created up temperature.

But have you done some scoping calculations -- here we're on the envelopes again -- to look at these volumetric effects, which are small, in small fractures within the system and what they might constitute in terms of an effect overall.

DR. POTT: What we have done so far is laboratory thermal tests where we have small samples. But, see, on those small samples, we see large effects and that's what identified them to us. So if those minerals are distributed evenly, then they still would have perhaps a large effect.

DR. LANGMUIR: Isn't one of the biggest issues what's going to happen in a drift when you have eventually spalling effects if you don't have a backfill and you start dropping pieces of material down on the waste packages? I didn't see that as a consideration. I presume it's part of what you're going to be addressing.

DR. POTT: We sort of incorporated that in drift stability, things like that. That's the kind of thing we are looking at.

DR. LANGMUIR: Dennis Price.

DR. PRICE: What do you know now and what will you know more after these tests about the long-term intense combined effects of thermal and nuclear radiation on the rock and how important is it?

DR. POTT: I don't think we have addressed anything to do with radiation. I don't even know on the lab scale that we'll know anything in terms of coupled radiological, mechanical, thermal.

DR. PRICE: And how important?

DR. POTT: Right.

DR. PRICE: Right what? Is it important?

DR. POTT: Is it important?

DR. PRICE: Yes.

DR. POTT: You're not asking the right person. I'm only guessing the fact that we're not considering it. Is there some indication that it's not important? But I can't -- I don't know what studies have been done or what that would be --

DR. PRICE: The two primary characteristics of the source and its interaction with the host, to me, seems like it's the thermal and the radiation.

DR. POTT: Right. And the closest I've come is the effect of radiation on thermal expansion.

DR. GIBSON: Buz Gibson, M&O. I can answer at least partially from a prior life in the world of nuclear effects. The radiation levels in there, even cumulative over time, are going to have very little impact on the stability or the mechanical properties of the rock, even on the near surface. And you get very far back into the rock, and it will drop to virtually nothing.

DR. PRICE: With a potential for chemical changes, how about the processes and so forth? Zeolites or anything like that?

DR. GIBSON: Not very likely, simply because the rates associated with that will be dominated by the chemical rates and you won't see any impact as a result of the radiation. If you had very high dose rates that were at the level or high enough to compete with the rates at which the chemical processes are occurring, then the answer would be possibly. But they aren't.

DR. PRICE: And this confidence you have, is it based on thousands of years of radiation?

DR. GIBSON: It's based on very large doses at very high dose rates. In this case, we're having large doses, but spread way out over time. It takes extremely high doses to actually do much damage or see any mechanical impacts in materials like that and it takes reasonably high rates to have an impact on the chemical processes going on.

DR. LANGMUIR: We're just about on schedule. We cheated to do it. But let's take our lunch break and reconvene at 1:30.

[Whereupon, at 12:00 noon, the meeting was recessed for lunch, to reconvene at 1:30 p.m., this same day.]

AFTERNOON SESSION

[1:30 p.m.]

DR. LANGMUIR: Please take your seats. Our first presentation of the afternoon is underground test coordination. The speaker is William Boyle. As you may remember, Bill did not get a chance to give this before lunch. So he'll start our presentations now.

UNDERGROUND TEST COORDINATION

[Slide.]

MR. BOYLE: For those of you who weren't here, my not speaking until now is perhaps an example of the PPA, things being deferred or thrown over the fence. I'm going to give a talk on underground test coordination, about the steps that will be followed in implementing the activities that were described by Bill Halsey and John Pott.

[Slide.]

MR. BOYLE: I think this has already been mentioned more than once by a number of speakers and perhaps the most important point is the second one. I think Bill and John both mentioned this, Ardyth mentioned it, and I would say this is one of our highest priority items right now for these in situ heated tests in the ESF.

People can use different words. Here I used refining, modified, however you want to think of it. But will probably happen with the tests that have been described is there will probably be a consolidation, optimization, if you will, of the tests to get the information we need in a reasonable timeframe and for a reasonable cost, without duplication.

[Slide.]

MR. BOYLE: So here is what the rest of the talk is going to be about, underground test coordination. First, I'll talk about the components involved in test coordination, pre-test planning, test planning, test implementation. And then for those people who like graphic representation of information, I'll show a figure that links all those components in a process and then perhaps the more interesting part will be at the end, some indication of where and when for the test.

Now, I will tailor the talk a bit towards thermal tests, but this process of underground test coordination has been developed by the Test Coordination Office that Los Alamos has for the DOE on the project, and it's already been applied to the activities that have gone on in the ESF, the construction monitoring activities at Sandia, the USGS test. So the process works and it works well.

[Slide.]

MR. BOYLE: Here is an overview of the pre-test planning activities. Now, these activities -- in some part, it's serial in that items one and two will generally occur before five and six. But some of the activities also occur in parallel. And where I'd say we're at right now is in activities one and two, where we're trying to define the tests, determine which tests of those that Bill and John had referred to, how we might consolidate them, marry them, if we can.

But these are the steps in the process for pre-test planning and over the next few sheets I will say a little bit more about each of the steps.

[Slide.]

MR. BOYLE: As I had mentioned, this is where we're at now. We're involved in this first step and I would say as an estimate for these first two steps, which pretty much go on at the same time -- these are two of the steps that occur in parallel -- that by the end of this calendar year, we would like to have a very good idea of what it is we're going to do in our first heated test in the ESF.

[Slide.]

MR. BOYLE: After that, we would start interacting more formally with the ESF designers, let them know which design packages would be involved, whether it would be 2C or 8A or any of the other packages that they have. We would also start talking about the timing of the test support and those sorts of items.

[Slide.]

MR. BOYLE: All the tests that we do, all the activities would go through these two steps, if necessary. If the study plan needs revision, that will take place before any of the activities go on. If the study plan isn't in place and up to snuff, then the work will not happen until that occurs.

[Slide.]

MR. BOYLE: The ESF design requirements document, Appendix B, has requirements for each test. Each test has these items for it in Appendix B. And the capitalization and the word test doesn't mean anything. The spell checkers work fine, but the capitalization wasn't quite right.

As the tests are further defined, then Appendix B is further refined, also. You can think of it now, for those of you familiar with DOE's Title I and Title II designs, the information in Appendix B now might be thought of as at a Title I level and as the tests are further designed, this information gets up to more of a Title II level.

[Slide.]

MR. BOYLE: This was the last pre-test planning activities. We would have a conceptual facility design. We would either have a new design package or a revision of an existing one and we would start procurements. This step we would hope to have done in FY-95. When remains to be seen, but certainly before the end of FY-95.

[Slide.]

MR. BOYLE: Those were the steps of the pre-test planning process. Now here are the steps in the test planning process. Things are evolving even more. These steps also -- they haven't been applied yet for the thermal tests, we're not there yet, but these same steps that are followed for the surface based testing program, they've been followed for the activities that have already gone on in the ESF.

So this process is in place. It works well. We'll just apply it to the thermal tests. And I think you can read as well as I can speak as to what will go on and just bearing in mind that the test planning packages are a further development or refinement of what went on in pre-test planning and then the job packages are even more refinement.

The guess at time there is by the end of FY-95 or early in FY-95 we would hope to have those activities completed for the first heater test in the ESF.

[Slide.]

MR. BOYLE: Then the last step in this process is test implementation. The test planning packages and job packages are controlled documents. This is a slightly different document. That's what is meant by an administrative work plan. The test planning packages and job packages, they can be audited, too. QA can come in and ask you to prove that what you said you were going to do you were doing and you have the right documents.

This is more of a document that is a recipe, I think is a word that Ned Elkins used in his preparation of these materials. It's a recipe for getting the work done. So these documents would also have to be in place before any of the tests, any of the thermal tests went on in the ESF.

Now, when the tests will occur we're not totally in control of, the people in the testing community. It depends on the progress of the TBM. In that last viewgraph I'll show, you can get some hint of one estimate of where the TBM might be. But if the TBM goes faster than people think, then that puts more of a burden on Sandia, Livermore and scientific programs to get -- and Los Alamos and their Test Coordination Office to get all this paperwork in place and get the process completed before any of the tests go on.

[Slide.]

MR. BOYLE: Here is a graphical representation of what I have been talking about in terms of the steps that are gone through. Toward the very end, you have field test initiation and documentation. This is a handy reference for people who are concerned how does information get to the designers in time for them to design an alcove or take into consideration what are the electrical requirements and that sort of thing for a test.

There is a formal process. It hasn't been applied yet fully to the thermal tests, but it has worked well for the activities that have already gone on in the ESF.

[Slide.]

MR. BOYLE: Now, the last overhead actually has quite a bit of information on it and it has information on the schedule. I know that Russ McFarland was talking to me about it earlier today. Here are some dates and Ned has been nice enough to say where those dates came from.

The planned progress to this point is based on schedules developed for FY-95. It's actually on -- the official number is by September 30 of the next calendar year. The plan, the schedule is to advance 1,280 meters. They may go faster, they may go slower. I don't know if that's a conservative estimate, an optimistic estimate, best estimate. I haven't quizzed anyone. But that's the number in mind.

You can calculate a rate of progress for that point. We have another date down here that says 2,800 meters by February or March of '96. So roughly in six months time, you go 1,600 meters, whereas it takes approximately a year to go 1,200 meters. And some of that difference in rate is due to the fact that the conveyor will be installed during this timeframe, the mapping platform will be installed in that timeframe. There are three alcoves before 1,200 or right at 1,200 and before it, the Bow Ridge fault and the contacts related to the PTn unit.

Now, this gives an idea of where we can conduct some tests. What's been outlined on here from 1,200 meters to 1,700 meters is a zone that you can call the TSw-1 non-lithophysal zone. And one thing I'd like to point out is these thermal/mechanical units, TSw-1 and TSw-2, I think of them in terms of fruit baskets. There's all different kinds of rocks in there. We don't have a geology where we have a limestone against a granite or something like that.

There are parts of TSw-1 that are more like some parts of TSw-2, then some parts of TSw-2 are like its neighbors of TSw-2. So there has been some consideration of conducting the first heated tests in TSw-1. Some people might question the representativeness of rocks in TSw-1, but as I've just said, these rocks are actually quite variable.

People have looked at this and there are units in TSw-1 that look very much like units in TSw-2 in terms of lithophysal content, style of fracturing, mineral content. It may be an opportunity to conduct tests earlier, which is one big advantage TSw-1 has over any other test location -- we get there first. So that is actively under consideration right now.

Another thing we can consider is to get early access to TSw-2, that's what this diagram and this little stub shows. You can think of it in some ways as a north ramp stub instead of a north ramp extension, that this would not -- one of the purposes of the north ramp extension was to go across the block and you could get an idea of the structure.

Another purpose was to provide access for thermal tests. Well, you can still do that and what Ned has done is given us an example of how we might do it. This little stub takes off and goes up-dip, if you will, to minimize the distance required to get to TSw-2, and that's what this cross-section shows. At this point, they know that the north ramp is 20 meters above the TSw-1/TSw-2 contact. They're driving down at two degrees and the geologic -- the dip, they're going up-dip.

If they do it at two degrees, it takes 185 meters to reach that contact and as the little note says down here, if they were to go down at ten degrees, it only takes a 72-meter drift to get to the contact.

Now, once people get out here, it's possible to go farther and by doing so you're starting to get the vertical variability if you continue on or you could go along strike and examine the lateral variability and do any number of heater tests in that area.

I said it was a busy diagram, but I think I've addressed most of the information. That is the current thinking. And if things were to go well and we were to field our first test somewhere here based on that schedule, it would be perhaps by the end of next calendar year that we might have our heater tests started.

Any questions?

DR. LANGMUIR: Bill, I didn't see any discussion -- maybe it wasn't appropriate for you to do it -- of the size of the tests you'd need. Presumably, if you're going to do a low loading test, it's a different scale study, a different scale block than a high loading test. The phenomena are different, the rates at which they -- maybe this is for Bill Halsey. The rates at which they impact the rock block differ. The observation time scales differ and a whole host of things are going to be different about them.

So I would presume you couldn't just take the same block and do all three loading tests on the same block. Is that a presumption that's incorrect?

MR. BOYLE: I will address that. People have mentioned that we haven't gone through that last round of consolidating the tests or coming up with our final test plan. And I agree. Bill Halsey is the right person to ask, in some ways, and so is John Pott of Sandia.

The department is going to rely upon them and the

M&O to come up with a test plan that's going to work and address all the issues of concern for technical site suitability.

DR. HALSEY: Bill Halsey, Lawrence Livermore. The work that was -- well, Dale Wilder yesterday went through quite a bit of the thermal tests and the hypotheses that you need to test for both the higher and the lower thermal loading regimes and they are both addressed by fairly large aggressive tests.

So it's not substantially different if you're trying to focus more on the lower than on both. It's very similar to the size scale test that I was showing and that's the kind of test that we would like to run. Perhaps Ardyth or Tom could elucidate.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. I think as we're seeing in some of these local drift scale calculations that we could be seeing substantial dry-out. I think they call those an LED, localized extended dry calculations. I think that all eight of those hypotheses could be needed even for a low thermal load, where we found we were mobilizing a lot of water and creating some dry-out.

The first four hypotheses are applicable to even a theoretical low as well as the higher thermal load. So Bill is right. We really do need to run tests looking at the same phenomenology for both the high and the low thermal load.

DR. LANGMUIR: Ed Cording.

DR. CORDING: I was wondering in the tests for the -- the three-drift tests that Bill showed, one would be -- one series would be a high rate short-term and the other would be a lower rate. So are you talking about two setups, like what you showed in your overhead?

DR. HALSEY: Yes. It's also possible that we would like to go to an even larger test for the long term to get the larger spatial scale that I showed in the very large test, seven-year test.

DR. CORDING: And the drift size is on the order -- or the size of the tunnels that you're looking at, what sizes were you thinking of there? Just diameter.

DR. BUSCHECK: About four to five meters.

DR. CORDING: Then I had one question for you, Bill. If you had -- if the tunnel boring machine could be where you would most like it to be when you're ready, where would you like that? Where would you like to put the thermal tests? What would be your preference, assuming it could be done on your schedule?

MR. BOYLE: This is my personal preference.

DR. CORDING: Yes. Well, whatever preference you want to describe.

MR. BOYLE: But this is my personal preference and in our discussions and consolidation, I'd argue for TSw-1.

DR. CORDING: As opposed to?

MR. BOYLE: I would argue instead of waiting to get something that's more representative of TSw-2 --

DR. CORDING: No. I'm not talking about waiting. I'm saying don't -- I mean, that somebody could magically put you where you wanted to be in about a year.

MR. BOYLE: TSw-2.

DR. CORDING: Okay.

DR. LANGMUIR: Russ McFarland.

MR. McFARLAND: Bill, you made a comment with regard to Sandia and Livermore being ready to start tests based on the rate of progress of the tunnel boring machine. Would you explain that?

MR. BOYLE: It's just that it puts more of a burden on them and the DOE. If the TBM goes faster, we'd really like -- if we decide to test here, we want to be ready to go shortly after the TBM is there. We don't want to reach here quickly and then wait six months because we're not ready.

MR. McFARLAND: But what you're saying then is essentially, if I hear you right, that wherever you're going to build an alcove, you want to stop the machine.

MR. BOYLE: I don't know those -- I'm not involved in --

MR. McFARLAND: I don't understand what you're saying then. You could always excavate beyond the place that you're going to have the alcove. When the laboratories are ready to build their alcove, build it. I don't understand.

MR. BOYLE: But what I'm trying to get at here is there was a question earlier by Pat Domenico whether some of this information -- what information would be ready for license application. The sooner we get the tests started, the more information we have by the time of license application. That's the reason for starting it as soon as we can.

MR. McFARLAND: Thank you.

DR. LANGMUIR: Anymore questions from the Board?

[No response.]

DR. LANGMUIR: If not, thank you. Thanks, Bill. The next presentation is Steve Brocoum, giving us a summary of the DOE's presentations and approach at this point.

SUMMARY

[Slide.]

DR. BROCOUM: I was just going to summarize very briefly and also talk a little bit about our planning process. As we can tell from this whole meeting we've had the last few days and now getting ready, this is -- thermal loading cuts across the whole program, more than just about any other subject we have. We need to get Office of Waste Acceptance, Storage and Transportation, the project management. What do you guys call PMI integration, Ron Milner's project office?

We've been recently reorganized, both at Headquarters and out at the project and we're trying to improve our planning. Dan Dreyfus is a very strong believer in strategic planning and he's put us through a lot of strategic planning, and I want to spend a couple of seconds talking about that.

So I'm going to talk a little bit about our program, our five-year plan, our program approach. I want to quickly summarize the thermal management strategy and the options and show you a diagram that tries to pull it all together, how we think we're going at this point in time.

[Slide.]

DR. BROCOUM: At the last TRB meeting, we did give out our draft five-year plan. That five-year plan is volume two of a three volume document that we hope will be issued this year. The first volume will be kind of an overview volume. The second volume is what we gave out, at least in draft form, at the last meeting, and the third volume is the balance of the program.

This is our attempt to pull everything together under one programmatic document. You can tell from all the meetings we've had that this is a big job. The volume we've issued so far has our major activities for the program approach, has interim milestones. We're trying to focus the program more on deliverables. It tries to clarify the relationship of activities, products and budgets. It tries to show that they address suitability, NEPA, licensing, all management compliance. Every product has to have, in a sense, a customer.

[Slide.]

DR. BROCOUM: We're trying to implement not only a strategic planning process, but a better planning process for the project itself. We're trying to identify, as I said, our products. We're trying to do assessments to see what information we need for those products, products for the site suitability, the draft and final EIS, the license application, the annotated outline. We're focusing very much in those directions.

So that defines the information we need. We have our five-year plan, which is, in a sense, the long range plan. We'll have the annual plans. We'll have the tips, which will be out the end of this month. We do all our studies of deliverables. We have feedback and we have a planning process that will start about January of each year and lead to the next fiscal year's work to get us all these key products.

So we're kind of trying to unify the program, both at the project level and throughout the whole program.

[Slide.]

DR. BROCOUM: Obviously, thermal management is a key element. We're trying to phase our program suitability, NEPA, EIS and so on, licensing. So we need to have flexibility. We're trying to pick a thermal loading that will support for the NRC to find a reasonable assurance finding when we submit our license application, and we talked about all these things before.

And the important point to make here is the idea of a concept of -- we'll probably be on the low range of the thermal range that we can accommodate by design is a technical decision. It's a strategic decision as to how we could move forward. That's the important thing. There's a lot of debate, actually. I was hearing people say we think we ought to go high, we think we ought to go low. That's a technical debate.

Our decision to proceed the way we're proceeding is a strategic decision to allow us to keep moving forward and making progress. That's the point that has to be absolutely clear.

Obviously, we're trying to keep a flexible system design to allow us to take whatever will be the appropriate, from a technical perspective, thermal load for the repository. So in a sense, we're trying to demonstrate progress, on the one hand, and we're trying to keep from making decisions that will lock us up too early so that scientists can get the work done.

[Slide.]

DR. BROCOUM: A lot of options were discussed. Again, this is, again, looking at the program from a very programmatic view. A lot of these options up here on the age and burn-up and latch storage and repository storage, all of these have to do with Office of Waste acceptance. Buz Gibson talked about a possibility for arranging a fuel types repository.

We realized initially that the prime area, based on what we submit to the NRC, may not be able to accommodate all 70,000 metric tons. We need to consider expansion areas. We need to worry about the size of the MPC. We need to look at things like ventilation.

We've got to keep those options -- that's the whole purpose -- available to us. We don't want to preclude them before we've actually made the proper decision. And that leads to the tensions, if you like, that we have between the designers, who would like to know what their requirements are so they could move forward in the design, and the scientists and the regulatory people, who would like to have more time to think about these things.

[Slide.]

DR. BROCOUM: This is an attempt to try to put it all together on a simple diagram, which lays out some of the options here, which leads to a flexible design, lays out the key decisions we think we have to make in the thermal area, and lays out in three groups the types of thermal testing -- the short, the laboratory and the short-term, the longer-term in situ tests, and the really long-term test which would be the result of actually loading waste packages in the repository.

So we have kind of a continuum here where we start with a -- that's an NRC term again -- the maximum design basis. We would update this about the year 2008. We would confirm it and update it during the operational phase.

Whenever you put a diagram like this together, of course, you can't cover all eventualities, but it's an attempt in one diagram to capture where we are at this time today. Again, I have to emphasize this is a strategy and not a technical decision and that strategy can change as we get new information.

That's all I really have to say.

DR. LANGMUIR: Thank you, Steve. That's a lot in a hurry. Any questions from the Board?

[No response.]

DR. LANGMUIR: I had one that's maybe not substantive, but I noticed that some wording, philosophical wording differs in what I saw here than what I'm used to seeing from the DOE. That was that you state that the EBS is to inhibit radionuclide mobility and to compensate for uncertainties in the natural barrier performance as opposed to being redundant. We've heard on the redundancy that the defense-in-depth -- this is a -- compensation is a different argument.

It implies to me that you have less confidence that you're isolating things, that you have to compensate perhaps or you might have to compensate.

DR. BROCOUM: I don't think it was meant to necessarily imply that. But we do realize there will be residual uncertainties no matter how many studies you do of the natural barriers. But we also are trying to emphasize that during the operational phase and the early post-closure phase, we are putting a lot of allocation, if you like, on the engineered barrier system.

I think you mentioned you thought that was a -- that's been evolving now for several years within the DOE, even before the program approach of the more robust waste packages. So that's been a constant.

DR. LANGMUIR: John Cantlon.

DR. CANTLON: Steve, I'd come back to something we chatted about on your first presentation and that is as you try to maintain the flexibility by going in with the low temperature approach as a strategic way of keeping the process going, it seems to me that that has a degree of incompatibility with your program trimming of what data you must bring to that set of decisions.

And we mentioned yesterday the idea that if you have a low temperature, you need a lot more space and, therefore, you need to know more about a bigger portion of the block by making that choice.

The other thing, it seems to me, that goes in the same direction, the lower temperature gives you a much more difficult set of challenges in corrosion than the high temperature regime, which means that you probably want to look at the lower substrates below, because you're going to now be dependent on the natural barrier to a much larger extent.

Could you address what seems to me to be a disharmony there?

DR. BROCOUM: You're right and it does bring some problems or issues. On the space issue, there are different ways you can approach that. For example, your initial license application, and I think Steve Saterlie mentioned that the other day, may not be for the full 70,000 metric tons. It could be for what you think you could support at that time in the prime area, if that's the area that you have confidence in and you don't have the same degree of understanding of the expansion area.

That's a possibility. I'm not -- I'm just talking possibilities now. And then through time, as you update your license application, you can not only update to add expansion areas, you may update to reconfigure the existing planned repository or some combination of the two. It really depends on what kind of a thermal loading you think you can go to.

In terms of the corrosion problem, that is one of the issues we have to worry about because the designers say, well, we may need a different package if we're planning for a relatively low temperature repository. So then you may design a different sleeve or outer sleeve.

You may then -- if you decide to go high, you may have to incur a cost of pulling those -- it's conceivable you have to pull them out and change the sleeve. So that is an issue. We're aware of it. I don't really have an answer for you.

DR. CANTLON: Thank you.

DR. LANGMUIR: Any more questions from the Board or Board staff?

[No response.]

DR. LANGMUIR: Any questions from the audience at this point?

[No response.]

DR. LANGMUIR: We're scheduled right now to go to what we call perspectives and we have listed on the agenda the opportunity for someone from the State of Nevada to make a presentation, a perspective view. I understood that this was to be Steve Frishman, am I correct? Steve Frishman. Go for it.

PERSPECTIVES

STATE OF NEVADA

MR. FRISHMAN: I will do my usual perspective without viewgraphs. It makes life a lot easier. You're all getting very used to seeing my notes.

After listening for the last two days, I think I will do what I guess we've gotten used to me doing at the ends of your meetings, and that's giving you an idea of some of the things that I think about what I heard and what I hope you heard.

I also need to sort of think back to how long I've been involved in this program and, once again, it's absolutely obvious that the only thing that's standing still is the schedule. We've spent two days talking about plans for plans one more time.

On the subject of thermal loading, I keep listening to the things that Steve Brocoum says as the PPA evolves into the PA and now evolves into -- we're more flexible, we're still just designing the program and so on. Now, he just told you that the thermal loading decision for the technical site suitability decision is just a strategic decision. Well, we've also heard that the technical site suitability decision is just a management decision and ultimately the Secretary is going to have to have a decision under the Nuclear Waste Policy Act to recommend the site to the President, and that is a suitability decision.

I'm having a very difficult time with trying to understand the logic of this thermal loading strategy approach. We've gone through the different opportunities that are available, below boiling, some type of a mid-temperature that has boiling maybe around the package or trying to create a boiling temperature in the entire bulk of the system.

Well, first of all, if you remember, a couple of years ago, I boldly suggested that the waste package was going to end up driving repository safety. Well, it looks like that's what is happening. And one of the new things that we saw today that I had never seen before was in one of the presentations, it actually talked about impacts of the MPC. The MPC is something of a liability in this system because for all the flexibility everybody seems to be looking for, the MPC is locking you into things. Like it's virtually impossible -- unless you want to wait a lot of years, it is virtually impossible to have a below-boiling system.

It's going to boil around that package anyway if you operate on the schedules that you claim that you want to operate on. So you have already knocked a major piece of flexibility out of the repository system, where safety is paramount, to take care of a political problem that the Department of Energy has. And I say that that's a very bad decision at this point. It has put you in a position where -- or put all of us in a position where we're going to have to, first of all, except for a technical site suitability determination in 1998, going to have to accept the concept of a cold repository, even though it's not going to be cold.

We're going to have to accept the strategy that says we'll be -- with increased information, we're going to be looking to make it hotter and hotter, and who said hot is good. There's an objective to make it hotter and hotter, meaning to move away from that cold boundary. But we also have heard and we've heard even more boldly in other presentations than what we heard here in the last couple of days that the midrange looks to be the most risky, most risky in terms of you have failed waste packages and reflux.

So if you're going to sneak up on a warmer and warmer repository, you're going the wrong direction. If you're going to start out saying that it's going to be cold, even though it's not, and want to go just a little bit towards hot, then you're actually, from what the presentations say, you're actually creating more problems and you're setting up a system where you have less and less certainty in what the release rate is really going to be, because you don't understand the flux system and probably never will because of the extent to which the site is disturbed, meaning cut with faults, cut with fracture zones, and the whole other suite of uncertainties that are involved.

So I think this strategy for thermal loading doesn't do what Steve has told us for -- ever since the beginning of the proposed program approach -- has told us is the end point of the site suitability process. The end point of that is, as he put it one time, the Secretary's recommendation of the site for a license application is the Department's safety decision.

As he's put it, we have a management decision in 1998. In the year 2000, we have a safety decision by the Department of Energy. Well, I put to you is there going to be enough information out of this thermal loading strategy to make a safety decision in the year 2000. I say no.

Now, there's another piece that has not been mentioned. We've been hearing the discussion of the models. We've been hearing the discussion of a bulk thermal loading. Well, we have to remember -- and it is mentioned occasionally, but I think its ramifications aren't really appreciated, at least in the modeling at this point. You have to remember the extent to which this site is inhomogeneous. We've got the Ghost Dance fault that is probably a 1,000 feet wide fracture zone and there is good evidence that at depth it looks pretty much the same as it does on the surface.

We've got another fault zone running off to the northwest that has similar dimensions. If you add up -- if those are of that dimension at repository depth, add up the area involved and you're looking at about a 20 percent decrease in the entire size of the repository footprint and it cross-cuts.

So now how are we going to deal with looking at bulk thermal loading when we know that we have a couple thousand foot-wide stripes taken out of the system, where if we can believe the earlier thought that they're not going to be used for emplacement areas. What is that going to do to the thermal management? You're going to be managing some very odd size pieces of rock and you're going to have them broken by what may, in fact, be hydrologic conduits, what we certainly know are unlike the rest of the rock in terms of vein filling and so on.

So you're not managing a repository block. What you're doing is managing a bunch of blocks with some things cross-cutting or between the blocks that you don't really understand and probably never will in terms of their hydrology and how it affects the rest of the area under Yucca Mountain. We're not talking about a block.

So I think if the modelers want to continue with the pretty pictures that we've been seeing, we need to start looking at some of the realities of this site. The major realities are the breaks in the site that keep it from being a block, even though for years it's been referred to as one.

Now, I guess a process question for the Board. Given the site suitability evaluation process that you've had presented to you, given what we now see as a strategy for making a thermal loading decision and what we think will be known and not known at various stages through that strategy, how is the Board going to satisfy its charge to comment on the technical validity of the Department's work when the Department begins pushing out these technical basis documents?

How are you going to be able to tell the Secretary, tell Congress, tell us in Nevada what you think of the validity and the quality of that technical basis report when the process is set up in such a way to where you have a statutory duty to report, and I know you have your twice a year reports, we have -- we, the State of Nevada, have a statutory duty to oversee this program and weigh in on a technical basis on suitability first, forget licensing for the time being, just suitability first, because that is the Secretary's safety decision.

I think it's something that you need to start thinking about. I've been thinking about it in terms of how the state is going to be able to evaluate those technical basis reports, how we're going to be able to deal with what is becoming a more and more broadly embedded reduction in the amount of information that all of us were led to expect at the time a suitability determination would be made.

I think it's something that I would like to have the Board at least think about how they're going to handle their end of that. We're not sure at the state level how we're going to handle our end of it. But I think it's important for the Board because your reports every six months or so certainly are not going to be in sync with the schedules that you've seen.

You're certainly not going to be able to keep up with the fast pace of the topical technical basis reports that, at least if the schedule holds, are going to be dumped out. And if you don't weigh in on the technical basis, where are you going to? It seems to me that that's the first place that you need to.

So the site suitability evaluation process has put us all sort of in a new world and in the process of that, to repeat myself somewhat, some of the major decisions that need to be made that might make the site suitable or not are essentially deferred or they're hidden under the name of strategic decision.

So I guess I'm becoming more and more concerned that the program is setting out to build a repository, solve whatever problems come up at the time they need to be solved, and the concept of making a safety decision is sort of being put off. Ultimately, it's being put off to what do we need to do for licensing and now what we're seeing is a licensing scheme that I don't think there's any precedent for with the Nuclear Regulatory Commission.

And the Nuclear Regulatory Commission has not said that this phased licensing scheme is okay. I've heard what they've said and they have not said that it's okay.

So we're in a situation where the Department is going to make decisions, whether we think they're good or not. They're going to lead to a license application, whether the NRC thinks it's good or not. And we're never going to quite get to what we all expected when some of us who were involved in writing the Waste Policy Act in '82, what some of us expected.

What we expected was an orderly scientific process to be able to get to what we considered a reasonable scientific decision, because we at that time believed the geologic isolation was probably possible. We're just not going to get to that. What we're going to get to is something underground that goes on for a hundred or more years while people are deciding when to slam the door; not whether it's safe, but when to slam the door.

As you can tell, my mood is deteriorating fast because the program is sliding out from under -- sliding out from everything that I think we all believed it was supposed to be when we started out with the Nuclear Waste Policy Act.

And this thermal loading decision, which I think is maybe one of the most critical in the whole program, is now being relegated to a strategic decision at just the time when a decision is going to be made that it probably puts us on an irreversible track. And I'm not sure that the information is there to make any decision, but as I said to start with, the only thing that is standing still is the schedule one more time.

I'm sure there are questions.

DR. LANGMUIR: Bill Barnard. Thank you, Steve.

DR. BARNARD: Bill Barnard, Board staff. How does the state view the site suitability decision? How important is it, things like that? What does it mean to you?

MR. FRISHMAN: Aside from the way the Department has dissected it in their technical site suitability decision, the site suitability decision at the end of site characterization as laid out in the Act is probably the most important decision that's made in the entire effort for geologic disposal, because you have the Department of Energy, which is a proponent of a project for geologic disposal, you have the Secretary of that Department making a policy decision for the nation.

And it is supposed to be an accountable policy decision. Site suitability is the scientific basis for the Secretary's decision. If the Secretary makes a decision that is not scientifically based, as required by the Act, then the Secretary is vulnerable, first, to a lawsuit for an arbitrary and capricious decision, which, if things go as I see them going, it's not unlikely that the State of Nevada could win a case like that and that would set the country back not the 16 years that we're behind now, but probably to 32.

So, yes, I see that as the key decision because lets face it, if this goes to a license application, it is unlikely -- in fact, it's probably impossible that it would not be licensed. The Nuclear Regulatory Commission licenses things. So it's the most important decision and the most accountable decision in the entire process, I think.

DR. LANGMUIR: Steve Brocoum, you didn't raise your hand, but would you like to comment?

DR. BROCOUM: There aren't many times that Steve Frishman and I agree, but I think we both agree that the site -- I always say that the site suitability decision is the most important decision DOE will make.

I just wanted to make one comment, if I can. To get to the technical site suitability, we're going to go through very many smaller decision points. We're going to make findings on all the guidelines. Only when we make findings on all the guidelines can then the Director make a decision on technical site suitability.

So it's a very elaborate, as you understand, process. We've worked with the external parties to develop it. We're finalizing it. We've had numerous meetings. There are several meetings planned for December to explain the process and answer questions for the parties and then we're embarking down this road next year with our very first technical basis report, which goes to the National Academy and to the involved parties for peer review.

But the formal decision from the Department of Energy is a decision that the Secretary makes when she sends her site recommendation report to the President of the United States. I'm just trying to make that little distinction. That's the formal decision. I think Steve also characterized it correctly there.

MR. FRISHMAN: I think, among other things, that points out the urgency of the question that I have laid on the table for you as a Board to try to figure out how you're going to deal with the site suitability evaluation process and, first, how you're going to deal with the technical basis report when none of us even yet know what a technical basis report is going to look like.

DR. LANGMUIR: I might just say the Board is concerned about that and we are internally looking at those issues and trying to resolve for ourselves where we think things need to be.

Thank you, Steve. Are there further questions? Steve Brocoum, again.

DR. BROCOUM: One more comment. Last -- it must have been February, we had that one-week technical review. We're planning a technical review for about a week in the middle of the February. Last year was very much from the principal investigators' perspectives. This year, we haven't finished planning it, but we're actually planning it about the technical basis reports.

So what we expect in them and what the principal investigators who are involved in the program think they --how they can think they can give that information that we're expecting. So that's how we're planning. I think it's the week of February 17, but I don't have a calendar in front of me.

So we are planning that what we call a TPR, technical project review, program review.

MR. FRISHMAN: And that will be the managers actually saying what they think they can put in a report or what they think the report ought to be?

DR. BROCOUM: That will be the people who are responsible in the regulatory area for defining what they think should be in those reports and it will be for the scientists, through the principal investigators, telling us how they think they can get us that information. It will be trying to increase that communication between the scientists, on the one hand, and the people that are trying to decide what we need to make those various findings in 960.

MR. FRISHMAN: It seems to me that with all the time that has gone into this program and will be going into it, at least if we believe what we've heard in the last couple days, it seems to me the technical basis report ought to be separate from any regulatory consideration. It ought to be the Department putting on the table everything it thinks it knows about that particular topic relative to the site rather than filtering it for some type of a manipulated regulatory determination. Ask the scientists what they know.

MR. BROCOUM: That's correct, but we're trying to get everybody to work together to define what we think should be in them and let the scientists help fill -- I mean, the scientists are going to provide all the data from which these reports are built.

MR. FRISHMAN: I guess I'm just suggesting that it should not be regulatorily driven. It should be driven by what the scientists think they know about the site and don't know.

DR. BROCOUM: Unfortunately, the program is regulatorily driven.

DR. LANGMUIR: Thank you both. This next perspective I believe is John Kessler with the Electrical Power Research Institute.

ELECTRICAL POWER RESEARCH INSTITUTE

DR. KESSLER: Now for something completely different. I guess I will start off philosophically. When you're trying to dispose of a waste that has a thermal kick to it, thermally generating waste, you're bound to disturb any geologic site no matter where it is. If it's Yucca Mountain or somewhere else, you're going to cause a thermal disturbance. I think we need to just remember that as we go on.

So if we look at the thermal loadings that we're talking about and if I wanted to take the perspective of comparing it to an undisturbed site, from what I heard at the ANS session on Wednesday, the lowest thermal loading that DOE is thinking about is 100 times that of the natural thermal gradient that's occurring at Yucca Mountain. The highest one is only 500 times. So we're talking about an order of magnitude increase for any of these low -- the low, the reference or the high scenarios.

So we should really be thinking of them not necessarily as low, reference and high, but as higher, a bit higher and a bit higher than that. They all have some influence on the thermal loading or certainly on the site, rather.

[Slide.]

DR. KESSLER: So if we're going to disturb the site, and that's a given and I think we should all start from that, we know we're going to disturb any geologic site we come to, what should be our decisions then. Now, I put these comments together before I went to the ANS session and listened for two days and I was rather concerned that DOE had made the decision to go low and stay low compared to what was in the SCP.

I think that some of my concerns are still pretty valid after listening to three days of discussions. I realized that what they're talking about is a program approach and not a final decision on site loading. But my concern about corporate memory with DOE is such that if they make this early decision to start low, they need to leave themselves some breathing room. So I'd like to talk about their reduction in the thermal loading from what they had in the SCP and a few concerns that gives me.

[Slide.]

DR. KESSLER: We think that the uncertainty about the effect of thermally driven processes is certainly the focus of significant DOE and NRC time and effort, as we have heard. We're also, though, creating an assumption that lowering the thermal loading will automatically reduce uncertainty and, therefore, ease the licensing process.

EPRI is concerned that this assumption hasn't been adequately tested. There's just this gut feeling it will ease the licensing process, but how do we know that that's really it. If that's driving DOE's whole approach to how they're going to enter into licensing and what way they decide to run the program, we need to make sure that this major assumption is true.

So a decision to lower the thermal loading may be a strategy with little positive benefit if it doesn't really ease the licensing process, but it also may significantly reduce system flexibility.

[Slide.]

DR. KESSLER: So can DOE demonstrate and ultimately NRC agree that a low thermal loading or a lower thermal loading will really reduce uncertainty? Well, the issues. Are there a number of thermal mechanisms reduced? Well, we've heard not much. Active boiling may be gone if the maximum temperature is below about 96. Is active boiling really a concern at any thermal loading? We've heard a little bit that it is, but I'm not totally convinced.

What mechanisms does it have that aren't generic to vaporization? We really only heard one. Personally, I think there is nothing magical about 96. We keep hearing about worrying about going above or below boiling. To me, you just have a steady change in the effect and the input of processes. This arbitrary mind game of 96 I think is just that. We need to get over that.

So for a repository in the unsaturated zone, is staying below boiling really important or does it just sound good? Others remain, that is other mechanisms remain, although their magnitudes, admittedly, are going to be lower. Vaporization and condensation, vapor and condensate transport, all the ones we've heard, thermomechanical stresses, geochemical alteration.

[Slide.]

DR. KESSLER: So if all of these thermally-generated mechanisms can't be eliminated, how is modeling going to be any easier? NRC, in Mal Knapp's TRB presentation to this panel in July, said that his experts said that modeling would be easier. It would be nice if the basis for that claim was documented. I think that would help the program a lot if we understood very clearly why NRC thought that.

The dimensions of the thermally-altered zone may be reduced. So, yes, this could potentially lead to a more manageable model if we don't have to model so much or somehow it makes that modeling easier. But we don't even know what the definition of a thermally-altered zone is to be able to bound that problem.

[Slide.]

DR. KESSLER: Will lowering the thermal loading cause a reduction of uncertainty in the long-term hydrothermal behavior? Well, perhaps, if it could be demonstrated that this approach eliminates the need to evaluate rather complicated coupled processes. Some examples might be this existence of a condensation cap. Yes, you might be able to get rid of some of that. I would argue you'll still have a condensation cap even if you're below boiling in some cases.

The way the cap operates will certainly be different, but it's still there. You still have to worry about it. The dry-out zone causing flow and diversion or shedding is certainly something that might go away. No thermally-driven flow in the saturated zone is another possibility of something that might be reduced, but I really haven't heard much about DOE not worrying about it if we go to a low thermal loading.

I keep hearing they're going to keep worrying about everything no matter what thermal loading they choose at this point. Therefore, EPRI is skeptical, without some demonstration by both DOE and NRC, that really lowering the thermal loading will ease the licensing path. So I'm not sure what we're gaining here by considering going to a lower thermal loading.

[Slide.]

DR. KESSLER: Selection of a lower thermal loading may have -- probably will have a negative effect on the current MPC design. We've already heard, I believe, Steve Saterlie and Buz Gibson describe that impact. The current MPC designs at the worst will require an abandonment due to the relatively high thermal output or at the least it will disrupt repository system planning regarding their use by requiring much longer surface storage prior to disposal.

In either case, we have this definitive negative impact by going to this lower thermal loading. As we also heard, it also calls into question the ability to meet the 70,000 metric ton equivalent maximum inventory since more storage area will be required. That opens up a whole can of worms that I don't even want to think about at this point.

[Slide.]

DR. KESSLER: We've also heard -- I think it was in the ANS session on Wednesday. Dave Stahl said that, well, if -- there was some discussion that we've heard here also about that when you look at the PAs, comparing different thermal loadings out in the longer timeframes, the difference between any initial thermal loading disappears. So then the question is, well, if that can't sort out what thermal loading you would choose, what are you left with.

Well, you may be left with the substantially complete containment requirement that's at about a thousand years. The question was asked, well, can DOE demonstrate substantially complete containment easier for high, reference or low thermal loading and Dave Stahl said, well, from a materials standpoint, the only thing that we think we could demonstrate by 1998 is the high thermal loading; that microbial-induced corrosion is an issue for the lower ones that we really don't feel we could nail down in that timeframe.

So that would argue high. I'm not saying I'm arguing high, that that's the way to go. I'm just saying that the emphasis on low and the idea that that may be the easier licensing path is not something I agree with, at least not today.

So as of today, reducing the thermal loading at Yucca Mountain may not significantly ease the licensing process, but it will certainly have a negative effect on MPC strategy and the repository area required. Therefore, an early decision to drastically lower the thermal loading is premature.

A couple other issues that I'd like to talk about. Again, the idea that one of the things we haven't heard much about is what if EPA comes out with a dose-based criterion that has no upper time limit and DOE, in their PA calcs, and EPRI has also shown that that thermal loading issue really starts to disappear. Again, what we may be left with to sort out what we need for at least a container design is a substantially complete containment.

So if we're going to a dose-based standard -- now, all of these containers we've looked at at all of the thermal loadings are eventually going to fail and it's unlikely that we can even determine whether different thermal loadings will cause differences in either the number of containers that are ultimately leached of their contents or the rate that this happens.

So I bring this up as a point as to where does DOE need to be making its emphasis if that reg should be changing. I don't know whether DOE has any plans or is currently thinking about how they might want to change their program if there is that kind of a change in reg.

I think that's about all I want to say in my ten minutes here today. Questions?

DR. LANGMUIR: Thank you, John. Any questions? By the way, we're going to be convening the roundtable shortly and several things that John brought up are appropriate issues for the roundtable to address. So we could wait till then or if you have some pressing questions right now, please feel free. Bill Barnard.

DR. BARNARD: John, this is sort of a point of order more than anything else. You work for the Electric Power Research Institute.

DR. KESSLER: Right.

DR. BARNARD: That does research for various electric utilities and groups of utilities.

DR. KESSLER: Right.

DR. BARNARD: Are you representing a utility viewpoint, nuclear utility viewpoint, or is this your own viewpoint or is this sort of an EPRI institutional viewpoint or what?

DR. KESSLER: Yes.

DR. BARNARD: I appreciate the clarification.

DR. KESSLER: We are funded exclusively by electric utilities to conduct research on their behalf. We're not funded by all of them. Certainly, you won't get all of the electric utilities to agree on some of these issues. But I think you would get all of the electric utilities to agree that they want their money spent wisely. They'd like their fuel taken care of in a rather quick fashion.

We view that some of the approaches that DOE is taking would not cause that to occur. So I think in that sense, I'm very safe in speaking for all utilities.

DR. LANGMUIR: Any more questions?

[No response.]

DR. LANGMUIR: The agenda shows a slot here for the perspective from an affected local unit of government. No one had spoken to me before the meeting or this morning about that. But would someone like to make such a presentation?

[No response.]

DR. LANGMUIR: If not, let's take our break and reconvene for the roundtable at 2:50, if you would.

[Recess.]

DR. LANGMUIR: Please take your seats. Those who are scheduled to be at the roundtable please come in and take your seats. Please be seated and let's start.

ROUNDTABLE DISCUSSION

DR. LANGMUIR: Before we begin general discussion, Ardyth Simmons would like to make a point of clarification with regard to the testing that she presented to us earlier today.

DR. SIMMONS: Is that the point that we were just talking about?

DR. LANGMUIR: I don't know. I was told you had something to say.

DR. SIMMONS: Well, there may be several things. I wasn't sure which one was being referred to. The question came up earlier as to when the effort that Bill Boyle was discussing as far as evaluating the planned ESF tests and looking at their potential consolidation and meeting information needs and so forth, when all of that would be completed and how that would relate to the review effort that I was referring to in my presentation that had to do with the thermohydrologic modeling and testing.

Bill pointed out that the review effort that he is going to be looking at will end in calendar year '94. So that's a very short-term effort, like six weeks. The effort that I'm looking at, at least the internal part of it where we write this white paper and we assemble all the information that's been done on thermal hydrologic testing and modeling and the plans for the future, that we hope to have finished by January '95.

But there are two somewhat different efforts. It's very clear that they must be coordinated. But he's looking at a bigger effort with all of the ESF tests, not just the ones that have thermal conditions associated with them.

So the plan that Bill and I have is to maintain close communication, especially -- our offices are just two doors away and a lot of the same PIs would be involved in both efforts, and we'll make sure that we have the same information used for the review effort that he's doing and the one that I'm doing. They're not duplicative, but the information on the thermal hydrologic models will certainly be of assistance, I think, to Bill and his group in taking a look at the configuration and the test design for those tests which are part of his overall review.

Maybe you want to add to that, I don't know, Bill.

MR. BOYLE: Not much, other than to state that it really will pretty much be the test with the heat for the time being in the sense that if you look at the deferred and non-deferred alcoves, the only other tests that are currently planned, other than the heater tests, are the non-deferred alcoves. Those are the tests of the USGS in terms of hydrochemistry and radial bore hole tests. And I think they will continue to be in the schedule.

DR. LANGMUIR: Let me shift gears here, if I may, unless there is more to pursue on that. Todd Rasmussen has offered to give us a little presentation on the Apache Leap site and I think we'd all like to see that. This is another situation with tuff, unsaturated materials, which hopefully will provide some insights for us with regard to Yucca Mountain. Todd Rasmussen.

[Slide.]

DR. RASMUSSEN: This type of meeting is new to me. I'm usually more comfortable in the technical environment. But I think some of the field data might be of some relevance to this problem. What we had was tuff heater experiments. It was part of an INTRAVAL validation program. INTRAVAL is an international program. A number of countries are involved in this. We're testing geosphere transport codes related to field data experiments.

We're trying to build confidence in field data sets and the ability to collect the data, as well as to interpret the data.

[Slide.]

DR. RASMUSSEN: So we had a number of different experiments that we looked at from both the data collection, monitoring and interpretation. We had a core scale heater experiment in unfractured, variably saturated. It was a coupled thermal/hydrologic/geochemical or chemical experiment where we demonstrated a significant heat pipe for that reflux, and that was over a range of 15 degrees to 45 degrees.

The point is that you don't need boiling conditions to get some of this and as somebody pointed out earlier, it's hot, hotter and a little more hotter. What was intriguing was the coupling was solutes in the sense that if you had a hot end of the core and a cold end, if there were no solutes in the system, the hot end dried out and the cold end got wetter and you had a boiling phase or a vaporization.

I don't know where boiling comes from. I think it's just vaporization at this end and condensation at the cold end. But you would essentially knock off most of that refluxing by just incorporation of solutes. We would see a significant increase in solutes at the vaporizing front. That would lower the vapor pressure and you would change that whole environment. So there is coupling here that you really do need to consider in any of this reflux.

That was modeled successfully. We have a field scale experiment where -- well, several versions of this, actually. We have one in fractured, variably saturated. That was a coupled thermal hydrologic and a heterogeneous material. We learn some thing every time we do this.

One of the interesting things is near the heater bore hole, we had an extent of empty bore hole and we had water, free water accumulate in that monitoring bore hole. So the point of this is that if you have any kind of void at all where you have a thermal gradient down the void, you're going to collect tremendous amounts of water at the end of that bore hole.

One of the big things we noticed was that instrumentation was extremely difficult to maintain under these kinds of environments. The corrosion, the thermal perturbations means that even with triplicate emplacement of sensors, that you ended up losing most of them. It's a rough environment to monitor.

Also, on the field scale, we had modeling by Charlie Voss up at Golder who showed that for a range of conditions, we could get anoxic conditions. The oxygen would be driven off by the vigorous vaporization of water that would force the air out away from it. So we would end up in low permeability zones. We were getting -- showing that anoxic conditions could be maintained at least temporarily. That was due to the increased total pressure driving out air and replacing it with pure water vapor.

The other thing we noticed was the dynamic changes in thermal mechanical properties. We had fractures that would open and close. That was generally consistent with what they found at the Climax mine. I think Dale Wilder mentioned that earlier.

And the implications, maybe just quickly, on waste management are that if you have two canisters and you put the coldest one in first, that's the oldest fuel first, then if you were to come in later and put hot fuel next to it, you would be driving a lot of the vapor and the cooler fuel cell would end up being a cooler site. And so you may actually increase tremendously the amount of condensation on your older fuel.

It may be better to reverse that and put your hottest fuel in first, drive out the water vapor, bring the cooler fuel in later. I think there is some scenario analysis that ought to be at least examined for fuel management in terms of opportunities for getting different results from this system that you wanted.

[Slide.]

DR. RASMUSSEN: There are just a couple of findings I just wanted to summarize very quickly. These are lessons learned, let's put it that way. Currently our field data collection focuses -- these are the kinds of things we can measure in the field. We can measure temperature, gas composition, the forces, the rock mechanical forces, displacements, maybe a few other minor things. But we need to interpret these data. We really don't have direct measurements, able to measure or monitor hydrologic or geochemical processes.

You'll notice that the things we can measure don't include any hydrologic variables. Unsaturated and --

fractured and unsaturated -- I have to qualify this by saying that that's in fractured unsaturated heated geologic media.

[Slide.]

DR. RASMUSSEN: To make a point here, it could be that there are some extremely high tech prototype tools that might be able to get around this on the edges, but we cannot measure content. We can measure water potential. We cannot measure water chemistry, water fluxes and fractures. That's just not possible at this time.

We cannot measure water content, water potential, water chemistry or water fluxes in unfractured rock at elevated temperatures at this time. We can't measure any of the variables we'd like to put in our models. We measure temperature and from that temperature we try and infer what the water content distribution is.

We're measuring the surrogate variables and then we're relying on models to tell us what our other primary variables are. So one of the limitations we found early on is just monitorability. We cannot measure water chemistry in situ in fractured unsaturated media.

The bottom line of all of this is that it is very hard very early on. We need to develop tools that allow us to obtain the information that we needed for performance assessment. They do not currently exist for anticipated conditions.

So I would say it's strongly driven by the need to develop tools.

[Slide.]

DR. RASMUSSEN: I borrowed this overhead of the proposed ESF facility and they have a number of, I guess, geochemical and moisture sensors. I'm real curious about the neutron logging. We found it wasn't possible at elevated temperatures. Temperature is your best data. There are some -- I guess these are the nuts and bolts that I'd really be interested in. What are the uncertainties in these new prototype development tools? We talk about modeling uncertainties.

DR. PREUSS: What is a geochemishial?

DR. RASMUSSEN: That's probably why he didn't show it.

DR. LANGMUIR: Blame the DOE for that.

DR. RASMUSSEN: This is not mine. I mean, geomechanical we can handle, temperature we can handle, but all the hydrologic, there's tremendous uncertainties in calibrating and just maintaining that type of equipment.

[Slide.]

DR. RASMUSSEN: Just very quickly, my last slide is on a second major finding we found is that air moving through the system had a profound effect on the whole process. If we were to be able to control the ventilation of the repository, we would -- and our objective, say, was to maximize the dry zone by increasing advective gas flux out of it, Ed Weeks at Yucca Mountain had shown that just breathing bore holes could dominate the natural hydrologic environment.

Essentially, those breathing bore holes allowed vapor to be advected out of the mountain. If you were to engineer the ventilation system to remove all of the water vapor that's liberated due to the latent heat changes, you would never have refluxing. You would never have weeps, you would never have seeps.

If you can remove that, and I would argue it's fairly straightforward to remove most of the water vapor from the repository horizon just through ventilation, you've lost the hydrology problem. The hydrology problem has gone away. You may still have rock mechanical problems, thermal transport, but I would argue ventilation can be used, extended ventilation can be used to remove heat, as well.

The one problem is that our models won't let us predict this. The fracture transport, the porous media models, aren't going to work if you're going to be looking at ventilation through dual porosity materials. We need better fracture -- you just -- porous media models don't work for that.

So we need to get away from models that ignore fracture flow mechanics. And as I said, the reflux and re-wetting problems would be -- could be minimized or mitigated by extended ventilation of the process.

In terms of the bottom line for thermal management, I think that we can't collect the data you need to answer that question, but there are other ways we might be able to do an end run around it so we don't need to collect that data, if we can manage the water in other ways, other than -- I think going in and trying to live with the system the way it is, I would suggest that there's a better way of doing that.

DR. LANGMUIR: It sounds to me like we have lots of issues to comment on and discuss here. I'd love to hear from the DOE folks, maybe Bill Halsey perhaps or Karsten Preuss. What do you think of the problems, for examples, the problems -- are you aware -- I'm sure you are, Karsten, and maybe you are, Bill, of the INTRAVAL program, for one.

I mean, how much interplay do we have of this sort of science internationally? I know DOE goes to these meetings. I'm not aware that this feeds back into the design of the test work that DOE is into now. I presume it does. That's one issue.

Another is the measurability of things in these in situ tests. We could go on from there, but that's a start perhaps for whoever wishes to respond.

DR. PREUSS: Karsten Preuss. Just a brief response. We are involved in various international cooperation agreements with Switzerland, Sweden, Canada, who have provided us a timely access to the subsurface, and there is very intense development of instrumentation that is going on and we're participating.

I think I agree with Todd that we have a way to go, but I think that this development is also proceeding quite rapidly.

DR. CORDING: That is in regard to the instrument development.

DR. PREUSS: Yes.

DR. LANGMUIR: Where do you stand currently with the planned instrumentation, for example, in the in situ ESF tests, Bill? Do you need to do some of the things that Todd said he couldn't do or will you be able to?

DR. HALSEY: I loaned him the viewgraph that Dale didn't have time to speak to. I believe it's in his package. I agree in part that some of these measurements are difficult and that the instrumentation is still evolving. But many of those measurements can be made, have been made. Some of them are made partially by indirect methods of humidity measurements, saturation measurements.

We can do neutron measurements at fairly extreme conditions, things like that. There are complex -- it does get more difficult. For example, at G-tunnel, we tried out some of those instrumentation techniques and some of them worked and some of them worked for a while and then failed. We learned from that.

So, yes, it's an issue, but I don't believe that it's quite as bleak as was indicated. In terms of the modeling, I agree. We are trying to develop models that deal with focused flow, where we have issues that are dominated by focused flow, and use equivalent continuum models where the processes can be adequately represented.

Going between those is a difficult and evolving process. I think that some of the previous presentations to the Board on some of the model development have indicated some of those evolving processes.

DR. LANGMUIR: Ardyth?

DR. SIMMONS: I'd like to add just briefly to that. One of the major objectives of the large block test that we're conducting at Fran Ridge is to do the instrument calibration and testing that we would be using in the in situ tests underground. We expect that we will learn a lot along the way.

We'll probably have some surprises, but the instrument testing for that large block test is being conducted this year. We have instruments designed that will go into that. I think it's our anticipation that we will be able to obtain that data.

Perhaps Tom Buscheck could talk specifically about the various kinds of instruments. But I know, for example, with regard to geochemistry, we were going to use something called a SEAMIST system. There are various different kinds of instruments available. It's a challenge, no doubt, but we think that in time we'll be able to get there.

DR. BUSCHECK: May I make one short comment?

DR. LANGMUIR: Yes. Short comment?

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. I don't want to give the details because Wunan Lin would do a very fine job of that and I don't -- but I just wanted to mention one thing. Karsten mentioned opportunities in the international program.

At Lawrence Livermore, we have an underground program, called the dynamic stripping project, which uses steam injection and Joule heating to extract non-aqueous phase liquids from the unsaturated zone and the saturated zone.

So people like Abe Ramirez and Bill Daily who have worked on the G-tunnel heater tests have been working very -- have been very active in that project. And even though they've been -- you haven't heard their names in recent years, they've been improving their instrumentation, ERT and other things, for monitoring displacement of these fronts at Livermore for the past several years.

DR. LANGMUIR: Thank you. Let me shift gears a little bit. Todd Rasmussen suggested something else which some of the heads went yes and some went no on his comment. I'd like to hear how we all feel about ventilation to avoid refluxion as a possible route to preventing refluxion.

My sense is that it's going to limit refluxion to some degree in the vicinity of the ESF, but that you're going to have a lot of water escaping ESF that's already up in the rock above in the ESF in the heating process. But I'd like to hear someone who is more into the modeling and the quantitative analysis of that. Maybe it's Tom Buscheck, maybe it's someone else. Bill?

DR. HALSEY: Just in putting together things that were presented by several speakers during the meeting, the amount of dry-out from the rock matrix into the drifts during the operational timeframe is still an unknown. It has to do with the vapor diffusion coefficients and the fracture network, how communicative it is with the matrix for gas phase.

I believe -- I don't remember who showed it, but just over a range of possible parameters for some of the controlling processes, during the operational timeframe you may get very deep penetration of dry-out into the rock matrix or very shallow, and those are some of the things we want to measure. So it's still an unknown.

It was also pointed out that you can remove, in principal, quite a bit of water and perhaps, more importantly, by the latent heat, remove an awful lot of thermal energy with the ventilation.

The final part of that -- this still may not preclude refluxing because the water which is originally in the pores of the rock matrix is only one contributor to the mobile water in the repository. Some of it is water hydration in the minerals. Some of it, of course, is infiltration from the surface, meteoric sources, and some of it is thermally mobilized water from the water table.

Your timeframe of operation will not allow you to remove those waters. They'll come and get you later.

DR. LANGMUIR: There was a point made by two speakers, one was Steve Frishman and one was John Kessler, which had to do with a concern about what MPC design might do to our options in thermal loading. I wonder if Steve Brocoum could comment on that.

The argument was that this would preclude perhaps the low temperature option if we went to the MPC as currently conceived.

DR. BROCOUM: I think where we are aware certainly with the large MPC that it may constrain us. I think we're looking at that in-house. I don't really have anything more to say. That's an evolving issue. We have a series of issues like that that we're concerned about that and the ability to interface between the MPC and the repository is a key one and we're working more than ever.

One of the things I failed to say in my closing statement. The M&O has this whole team that cuts across all different parts of the program working these issues. I don't know if anybody else wants to add anything to that, but I can't give you any definitive answer at this point.

DR. RICKERTSEN: Can I just add one thing? Larry Rickertsen from the M&O. The point that you made about the fact that things might be jeopardized is on the basis that the goals might change. That was the essence of it. But I would just say that the limits for the design specifications for the MPC so far, even for the large MPC, are based on the full range of areal mass loadings that we're presently considering, including the lowest. So, in fact, that's the most constraining one.

If you were looking at the higher loadings, the loading limit to the MPC would be much higher. You could allow a higher loading for that. So that 14.2 kilowatts corresponds to the lowest loading, given the set of goals that we now have. If those goals change, that might change.

DR. LANGMUIR: John Kessler.

DR. KESSLER: For the lowest thermal loading locally, you're going to find something. I believe I saw it presented that you're going to go over this magic boiling number. Maybe not repository-wide on an average for the lowest thermal loadings, but somewhere near the canister you're going to go over this arbitrary boiling criterion, and that this was some sort of a reaction to try -- you know, if the goal is everywhere to keep boiling away, then what I heard, maybe wrong, was that the lowest thermal option doesn't allow the use of the MPC with its thermal output to be used if you want to avoid boiling everywhere.

DR. RICKERTSEN: The option, the lowest loading option is not quite the same as a no boiling option. We don't know exactly what the lowest loading is going to do for us yet. That's part of why you're going to do the testing. We think it will correspond to the lowest disturbance, certainly, but we don't know what that low disturbance is yet.

That distinction that was made in that strategy of talking about low loading wasn't just an accident. We didn't mean to say minimal disturbance. We meant to say it's the low loading. As we investigate that, we will find out what the conditions are. We'll find out what the degree of disturbance is and whether that's -- and do some evaluations to find out how acceptable that is or not.

The point was that at the low loading case, on the order of 20 mtu per acre, on the order of 25 kilowatts per acre, those goals of 350 degrees C and 200 degrees C would be met. Those are the only goals we have so far for the lowest one. We weren't looking at the highest one to constrain us. That was the point I was trying to make here.

In the picture that we have, the MPC is certainly consistent with the goals that have been established so far. If those goals change, we might have to consider it. So your point is quite valid. But the point I want to make so far is that on the basis of where we're going, the MPC is consistent so far with the low loadings.

DR. LANGMUIR: In a related vein, I guess I'd like some comments from geochemists and hydrologists in the room as to the -- and those who work in performance assessment who have to take their subsystem models into play, what do you think about the uncertainties and the magnitude of the uncertainties that affect your ability to predict long-term performance, looking at the low versus the higher thermal loading options?

I have my own opinions on that one, but I'd be curious what some others might think of that. Bill?

DR. HALSEY: Yesterday Steve Brocoum asked if anyone wanted to argue that the uncertainties were indeed lower at the higher thermal loading regime and Dave Stahl commented from the point of view of the EBS. Most of my work has been focused on the EBS in the near field environment and I would say in response, Steve, that for the EBS in the near field, the higher thermal loading regime looks easier to demonstrate and is a better performer for the limited timeframe that we have subsystem requirements.

Conversely, I agree with the broader opinion that that probably is not true for the system as a whole, for the farther field coupled processes, the unsaturated zone and saturated zone processes. There's probably more that occurs with less certainty in the timeframe that we're asking to demonstrate it at the higher thermal loadings.

So it depends. If you're asking me about the near field, I think that the uncertainties can be reduced at the higher thermal load because you're driving to fairly simple processes. But for the global system, I think that the lower thermal loading probably does have globally the lower uncertainties.

DR. BROCOUM: I know you guys keep a detailed record, but I think my question yesterday was where can we get the information fastest to make our case to prepare -- to make a suitability decision and a license application, not which is the best technical case. So I was asking a different question than you answered.

DR. HALSEY: Actually, I think it's the same answer. We can get the information for the near field faster for the high, but I don't believe we can get the information faster for the entire system. I think that's what you were really asking. So I concur with you there.

DR. LANGMUIR: I'm moving my questions around a little bit in terms of subject matter, but Steve Brocoum used the key word "suitability" again. So it reminded me of another question I wanted to ask him.

All of us on the Board have been wondering and concerned as to how a decision of suitability in '98 really differs in substance from a decision to license in 2001. What do you have to know that differs?

My personal sense is you've got to know the same things. If it's suitable, it's licensable. If it isn't suitable, it's not licensable. But I'd like to know what your thinking is on this. I know that the DOE -- you haven't promulgated in writing at least the differences in those definitions that we've seen yet. What is your current thinking?

DR. BROCOUM: A lot of people have asked that question. It's a difficult question and it's also a philosophical question. But, fundamentally, suitability focuses on the site and, of course, focusing on a DOE decision. Licensing focuses on the ability to license a whole system, having a fairly well designed well advanced design in certain waste packages, at least we're laying out our strategy.

So that's the fundamental difference. We're trying to see at the suitability decision can we -- is there anything at the site that precludes us from designing a repository that will fit that site. That's one way of looking at it.

Are we in some way constricted? Is there a fatal flaw, if one can define a fatal flaw? So it's really focusing on the site. For licensing, you're focusing on the whole system.

DR. LANGMUIR: Does anybody else want to pursue that or is it all crystal clear?

DR. DOMENICO: Crystal.

DR. LANGMUIR: Any questions from anyone else but me? Leon Reiter of the staff.

DR. REITER: I just want to pursue that a little bit. Steve, could you focus in one -- I think I asked you before -- on thermal loading, what kind of statement are you going to make in 1998 as to thermal loading as compared to the 2001? The reason for the confusion is that on some of the charts, we see 1998 is concepts and 2001 bounding. And then I may have been mistaken, but I think in your presentation you said that by both 1998 and 2001, you're going to go with the low thermal loading option. Maybe you can clarify that.

DR. BROCOUM: We'll be collecting a lot of information. We're obviously going to have more information in 2000 or 2001 than we're going to have in 1998. A lot of thermal tests certainly will be just getting -- having only been underway for a short period of time by 1998. So obviously we're going to have a lot more information in 2001, on the one hand.

On the other hand, we're going to go through each guideline, put the information together, get it peer reviewed. Depending on what the peer review says, we may make a finding. So before we make a technical site suitability finding, we will have done each individual guideline. We will have made a finding on each individual guideline. The technical basis to back up that finding will have been peer reviewed.

So it's not an issue of -- I mean, you can ask the question what will we have in thermal and the scientific people can tell you what exact information we'll have. It's an issue of based on all the information we have available, do we feel we could make a finding on a particular guideline or in some -- all the guidelines put together in such a way that we feel we could make a higher level positive finding in terms of finding the site to be suitable.

DR. LANGMUIR: Pat Domenico.

DR. DOMENICO: Steve, I'm not that familiar with the findings, but are there any findings that deal directly or indirectly with a thermal hydrology?

DR. BROCOUM: There are hydrological findings.

DR. DOMENICO: I know hydrological.

DR. BROCOUM: And I think you have to accommodate -- you may have to accommodate in making that finding thermal considerations. I don't think there's any guideline that addresses specifically a thermal consideration.

DR. DOMENICO: So you mean you could find a site suitable independent of a loading strategy.

MR. BOYLE: No. There's a finding on the pre-closure rock characteristics that takes you through the end of the operations period and we have to take into account what the effects of the heat will be, at least for pre-closure rock characteristics.

As far as geohydrology goes, I'm not as familiar. Ardyth might speak to that.

DR. DOMENICO: By rock characteristics, you now mean mechanical, thermal mechanical.

MR. BOYLE: Yes. But I'd like to make a point that I meant to make earlier. In many respects, I think it's a mistake to divide things in terms of thermal hydrologic, thermal mechanical. All the mechanical properties that we're interested in in terms of strength and deformation are functions of the water that's present, the saturation levels, sheer strength on a joint, if there is water present, what is the pore pressure.

And it's the same for thermal hydrologic, particularly in the near field. If you heat up the rock and the joints open or close or whatever, it might affect -- it will affect any fluids flowing through that joint.

So when we look at pre-closure rock characteristics, to the extent that we have to incorporate what the water is doing, we will.

DR. DOMENICO: Well, the distinction between thermal mechanical and thermal hydrologic is yours, not ours. That was in your slides. But I think that the thermal mechanical are a bit easier, myself. But my question is this. You can find a -- come to a suitability analysis independent of any thermal load you might impose on the system or you can't?

DR. BROCOUM: I'm not sure what your question is.

DR. DOMENICO: You can come or maybe you cannot come to a site suitability analysis independent of either a high loading or low loading, low temperature or high temperature loading, however you want to call it. Does the thermal load have to be taken into consideration in site suitability?

DR. BROCOUM: I think you could look at the knowledge you have at the range of thermal loads you're considering to see if anything in that range of thermal loadings would preclude you from designing a repository for that site. That's a way you could approach it.

If you don't see anything that would preclude you at that time, I think you may be able to make a finding. I don't want to prejudge how these findings are going to go because I don't know how they can go at this point in time.

DR. DOMENICO: I know something about the findings, but I can't recall one that dealt directly with influences like thermal loadings on the rocks.

DR. BROCOUM: No. Let me put the one on pre-closure rock characteristics, post-closure, and is there anything -- and there's nothing specifically in here about -- is there?

DR. RICKERTSEN: Yes. The post-closure rock characteristics guideline, technical guideline, asks you to make your evaluation in view of the thermal stresses on the rock, whatever that means. The geochemistry guideline also talks about thermal interactions with the materials of the waste package.

So there are two places that talk about it. We don't know quite how we're going to handle that yet. It might be in terms of a bounding analysis. It might be in terms of looking at the range of possibilities for the range of thermal loadings that you're considering at that site suitability determination stage.

DR. GAMBLE: Bob Gamble, M&O. An additional point of clarification for what Larry said. The post-closure rock characteristics guideline does, in fact, talk about the characteristics of the site and, given the rock characteristics, whether or not they are capable of accommodating the thermal chemical, mechanical and radiation stresses that are expected due to the existence of a repository of some design.

It also talks about the expected interactions among the waste, the host rock and the groundwater and the engineered components of the system. So the qualifying condition of the post-closure rock characteristics guideline does specifically ask the DOE to look at the interactions of the various processes at the site, thermal loading being --thermal effects being one of those processes.

DR. LANGMUIR: Leon Reiter.

DR. REITER: I think it would be kind of difficult to meet your system guideline, post-closure system guideline, and do a performance assessment without taking into account thermal loading of some sort.

DR. BROCOUM: But if you use the advanced conceptual design, which presumably will encompass the full range of thermal options, wouldn't that cover or bound your case?

DR. RICKERTSEN: That means that the site suitability is going to show suitability for both low, medium and high thermal loadings or some range of low. I'm not quite sure.

DR. BROCOUM: I don't know what it will show right now. I'm just kind of speculating. You gave me kind of a speculative type of question. But the point we've made is we will use the advanced conceptual design as a basis for doing the guideline findings.

That advanced conceptual design encompasses the range of thermal loadings you're considering, and, therefore, you would be considering those. So if we follow through with the flexible design, the advanced conceptual design, and it encompasses that range, that would be included in your analysis.

DR. LANGMUIR: Another disconnected question from me. Srikanta Mishra presented the M&O's TSPA status at present and the evolution of things since '93, the '93 documents. I understood there, because I've been -- unfortunately for me, I chose the Sandia '93 to study carefully, not the M&O one. Now it's no longer part of the flowchart.

I'd be interested to know how the parallel Sandia effort has been combined with the M&O effort in a single

TSPA approach that's underway now and for the future at the DOE. Obviously, it's a very complex undertaking, but how are you bringing this all into one path.

DR. MISHRA: Once again, I would like to defer this to Bob and, unfortunately, he's not here. He has been primarily involved in the planning process of the TSPA with the DOE WBS managers. My understanding is that the next iteration of the full TSPA will be an M&O project and Sandia National Labs will focus their TSPA efforts on developing scenarios and having a scenario analysis, particularly those of disruptive events.

So the basic TSPA will be conducted by the M&O and we will try to include as much refinement as possible in such areas as the waste package degradation model, the waste form alteration model, and the geosphere of transport. But in terms of the external features and events, those will be done by the Sandia National Labs.

DR. LANGMUIR: What about the hydrologic flow model effort? My sense was that the Sandia approach involved looking at both weeps and composite porosity and then I understand from you that the M&O approach has been composite porosity only. What are we doing here?

DR. MISHRA: We have basically used an equivalent continuum model. For the next iteration of the TSPA, we are looking at some dual continuum kinds of representations. We are also going to be working with the scientists at LBL and USGS who are developing the site scale model of hydrology. So we will try to link our efforts with their representations of the site hydrology so that there is some consistency in the hydrologic description, as well as in the total system description.

DR. BULLEN: This is Dan Bullin from Iowa State. Since we got to PA, I thought I'd throw in my two cents worth. I want to reiterate the fact that as you try and model waste package performance and engineered barrier system failure and release of radionuclides, that you don't take a too overly conservative approach. If you get too overly conservative, basically by saying that everything disintegrates, then you don't get the credit for being a smart engineer.

I also would like to point out -- maybe reiterate something that John Kessler said. Maybe we want to look at substantially complete containment first and see if the engineered barrier system helps us achieve that goal, then take a look at repository performance based on transport out of the near field environment, the engineered barrier system, and then the long-range coupling with refluxing and all of the other aspects associated with it.

And in doing all of those things, keep in mind the impact of the thermal loading on waste package performance and waste package failure, because I believe those will have significant effects, particularly when you want to do the evaluation of substantially complete containment and then perhaps even a more significant impact as you realistically model transport from a breached container, but not a container which completely disappears at failure and immediately dissolves all of the radionuclides into the accessible environment for transport.

DR. LANGMUIR: Any comments?

DR. MISHRA: I think all of your points are very well taken. As Bill Halsey pointed out yesterday, the 1993 TSPA was essentially the first effort at incorporating all the thermal dependencies. So most of the models that we have should perhaps be thought of as place holders for the time being and being place holders and being the initial place holders, we do have a lot of conservatism in most of these.

But as the TSPA is matured in the iteration that we are going to conduct in FY-95 and in the iteration that we will conduct in FY-97 which directly flows into the technical site suitability determination, we hope that most of our models will be more robust, more defensible and less conservative and there will be some enhanced degree of realism in each of these.

Now, as to whether this will make any difference or not in terms of the performance both with respect to the substantially complete containment aspect and with the controlled release from the EBS, with respect to the total system performance, I don't think I know which way it will go.

But, once again, there is an effort at making the underlying models a scientifically defensible and robust as possible.

DR. LANGMUIR: It's Friday. I didn't have any more. If other people would like to bring up issues, we have time. In principal, we do. The schedule suggests we don't have to quit yet. If we're through here at the table, though, it's appropriate for those from the audience who might wish to comment or ask questions to do so, if they haven't had a chance.

COMMENTS FROM THE AUDIENCE

DR. RICKERTSEN: I just had a technical question on the monitoring of fluid fluxes through the experimental plans. One way of monitoring fluids would be to look at how much fluid is present, but actually monitoring flux rates or flow rates through fractures. I'm wondering what strategies are being proposed for looking at fluid velocities and transport rates.

DR. LANGMUIR: Is anybody here aware of such work? Any hydrologists?

DR. BUSCHECK: Karsten, do you have an idea about that?

DR. PREUSS: I'm not a proponent of either, sorry.

DR. BUSCHECK: We haven't said in any of our plans that we are going to be explicitly measuring rates. We will be in the large block test, however, placing open bore holes where we feel that refluxing is most likely to occur. We were planning to -- I'm not sure if the funding is available at this point -- to utilize the SEAMIST probes, which would be collecting moisture in the refluxing zone and in real time measuring chemistry changes.

I don't want to say that we have a magic way of actually measuring flux. However, I think we have -- through the use of models, perhaps we have a way of interpreting what that flux may have been to give us the thermal hydrological conditions. I think that's the best we have right now in terms of trying to understand the details of what's happening in the fracture.

DR. DOMENICO: I think in the ESF they've planned some conservative tracer tests in a few of the fractures. I believe that's part of the plan in the ESF. Conservative tracer tests.

DR. LANGMUIR: In fact, I worked on that. I had forgotten. I worked on that for a number of years when I was in this program, selecting conservative tracers to be used in hydrologic tests. That's definitely planned.

DR. DOMENICO: Do you give that a high priority, Todd?

DR. RASMUSSEN: Normally what happens is you have such a small volume change in a fracture that you can't actually monitor what the content within a fracture is. What we have noticed at Apache Leap is that it migrates long distances very quickly.

So since you can't measure whether there's water in a fracture or not or how far it's moving or where it's moving and it can move large distances in a short period of time, I'd put a large priority on knowing that.

DR. DOMENICO: I thought we would know that without monitoring and if you get water in a fracture, it's going to move long distances in short times. I would think that we would probably all agree with that.

DR. RASMUSSEN: But you can't predict that using an equivalent porous medium.

DR. DOMENICO: No, no. There is no such thing as fractures in equivalent porous medium.

DR. BLINK: This is Jim Blink from Lawrence Livermore. I can think of three ways that we've been able to measure fluid velocity, if not fluid volumetric flow rate, at the large block site and in the laboratory. Sandia has used an x-ray technique -- Bob Glass is the PI to follow fluid fronts -- and in the field at the large block test, we did three fluid flow experiments prior to excavating the area around the block.

These experiments were about a three-meter-by-three-meter area that was dammed and flooded with water. The water was traced, as it turns out, with food coloring. It seemed to satisfy the EPA real well. And then when we excavated, we looked at the traces in the fractures that were colored by the food coloring.

In addition, as it happened, we had drilled some holes around and one in the middle of those blocks and put electrical resistive tomography electrodes in and were able to follow the fluid front as it passed through. So that technique could be used to measure velocity and one could do it in the field at selected areas.

But as far as flow rate, that's another story.

DR. LANGMUIR: The tracer business has become very sophisticated with the use of mixing a variety of related tracers, like CFCs, that are subtly different chemically. So you can look at not necessarily volumes, but you can look at exact proportions in mixtures from different locations coming together in a fracture system using a variety of traces together.

Any more comments or questions from the audience? If not, from the table? Karsten?

DR. PREUSS: I'd like to repeat a little more on the modeling, if I may. The thermal hydrologic test, in particular, the thermal hydrologic modeling that Bill Halsey showed this morning looked suspiciously like effective continuum approach. As far as I'm concerned, on the scale that you want to work, that's already discredited by the G-tunnel experiments that showed very different behavior from what effective continuum models predicted.

So I'm very concerned that before you do the test, that models would be applied that would be as realistic as they can be and explicitly represent fractures and the like. Could you comment whether that's happening?

DR. HALSEY: Prior to having access to the rock, we can't really explicitly model the fractures that might be there. Certainly, the results that we were showing are predictions without fracture perturbations and the true test results would be different from that.

What we're trying to do is scope the tests to compromise the scale between time and what can be measured about these various processes in the hypotheses that we're trying to test. We don't believe that the results will exactly match the results that were modeled, but we believe that equivalent continuum modeling can tell us something about what size test measures the processes we're looking for.

DR. PREUSS: Just a brief remark. I think that it's vital that you model explicit fractures before you actually go underground and before you know where the fractures are. You do know the fractures will be there and you have to develop and show a capability of modeling the behavior of individual fractures, even if -- you know, prior to going underground. Take the hypothetical and not the actually ones that you then later map.

But how else would you design and how else would you know what kind of a response you expect from a hydrologic system and what kind of monitoring you will need to pick this response up if you never model the kind of realistic behavior that will be dominated by fracture flow?

DR. HALSEY: The fracture network is certainly represented in the bulk permeabilities that were used as the basis for those predictions.

DR. BUSCHECK: Because we're in a different time zone, after every meeting, I get to call back to the office and during this week, John Nitao has been making progress on revisiting the G-tunnel heater tests. We had planned to do that. Dwayne Chesnut has been working with him on that. We have been attempting to more -- well, actually, to do a discreet representation of if not the actual fracture distribution, a hypothetical representation or idealization of it, but representing the discreet behavior so we could somehow show the fact that we did not see a condensate buildup and we did, in fact, show shedding around the boiling zone.

To say that one cannot look at the effect of heterogeneity independent of having a discreet fracture model is to say that we'll never be able to solve the problem perhaps because in a test like this, we probably may have thousands of discreet fractures. Certainly in the large block we may have thousands of discreet fractures.

We have looked at the effect of heterogeneity in a couple of different ways, one of which is through having -- using equivalent continuum modeling and modeling it heterogeneously, and we have managed to get extremely heterogeneous flow. We have published those results in the last year.

We also have Dwayne Chesnut's model which represents drainage in a log normal way distribution. So there may be ways of looking at this problem statistically. We may not be able to isolate exactly where this non-average behavior occurs, but we may be able to get an idea of what type of extreme behavior may happen about the mean, which the equivalent continuum model -- I don't know if you could call it exactly.

I mean, certainly with respect to dry-out we found at G-tunnel, we got a fairly reasonable representation of dry-out. So I think some of the mean behavior can be represented. And other alternate means can be used in conjunction with those modeling techniques to get an understanding of how the actual behavior deviates from that mean behavior.

DR. CORDING: Tom, you showed us about two years ago essentially a one-dimensional model with a matrix and interaction between matrix and joint flow and true joint. Is that type of model one that you can apply in a more complex -- in any way under more complex boundary conditions you would have in a test, a thermal test?

DR. BUSCHECK: To use that technique explicitly to model our system, the way we did for those calculations, would literally require on the order of probably half-a-million grid blocks. I know they're looking at this also at INTERA. There have to be some sort of ways that represent the imbibition behavior in the matrix blocks without discretizing it explicitly as we did in those calculations.

Karsten, do you have a comment on that?

DR. CORDING: In other words, you're looking a true three-dimensional -- what you would have to do if you had a true three-dimensional model. There may be some other ways of cutting it.

DR. BUSCHECK: There are two things that bulk permeability distribution will do to you in terms of non-uniform behavior, one of which is they cause non-uniform behaviors of the gas flow. In that setting, the equivalent continuum model in terms of a heterogeneous gas flow is probably not as bad as it is for heterogeneous condensate flow or infiltration flow.

So we may be able to capture some of the heterogeneous behavior insofar as it plays in heterogeneous vapor flow. We have made progress in that type of modeling.

DR. KESSLER: I just had a general comment about the push to model discreet fractures. That is where are we going with this. If I look at the TSPA-93 results of Sandia, my gut reaction might be, gee, good, I'm glad we've got flow in fractures. That means not every canister is going to get wet like an equivalent continuum model. So it might be a better thing.

But what are we going to do with the information once we know discrete fractures? For instance, Karsten, I know LBL was heavily involved with STRIPA. They looked at all the fractures, they saw where things were going. What could be done with that to make a safety case for some system like STRIPA, which, other than being fully saturated, might be pretty analogous to Yucca Mountain.

Once you've got all that information, how are you going to use it to maybe bound the performance of your system just because you've modeled discreet fractures?

DR. LANGMUIR: Let me pick up on that and expand it a little bit and throw it towards Steve Brocoum. What we've been hearing so far in the last couple of days is how flexible the program has become and we've seen evidence of that and some creative thinking about ventilation systems and so on.

The most creative of all possibilities perhaps would be to say I've got a fracture, great, it's draining the water away from my repository, so don't put the fuel in the fracture and I'll do it this way and as long as there's enough space in the mountain for my waste packages, I can accomplish it that way and minimize water coming in contact with the waste by using fractures rather than worrying about them in the conventional sense.

How flexible are we? Is that a permissible way to think about it?

DR. KESSLER: I would just add one thing to that question. Even if we're not smart enough or aren't capable of knowing where all the fractures are to be able to offset them from every fracture, one of the things that we may be able to infer from the Sandia TSPA-93 is, okay, so a couple containers get hit, a lot of them because of fracture flow don't, and can we use that information.

DR. BROCOUM: That's an interesting question. That is why it's so hard to define discreet features of a site that would disqualify it. One might think a fault -- Ghost Dance fault through the middle of the site, the state keeps bringing that up as it being a bad feature. But one could conceivably think of a design of a repository that would use faults or fractures to drain water away from the waste packages.

So you can come up with a design that takes advantage of the attributes, if you like, or the features of the site to give you a better system. That's why a lot of people have always had problems and it's very hard to list down on a piece of paper what features should disqualify a site, because it's very hard to do that independently of a concept of the repository you're thinking about.

DR. SIMMONS: May I just add to that and follow this on from what Steve was saying? We've been talking quite a bit at this meeting about the engineered barrier system, but one thing that we haven't really discussed is some of the attributes of the geological system in the far field that would be helpful.

And with regard to fracture flow, if we were able to show, as we anticipate, that much of the flow will occur in discreet fractures and that it may perhaps be episodic, it may be possible to show that the Calico Hills even beneath the repository is a very effective barrier to radionuclide retardation.

In a scenario in which a number of discreet fractures are responsible -- that are spaced at some regular interval perhaps or at least that we were able to model, we may be able to demonstrate the transport pathways that would carry radionuclides and be then retarded in the Calico Hills. So it's a matter of putting together your information about the engineered barrier with the geologic area.

DR. DOMENICO: I have a question. With regard to the Fran Ridge block test, maybe somebody could tell me. Certainly that block has been mapped, the fractures have been mapped and apertures measured and things of that sort. Is somebody going to apply Jane Long's black magic to those fracture sets before and after the heating? Is that part of the scheme on the calculations before and after heating?

DR. BUSCHECK: We've had preliminary discussions with Dave Dobson at Golder Associates about the use of FRACMAN to try to and -- so that is still in its early stages. I discussed that with Ardyth yesterday and something along those lines is going to be pursued.

The practical problem with this is that how we represent that discreet representation. We'll probably need to use something like FRACMAN to see what type -- what the effects of discretization are. From a practical standpoint, can we incorporate some of the realism and do something that's significantly better than just assuming bulk equivalent continuum properties.

DR. SIMMONS: That's what you were referring to by Jane Long's black magic?

DR. DOMENICO: No. FRACMAN is different.

DR. SIMMONS: Okay.

DR. LANGMUIR: Any further questions or comments?

[No response.]

DR. LANGMUIR: Are we burned out? Bill Halsey, the final word, perhaps.

DR. HALSEY: I was thinking about some of the requirements to follow the pathway that's being laid out. This developed during the ANS meeting on Wednesday and I think it's even clearer now that the program approach that we're following, where we're looking at each of the major milestones and trying to figure out what is it that we really need at that point and then move on to the next one, is something a little different than what was viewed from many years back in the SCP, where it laid out everything.

I would use the analogy that the SCP approach was sort of like the yellow brick road. We were going to pave it all and then we could walk along it. And now we don't have the time and all the bricks. We're going to look at where it is that we need to stop and we're putting down milestones and we're going to hop over to that one and then this one and play a little hopscotch along this pathway.

That's okay. That will get you there. And the fact that you have to land with your left foot here and your right foot there is all right, too, because the requirements for the different milestones are different. The site suitability is a requirement on -- is a judgment of the site's ability to host a repository.

The initial license application is a judgment on the ability of that design you are asking to start construction to comply. I think that's a much more efficient way of getting down the path and a quicker way.

I just believe that it also requires a little bit more in the way of dexterity and agility. I see four components -- the engineering and design components, the testing, the gains, the mechanistic knowledge, the model development and the performance predictions, and the licensing approach and the interactions with the public and the regulatory bodies that now all have to pop together. They have to be in sync on this or we're going to trip ourselves up.

I think that puts a greater burden on the systems analysis that we heard discussed yesterday, on the program integration. And I agree with Steve Frishman's observation that that puts a greater burden on the Board because now you have to understand the path that we're following rather than judging whether we've built a fundamentally complete road. That's just an observation.

DR. PALCIAUSKAS: I really like that observation about hopping between these little patches on your road to site suitability. We keep looking at the path and we're looking for the big gap between two patches which you just might not be able to clear. That's the role of the Board, I guess.

DR. CORDING: We also need different tools. We need machetes instead of trowels.

DR. LANGMUIR: With that, let's thank you all for attending. It's been very productive. We appreciate your contributions. We are adjourned.

[Whereupon, at 4:00 p.m., the meeting was concluded.]