UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

Evaluation of Ranges of Thermal Loading for High-Level Waste Disposal in Geologic Repositories

October 10, 1991

St. Tropez Hotel
Monte Carlo Ballroom II & III
455 E. Harmon Avenue
Las Vegas, Nevada 89109

<u>BOARD MEMBERS PRESENT</u>

Dr. Don U. Deere, Chairman
Dr. John E. Cantlon, Chair Morning session
Dr. Clarence R. Allen, Chair Afternoon session
Dr. D. Warner North
Dr. Dennis Price
Dr. Donald Langmuir
Dr. Patrick Domenico
Dr. Ellis D. Verink

<u>NWTRB STAFF PRESENT</u>

Dr. William D. Barnard, Executive Director
Dr. Sherwood C. Chu, Senior Professional Staff
Dr. Leon Reiter, Senior Professional Staff
Mr. Russell K. McFarland, Senior Professional Staff
Dr. Edward J. Cording, Consultant
Dr. Roy E. Williams, Consultant
Ms. Paula N. Alford, Director, External Affairs
Ms. Nancy E. Derr, Writer/Editor

<u>FOREIGN GUESTS</u>

Dr. Klaus Kuhn, National Research Laboratory for Environment and Health; Germany

Gary Simmons, Atomic Energy of Canada, Ltd. (AECL)

Nils Rydell, National Board for Spent Nuclear Fuel (SKN); Sweden
Peter Stevens-Guille, Head, Radioactive Materials Management Ontario Hydro; Canada

I N D E X

P R O C E E D I N G S

8:30 a.m.

DR. DEERE: Good morning. Good morning, Ladies and Gentlemen. Welcome back to the third day of this meeting on thermal loading.

The chairman of this morning's session is a fellow Board member, Dr. John Cantlon, who is chairman of the Board's Panel on Quality Assurance and recently appointed chairman of the Board's Panel on Environment and Public Health.

John?

DR. CANTLON: Thank you, Don.

I have one logistical item. We have found an unused airline coupon and if someone can identify it appropriately to the people on the desk on the foyer, we'll surrender it back to its owner. Hopefully no one will be marooned.

For the past two days, we have heard about various thermal loading considerations and how they may affect repository design and system operation here and in other countries. Also, we have discussed a range of uncertainties associated with alternative thermal loading concepts.

Today, we will talk about the implications of high and low thermal loading. We will begin the presentation with a group from EPRI who will look at performance assessment considerations associated with thermal loading, waste package integrity, near-field effects and overall performance.

Following this, we'll have a break and then we will review the comparative cost of high versus low thermal loading. We will also discuss how the various alternatives affect the cost associated with the high level waste system's storage, transportation and repository components.

For example, cooling of the spent fuel for extended periods before disposal will lower thermal loading, but will increase storage cost. Thermal loading could also be reduced by decreasing the amount of spent fuel in each waste container or by increasing the spacing between waste packages. However, the desirability of these alternatives must be balanced against the need for cost--needs for and cost of a larger emplacement area, more waste packages, greater risks for more handling, and even for additional repositories.

Thermal loading also has important legislative and regulatory implications, and we will hear these discussed this morning.

As suggested in its draft mission plan amendment, the DOE, and I quote, "will need to be able to demonstrate during licensing that we understand the effects of thermal pulse on the repository and the engineered barrier system, and that the performance of all elements of the system is acceptable with respect to established standards."

We look forward to hearing presentations on these important issues, as well as a discussion of conceptual considerations for total system performance.

This afternoon, we look forward to an especially exciting discussion, round table, of the issues that have been identified in the previous sessions. And let me ask Dr. Clarence Allen, who will chair that session, to give you a brief outline of the operating principles. Clarence?

DR. ALLEN: Thank you, John.

I notice the program says this session will provide an opportunity for participants to reach conclusions on the risks and uncertainties, et cetera, et cetera, so we're looking forward to what conclusions have been reached.

I think our--the format for this will not be to appoint a given round table with specific assignments, instead, we will sort of have this a round table as a whole. I think we may turn the table around here so that we're facing each other and have at it in an informal fashion.

I have invited each of our foreign guests to perhaps lead off with some of the reactions they might have to the three days of the meeting. At least two other people have already expressed a desire to make statements, and certainly we will do so. We will, I hope, set up so the viewgraph can be used.

So, please, during the morning, all of you keep in mind what you might wish to add to this session, either in the way of presentations or in the way of questions, and I particularly urge the Board members to think of provocative questions or provocative statements that might keep the discussion going this afternoon.

Thank you.

DR. CANTLON: Thank you, Clarence.

Well, let's start then with the performance assessment considerations from the EPRI group. Bob Shaw will handle the introductions of the group. Bob?

MR. SHAW: Thank you, John. It's a real pleasure to be here and we appreciate the opportunity to come before you and give you some results that we've been working on.

We're mildly apologetic for the fact that we weren't able to arrive until last night, but I have a Methodology Development Team which I had a six month standing date to meet Monday, Tuesday and Wednesday in Palo Alto, which we did. So a lot of the results that you're going to see here are literally hot off the press. They are things that we put together and worked on over the last three days, Monday, Tuesday and Wednesday of this week.

EPRI is the research arm of the electric utility industry. There's reluctance in the electric utility industry to spend much money in EPRI or other organizations on this particular arena because they already see themselves as being the prime funding for the Nuclear Waste Fund for the efforts that are going on with the elite. Nonetheless, they have been willing to provide some funding for work that we've been doing in an overview sense.

About a year ago, we published the first of our reports, NP7057, on the work that we had done up to that point on a risk based performance assessment for a high level waste repository. I think many of you are aware of that, and we have made presentations to the TRB on the basis of that.

We also currently have a couple activities that are going on that are worth mentioning before I come back to where we are and what we've been doing more recently.

One activity that, again, I think many of you are aware of is that about two weeks ago, we sponsored a workshop on the EPA criteria. This was an attempt to pull together the information that was established actually a year ago when the National Academy of Sciences had their forum on the EPA criteria. We had what I feel was a very successful workshop two weeks ago in which many participants, including major people from EPA, got together and said what are the real issues that we're confronted with. The measure of its success, of course, will be what happens now. How does EPA make use of the output that came from that particular meeting.

The second effort that we have under way is our work in trying to establish a means whereby expert judgment can be used in such performance assessments. We are establishing a set of workshops in the seismic arena, the first of which will be in mid November, where we have now identified six experts in seismicity, we'll come together, we'll discuss data, models and other features, and we will have a group of experts who will come and elicit their expert judgment with the prime aim of being what are the uncertainties that are really associated with these particular fields.

Then it will be our attempt to take the results of such a meeting and blend that into our performance assessment to show how we can make use of expert judgment in an overview performance assessment.

Now, let me go back and just say a few things about where we are with regard to our performance assessment in an overview and survey sense.

As many of you know, our performance assessment is based on a logic tree analysis for a risk based approach for an overview model of performance assessment. In what we're calling Phase 2, the work that we have under way right now, we considered a number of additions to our Phase 1. And in particular, they included gaseous release, time dependent inputs such as changes in climate that could appear many thousands of years out, human intrusion, thermal loading, an enhancement of the interfaces between the various technologies, we expanded our list of radioisotopes from one to thirteen, and we've really converted from what we considered to be an illustrative performance assessment to now a situation where we consider that we have a usable performance assessment, something that can be used to establish priorities to come to some preliminary conclusions.

As I mentioned during the last three days in this week, we have collected together our Methodology Development Team which numbers thirteen people, each of which has an expertise from a different area. This has been the basis for our performance assessment methodology. We also had three observers and three EPRI people there.

We're going to, as it shows on your agenda here, march through a select few of the particular technologies that we've had of interest. But first let me just give you the overview logic tree that we are currently using so that you'll have an overview sense of how this performance assessment works.

What you're looking at here is a progression from left to right of the nodes and branches that we've constructed. And I'm not going to march you all the way through all of these, but I do think that it's important to have a sense of this so that you understand how the various constituents that we're going to discuss fit into the overall pattern.

And if you quickly trace through these, you'll find that about the first five--actually, the first six have to do with the general hydrology of the area, such things as flux, which we determine from, first of all, precipitation and then, secondly, soil physics to determine how much penetration you actually get through, lateral redistribution, change in the water table that results from flux, fracture matrix coupling, saturated flow velocities and then matrix reductation values.

And then there's a second grouping here that has to do with the obtrusive kind of events. Here are volcanos and the water table change, earthquakes and the water table change, and actually at the end we have another one, which is human intrusion. And then 11, 12 and 13 sort of wrap together the engineered barrier system, the source term, near-field and the transport back into the hydrology, which of course is transported all the way through.

And we do use a node and branch technique. This is not a probability density function type of analysis but, rather, we asked each of our experts to break down their particular system into a branch type of analysis. So this is the manner in which we have done this work, and now we're going to proceed to march through a few of our experts here and have them tell you how we proceed. And the first one who's coming up will be Ben Ross, who will speak to you about temperature profiles and gaseous release.

DR. ROSS: I'm actually giving two talks, but one of them is about how temperature affects gas flow and the other one is about how gas flow affects temperature, so I just made one title slide.

I'll talk first about temperature, and this is largely work done for EPRI as part of this project. And what we did essentially is reviewed the literature first of all, and mostly, and we found that there are quite a number of different physical processes that can have a significant effect on temperature. And I know this has been talked about. There's heat conduction. There's convection of both sensible and latent heat. There's gas flow away from zones in which water is evaporating. There's buoyant gas flow. The gas flow can remove water from the system. You can have liquid flow driven by suction. You can also, and I forgot to put this down, have liquid flow driven by gravity. There is gas-phase diffusion. Silica redistribution could affect the permeability, and the ventilation of the repository can remove both water and heat. So you have a complicated problem.

And I focus in particular on two phenomena that have the potential in some circumstances of really significantly lowering the repository temperatures, and those are the heat pipe effect, which is what was discussed yesterday afternoon, but I'm referring to a natural heat pipe rather than one installed, and repository scale buoyant gas flow, which is important on its own for worrying about Carbon-14 and will be the second talk I give.

So we have here a cartoon of the different heat transfer regimes that can exist around the waste. In the center is the waste package. Furthest away you have an area below the boiling point where both conduction and convection may be important. Then you have a heat pipe region in which the temperature is fixed at around the boiling point. Then you'll have an inner conduction zone where the rock has dried out, the temperature is above the boiling point, and conduction is the significant heat transfer mechanism. And one or more of these regimes can be missing in any--may turn out to be missing.

Now, if you'll look at the published temperature analyses, and this is published literature, does not include what's been done earlier at this meeting, you'll find that no one's solved the complete problem. There are numerous conduction only analyses in the literature. There's a paper in Water Resources Research by Tsang and Preuss which has a very low rock permeability compared to the number I like. You have the model of John Nitao which I'll talk more about in a minute, which is a predecessor of the results that Tom Buscheck presented yesterday. But what he did is not repository scale; it's only one room wide, one room and pillar wide. So you don't have the horizontal temperature contrast between inside and outside repository.

Then you have some very interesting similarity solutions, semi-analytic solutions by Chris Doughty and Karsten Pruess, but they omit gravity and also have a restricted geometry.

But in looking at all of this literature, I've identified three regimes that I think are plausible to exist around a repository, three different heat transfer regimes. One is the case in which there's a low bulk permeability. And, very roughly, I'd estimate that as being less than 10 to the minus 13 square meters, and that would refer to the total, essentially the gas permeability of the rock.

The second is a regime in which there's a high gas permeability and the water is mobile in the fractures. And a third regime is where there is a high bulk permeability and the water is immobile in the fractures.

In the low bulk permeability regime, liquid is drawn towards the heat source by suction. Vapor, because of the low bulk permeability, moves away by diffusion rather than bulk flow, or at least diffusion is significant compared to bulk flow. The result is a pressure build-up near the heat source. The heat transfer is conduction dominated with little buoyant flow, and the canister temperatures will exceed 95 degrees, which is the boiling point.

The other extreme is the regime with high bulk permeability and fracture flow. In this case, you develop a strong heat pipe effect. You find that the buoyant gas flow can be effective as a heat removal mechanism, and there is no pressure build-up near the repository.

Finally, in the case of high permeability without fracture flow, you find that the strength of the heat pipe depends on the matrix permeability. You get strong buoyant gas flow, but how effective that is in heat removal might be limited by drying out of the rock. Some pressure build-up may be possible and you will have the one time removal of latent heat by evaporation, and I'm not sure how important that will be, and I'm not sure what the canister temperatures will look like.

Now, in order to come up with some results that would be usable as inputs to the overall EPRI model, we had to have a starting point, and I started from the results published by John Nitao, which as I say, was a forerunner of what Tom Buscheck presented yesterday.

He allows some water flow in fractures, but only when the system gets very wet. So it's sort of in between the no fracture flow and lots of fracture flow cases.

He models a column from the water table to the surface, but it was only one room and pillar wide, and he had a high bulk permeability, which I should add that the results Tom Buscheck showed yesterday had a lower permeability than Nitao did in the earlier work.

The other one was also the same, so they were both 2.5 times 10 to the minus 13. Okay. And his conclusion was that the rock dries out around the canisters for about a thousand years, and the pillar goes into the heat pipe zone and never gets into the inner conduction zone. It stays in the heat pipe zone and eventually will cool off, but only a fairly late time.

Now I'm going to present reasons why the canisters might get that hot or even be warmer, and then I'll present some reasons why they might not be that hot.

Reasons why it might get that hot or even be hotter, that the water you would need for a heat pipe could run down through the pillars or cold spots and leave the system. The water could also run down even through a hot zone through open fractures where it could move fast. There may be water removal by the ventilation system before the repository starts. You could also have mineral precipitants that plug fractures and block the heat pipe, both liquid and gas phases.

Now, some reasons why it might get cooler. In Nitao's model, he seems to indicate a 5 to 10 per cent convection effect on the temperature on the Delta T. This is not at the repository; this is away from the repository. But in the modeling we've done, we get the gas flux is 100 times larger than he calculated, so if he's already seeing some convection flux, you know, we thought we'd see a lot more.

Second, he starts with, in his older work, he started with eight and a half year old waste, which may not be realistic. As I mentioned, he requires the system to get quite wet before liquid flows in the fractures, and finally heat could be removed by ventilation during the operating phase.

So we came up with three cases that we thought were worth considering, what we call a hot case in which most of the canisters are at the temperatures that Nitao calculated, a warm case in which most canisters get pinned at the boiling point by a heat pipe effect, and some get hotter, and a cold case with a maximum temperature which was an arbitrary number a little bit less than the boiling point.

And we derive these three curves; the lowest curve is just done by scaling down the top one. But there's clearly going to be inhomogeneity in the repository, so we thought that in each scenario, even if you had a pretty good heat pipe, there would be some canisters that didn't have good access to water and they'd get hot anyway, and vice versa, you'd have a few cold ones, even if most of them were hot. So we more or less made up these numbers.

In the first scenario, we said 90 per cent of them follow the hot curve and 10 per cent the next curve, and so on. And we assigned them probabilities which were somewhat of a compromise between my own thinking and what the general feeling of the community seems to be.

Now, just to demonstrate to you that the idea that the thing won't get very hot at all is not far out, I want to show you some brand new and very preliminary results. This is work that is funded by Sandia. We also work for Sandia and most of our gas flow work has been done for Sandia.

We now have coupled to our gas flow model a transient temperature model, which takes into account latent and sensible heat convection, and this has only been running for about two weeks and still has some instabilities we're working on. And this is the grid we've been solving. The points circled in red I'll show you curves for in a minute.

DR. NORTH: What's the scale on that?

DR. ROSS: This is the whole repository; the top is the ground surface, and the bottom is the contact between the Topopah Spring and the Calico Hills. Calico Hills is basically a barrier to gas flow.

Here are temperatures at 150 years. The solid line is conduction only; the dotted line is convection and conduction. You can see that at 150 years already the hottest region has started to move up away from the repository. This is done with parameter values chosen to be most favorable to convection, but within the realm of reality. Again, this is the repository scale. What you see here is the same; the top is--

DR. LANGMUIR: Where is the repository?

DR. ROSS: The repository is basically where this 352 solid contour is. This is Kelvins. In this case, as I say, we have five times 10 to the minus 11 permeability, which is five times higher than the number I think that I like the best, but is within the realm of reality. And we had a heat source that ramped up linearly from 15 years to 60 years, so it was less of a heat source than a lot of other calculations you'll see.

And this shows the temperature as a function of time. The peak repository temperature in this case was reached in the center of the repository, which is the highest pair of curves, was 82 degrees C. and was reached around 90 or 100 years. And recall that the heat source didn't reach its peak until 60. The model went unstable at 160 years in numerical instability that we're working on right now, and we think this model has a lot of applications. First of all, our original motivation is better Carbon-14 travel times, but we think it can also be used for all sorts of applications for temperature calculations.

These four curves here in the middle are the ends of the repository. And one thing you'll see is that convection, strong convection effects increase the temperature difference between the center and edge of the repository because the convection is stronger at the edge because you have the localized temperature contrast.

DR. DEERE: Ben?

DR. ROSS: Yes?

DR. DEERE: Did you mention your loading?

DR. ROSS: Yes, what we did was we simply took 70,000 metric tons of heavy metal, divided it by the repository area, took a radioactive decay curve and phased it in linearly from 15 to 60 years after the waste comes out of the reactor.

DR. DEERE: What did that come out to?

DR. ROSS: I'm not sure what the peak is at 60 years.

DR. DEERE: 70,000 metric tons?

DR. ROSS: Metric tons, divided by--

DR. DEERE: What was the area?

DR. ROSS: Was it 6.2 square kilometers? 5.6 square kilometers. And this also doesn't have a confining bed in there, but at these early times I don't think that makes much difference.

Now, the second topic I will deal with is gas flow as a means of transporting Carbon-14. And this is a model that we've developed, again I should make clear, for Sandia and some of the results I'll show were originally calculated for Sandia and then for EPRI, we took those results and did some additional runs to provide inputs for their model.

We take into account there are a number of forces that drive the flow of the gas at Yucca Mountain. There's the natural geothermal gradient beneath the mountain, there's the heat of the repository. Both of those are in our model. There are seasonal and diurnal temperature fluctuations and barometric pressure fluctuations, which on long time scale should average to about zero.

The wind is a significant driving force which we don't model, and there seems to be something else that hasn't been figured out yet, perhaps comparable in importance to the wind.

The approach in our model is that we fix the relative humidity at 100 per cent and assume that the water can move around to keep it that way and don't worry about the water. We solve for a "Fresh-water head" variable, which makes it numerically more tractable, and the results I'll be showing here are earlier results where we got our temperatures from a steady state conduction model, which we assumed a fixed temperature at the repository and basically used that just as an interpolator to get some smoothly varying temperatures. And all the results I showed before and these were done by finite differences on a PC, and in fact we can do 8,000 nodes without going into extended memory.

Now, in addition to the gas flow, we look at the transport of Carbon-14, and we did some geochemical modeling a few years ago where we assumed, first of all, isotopic equilibrium of the CO2 gas and aqueous bicarbonate, which is an extremely safe assumption. We assumed the water to be an equilibrium with calcite, which is a much less safe assumption, but the people who, you know, some people who I trust say that their gut feeling is that's what it really is, but nobody can, I don't think, demonstrate it from clear data at this point.

We calculated equilibria using PHREEQE and using water compositions measured by Al Yang. And then to get travel times, we used the retardation factors we got from that and did particle tracking by integration inside the grid blocks using a method from Dave Pollock of the Survey.

The chemical model calculated retardation factors, which depend on temperature, we got different values for the three different units. That just reflects different relative proportions of gas and liquid, and they go down by temperature because of the solubility of calcite.

We modeled the gas flow in four parallel cross-sections across Yucca Mountain, each due east-west. This will give you an idea of the geometries we were using. We have three permeability zones. We treated the non-welded Paintbrush unit as having a higher permeability, but in the area that's heavily fractured, we gave it an intermediate permeability. And we solved this on a grid of about 4,000 nodes.

And here are some typical calculated gas flow lines to give you an idea of what the flow pattern looks like. You get convection cells around the edge of the repository that are closed. This is with the repository heated to 57 degrees C. And superimposed on that is a mountain scale convective circulation, and this particular unit has 100 times permeability contrast with the confining bed.

And with that approach, we're able to calculate Carbon-14 travel times, and they turn out to be very geometry dependent; depends where you start out in the repository. This is with no repository heat, and we get a wide distribution ranging from a little less than 10,000 years to around 35,000 years. And with the repository heated to 50 degrees, you can see it gets much shorter and remains widely distributed. But, still, a lot of them have a significant delay time comparable to the half-life and the 10,000 years.

Okay, thank you.

DR. DEERE: I think Buscheck said yesterday that he calculated and, Tom, correct me if I'm wrong, that the gas transport, in the absence of heat, was something on the order of 100 years. Is that right, Tom? Or the Carbon-14 transport. And I'm hearing you saying it's closer to 10,000 years?

MR. BUSCHECK: That's with no retardation.

DR. DEERE: Could you identify yourself?

MR. BUSCHECK: Tom Buscheck. That's with no retardation.

DR. ROSS: That's the same kind of number we get.

DR. DEERE: 100 years?

DR. ROSS: With no retardation, yes.

DR. DEERE: Okay.

DR. ROSS: And is that with no heat or with--

MR. BUSCHECK: No heat; that's with just the geothermal gradient.

DR. ROSS: Yeah. It's the same order of magnitude.

DR. CANTLON: Other questions? If not, we're running a little bit behind, so I think we'll go on to the next speaker, and then discuss the group at the end.

MR. BULLEN: I'm Dan Bullen and I'm with the Nuclear Engineering and Health Physics Program, on the faculty at Georgia Institute of Technology, and I'd like to discuss the portion of the EPRI model relating to the engineered barrier system, and specifically failure of the container and failure of the waste forms.

What you have in your packets, by the way, is a summary of all the overheads that I presented at the EPRI meeting previously this week. I'm not going to go through those. In the interest of brevity, I'll go through the highlights, and I'd be happy to discuss any questions that you may have on the viewgraphs that I don't discuss later this afternoon or at the breaks.

By way of introduction, I'd like to reiterate the goal of this portion of the project was essentially to develop a model for the evaluation of the impact of the following: the container failure mechanisms, the container failure rates, and the waste form failure rates.

Now, in the EPRI presentation, I did a review of potential degradation pathways, and in the current engineered barrier system design and in an alternate engineered barrier system design. The emphasis I wanted to make here is that this model is applicable to multiple alternate designs and different failure mechanisms. This is a high order model that can use mechanistic models at a lower level as inputs to give you the overall system performance.

The container failure models that were developed are essentially a single metal barrier failure, a multiple barrier failure, and I also have a parameter that includes the premature failure of containers.

And in keeping with the crux of the EPRI model, what I tried to do was to take a look at the conditions that Ben just discussed, those being hot, warm and cold conditions, talk a little bit about wet versus dry and oxidizing versus anoxic conditions, whether or not the repository sees any of these, and I'll give you brief results of the initial application of this model.

Again, a brief review of the engineered barrier system failure models, essentially you identify for a metallic barrier, general oxidation and corrosion, localized corrosion, which includes both crevice and fitting corrosion, stress corrosion cracking, and then phase instability or hydride embrittlement or any other metallurgical problems associated with the metal barrier.

I identified models of uniform oxidation and corrosion, localized corrosion, stress corrosion cracking, and I also looked at additional information in Phase 2 which took a look at the failure of the cladding to try and include the impact of the failure of the cladding. The two mechanisms were creep rupture and hydride reorientation.

When we first started this program, it became inherently obvious that it was difficult to consider all of the possible degradation models. There was also an uncertainty in the repository environment and currently uncertainty in the engineered barrier system design, where you have whatever type of package you would want to in place. So I decided to employ statistical techniques that had been used for component lifetime prediction and selected the 3-parameter Weibull function to determine the late container failure rate, and I employed an exponential distribution to account for the early container failures. And then I calculated a fraction of containers failed as a function of time to provide an input to the source term.

The equations that were developed, in fact this is just the multiple barrier rate equation, but what you see here is a failure rate as a function of time that has a fraction of containers that would be susceptible to early failure, and this will vary as a function of repository conditions and of engineered barrier system design, and it's just an exponential with an average time to early failure.

And then you kick in with Weibull distributions for each of the three, with a threshold to failure for the individual barriers and the cladding or the waste form, the poor container if you want to take credit for it. If you want to include it as an inclusive part of your model or an overall part of your model, the barrier.

The average failure times, this is a threshold time, this is the average failure time, and this is the Weibull parameter, which is the slope at the mean time to failure.

The parameters that I'm going to show you are for two cases; one is a single metal barrier failing in a cold temperature, the other is for a multiple barrier failing at a hot temperature. And for purposes of description here, I've gone through the literature and gone through some previous models and tried to determine how I would plug in Weibull parameters or the three parameters that I have as adjustable parameters in my model to mimic what I think the response of the repository would be.

Now, the purpose of this is to show you that I think the model works well within the EPRI scenario. The models that I use and the numbers that I derived are my own personal models. There are no panel of experts that have derived these models. But for the low temperature single metal barrier case with 5 per cent of the containers susceptible to early failure with a mean time to failure of 1,000 years and a threshold to failure of 1,000 and 3,000 years for the engineered barrier and for the cladding, and a mean time to failure of 5,000 years, and then an additional 4,000 years for failure for the cladding, with rate parameters of basically a uniform slope at the engineered barrier and an accelerated failure at the cladding.

You end up with a distribution that looks something like this where we have fraction failed on the "Y" axis as a function of time. And note the slope down here. Basically this is the early failure parameter, and then the Weibull statistics that describe the cumulative failure distribution.

Now, by way of illustration for the hot containers, T greater than 96, smaller fraction were susceptible to early failure, with a longer mean time. Thresholds for failure for the three barriers in this case, it's a multiple barrier case, of 2,000, 4,000, and then I took no credit for the cladding in the hot regime. Mean time to failure of 10,000 years for the first barrier, 10,000 years for the second barrier, and no additional time for the cladding. And failure rate parameters of slow failure rate, moderate failure rate and high failure rate. And, again, the distribution you get is similar and, in fact, for illustrative purposes, it works well to overlay the two, and you can see the change, this being the cold case and this being the warm case.

DR. CANTLON: And one being single and the other being multiple.

MR. BULLEN: Yes, excuse me, one being single and one being multiple barrier also.

So a quick summary; what I wanted to point out essentially was that the failure rates for single and multiple barriers in different temperature regimes have been calculated. I identified my parameters for the Weibull statistics. The applicability of this model would be that you could take a mechanistic or deterministic model, come up with failure rates that you think would be applicable, a mean time to failure, a failure rate at the mean time to failure, any incubation time in which you would expect no failures, plug it into this model and see the overall system performance.

That summarizes what I'd like to say. I'll turn it over to Mick Apted if there are no questions.

DR. CANTLON: Questions? Ellis?

MR. ELLIS: No.

DR. CANTLON: Okay.

MR. APTED: Good morning. I want to clear up a few informational problems and introduce myself. My twin brother, Nick Apted, couldn't be here this morning, but I'm Mick Apted. Also I'm not, although we are contracted with EPRI for this particular program, I am with a different company, Intera Information Technologies. And just a quick word, that's not the Intera; it's a different Intera than the Intera that's involved with the M&O contract. And if anybody wants a long history on that, they can buy me a coffee this morning and we'll go over, but anyway, I'm here to talk to you about the release rate models for source-term calculations that have been implemented during Phase 2 into this EPRI model.

My presentation, I have three parts to, and one is a short discussion of strategy and assumptions that are fundamental to what we're doing, and constraining, in a sense, what we're going to do, basically describing how the source-term model is driven by so much. If you remember these other parameters, if you remember the slide that Bob Shaw put up with all the various nodes, source-term comes actually rather late downstream in that series, so all these effects of flux, earthquakes, volcanic activities, how they change environmental parameters in the near-field are going to be talked about here.

And then I'll talk briefly and compress a lot into release modes and models, and talk a little bit about, by inspection, some of the important parameters that are going to come out of that, which of these are particularly sensitive perhaps to the issue at this meeting on thermal loading, and then get a little deeper into that, discussing just the tip of the iceberg on EBS data that is available or needed.

I'm not going to talk all these points, but I want to point out a few of them that are particularly important. One, from the beginning, the strategy for the EPRI model, as was similar to the strategy adopted by the Yucca Mountain project and the PACE-90 set of calculations, work that I was involved with with Tom Pigford and the Livermore group, basically that we're trying to identify all relevant release modes, identify those, with no a priori judgment about which, what is their likelihood of occurrence or probability of occurrence. That comes in further down the field, but we didn't want to get into the initial argument, how likely is that.

The models and parameters as I show will be identified for each mode, and the basically different scenarios, if you will, defined by different environmental conditions. These different environmental conditions, in turn, are driven along this fault-tree branching by a number of factors, seismic disturbances, thermally induced failure of the air gap is one that's particularly sensitive, the near-field is sensitive to that particular event if it occurs, elevation of the water table, et cetera, et cetera.

Basically within the proportion of waste packages that are undergoing release by a certain mode, individual waste packages having different parameters can be independently simulated also. So the repository isn't divided into just blocks of waste packages that are all performing in lock step release. So if some are undergoing, let's say, a mode in which water is dripping down there, the actual drip rate on different packages can be simulated independently.

And, finally, where we leave off, where we hand the ball off to the next group, is that the release rate in either units of grams or curies per year in terms of a release rate, which again is for the near-field people, of course, under the NRC regulations is one of the things that is looked at, how well is the engineered system working in terms of this one part and 10 to the fifth release rate mode, but we can also provide concentrations that go into the tuff host rock. So this is where we're cutting off our source-term, if you will.

Assumptions: basically we're going to look at two groups of radionuclides, ones that we believe will be insoluble and, hence, solubility limited. These may include cesium, tin, uranium, neptunium, plutonium and americium isotopes. Soluble or reaction rate with these nuclides may be constrained by their actual dissolution rate of the waste form rather than by their solubility of waste matrix or individual radioelement solubilities. But these include selenium, possibly technetium, iodine and carbon.

We'll get into some of this other information about where some of this inventory is located and how that affects the models.

Getting just briefly ahead of myself in terms of the viewgraphs I'll show, there are three basic broad categories that we're looking at where the mode of release is either by a wet-drip, which is a discontinuous model that the water, there is not a physical contact between the water that might be in the package and the water that's in the partially saturated surrounding host rock. If some continuous pathway were to form, we have a different set of models, varying from just moist conditions, unsaturated, to fully saturated, and finally the dry modes, in which basically we're concerned with release of Carbon-14. Anyway, so those are the three models that we're going to talk about, and I'm not going to get into all these other details.

All right, release modes and models. Here's a schematic view of the EBS release modes. It's getting perhaps a bit long in the tooth at the moment, but I think it's illustrative of the general modes that we want to identify.

For some large percentage of the packages, the expected conditions as to the package will remain dry. The air gap here design will continue to serve its function of representing a hydrologic break, and water will be diverted around that, and we're dealing basically with gaseous release of Carbon-14 is the mode.

Another circumstance might be that in some way water gets directed into an emplacement hole, causes a failure here near the top, and water either comes in and fills this container up like a bathtub, or there's a hole here at the top and at the bottom, and we have a trickle through. But, again, this is a drip model where there's not a continuous pathway between the waste form and the host rock.

Finally, there is, if there's a number of factors and some way that the air gap particularly is compromised or that a portion of the repository would become temporarily and locally returned to saturation, we have models in which there's a wet or a moist continuous pathway. Failure of the air gap in some way, tilting of the packages, contact basically, direct contact of the package and material that would allow diffusive, or diffusive conducted pathways from the package.

As I say, this type of broad break-out of models is very similar, actually it's identical to the strategy that was employed in the PACE-90 calculations for the Yucca Mountain Project.

Another way to look at it, and I should acknowledge contributions and a lot of help by Joe Pearson in this. This is Joe's slide. Basically, let me take you through how the function, continued function of different barriers here in the engineered barrier system affect which release mode. We have a canister or container, and if it remains intact, we're in pretty good shape. We don't get releases through the package.

If that fails, however, the next step down is to consider what protection might be afforded by the cladding. If that's intact, then basically we're looking at releases of perhaps Carbon-14 that is on the outside of the cladding. Eventually if we assume in some circumstances the cladding also fails and yet the air gap is intact, then we're either in this dry condition or perhaps in a wet-drip condition.

Finally, under a certain set of conditions if the air gap has failed in some way, either under unsaturated conditions or saturated conditions, flow or no flow. So there's a broad nodal branching showing you the different conditions by which we reach different release models.

Now, out of that broad category, we took a preliminary selection of five release modes; saturated conditions with hydrologic flow, saturated conditions with no hydrologic flow, these are wet continuous models, saturated conditions with the air gap intact, no dripping water, the dry condition, unsaturated conditions with the air gap intact and yet there is somehow dripping water, a filled bathtub and, finally, unsaturated conditions with a failed air gap where now there's thin film water, aqueous pathways for diffusion from the waste form into the host rock.

The five cases I've shown here each are subdivided into two cases. And, remember, I made the point that we're going to have to look at soluble radionuclides and insoluble radionuclides. Some are going to be controlled, and it's an important constraint by solubility. In other case, some may not be limited by solubility, and we're going to need information on the reaction rate of the waste form.

Again, it's a lot of mathematics here that one best follow in the references that will be in the EPRI report. This model was developed by Chambre and Pigford and others at University of California at Berkeley, as some of these other models have been, that we've basically taken some of their work and some of the guidance and previous work from Livermore, try to implement those into these models that we have.

Finally, EBS data, I really haven't gotten deeply into the theme of this particular soiree we've been having, but I want to talk about two features of the EBS data that have to do with transport considerations first, and then chemical consideration and chemical properties.

I think the transport considerations, unfortunately, are often neglected in the near-field. And, again, when one sees especially European and non-U.S. repository programs talking, and they talk this favorite word "robustness". One of the key aspects of the robustness is that they're having a good understanding and control of the mass transport characteristics of their near-field. That's where they're getting a lot of their performance, if you will, is in their understanding and engineering to the point of a robustness in transport considerations.

This is work by Jim Conca using an ultra-centrifuge, looking at the effective diffusion coefficient, this is in tuff gravel, as a function of volumetric water content. This is work that was published in the 1990 International Waste Management meeting that was held here in Vegas. Basically, this gravel has a porosity of about 45 per cent, so this value up here is about what we'd expect. All the pores are saturated in there. There are no chemical retardation effects that would mask true diffusion here. And so we get a value of what we'd expect, about 10 to the minus 5th centimeters squared per second.

But, interestingly, the gravel, now the pores are not filled with water, but much less filled with water, there's a dramatic fall-off in the diffusion coefficient.

I want to point out that for those nuclides that are going to be diffusion limited under partially saturated conditions, basically the release rate for all radionuclides are going to scale directly with this term. So that's not just soluble or non-problem radionuclides. Even the problem radionuclides, the iodines, the seleniums and so on, are going to scale directly with this factor.

DR. DOMENICO: Excuse me; Domenico. What's the porosity?

MR. APTED: The porosity is about 45 per cent.

DR. DOMENICO: 45 per cent?

MR. APTED: So this would be about saturation right here. I think the exact number would be in Jim's paper.

Well, let me go on. Okay, chemical data. We've talked in the past about that fuel, and the models were created particularly from fuel, is a very heterogeneous waste form, different materials containing different inventories of radionuclides that react differently with water, leading to my analogy is it's like a room full of a lot of radios set to different stations, and unravelling the actual contributions of fuel is still being done, both in close coordination between, I know, the Livermore workers in the modeling and the data collectors in that group.

Chemical data that we've used in this preliminary assessment, we were somewhat ambitious and threw a large net over a number of radioelements and radionuclides we thought might be of interest. This is very recent data that Livermore kindly supplied to us and to Golder Associates at a June meeting -- some of their best estimates currently, and I think these are based on EQ3/6 calculations, perhaps confirmed in some cases by tests on waste forms.

Here's some previous values that we used in a P&L report years ago, three or four years ago, and basically to fill in some of the gaps where there was some missing information, I want to point out that whether a nuclide is solubility limited or not, for example if technetium has a solubility of 9.9 times 10 to the 5th grams per cubic meter, it's definitely going to be reaction rate controlled. We go to this new value which is definitely going to be solubility controlled. So it's an important cross-over in terms of getting information to use in these models.

Finally, I'll talk a little bit also, there are some radionuclides that almost certainly will be controlled by the dissolution rate of UO2 matrix of fuel. This is work by Gray and Wilson using Approved Testing Material-105, ATM-1.

Basically this is in a stirred flowed reactor with oxidizing solution coming in. Basically we're looking at the pore dissolution rate, not a final dissolution rate, but the maximum dissolution rate to be expected for UO2 matrix.

Here, they're measuring uranium and cesium profiles or concentrations as a function of time. And from this, developing rate information about the rate as a function of temperature for the dissolution of UO2. From the beginning of this, we can now begin to put implicitly some temperature information into the release models.

Thank you.

DR. CANTLON: Thank you. Questions? All right, we'll proceed then with the next speaker and hold our discussion.

MR. McGUIRE: Good morning. My name is Robin McGuire. I'm with Risk Engineering, a contractor to EPRI. Our involvement is in integrating all of the inputs to perform an assessment and deriving some outputs and some conclusions there from. I'm being assisted by John Vlasity, also of Risk Engineering, and he really deserves the credit for all of the computer graphics that you'll see this morning.

Bob Shaw gave an introduction to the model that's illustrated here in the logic tree format where we take the major uncertainty in the problem and quantify those in terms of discrete alternatives and associated probabilities. This is set up so that the more independent terms are on the left side, the more dependent terms are on the right side, so that as a function of the terms on the left, downstream terms can be made dependent on those upstream terms.

So, for example, with respect to the topic of discussion this morning, the thermal pulse, we have that here as Node 11. We have a node here representing fractures of the bore holes. That is dependent on the thermal pulse. So values and probabilities are dependent on which branch precedes that node of bore hole fractures.

Similarly, as discussed by Professor Bullen, canister performance is a function of the thermal pulse. In addition, we have several other parts of the model that are a function of the thermal pulse that are not represented by uncertainties. Specifically parts of the source-term, the matrix dissolution rate is a function of temperature, and the near-field conditions, specifically there's no hydrologic or aqueous transport if the temperature is above 96 degrees C., that is, the containers are dry so there's no chance for transport of nuclides in that condition, and that is certainly a function of the temperature profile.

So that represents the model, the overall model. We have, in addition eruptions that result from the volcanic model and drilling and excavation scenarios that result from the human intrusion model, and in addition, we have a gaseous transport model, all of which I'll not be illustrating this morning. I'll only be talking about the aqueous pathways.

All of the inputs to this model have been prepared from other consultants to the project. The previous slide really represents a synthesis of a great deal of work from all of those consultants. So when you see a node here representing thermal pulse with three values and three probabilities, that really doesn't do justice to all of the work that's gone on to evaluate that node.

In particular, this one is relatively simple. We have, as described by Dr. Ross earlier, we have three temperature profiles. We weight those and have basically three scenarios, a hot, warm and cool scenario, representing those various fractions of those temperature profiles and, again, representing various temperature profile fractions among the containers. The last one being cool, the lowest curve having a weight of unity.

As further background to the model, what I demonstrated earlier represents one package here that loops over some 30,000 environmental engineering and nuclide characteristics. As part of the input, we have some other parts of the--some other modules that really give what we call transfer functions as input to the basic or the overall model.

Particular for this application, we have hydrologic transport that goes through hydrologic calculations and transport calculations for all of the relevant combination of parameters.

DR. CANTLON: Could we interrupt you? We're having a little trouble with your mike. We want to change the battery.

MR. McGUIRE: (after pause) Okay, let me repeat a little bit so that that can be picked up there.

We have several modules here that are inputs to the macro model. As an example, the hydrologic transport model goes through several calculations among the alternatives of the relevant parameters and gives values of grams output per container at time "T" as a function of unit input for all the different combinations of parameters that affect hydrologic transport and nuclide transport.

The advantage to that is that for these 30,000 combinations in that logic tree, then we don't have to go through 30,000 hydrologic transport calculations.

We have a similar case for the source-term. We go through many combinations there, but those provide an input to this overall model. So that we can very efficiently through this 30,000 set of calculations basically just doing arithmetic to get all of the combinations of parameters and the resulting concentrations of nuclides coming out for the 13 nuclides.

So integrate those two inputs, we have a similar set of transport functions for gaseous transport. That provides calculations of curies versus time for all 30,000 combinations of parameters. We then go to the display program that you'll see in a minute to plot those results.

DR. DOMENICO: Robin, excuse me; Domenico. If I read that correctly, the role of temperature in this whole analysis is strictly associated with the source-term; is that correct? It has no influence on the hydrolic transport?

MR. McGUIRE: That's correct.

DR. DOMENICO: That is correct; thank you.

MR. McGUIRE: In this model. It affects the gas transport. We're not talking about that today. Yeah, that's right.

So let's go to the results of those calculations. Again, for each of those sets of combinations, we calculate curies versus time, and what I'm showing here is, as a choice for Selenium 79, and this shows curies versus time here from 10 to the minus 3 at the bottom to 1,000 curies versus time from zero to 10,000 years. There's a dash line here at 100 curies, which is the EPA limit in the proposed standards. And the green curves show curies versus time released to the accessible environment downstream, in our model, 5 kilometers from the repository for all the different combinations of parameters in that logic tree.

What you see actually here are about 14,000 curves; the other 16,000 are off scale to the bottom, so those aren't plotted. What we do then is, of course each of these curves has with it an associated probability that it's just the product of probabilities along the path that leads to that end branch, so we can form a probability distribution of release at 10,000 years by going down from the top and forming a CCDF, a complimentary cumulative distribution function. And that's what we'll be showing for the remainder of the demonstration.

Now what we're showing here are the CCDF's at 10,000 years for all of the nuclides that we've looked at. It turns out some of them are off scale so you don't see them. We have the most important ones being Selenium 79 here and Iodine 129 being the green and the red, and also the total curve, which is the total of all nuclides. And now what we're using on the bottom scale is a normalized release. Since the EPA limit for some of these nuclides is not 100 curies, but 1,000 curies or 10,000 curies, what we do is normalize by that, and that is the proper normalization with which to compare this CCDF to the EPA criteria of probability of .1 at normalized release of 1, and a probability of 10 to the minus 3 at an EPA limit of 1.

So the total curve is the dashed red here, and you'll see very close to it is that Selenium 79 curve, the green curve, meaning that the other nuclides really contribute very little to the total on this normalized scale of EPA release. The other ones in particular over here, and other ones that are off scale really contribute negligible amounts. That's an interesting curve and it's the reason why I showed you the Selenium 79 earlier.

Another way, however, to look at the results and the sensitivity to the results is to plot separate CCDF's conditioning on some of the choices of the alternatives. It's equivalent to putting a probability, for example, of unity on high thermal loading and zero on the other alternatives instead of the weights of .6, .3, .1 as we've done, which leads to this total curve here. So if we can do that and show the influence on the CCDF of the range of choices of some of the parameters in the system.

What we're doing now is just pulling up specific CCDF's that we prepared in advance here from the cumulative files, and the ones we'll show first are the high temperature curve, the moderate temperature curve, the low temperature curve, and then we'll give you the base, that is, the integrated overall choices curve for Selenium 79.

So, again, here if you'd pull up the legend just for a minute, the dashed green is the base case or the integrated curve, overall temperature profiles, the green curve is the high temperature curve, and the red and blue are the moderate and low temperature curves.

And what you see is initially what you expect, the high curve leads to lower releases, and that makes sense because the repository is dry for the first thousand years or so, and the low curve, low temperature curve, is up here. That's very closely followed by the moderate temperature curve, which also is dry for the first thousand years. And you could ask, well, why is that, that should be more like the low temperature curve. And the reason is in the container performance calculations, the container performance equations.

From Professor Bullen, the equations for the moderate temperature curve are much more similar to the low temperature than to the high temperature. So that really governs where this CCDF comes out, more so, much more so than the temperature in the first thousand years, that is, whether it's at or above boiling or below boiling.

Okay, let's go to those specific canister performance curves, and we'll pull up another plot here, the first one being canister single barrier, the second being canister multiple barrier, and then again we'll show the base case.

So the single barrier curve, if you'd pull up the legend, please, the single barrier curve is the green, the high one. That leads to higher failures, early failures and higher releases relative to the integrated curve, the blue one. And the red curve here is the container multiple barrier, which leads to lower releases.

That represents the kinds of sensitivities and the results that we'll be performing over the next few months for EPRI. We'll be adding some models in addition, or some parts in addition. In particular, for instance, we have not represented uncertainties in solubility in this model, and we'll be adding that as a result of our discussion over the last three days.

But I think it's our position, or our observation, that this method of integrating over many different alternatives is really an important way to evaluate the importance of various factors in evaluating the potential releases from a repository.

I'll stop there and turn back over to Bob Shaw to give some summary comments.

MR. SHAW: We apologize for the handouts not parallelling precisely the presentations here. I realize in some cases that you do have information that's exactly the same as we've viewed up here, and in other cases you don't. It's a reflection of the fact that over the last three days, we were working through this, and as a matter of fact, many of the results that you saw up here will be changed as a result of discussions that we had in the last few days. There are a number of technical considerations in other areas where we have made decisions to change things.

What I would like to do here is summarize preliminary conclusions. I say preliminary for the reason that I just mentioned. As a result of our discussions over the last three days with my Methodology Development Team, there are changes that we are going to institute into the program, and as a result, some of the CCDF's and the sensitivities that you saw there will undoubtedly change. We will be putting together a report, our aim is to have it out very early next year in 1992, hopefully in January, that will go through all of the illustrations that you have here.

In addition, one of the items that was not included in the CDF that you saw here is Carbon-14. It was on the CDF's that were presented to us on Tuesday when we first saw it. We decided that there were some aspects of Carbon-14 transport that were not properly accounted for in there, and we didn't have time to reaccount for them, so Carbon-14 was deleted from that list that you saw there. Nonetheless, it will be in the final report as a part of the CCDF calculations that we make.

So these conclusions are preliminary only in the sense that in the next few weeks to a couple of months, we will be fine tuning on those.

Our first conclusion is that radioisotope releases are not very sensitive to the three heat transfer scenarios. The first CCDF set that Robin showed you showed the hot, moderate and cool scenarios, and it showed that for the case of Selenium, which is our controlling radioisotope in our CCDF, that it did not change very much as a function of those.

So our preliminary conclusion here is that it's not highly dependent on which of those three scenarios, and in a sense, that says it's not highly dependent upon whether you have a hot versus a cold repository.

DR. DOMENICO: Excuse me. With regard to that, Bob, this is Domenico, doesn't the hot scenario preserve the canister longer at least?

MR. SHAW: Yes, it does, and that's part of our scenario.

DR. DOMENICO: But you still come up with this conclusion?

MR. SHAW: That's correct. When we integrate all of the features together and don't look at just that one feature, this is the conclusion we come up with.

DR. DOMENICO: Thank you.

DR. NORTH: I wonder if you could give me some insight as to why the Selenium isotope should be the leading term.

MR. McGUIRE: It's the one with the highest amount and high solubility in the waste, so it has both those factors leading to large releases.

DR. NORTH: Is it dominated by one of Mick Apted's scenarios such as wet-drip? Is there an easy way to think through that issue?

MR. APTED: No, it's more the--all the top ones that you'll seen, Selenium, Iodine, and in some cases where technetium has a high solubility, it's more related to the fact that these are going to be controlled by their poor dissolution of the waste form and, hence, those are generally higher release rates than if they were controlled by the actinides and Cesium, are controlled at much lower concentrations right at the waste form surface. So it's not so much that as, the drip versus the transport.

Surprisingly, for the values we used, the release rates for those two very different modes of release are generally very much in the same ballpark, maybe one or two orders of magnitude for the same element compared between two modes. So it has much more to do with the performance of the waste form and the radioelement chemistry than it does with the mode of release.

MR. SHAW; The question you're asking, Warner, is really part of the work that we have in front of us for the next four to six weeks, is to try and look at this whole system and say why are certain radioisotopes controlling, can we identify the particular steps or set of steps that lead to that conclusion.

DR. NORTH: Yes, I'll look forward to those insights.

DR. LANGMUIR: Bob, I presume that at the same time, you're going to be looking at where the largest uncertainties are in arriving at that conclusion.

MR. SHAW: That's correct.

DR. LANGMUIR: Or do you have some preliminary ideas on that now?

MR. SHAW: That's right.

DR. LANGMUIR: At this point, you can't comment on that?

MR. SHAW: That's correct. So our system will be out in early next year.

Actually, what people are saying here is that there's an awful lot of insights that you can gain from that, and some of these, the most sensitive ones we will be aiming at, the controlling factors that Warner mentioned and the greatest uncertainties that you're mentioning.

Second conclusion we come to is that waste package behavior is a key ingredient in the model. The selections of that Dan Bullen mentioned to you for waste package Weibull diagrams are ones that he has selected, and he mentioned that a number of times, it was his selection, but they are based on technological deterministic kinds of calculations that have been done in the past, and that's their basis. But we do find that as you change those numbers, you do get significant changes in the release rate calculations.

So the kind of waste package integrity lifetime that is used as a part of this is a very key ingredient in this model.

Next conclusion?

DR. DOMENICO: Excuse me.

MR. SHAW: Yes.

DR. DOMENICO: Doesn't that seem to contradict the first statement?

MR. SHAW: Doesn't that seem to contradict the first--

DR. DOMENICO: The first conclusion, yes. I asked obviously the hotter the repository, the longer lasting the canister, but that doesn't seem to affect the radioactive release rates. The second conclusion is the waste package behavior is a key ingredient in the model. It seems to me those are contradictory statements.

MR. SHAW: The waste package behavior is the key ingredient in the sense that it determines the source term. The three scenarios that we chose among in the list come to the same conclusion as I have up there, that it's not very sensitive to those three heat transfer scenarios. I don't think those are self-contradictory. I think those are independent assessments. We'll look at that question.

DR. LANGMUIR: Does that insensitivity also correspond to all the other isotopes you mentioned, or only to the Selenium, since the Selenium is most of the release?

MR. SHAW: Do we know the answer to that question, Robin?

MR. McGUIRE: No.

MR. SHAW: We don't know the answer to that question yet. We only looked at the controlling one at this point, or the major one.

DR. NORTH: One of the scenarios I find most interesting is a combination of pluvial climate plus migration of actinide complexes with organics or in colloidal form. Have you considered that? And what preliminary insights do you have on the importance of that scenario?

MR. SHAW: Let me ask my crew that are here to support me or disagree with this conclusion, but I'll make the following statement. First of all, we do include pluvial conditions as one of the climate conditions as we move out. Secondly, organic complexing and/or colloidal forms are not included directly as a part of our scenarios. Is that consistent? Yes, that's where we are; yes, could be adopted, but is not part of what we're doing now.

DR. NORTH: I will urge that in future iterations for your exercise, that of the Department of Energy and other players in the performance assessment game, let's take a look at that one, please.

MR. SHAW: Okay. Next conclusion. Hydrology modeling is very complicated, highly uncertain and likely to remain so.

DR. DEERE: No question.

MR. SHAW: That is not necessarily something that falls out of the modeling, per se, that we've done, but it certainly comes out of all the discussions of my Methodology Development Team, and so you might call this my personal conclusion with regard to the discussions and interactions that we've had. It's not quantitative in the sense that the first two are.

DR. CANTLON: Bob, do you expect that to improve somewhat when we get a ramp down and get some insight to data?

MR. SHAW: I think that when we get down and we get better data, that will improve our confidence in certain parameters, and I also think as we get down and get better data, we'll find some surprises that will further complicate maybe the scenarios or the various pathways and other things. I think it's almost a net-zero-sum game, but I think we will improve confidence in some areas, hopefully the key areas. But I'm not confident that by getting down in there, that we are going to dramatically improve our awareness of the hydrology.

Despite the fact that we didn't link them very strongly, hydrology and temperature are intimately linked. Ben Ross talked about some of the features on Nitao's model that could cause it to be warmer, colder, and so on and so forth, and a number of those were linked with the hydrology pathways.

And, finally, and maybe most importantly, integrated performance assessment is vital. Just as we saw a couple attempts right here at questions that would focus on a particular scenario or a particular portion of what's going on, waste package or the pathway that Warner North just mentioned a moment ago, those are very interesting scenarios, but they all must be taken into consideration with regard to their likelihood and their probability, and only when we put them together into a complete performance assessment can we evaluate the relative importance of the various features that we're talking about.

We could say that for the last item here, too. When we get underground and we make better measurements, we're able to take performance assessment right now and say, okay, if we have a scientist who says I get underground, I'm going to improve my uncertainty on a particular parameter, plug it in, see what advantage that gives you, because we should be able to make those kinds of estimates right now, or at least reasonable guesses, and then one can come to some conclusions about how much that does benefit you.

Even if, and we confess our performance assessment model is rather crude, it's rather preliminary, it runs on a PC, it doesn't need seven Crays', you know, and so there's a lot of things that don't go into it, nonetheless, these kinds of performance assessments do give us a sense of where we're going and where we should be going.

So that completes our presentation, and all of us are available for any questions you might have.

DR. NORTH: I'd like to add some rather obvious comments I've made on previous occasions. I think you could add to your last conclusion, in addition to vital, difficult, time consuming and it will take numerous iterations for it to become adequate. This is not something that can be done simply by putting all the pieces together in an obvious way. Because of issues like the linkage between hydrology and temperature, it's going to be a long difficult process to get the understanding of these complicated issues to the point where you can get a consensus that we have them under control.

So let me not be unclear with respect to the challenge I threw out to you for another scenario to be considered, I applaud the work that EPRI has done in coming this far and would urge that DOE proceed with its exercise with all deliberate speed, and from these exercises, try to derive conclusions as to what are we going to do about temperature and what are we going to do about hydrology and how important are these issues relative to many other issues that might be raised such as the organic complexing of actinides that I just threw out as something that I want to make sure stays on the agenda.

MR. SHAW: Almost everything you said I agree with, but I'd like to take a little issue with a couple points you made.

You talked about the effort for performance assessment as being difficult and time consuming. There are certain elements of that, but it's important that we don't over emphasize that. There are really some very simplifying assumptions that you can put into this. There are transfer functions that one can use that allow you to go to the PC kind of calculation; that we shouldn't over emphasize difficulty and time consuming and use that as a way to prevent us from moving rapidly ahead to do performance assessments simultaneously with the more complicated modeling that is going on. And I'm pretty sure you agree with me, and I just wanted to point out that let's please not over emphasize the difficulty or the time consuming that goes into performance assessment modeling, especially over-view modeling.

DR. NORTH: I agree with you, lest there be any uncertainty, but the point of my little speech is let's get on with it, let's not wait.

MR. SHAW: That's where we are.

DR. DOMENICO: I'm surprised in your preliminary assessments you didn't mention the role, the importance of the source term, or is that locked up into the waste package behavior?

MR. SHAW: Yes.

DR. DOMENICO: Because some of us believe that the source term is so important that one day eventually we should have another meeting like this just dealing exclusively with the source term. Could you comment on that?

MR. SHAW: You could put your words into this conclusion that says waste package. Source term is another way of saying that same thing, so I totally agree with you.

There are questions out there?

DR. CANTLON: Audience questions?

MR. BUSCHECK; I agree with you about, you know, using PC's for performance high level modeling. However, for basic mechanistic modeling, you have to rely on a large mainframe, because we did sensitivity analysis for grid spacing and we found that if you use a coarse grid spacing, you could completely preclude any boiling behavior because you greatly increase the thermal dispersion and, therefore, you do not build up an adequate amount of heat in near-field.

Another comment is that with regard to low temperature evaluation, I'm positive that you've used something like a continuum model which is a great homogenizer. It makes many different scenarios look very, very similar because it does not include the effects of non-equilibrium, fracture flow wherein fracture flow can get to the mountain under low thermal loads in less than an hour. So, therefore, your model is not nearly sensitive enough at low thermal loads to look at the consequences if you don't have a dry-out zone.

The other things is we've been--excuse me for my nervousness, I'm going back to my old nervous ways in public, but we have been under a QA program, we have benchmarked the VTOUGH code extensively over a six year period of time. I would never present publicly results of a code that's been running for one and one-half weeks. I think that is very important, especially a code which has admittedly stability problems. I would never go forth and make conclusions with a code that has such a short pedigree. And I'd like to find out if there's been any benchmarking at all done against codes which have a longer pedigree.

Another thing that I'd like to point out is that

Ben's model assumes a semi infinite repository versus our three dimensional large scale model. You'll get substantially different buoyancy effects when you consider the finite areal extent of the repository versus one that's assumed to be infinite in the third dimension. So, therefore, your edge effects will be magnified by those boundary assumptions. I think it's important to look at the dimensionality of the problem before you make, you know, conclusions regarding that impact.

We have been looking at high fracture scenarios that are as high or higher than the ones Ben presented today, and we do not see a far-field, significant far-field effect on large scale convection. I've been on the phone with one of our programmers and, hopefully, will have faxed information that I didn't think I'd have time yesterday to present that, hopefully, will clarify some of that. But I do still feel that the far-field will be dominated by convection, and that the very strong cooling effects that are being shown by convection in the far-field I think will be shown not to be that significant, especially when a model with appropriate dimensionality is applied to it.

Also, the model I believe used a thermal conductivity value of 3.3. We used the values from the RIB for each and every hydrostratographic unit, all of which are substantially less than 3.3. A high thermal conductivity value will also lower repository temperatures, as will far-field convection.

Thank you.

MR. SHAW: I think Tom's comments and points dramatize a few points that I would like to, I guess, re-emphasize at this stage.

First of all, in developing a model of the nature that we've developed, we relied heavily on comments from others with regard to the strengths and weaknesses of what we have done. And that goes down to values of parameters, concepts and modeling that we've put into it.

Secondly, Tom's comments with regard to some programs that can't be run on PC's but have to be run on very detailed systems, is to us a very important element of what we're trying to do in our performance assessment. We rely heavily on those detailed calculations on very fine grids that other people are carrying out in order to give us input into our simplified model. And we look for what we've called transfer functions as a way of taking information of that nature, putting it in some kind of a simplified form, whether it's data, whether it's concepts, whether it's mechanisms, whatever it might be, to include in a simplified model.

So I'm keying in on saying both of these approaches have very important ingredients in the overall program. They have to be integrated. We went through the last two or three days, with our people, in getting to a point where some individuals wanted to get it down into great detail and we had to cut it off and say, no, no, that's too detailed for what we're trying to do.

So we have to draw a line, we have to draw a cutoff, and it's not so much we're saying it has to go on a PC, but a PC gives us a nice guideline for saying, hey, let's not get so complicated and into so much detail that we lose the broad picture. And the broad picture for us is how do you integrate these things, and how do you find out which, at this stage, seem to be the major important features that you end up with. What are the parameters which have great uncertainty, but seem to influence the result. What are the models which seem to influence it, and so on.

And to re-emphasize what Warner said, you have to go back and look at it again and again and again, because as you people are developing better details at the labs and at other contractors for what's happening, we have to then implement and integrate that into our performance assessment model.

DR. CANTLON: Other comments?

DR. ROSS: I just want to add something to what Bob said to make sure there's no misunderstanding.

There are certainly differences in parameters that went into my calculation versus Tom Buscheck's, very substantial differences. And I really don't know; I think the differences in the parameters are quite sufficient to explain the difference in the results, although they may not be the only explanation.

I would just add that what went into the EPRI report was not the result of that modeling that I showed at the end. That was just some new results by the way to show that the things that we said, you know, some of our extreme scenarios don't look like they're totally unreasonable. But they're not the basis of the numbers that went into the rest of the model.

DR. CANTLON: Other comments from the audience? If not then--

DR. DEERE: One comment.

DR. CANTLON: Oh, all right.

DR. DEERE: I would hope that the approach isn't such a homogenizer that any information which is in-situ ground true with respect to very permeable zone, impermeable zones, which are not going to make any difference, that whether we go underground or not, meaning whether we do or do not understand the geologic and hydrogeologic framework really is not going to change your conclusions. I would really question that.

MR. SHAW: I understand. I think it improves our understanding and it improves our confidence, but I don't think we should feel that just simply things are going to get better and better and better. I think when we get underground there's going to be other complications, other things we hadn't thought about, a few surprises. I think we already have situations where when we attempt to do hydrologic modeling, for example, and then compare that to what happens at other places like Rainier Mesa, we find it very difficult to take those hydrologic models and come out with the appropriate results that we see elsewhere.

And so it gives you question as to what extent are we really capable of modeling, especially a multistratographic system with a variety of heterogeneous systems, even within the given stratigraphy. And so I'm not saying let's not get underground, that it's not going to be of any benefit to us. I clearly believe that it will be. But I don't think we should be in the position of saying once we get underground, that's going to solve all our problems, we come out with a model and we'll be very confident in the model and in the parameters and we'll make the calculations and we'll submit it to NRC and they'll say oh, yes, of course, this is great, stamp it and, you know, off we go. That's the only skepticism I'm trying to lend into this particular discussion.

DR. DEERE: Yes, and I have the opposite; that if the model can't handle the complexities, it's not the geology that's wrong, it's the model that's wrong.

MR. WILDER: I'd like to just follow up on that slightly. I think that I agree that once we get underground, there's going to be a lot of things which are surprises. We saw that at G-tunnel. But I think also we have a history, a track record, if you will, at G-tunnel which has shown that the hydrology as you get underground can be better understood and that we can make tremendous strides in our understanding. And so I guess I'm not quite as pessimistic as I think I hear you expressing.

MR. SHAW: I accept that.

DR. CANTLON: Other comments? All right, if not then, we're recessed and we'll try to get back here at 10:30.

(Whereupon, a recess was taken.)

DR. CANTLON: Could we reconvene? I'd like to ask Mr. Cloninger to introduce the next speaker.

MR. CLONINGER: Mike Lugo of SAIC will present some of the regulatory legislative considerations regarding thermal loading. That will be followed up by Mike Voegele of SAIC who will be presenting the overall conceptual considerations for total system, this being the mined geologic disposal system again, the performance of that system. Then I will follow that with a very brief summary prior to the round table session this afternoon.

DR. CANTLON: Very good. Thank you, Mike.

Then let's start off with David Jones. I understand there was a motion that we change his name to Michael so we'd have all Michaels on this morning. But since symmetry isn't a requirement of the session, we'll proceed. David?

MR. JONES: Okay, as you all are aware, in the last two days and into this morning, the board has been inundated with technical information pertaining to various thermal loadings on the repository. My purpose for being here is to give you all an idea of the economic implications on the remainder of the high-level waste management system, in addition to the repository from the various thermal loadings.

I'd like to start off with a brief clarification of the subject. Right now, it's stated as a comparative cost presentation. It won't be a true comparative cost presentation because there really hasn't been a detailed cost analysis of various thermal loadings. All the system costs that have been calculated to date are based on the SCP/CDR design of maintaining 57 kW/acre. But what we do have are some numbers that will give you an idea for cost implications of some of the scenarios you've heard over the last couple of days in terms of aging the fuel and varying the subsurface area to achieve various thermal loadings.

In order to make an assessment of cost implications of the thermal loadings, one would first have to have an understanding of what the current life-cycle costs for the system are, and then in addition to that, what the basis for the development of those estimates are. So that will be the first area that I'll talk about this morning.

From there, there's two basic approaches to adjusting thermal loadings at the repository utilizing the system. One is to work with the current system designs and current system assumptions to achieve different thermal loadings, and the other that I'll talk briefly about is potential design changes with significant cost impacts on the system.

This first section will be a presentation on the current TSLCC estimates, as they're called. The TSLCC estimates feed into the program's annual evaluation of the adequacy of the fee. These estimates are taken from the last published set of cost estimates for the system, which is the 1990 TSLCC addendum report. All the numbers are presented in constant 1988 dollars, billions of 1988 dollars.

The TSLCC is comprised of five components; development and evaluation, transportation, repository, which is broken into first and second repository, MRS facility and benefit payments.

In addition, we evaluate various scenarios within the TSLCC in an attempt to try and bound the system costs for purposes of fee adequacy analysis. And for this reason, we evaluate both a single and a two repository system. In both cases, in the single repository, it's the tuff repository at Yucca Mountain; the two repository system, the first repository is the Yucca Mountain repository, the second repository is assumed to be a generic repository at an unspecified location.

As you can see, the single repository estimates currently is about $26 billion, and the two repository system is $34 billion, with the majority of the costs increase there due to the second repository itself.

Some of the underlying assumptions that feed into this, and I'm not going to go over all the detailed assumptions related to the cost development there, what I'm trying to do is focus on ones that have some relevance to the thermal loading issue.

First off, on the first repository, Yucca Mountain repository assumptions, the design is based on modified SCP/CDR and RCS designs for both the surface and subsurface. The first repository in both the single and two repository systems is assumed to begin in 2010. All spent fuel is assumed to be emplaced as intact assemblies in the hybrid disposal container that you heard Eric Ryder describe and present the day before yesterday. That's a thin walled stainless steel container, is the assumption for the cost developments at this point.

The repository capacity is dependent on the system. For the first repository, the single repository system, Yucca Mountain, is assumed to accept and emplace all the waste and the cost estimates there are based on 96,300. Of that, about 9,500 is high level waste, both defense and civilian high level waste; the remainder being spent fuel.

The two repository system, the one that most people are familiar with, the Yucca Mountain, capacity of 70,000 MTU. Here, about 10 per cent of that is high level waste.

The subsurface layout in all of these costs for the Yucca Mountain repository is based on maintaining that 57 kW/acre. Our cost model for the subsurface has been developed in conjunction with the project office and their contractors, and the basic methodology is that it makes adjustments based on the age and characteristics of the fuel on an annual basis to maintain a 57 kW/acre in the subsurface. This allows us to do various scenarios of predicted repository start dates.

The MRS facility assumptions, the MRS costs are based currently on a storage only facility. MRS facility for these cost estimates that have been presented is assumed to begin limited waste acceptance in 1998 with the full capability MRS beginning in the year 2000. This is based on the secretary's report, 90 day report.

The storage concept utilized and developed in the cost estimates is assumed to be a dry cask storage concept.

Additionally, the MRS is assumed to service only the first repository in the two repository system, and it is assumed to service the single repository for the entire life of the repository.

All spent fuel shipped from reactors was assumed to go directly to the MRS before it goes to the Yucca Mountain repository, and the Peak MRS facility capacity is 15,000 MTU, with a linkage to the repository schedule limiting it to 10,000 prior to repository operations.

In the transportation area, the transportation cask designs are based on reference ten year old spent fuel, and the acceptance and transportation logistics from reactors to the MRS facility was based on "oldest-fuel-first" acceptance priority.

Development and evaluation component of the TSLCC estimates include all the siting, preliminary design development, testing, regulatory, and institutional activities associated with a waste management system.

D&E costs also include the administrative costs for oversight by the Federal Government of the high level waste program. And also, under the design component of the D&E, includes all pre-LAD costs for the transportation and repository and MRS facility.

All right, from here I'd like to talk a little bit about the cost implications of different thermal loadings if you use the current system designs, working within the current regime of designs for transportation system, MRS and repository.

You've heard several options for achieving different thermal loadings. The two primary ones which have been addressed in this meeting fall under what I would say two categories or two techniques of achieving different thermal loadings within the current system design.

The first is customizing the emplacement of waste packages, basically making adjustments to the borehole and/or emplacement drift spacing to achieve a different thermal loading; a smaller subsurface repository, giving you a higher thermal loading, a larger giving you a cooler repository.

The second technique is one that I would classify as levelizing or heat tailoring thermal output. One approach to doing this is to use the MRS as a big surface storage facility to allow the fuel to age to get it to a point where you've got thermal output from the fuel at the time of emplacement giving you the desired thermal loading at the repository, a lower thermal loading.

Under customizing the emplacement of waste packages, again, working with a smaller subsurface to give you a higher thermal loading, and a larger subsurface to give you a lower thermal loading. What we've done is we've gotten some numbers and worked with Eric Ryder on his numbers. You saw him give several scenarios of kW/acre and some of his presentations and his videos.

What we've done is we've taken some numbers from him on the number of panels used, et cetera, and we've calculated some mined volumes for each for three target thermal loadings; 30, 57 and 80 kW/acre.

At 30 kW/acre, your mined volume is approximately 353 million cubic feet. 57, you're at about 300 million cubic feet. And 80 kW/ace, you're at about 255 million cubic feet. This includes the common areas. These are very rough calculations. These are not done to the precision that the numbers that are in the TSLCC are.

So what you're seeing there, at the reference 57 kW/acre, you're at about a subsurface cost of $3.1 billion. Going to the other two extremes, you're going up or down 400 million respectively to get to that different thermal loading.

Another point to make about this approach is that the only significant impact on your system cost is the subsurface costs. You're not impacting your transportation costs significantly. You're not impacting MRS facility or repository surface facility costs. You're achieving different thermal loadings with just adjustments to the subsurface.

Now, Eric did have a couple of different approaches tied in with his, one of which is aging the fuel in the 30 kW/acre scenario, and also he's assuming a levelized heat rate coming into the repository where we assume oldest-fuel-first. And the two next slides will address both of those in addition.

Providing long-term surface storage at the MRS facility prior to emplacement to achieve a lower thermal loading, we have done a case at the request of the MRS Commission where we looked at an MRS starting on time, a full functioning MRS starting in 2000, and a repository which was basically delayed about 45 years. The idea here is they wanted to get an idea of the cost estimates for a repository emplacing spent fuel at a minimum of 50 years of age.

What we found in doing this case is that the MRS operating costs increased $2 billion for the single repository system, on the order of $1 1/2 billion for the two repository system. In addition, your D&E costs are going to go up about $2 billion for both cases. The main reason here is due to the fact that you're increasing the period of time over which the government administration of the program has to be accounted for, and you're lengthening some of the other development programs for the other pieces of the system.

A key point to make on this is that it also assumes an unconstrained MRS facility which accepts basically the entire inventory of spent fuel prior to its being shipped to the repository.

In this case, there would be, again, no significant impact to the transportation system. If you're just going to use your MRS facility to do the aging, for the same area of repository for an on time repository, you can achieve a lower thermal loading without any significant impact on your repository costs also.

Now, in this particular case, it's providing about 45 years worth of storage. Eric presented a case where I think his 30 kW/acre relied on 30 years of additional storage. So this gives you an idea that this impact would go along with the $400 million savings that you saw in the previous slide in the particular case that Eric presented.

An additional option for achieving of different thermal loading which warrants further consideration, I would put this under the technique of levelizing or heat tailoring at the MRS facility, is that by using the MRS to provide you with a level heat pattern of fuel coming into the repository, you can achieve--the MRS system study found that producing a level pattern of annual average decay heat emplaced at the repository could be accomplished with an MRS facility with a capacity between 20 and 25,000 MTU, even accepting oldest-fuel-first from reactors.

Right now, the TSLCC estimates are based on a maximum capacity MRS of 15,000. So what you're talking about increasing about 10,000 MTU at the MRS facility in order to achieve that, and that was another one that Eric relied on another approach that he relied on as achieving a levelized heat pattern. So to accomplish this, you're talking about an extra $500 million increase in the MRS costs for a 10,000 MTU increase.

The next area I'd like to talk about is some of the potential design changes with significant cost implications on the system. All the previous discussion that you've heard is based on using the current system designs. Obviously, if we go to a different thermal loading, if we target a different thermal loading, there could be significant design changes that have ramifications throughout the entire system. I'd just like to point out a few of them and in some areas, I can address cost issues.

Repository, in the waste package area, changes in materials; again, I pointed out we're relying on a stainless steel waste package right now in the estimates that were in the 1990 TSLCC addendum. Making a simple change in the materials, we did an analysis in the MRS system study where we looked at a copper waste package, the same design, the same hybrid container, 3PWR, 4BWR. The stainless steel waste package has a unit cost of 32,000, is what we're assuming. Going to the copper, I think it was a $73,000 unit cost on that. So you're talking about almost a $2 billion increase in the single repository case in waste package just from a simple material change. Well, I won't say simple, but just from changing the materials.

In addition, you can have significant changes by changing the capacity, or we heard before about reducing the number of assemblies in a waste package and resulting in more waste packages. So waste package has some significant implications.

There's also been some talk about a universal cask or a multi-purpose package. That has some significant implications on the MRS transportation and repository throughout the entire system, as well as D&E. Any of these that are listed here under design changes will impact your D&E costs because you're going to have to go back and do some redesign, preliminary redesign work.

There has not been any attempt to try and quantify a lot of these just because of the fact there's too many variables, too many options. It's one thing that I would point out that's very important in terms of when you're looking at all the options at the repository, costs have to be a consideration. They are not probably the primary factor at this point in time, but they have to be a consideration, and in terms of not just on the repository, but the entire system.

Another area is subsurface layout. Again, all the discussion that has been to this point in this presentation has been on basically the same SCP or layout. If you go to horizontal emplacement or drift emplacement, there there's cost implications that have not really been assessed with the current system.

Surface facilities also; waste handling building, hotcells. We've heard about ventilation. If you're going to add additional ventilation, you're going to need some support facilities on the surface as well as considerations on the subsurface.

At the MRS facility, again, the storage concept, getting back into the issue of achieving a levelized heat rate at the repository. If you're going to go with that approach and you decide that you want your MRS to basically be a heat sink, that you can adjust what the annual average thermal delivery to the repository is. With a storage concept, a dry cask storage concept doesn't optimally do that for you, going to a modular vault where you have less repetitious handling of the spent fuel; having to pull in numerous casks versus being able to go to different vaults is a more optimal design and you have cost implications there.

Total storage area and also extended operating life implications. If you go to a long MRS surface storage, there's some extended operating life implications that can have some cost ramifications also.

Under transportation, again, the cask is the main concern here, with the materials, capacity and also on a universal cask, how you're going to handle a multi-purpose type of package.

I'll try and summarize some of this. If you're going to stick with utilizing the current system designs for the waste management system, in terms of the economic impacts, the approach that has the least impact on the entire system is making the adjustments in the subsurface only to achieve your different thermal loading. Whether this can be done with the quantity, the type and characteristics of the fuel you're going to be receiving, that gets into whether you're going to have to use your MRS facility as a heat sink.

But basically making the adjustments in the range of what we've heard throughout the three day session so far, you're talking on a ballpark estimate of about $400 million, which represents a 1 per cent increase or decrease in the total system costs.

At the MRS facility, if you go to long-term surface storage, what you're talking about doing there, depending on the system, is increasing the system cost by 16 per cent to 10 per cent, based on the current estimates and based on a dry cask storage design.

So, here, you have more of an impact. Also, if you're using this in conjunction with the adjustments to the subsurface, you've got to account for that because that can either increase or decrease the total impact on the system.

And, finally, the MRS facility utilizing that to achieve a level pattern of annual average decay heat, going to an inventory of about 25,000 MTU would represent a $0.5 billion dollar increase in your MRS operational costs without any significant impact on the remainder of the system.

So this is what I've been able to pull together for you today. Obviously, there's numerous scenarios that could be costed out. The problem is trying to determine which ones are feasible, which ones aren't feasible. We did not do or undertake an intensive cost analysis. As I pointed out, all the system costs to date are based on 57 kW/acre. At some point, it would be very worthwhile doing a cost analysis to try and get some trade-offs between some of the scenarios you've analyzed over the past few days.

DR. CANTLON: Okay, thank you. Discussion? Question?

DR. DEERE: Yes. When you use the word "large cost implication," you're including savings as well as increases?

MR. JONES: Yes.

DR. DEERE: Because obviously, when you start looking at the repository variables that you haven't yet costed out, different orientations, different size, reduction in volume by 50 per cent, things such as this are not going to be increased costs.

MR. JONES: That's right.

DR. DEERE: They're going to be decreased costs.

MR. JONES: That's right. I have not tried to quantify whether large means large increase or large decrease.

DR. DEERE: Thank you.

DR. DOMENICO: When you estimated the cost of increasing the size of the repository, did you just take into account the cost of removing the rock, or did you take into account the added investigations, the cost of the added investigations?

MR. JONES: You're talking about on the single repository case?

DR. DOMENICO: On the single repository.

MR. JONES: We included additional site characterization costs. We have some very basic assumptions in there in terms of expanding, for that repository expanding into the northern block. You've seen some diagrams I guess in the last two days about the additional areas that have the most promise in terms of expanding into. And we've included additional costs for site characterization of that additional block. We've included costs for an extra exhaust shaft into that area, costs for extending the mains up into the northern block. So, yes, there are additional costs accounted for.

DR. DOMENICO: Thank you.

DR. CANTLON: Other questions or comments? If not, we'll proceed then to Mike Lugo, SAIC.

MR. LUGO: Before I start, let me just carry on this thing about names we were talking about before, where Mick Apted talked about his twin brother, Nick. I just want to reassure that even though this says Miguel and your schedule says Michael and it says here Mike, my mother did not have triplets. Okay? She did have three kids, though.

With that out of the way, what I'm going to be discussing with you today are the regulatory considerations with high or lower thermal loading, and I'm going to touch on the key regulatory requirements that relate to this thermal loading issue, also touch on the concept of licensability and the compliance approach. This part of my presentation I sort of view as a lead-in or setting the stage for the next presentation that Mike's going to give where he's going to give more detail into the actual technical requirements that are in the regulations.

I'm also going to touch on the legislative implications to expand, and some of the things that they just finished talking about where if you wanted to use the MRS facility for extended storage and for cooling of the waste before you put it into the repository.

In 10 CFR 60, there are primarily two requirements that I guess the first one would be the key driver for this thermal loading consideration, where in 60.133(i), it basically says that you must account for the predicted thermal and thermomechanical responses in the design of your facility in meeting your performance objectives. And also you must include this in your license application, this anticipated response of the rock.

The performance objectives that are called out in 133(i) are basically, in the next couple of viewgraphs, for the preclosure time period, we have 111(a), which basically invokes the radiation protection requirements of 10 CFR 20 and also 40 CFR 191, as well as the waste retrievability requirements in Part 60.

For the postclosure period, we have the total system performance requirement, which again also references 191. We also have the requirements of the waste package containment for 300 to 1,000 years after waste emplacement, the 10 to the minus 5th release limit per year of the engineered barrier system, as well as the 1,000 year ground water travel time requirement.

Now, there's a lot of requirements that basically roll up or support demonstration or compliance with these performance objectives and that's what Mike is going to get into later on, he'll go into a little bit more.

What I wanted to say, though, about these requirements is that basically the regulations don't necessarily point to any preference between one thermal loading or the other. It basically says here are the requirements, you are the applicant, you have to design the system and you basically have to show compliance with the requirement. So, therefore, you don't really have any lesser or greater requirements imposed on you as a result of the thermal loading that you do choose.

What it does and it could affect would be the demonstration of compliance. And what I mean by that is, and since everybody else has been referring to Tom Buscheck's talk, I might as well do the same thing, where we talk about for the different thermal loadings, you could have a different drying effect. Well, obviously, if you drive away the water with a higher thermal loading versus not driving it away with lower thermal loading, that could be a different consideration in how you choose to demonstrate compliance with the requirements. So that's what I mean and I'll get a little bit into that later.

But before I get into that, I wanted to get philosophical here for one viewgraph at least. This whole concept of licensability, a lot of people seem to think that regulatory compliance is a magical thing. It's very, very much tied to technical considerations. And basically, licensability is largely a factor of how you go about demonstrating that the technical requirements have been satisfied.

Admittedly, there are also procedural requirements that you have to think about when you go to licensing, but in the form that we're talking about here, which is thermal loading, this is really what we have to think about.

Now, you know that when you submit a license application to NRC, the reviewer is going to continue to ask questions, and we know that based on history, until he or she is satisfied that these requirements have been met. And a few of the things that an NRC reviewer looks at when they look at a license application is, you know, basically these are four key items, how much data do you have available that supports your technical conclusions, also what kind of data is that, is that QA pedigreed or not, or is that something you just took off the shelf that you haven't qualified, for example. Also the precedence; is there any precedence to what you're trying to propose to the NRS, has this been done before, is this a first of a kind design that you're proposing. Also complexity. Obviously, like you say, in a Cadillac you've got more things to go wrong, so the more complex you have a system, the more defense I guess you have to have for that. So basically, the simpler system would be simpler to defend.

So with all that in mind, obviously a design with fewest uncertainties and the least controversy is likely to receive a more favorable review from the NRC.

Now, with that as a philosophical backdrop, with respect to how that affects demonstrating compliance with the regulations, just as an overview here, for the preclosure time frame, demonstration of compliance is mostly dependent on design of the engineered system. And I put in here also operating procedures, and I know we haven't touched on that too much except for I believe Eric Ryder's viewgraph which showed the decay of the radiation over time, but obviously if you use newer fuel, you have higher radiation potential so, therefore, here is where you have things like a laver coming into consideration. You would maybe need more shielding, you would need unit operations. Your procedures that affect those need to be different.

So basically that's something that in our view it's within reasonably available technology. It's been done before. It's not something, you know, in general, people talk about the repository as being the first of a kind, but when it comes to preclosure, it's really not the first time it's been done before. So from a regulatory perspective, we really don't see any major glitches or problems with meeting the preclosure requirements.

As far as postclosure is concerned, obviously, these requirements require an understanding of the EBS as well as the geologic setting. And just because of the time periods involved in the postclosure time frame, we're talking about 10,000 years and maybe further into the future, that in itself causes certain kinds of uncertainties aside from this whole thermal loading issue. This is just one aspect of it. And obviously the amount of regulatory uncertainty that we have when we go into licensing is going to be dependent on how much and what understanding we have on that.

And as has been pointed out by the technical speakers over the last couple of days, these uncertainties we believe are going to be addressed and I think reduced to a reasonable level during site characterization, waste package testing and performance confirmation.

Now, I want to bring something up here which I was sort of surprised hasn't been brought up yet by the other speakers, and that is the whole issue of uncertainty. I think what a lot of people are talking about are really unknowns. At this point in time in the program, we haven't gone underground, we haven't done the full testing program, we still don't know certain things. It doesn't mean that we're never going to know it; it just means that we have to go out and get that information. We know what we have to obtain. We just have to go out and get it. And I guess there maybe is some residual uncertainties, but I guess you may have heard from some of the speakers over the last couple of days about some areas that have higher uncertainties whether it's higher or lower thermal loading, and I think a lot of that is driven by just the fact that we just don't have the information yet. We just haven't done the testing program.

Okay, I'm going to change gears here for a second and talk about legislative implications. And as I said before, what I'm trying to home in on, and you've heard various ways, and Dave touched on some of those and so have the speakers over the last couple of days, on various ways to reduce thermal loading in the repository. A lot of those have to do with engineered type arrangements. The one I'm going to address is whether or not you decide to cool the fuel off site, and in my case here, at an MRS facility.

Now, as we all know here, the NWPA in its amendment actually set the federal policy on geologic disposal, and it did include a schedule of the program activities, even though it was at a very high level. Implicit in that schedule and in the NWPA is an emphasis on early or timely disposal, not on storage. And I know Carl Gertz alluded to this in his opening remarks, and basically that's the, I guess, the torch that DOE has been carrying over the last few years to try to get to that timely disposal.

Now, I used Congress here just so I wouldn't blame DOE or TRB or the state or anybody else, I figure there's nobody from Congress here. Assuming that we want to go and emphasize extended storage at an MRS rather than disposal, there's obviously various things that need to be done to the regulation, and I just picked out three which I believe are at the top of the list, and one is de-linking the MRS from the repository.

As you know, right now in the regulation, the NWPA, there are certain provisions there, for example you cannot select a site for an MRS until you have recommended a site for the repository to the president. Also, it talks about you cannot construct an MRS until you've received construction authorization for the repository. And there's at least one or two other ones in there.

The second one on the MRS capacity limits, right now, the NWPA limits the MRS before you start waste emplacement to 10,000 metric tons per year. And after you start waste emplacement at the repository, limits it to 15,000. Or is that total? It's per year? Total; that's what I thought, okay.

Also, right now, the NWPA only authorizes DOE to go for one MRS. And obviously if you did not get revision to capacity limits and, therefore, you wanted to have multiple MRS's, that would be another area that would need to be looked at.

Now, to turn to the side here and sort of close the loop now, assuming that these changes in the NWPA were brought forth and, therefore, it had resulted in an emphasis on extended storage rather than disposal, there are some impacts that we've got to consider to the Civilian Radioactive Waste Management Program and actually to the nuclear industry in general. And one is that basically it would take the focus away from finding a permanent solution for the high level waste problem, which was the whole underpinning for the NWPA. And indirectly, obviously, you would see that there could be an impact on getting new reactor licenses or extending the present licenses just due to the fact that there is no permanent solution yet.

And as a side impact, I put on here the fact that if you were to now use the MRS for extended storage, and when I say extended, I'm talking about long time periods, 50, 100 years or whatever, more so than is presently in the reference Waste Management System concept, I would say that since licensing is very much of a public forum and public views are very much part of the licensing process, where the MRS may end up being a harder facility to license if that were the case, primarily because of the fact that the public would view that as a de facto repository, or could view it.

So having said all that, this is what I would like to leave you with today, and that is that the regulatory requirements in themselves do not vary depending on the choice of thermal loading. They're there and you have to meet them. It's up to the applicant to show compliance with them. Also, the regulatory uncertainty, and that is licensability, is primarily a factor of the defensibility of technical conclusions. I know a lot of people like to separate regulatory and technical, but they're really very much intertwined.

Now, for the preclosure operations, a higher thermal loading is not expected to cause any regulatory concern. Like as I mentioned before, we believe that much of that, if not everything, is really within reasonable available technology. For the postclosure performance, and I don't think that this is mainly due to thermal loading, but just the whole time period that we're talking about, which is 10,000 years, that the level of regulatory challenge will depend on the extent to which the testing program could reduce those uncertainties or address them. Or like I said before, just obtain the unknowns that we're talking about.

And then as far as the legislation is concerned, an emphasis on cooling of waste at an MRS facility would require some legislative initiatives as well as re-focusing of the whole program.

DR. CANTLON: Okay, thank you, Mike. Comments or questions from the board? From the audience?

DR. RAMSPOTT: I just had a question, Mike. From your viewpoint, would the idea of cooling the waste in place during the 50 year retrievability period have any implication as far as either legislative or regulatory?

MR. LUGO: No. Is that quick enough? I've been to enough licensing hearings, I just say yes or no if that's the answer.

MR. SMITH: Jay Smith, Edison Electric Institute. Mike, do you foresee any troublesome aspects of repository licensing resulting from the impact of licensing precedence of nuclear power plant applications?

MR. LUGO: I assume you're talking about the preclosure, since postclosure is not really something that's been done before as far as nuclear power plants.

MR. SMITH: No, not necessarily. We're dealing with a geologic environment and some nuclear power plant licensing applications have been greatly troubled by geologic environments, faults and seismicity in particular, so I was just wondering if you see any precedence that might somehow, through the licensing process, be applied to the repository that might be troublesome.

MR. LUGO: I can't really think of any that will be troublesome. I can hopefully think of some that would be advantageous where, like I said before, a lot of the operations and things like that that we're talking about at a repository have been done before at spent fuel handling facilities and fuel handling buildings and things like that. And the fact that we want to always make the differentiation between the fact that a repository is what you would call a passive system versus an active system, which has a lot different types of, we will call, risks involved.

I guess the answer is no, I really can't think of any off the top of my head that would be negative.

DR. CANTLON: Other questions? Okay, thank you, Mike.

The next speaker then is Michael Voegele, also SAIC.

DR. VOEGELE: Members of the board, ladies and gentlemen, staff, good morning. It looks like it's become fashionable to start these presentations by making some reference to something to do with your name. I understand there's several people in the audience who have used the term "butchered" with respect to the way my name is pronounced. I apologize. We've been in the United States for a very long time. My great grandfather didn't even speak German, so I can't do much to help you with that. I can't explain why they haven't gotten rid of the extra "e".

DR. DEERE: There's two extra "e's".

DR. VOEGELE: No, no, there's only one. The last "e" is a diminutive; that's supposed to be there. That's supposed to be there; it's the umlat that fell over on its side that bothers most people.

Okay, I started the last presentation that I gave to this Panel by making reference to suggestions that you observe the reactions of the Structural Geology and Geoengineering Panel to see what they were doing getting ready for another fire hose treatment, and they look quite a bit more relaxed this morning and I think it's got something to do with the size of the pile of viewgraphs. Quite a bit smaller than they normally get from me, but I want to caution you don't relax too soon, there's a lot of words on these viewgraphs and we have a little bit less time than we normally have.

Let me make one final introductory remark. I also had a nightmare on Tuesday evening, and it has to do with a number that I gave in my presentation and a number that Eric Ryder gave in his presentation. And Eric's sleight of hand not withstanding, which is sort of like he had the hand over the viewgraph, I did say that there were 2,200 acres in the primary area, and Eric's viewgraph showed 1,850 acres. And you may remember that I mentioned something to you to the effect that the people who were doing that modeling were dealing with uncertainties in the orientations of the faults that bounded the block, and some estimates of how much usable area might physically be there with respect to stand-off distances from faults.

That 1,850 acre number is the number that takes what Dr. Chip Mansure from Sandia thought was a credible limit to the uncertainty in that 2,200 acre, and he just subtracted it out, so the number Eric used was 1,850, which takes out all, what was believed at that time to be a credible level of uncertainty in that area.

So from that perspective, Eric's number is probably a better number, but it is a much more conservative number than the number I was using.

Okay, I've been asked to talk about conceptual considerations for total system performance this morning. I'm going to do that in the context of the performance objectives of 10 CFR Part 60, and I'm going to try to use that as a vehicle to tie together some of the information that has been presented by the presenters over the past couple of days, so you'll see a slightly different approach I think to what you've heard from some of the other people.

My objectives are to examine some of the implications of higher and lower thermal loadings in the context of conceptual considerations related to total system performance. And as I noted, I'll do that by discussing relationships between the physical system components, the technical uncertainties, those six categories that we had speakers address over the past, yesterday I guess it was, and then the Part 60 technical criteria as well.

The approach we're going to use is to spend a little bit of time describing some of the thermal design related aspects of the 10 CFR Part 60 technical criteria. And I'm going to primarily have a postclosure emphasis on the criteria that I talk about. The reason, I guess my choice, it was, you know, rather than take an abstract concept, I settled on postclosure total system performance and the performance of the particular barriers as well to be the focus.

I will say some things about--identify some of the technical criteria that deal with preclosure concerns, but the emphasis of the talk will be on postclosure.

I'd like to do that by describing, start off by describing some of the relationships between the 10 CFR Part 60 performance objectives that Mike Lugo just had on his view graph, to 10 CFR Part 60 technical design criteria and the MGDS system components. And I probably should have just said repository system components there, although it does include the natural barriers.

And I'd like to summarize some of the geomechanical, hydrogeologic, geochemical, mineralogical, waste form materials and biological resource technical uncertainties that come about when evaluating these performance objectives.

I selected a number of criteria of Part 60 to begin this discussion with, and I wanted to do that, of course, in the context of how they're related to thermal loads. I don't want to leave anybody in the audience with the impression that this is a strictly correct flow-down of the way the pieces of the regulation fit together. However, it is a pretty good representation of the sorts of things that you have to consider from the design perspective when you're addressing the performance objectives. I don't think I missed anything, but I will not defend this as being a complete comprehensive capturing of all the Part 60 requirements that somebody might want to address when they're dealing with thermal criteria.

There are several sections in the content of the license application, that's 10 CFR 60.21, that clearly indicate that thermal issues are important in the license application, not the least of which is 60.21(c)(1)(i)(F), where we are asked to discuss the anticipated response of the system to the maximum thermal loads that will be imposed on the system.

Likewise, there are sections in 10 CFR 60.21 where we will be doing comparative evaluations of the major design features of the repository system. We've spoken with you about that on many instances. Certainly, the concept of the thermal loading features of the repository are relevant there.

Also, and this is one that really has a preclosure perspective, from my way of thinking of it, there's a requirement in 60.21 to include a discussion on the features that would be included in the repository design to facilitate closure of that system. And certainly stability of the excavations would be a typical component that you would address under that kind of a feature, and as Larry Costin showed you, that there are relationships between preclosure, stability type questions, and thermal loading.

The performance objectives themselves I've chosen to capture that have a relationship to thermal loading, 60.111(b)(1), we're directed to preserve the option for waste retrieval in our system. That not only is a 10 CFR 60 requirement; that's a specific requirement of the Waste Policy Act.

Now, somebody asked me a question the other day, and this is an appropriate time to point that out. I have asked several of the NRC staff members who were instrumental in the development of 10 CFR Part 60 if there was a consideration underlying 60.111 relative to the economic value of this material. We'd have it underground for 100 years and we might want to get it back out for economic reasons. They all assure me that that is not the case; that this is simply a requirement to make sure that after we've done our performance confirmation program and looked at the way this system responds, if we are unable to continue to validate the conditions of the license that we're given for emplacing radioactive waste, we could probably be asked to pull that material out, and that's the source of the requirement for retrieval. It really is a postclosure concern.

60.112, the overall system performance objective, that is the part of 10 CFR 60 that incorporates the EPA standard of 40 CFR 191 with some additional information, and it's the kind of thing that you saw presented in the EPRI presentation this morning.

60.113(a)(1), when you get into the 113 section of 10 CFR 60, you're talking about a section that's entitled performance of particular barriers after closure. These are the pieces of the regulation that deal with the defense in depth and the redundancy and how you provide additional assurance that the system will in fact meet this requirement. And those are, in fact we've talked about them as well this morning, the substantially complete containment, which is the 300 to 1,000 year waste package lifetime, and the gradual release rate, which is the 1 part in 100,000 release rate criterion that we have.

There also is a piece of the regulation in 60.113 that puts a limit on the pre-waste emplacement groundwater travel time of 1,000 years.

Now, I don't have it specifically on this viewgraph, but the items that fall under 60.113 are subject to, or I should say DOE is allowed an opportunity to propose an alternate for those particular performance objectives. But at this point in the program right now, we are focusing on trying to meet those particular objectives.

I've included in this figure three of the siting criteria that are in 60.122, and the first one is a favorable condition. And a site would be considered favorable if you had minimal thermal impacts on the minerals present. There are two potentially adverse conditions that are relative to thermal loading, and they are conditions that would require complex engineering to deal with, and certainly there could be thermal situations that would require complex engineering solutions.

Finally, there's a potentially adverse condition where you would have geomechanical properties that would not allow you to develop stable openings. So those are the performance objectives and three of the associated siting criteria.

The real focus of my talk is going to be on this slide and the following slide. 10 CFR 60 also includes design criteria for the geologic repository operations. Those design criteria are included in 10 CFR 60 and they're generally in the context of the things that you do in engineering design to try to make the system meet the performance objectives.

I want to skip 60.130. I'll come back to that one at the end. And I want to just discuss some of the ones I've highlighted here.

60.131(b)(9) basically is a requirement to be in compliance with mining regulations. And certainly thermal effects in the preclosure, operational kind of considerations, as well as temperatures that you would expect people to work in, are of a concern there.

When you get to 60.133, we're tending to get more into postclosure concerns, although there are some preclosure concerns at that point in time. In 60.133(a)(1), the DOE is directed to look at the geometry, the orientation, so forth, of the underground facility, as well as the engineered barriers of the waste package, and so forth. They need to be designed in such a manner as they would contribute to isolation.

Likewise, in 133(b), the facilities, the underground facilities need to be designed such that there's sufficient flexibility that they can deal with conditions that are encountered underground.

Once again, we have a requirement for a design to permit retrieval, likewise, one to ensure operations can be carried out reasonably and the retrievability option maintained. Those are very, to my way of thinking, clearly focused on geomechanical issues.

The ones that I think are the most important with respect to meeting the performance objectives follow in the latter part of 60.133. There's a requirement to reduce deleterious movement or fracturing of the rock mass. There is a requirement to limit the potential to create additional pathways for radionuclides to migrate. There is a requirement that the EBS should be designed to assist the geological setting, and there's a requirement that we look at the thermal/mechanical response and ensure that in fact it does not compromise the ability to isolate waste.

I skipped 60.130. That is a more general statement of a requirement for design features in the repository system that need to be developed with a mind towards achieving the performance objectives.

We specifically have used 60.130 in the types of evaluations and design concerns that we've addressed to date to deal with the question of water. We're in an unsaturated zone system and we wanted a place where we could specifically tie our concerns about the water that we would be introducing in the system through construction operations and drilling operations, so forth. We wanted to make sure that we managed that water as well as other materials in such a way that we did not impact our ability to achieve the performance objectives. And we generally talk about those concerns under this 60.130, which is a general statement. It says DOE must do everything they can--I don't think it says everything--but not relieved, just because it is not listed in the lists that followed, DOE is not relieved from having to consider it. It's that kind of a statement.

There's also two more sections in that design criteria as well; section 134 and section 135. Section 134 deals with sealing of boreholes and shafts. And that must be done in such a way that we do not create additional pathways. And, likewise, there's a specific requirement that the materials themselves, or the emplacement techniques that are used do not have effects on the transport of radionuclide waste.

And, finally, there's a very comprehensive section in 60.135. I've chosen to only list 60.135(a)(1), which says that the waste package should not compromise the performance of the site. It also says the converse; the site should not compromise the performance of the waste package. Very nice little piece of the regulation.

What I would like to do to set the stage for what follows is show you a very busy diagram. The diagram is there more for a concept than for the detailed specific elements of it, although I will discuss some of them.

We have four performance objectives that I've selected; the waste package lifetime, the release rate, the pre-waste emplacement groundwater travel time, and the total system performance objective. And I wanted to try to give you an impression of where these different design criteria that I was talking about would come into play with respect to these different performance objectives, and with some selected components of the system.

Now, I'll point out up front that I'm only going below the repository horizon. I recognize there's a comparable set of system components above the repository horizon that are relevant to meeting these performance objectives, but I wanted to keep this a little bit more manageable.

So I've also, you can consider this a flow down of regulatory requirements of sort, and I just wanted to point out the kinds of things that we were talking about, and it probably would be a good idea for me to put this other view-graph up at the same time so both of us can remember what some of those little numbers mean.

Well, let me just start with the waste package lifetime. We're really concerned there about the initial period where the canister would break apart. There's a requirement that that should be a 300 to 1,000 year lifetime period, and so what I'm looking at with respect to this are things that said, for instance, as I told you, the 60.130 is where we try to capture the effects of water. Would we have introduced any water in the system due to our construction methods that would have an impact on the lifetime of the waste package. Is there a way that you could orient the facility in such a way that it would either detract from or enhance that waste package lifetime?

One possible way would be if you were at a fractured rock mass, which we are, and it's reasonable to expect that there would be preferential orientation with respect to that fracturing in the rock mass system where you might have a piece of rock falling out of that borehole wall, whereas, other orientations, you wouldn't. So that's a concern.

60.133(b), flexible conditions. We deal with that primarily with avoiding conditions that might not be as good as other conditions within the rock mass. We've talked to the Board before about contingencies in our area usage underground where we might stand off some distance from a particular fault.

Likewise, the (e)(2) series has to do with creation of fractures. Again, this could be either allowing blocks of rock to fall off and hit the waste package and fracture it, or it could have to do with creating additional fractures that didn't exist before that would allow better pathways for water to get to the waste packages.

Let me choose a couple of other ones here. I've put a comparable set of these down above and below, and that's really because the waste package requirements themselves say that the waste package shouldn't influence the natural barriers and the natural barriers shouldn't influence the waste package. So I think you have to look at it from what the waste package does to the rock around it, as well as what the rock around the waste package does to its lifetime. So that's why there's a comparable set of these around there.

Now, I don't believe that the Calico Hills or the saturated system below it are probably going to be very important in the waste package lifetime, although I can imagine scenarios where that could matter. I've chosen to try to put down what I thought were the most important ones.

I have a comparable list for the release rate, and my arguments would be just exactly the same, whereas I would be less concerned about a piece of rock falling off the side hitting a canister and fracturing it with respect to the release rate. I would be concerned about the same physical mechanism, the creation of these fractures or the incorrect orientation or a less than appropriate orientation, I guess I should say, leading to an enhanced ability for the system to either provide water to dissolve that material and carry it away or vice versa. So those are very similar.

Now, with respect to the pre-waste emplacement groundwater travel time, the major concerns that I have on here have to do, as I mentioned on Tuesday morning when I spoke with you--Tuesday afternoon, excuse me--having to do with the extent of the disturbed zone. And we'll talk about that a little bit later on, but again we're talking about construction induced fracturing, construction induced water that would be there that wouldn't have been present before, the effects of stress redistribution due to both the excavation of the openings and the imposition of the thermal heat loading on that as well. So that's why I have those up here and likewise down here.

I vacillated on this. Any version of this view-graph, it's either on there or off there, and I decided to leave it on there because not only are you supposed to worry about what the natural barriers do to the waste package environment, there's that alternative interpretation in 135 that says what the waste package does to the natural barriers themselves. And so I felt it needed to be appropriate for completeness to leave it on there.

Now, the last one, total system performance, we have very similar questions. We're rolling up a lot of this directly. Although we are directed in the performance objectives to look at the performance of the particular barriers, the waste package lifetime of 300 to 1,000 years, that is not necessarily directly relevant to the total system performance, although it is, and I'll show you why I believe it is so.

Likewise, there's a specific requirement on the release rate. You may have a system that would meet the EPA standard with respect to vastly different release rates than are permitted under this portion of the regulation. The regulation requires you to look at those release rates themselves. So the same things that are over here show up over here, but they show up more in the context of a source-term rather than an absolute limit set on either the waste package life or the release rate.

And this point also brings in the questions related to sealing. Likewise, I've taken some liberty here by assuming that the Calico Hills would not be relevant to the extent of the disturbed zone, but I think I would have to certainly admit that that will have to be considered. We're looking at much larger volumes of disturbed material, disturbed in the broadest sense, when we're talking about higher thermal loading. So it's quite likely that Calico Hills could become a question with respect to the extent of the disturbed zone.

But the Calico Hills is relevant to the total system performance because that's where our major retardation would occur and that's where the bulk of the transport would have to occur. And likewise, as I mentioned before, the groundwater system itself, we, in the SCP, did not take very much credit for the ability of the groundwater to retard the material. And as I mentioned, that's primarily due to an uncertainty that exists currently in a value for the effective porosity.

I've seen us in the time I've been in the program lose a couple of orders of magnitude of groundwater travel time only because of the uncertainty in the effective porosity that you would use in that calculation.

Okay, now I want to tie that back to the sorts of things that Eric and I talked about on Tuesday afternoon. This is basically a summary of a couple of Eric's viewgraphs. But I wanted to remind you that we talked about the design considerations, both from the historical perspective that I talked about, and that Eric talked about with respect to developing thermal loading from specific criteria. These are the kinds of criteria that Eric was talking with you about, and I think that you can recognize in this set of criteria, those design considerations that exist in 10 CFR Part 60. That's really where we get them. That's what drives us with respect to our eventual license application.

My talk on Tuesday afternoon was more focused on the historical evolution of those, trying to show you where they came from. I'd like to point out to you now that in fact when I say rock slippage, we have to limit the impact rock failure or the continuous joint slippage. We're talking once again about a piece of the regulation down here in 60.133(e)(2), or 60.133(f), maybe 60.133(i). So there are very strong ties between these repository design considerations that we're working with and the additional design criteria in 10 CFR Part 60 that we need to consider in our demonstrations of meeting the performance objectives.

So now I would like to take that kind of information and move into a little bit different approach to talking about this. And what I've chosen to do is examine the technical uncertainty talks that you had on Wednesday in their relationship to the performance objectives. And I basically said I'm going to look at the four postclosure performance objectives and the technical uncertainties. Larry Costin talked about geomechanics, Tom Buscheck talked about hydrogeology, Brian Viani talked about the near-field geochemistry, Dave Bish talked about the mineralogy, Greg Gdowski talked about the waste form and materials, and Ted Ostler talked about biological resource concerns.

And I would also like to use the kinds of things we just finished talking about with respect to those system components, and so I'm going to look at the same system components we were just looking at before, the repository, the waste package, Topopah Spring, the Calico Hills and the groundwater. And that will lead me to a very interesting box that I want to talk about. I want to use this as the format to talk about where the checks are in this box, where the uncertainties are, what they mean in terms of the performance objectives.

Now, let me show you just why I want to do this. Okay? When Larry Costin stood up and talked before you, he did a slice through this block effectively this way. Okay? He talked about the geomechanics concerns. He didn't tie them strongly to these performance objectives, although I think if you hadn't realized it when Larry talked, I hope by the time I'm completed, you will recognize that the things Larry was saying really were in the context of these performance objectives. Okay? And then, likewise, Tom Buscheck went through hydrology and so forth. So I'm going to do that the other way. I'm going to go through the box along these slices, and I'll start out by putting up one that has to do with uncertainty in the waste package life.

We identified four boxes in that cube that I would like to make some comments about. They are the relationship between the geomechanics, the waste package life, the hydrogeology--excuse me--that's in the context of repository, the repository element. I'd like to talk about in the context of the waste package element itself, the waste form and materials, and then with respect to the Topopah Spring, some information about hydrology and the geochemistry in the near-field. And so that should be the next viewgraph in your package.

I may give you a little bit different spin on these things from the way Larry Costin might have said it or the way Tom Buscheck said it. Most of my spin differences will be with respect to Larry Costin. You have to consider my background as well. I am one of these kind of people rather than one of these kind of people, so I have more fun with this part of the diagram.

So the first thing I mentioned was the borehole stability question, again with respect to waste package lifetime. Is a block going to fall off the side of the canister--excuse me--off the borehole wall and damage that canister, or maybe not even damage it, but maybe tip it over to the side. If we're relying on the air gap that we currently build into this system and a block of rock pushes it over where it contacts the rock mass, it's a different situation from having an air gap which is effective.

I have a little bit different perspective on the question of creation of new fractures and opening or closing existing fractures. I don't feel quite as confident that that's a problem that we either fully understand, and I don't want to put any words in Larry's mouth. It's possible to interpret something that Larry said in such a way that he may not have felt that that was a significant concern. I see more uncertainty in it. And the reason is you not only have the potential to create new fractures, you have the potential to open or close existing fractures, and that is going to have some impacts on some of the things that Tom Buscheck was talking about, fractures promoting rapid condensate creation.

Well, those fractures may be open in our current understanding of the mountain, but when you pose compressive horizontal forces on this system of the magnitude that Larry's model was showing, you could close those, and you may prevent that condensate drainage that we're talking about in some of the models that you saw yesterday. I view that as a big uncertainty.

And I'd like to thank Mike for the extra five minutes because I think at the end of this, I'd like to show you a couple of figures that show what happens to a natural fracture, both with respect to a measurement of its opening and closing and with respect to what the permeability of that fracture is in a cyclic heat environment. I'd like to show you that information.

There are uncertainties with respect to the usable area and the flexibility. Again, this is a stand-off question once again. If we go into this characterization program anticipating that we may not be able to use the area that's within, say, 25 meters of a fault that may be transmissive. If the Ghost Dance fault turns out to be a transmissive fault and we have already said we would stand off from that fault so that we would not put those materials, waste package, waste form materials in a rock mass material that was more subject to flooding, that's a question for us.

I told you right now that, or when I started, that we believe a conservative estimate of that uncertainty is on the order of 350 acres out of 2,200 acres. We need to confirm that through characterization.

Then there's a question of lateral diversion. And again Tom Buscheck showed you lateral diversion occurring at a boundary between two distinct rock types. I think that if you're closing fractures in a system, it's likewise possible that you could divert moisture. So that's a consideration that I don't think's been--that we need to look at. That will come out exactly what I said. That is a consideration that we do need to look at.

With respect to the hydrogeological concerns, Tom Buscheck was showing you that high temperatures promote drying and extend the resaturation time, and they limit contact of fluids with these waste packages under his models. He viewed that as a very positive aspect of his model. I think there's uncertainty with respect to that.

I've already mentioned to you the topics of fractures promoting rapid condensate drainage in the context of the thermal loads on the system, closing some of those fractures, and then the usable area of flexibility question.

With respect to the geochemical uncertainty relationships that exist in that Topopah Spring unit, we're talking about changing the environment surrounding that waste package, and that affects the chemistry of the system, the dissolution that takes place within that system, precipitation of minerals, and sorption capabilities, all of which could have an impact on the waste package life if corrosion is a dominant mechanism in that life.

And that brings me to what I've tried to capture what Brian said in four words, and I think he's talking about some of the mechanistic aspects of corrosion are not perhaps as well understood as they need to be before we can put this question to bed.

With respect to the waste form and materials, now you're getting farther and farther away from my area of expertise, and so all I can say is exactly what Greg Gdowski said yesterday, that the container materials are above boiling, there are some advantages for corrosion rates and formation of protective oxides. That would be this box over here.

Okay, well let's move on to the next one, which is something about the technical uncertainty relationships with respect to the release rate. I didn't really need to change this box. The graphics people are angry with me that they had to color two of these because the boxes are colored in exactly the same, but the bullets are a little bit different.

Again, in the area of view mechanics, we're talking about the question of creating new fractures or opening and closing existing fractures, again that has to do with the amount of water that's available to move material away from the waste package. I believe that opening and closing of those fractures is a consideration, although it might not be. If high temperatures do promote drying to the extent that Tom Buscheck was suggesting, that would extend the resaturation time, again under consideration of opening and closing these pre-existing fractures or creating new fractures, again that's a consideration and very strongly coupled. And it could limit the amounts of fluids available.

However, the converse, if you will, is also true. If you've closed fractures in that system, you could in fact channel materials that would come right down and basically prevent that rapid condensate drainage from occurring.

The same concerns exist about the usable area of flexibility and lateral diversion with respect to this. Now, in this particular instance, I've used lateral diversion as a favorable aspect for the hydrogeological situation because we're talking about water moving away from the system. If you're moving that water upward in the system and you deal with those boundaries again, there may be that potential for lateral diversion.

With respect to the geochemistry, we have the same concerns of mechanistic aspects of corrosion and the environmental changes leading to chemistry, dissolution, precipitation and sorption changes. However, it's very appropriate to point out that both Dave Bish and Brian Viani pointed out that the expected phases that we would deal with, changes within the rock mass itself at the elevated temperatures, are zeolites and clays, and so that should be a positive situation for us.

You are dealing with a region of altered permeability and porosity and the extent of that region needs to be ascertained. Again, we're talking about release rate here. We're talking about the context of how much material, mass material, is actually available to move these radionuclides, dissolve them and move them.

With respect to the waste form and container materials, this is pretty much a direct quote from what Greg said yesterday, the container materials are above boiling; there are some advantages for corrosion rates and oxide formation. However, now we're not dealing with just the container. We have to talk about the waste forms as well with respect to this issue, and we're talking about spent fuel as one possible one, and Greg was pointing out that in the 100 degree to 250 degree C. range, there are some advantages with respect to cladding rupture, oxidation, pellets remaining intact and dissolution of the fuel.

For the borosilicate glass, on the other hand, the advantages occur when you're at or below boiling where you have more benign water/glass interactions.

Incidentally, if any of the gentlemen who I am paraphrasing would care to stand up and say no, that's perfectly all right.

I mentioned the other performance objective, pre-waste emplacement and groundwater travel time. What I've identified here are the concerns with respect to the geomechanics and hydrology at the repository horizon--excuse me--with respect to the engineered barriers themselves, and at the repository horizon, this should be the Topopah Spring. I can see I've been a little bit more generous in this diagram when I extended it down to the Calico Hills as a potential needed to be considered for the extent of the disturbed zone.

And I threw this in; it's kind of a recapitulation of something I said the other day. The pre-waste emplacement and travel time, these technical uncertainty relationships, remember we're talking about postclosure concerns right here, and this is a piece of the performance objectives that falls under the performance of a particular barrier after closure, and it is one of those aspects of the performance objectives that are intended to provide more assurance that the system will function if there's redundancy in the system. And the importance of the thermal loading with respect to the pre-waste emplacement and ground water travel time is only in the calculation of the extent of the disturbed zone. Okay? You would not put the heat in the system to calculate that groundwater travel time, although you would consider the effects of heat in determining how far the disturbed zone was, which is the point where you start calculating the groundwater travel time from.

And again, as I've said, the important issues there were stress redistribution, construction and excavation induced effects, thermomechanical effects and thermochemical effects. And as I told you the other day, NRC considers 5 opening diameters may be the minimum appropriate distance.

So the pieces that I've pulled out here, again, have to do--oh, let me remind you a point I made with respect to our approach to dealing with the disturbed zone. We looked at the volume of rock where the permeability would be changed significantly such that it would change the groundwater travel time significantly. So we recognize that many of the theoretical solutions that we're looking at for heat in a system, the effects extend, theoretically, to infinity, but we were looking for a more practical applicable aspect of that and so we tried to define it as the point in space where the effect of the permeability change was appreciable.

So much of what I'm saying here is in that context, and so we're looking once again, under geomechanics, at the effect of construction induced fractures and thermally created fractures and, again, this is in the context of opening or closing those existing fractures in such a way that it could modify that permeability to lead you to move farther away from the repository horizon to begin your travel time calculation.

With respect to the hydrogeological concerns, there is that concern for construction or operation induced fluid saturation changes. When you're in an unsaturated zone environment and you change the saturation of the rock mass system, you have changed the relationships between permeability. And, likewise, the lateral diversion question again. If it's induced by the creation, something to do with the creation of the repository facility underground, that would need to be considered in the pre-waste emplacement groundwater travel time.

Near-field geochemical effects, we talked about the development of that region of altered permeability and porosity. The extent of that region is of concern for the groundwater travel time.

And with respect to the mineralogical changes, now these are the far-field ones that Dave Bish talked about, we're talking about dehydration and contraction of minerals, potential for enlargement or contraction or clogging of transport pathways.

Now, with respect to my opening remark about the significant changes in the permeability of that rock mass zone, some of these could actually be beneficial. You know, if we could manage closing, contraction, if you will, or clogging of transport pathways, that would be a net benefit. I am not certain how you would treat that in the calculation of the extent of the disturbed zone. I would like to think you would take credit for that or you would not detract rock mass away from the system when you've made it better. You would only take rock mass away from the system when you've made it worse.

That's a wide open question. I think Raj and I talked about that the other day, about there's still a lot of debate that needs to follow on the extent of the disturbed zone.

Dave was mentioning that the short-term contractions appear to be reversible, and even though some of these reactions that occur in the mineralogy cause flow path modifications, they may be beneficial. I think Dave concluded that as well. That's where I got that.

And with respect to perhaps a heightened sensitivity to Tom Buscheck's model and Dave Bish's model, I did give Calico Hills a little check there on my matrix block.

Okay, finally, total system performance involves much more of the system. Okay, now we're talking about, and perhaps it might be appropriate just for a moment for me to put up a diagram that I had up earlier that I've taken down, and the reason that the list becomes more extensive with respect to this particular diagram is how much more all those pieces of the regulation, the additional design criteria that we're talking about having to use and address to demonstrate that we meet the total system performance, how much more pervasive they are with respect to all of the system elements now. So that's why the list gets a little bit longer on this block.

Okay, geomechanics. We're talking about the borehole stability. That's really a source term aspect with respect to total system performance, whether or not the borehole collapses, punctures your waste canister, pushes it over against the side so you have more contact with the rock mass. Again, the concern I've expressed about creating new fractures, I believe the more significant concern is in fact opening or closing existing fractures, and that usable area question flexibility, how much room do we have down there, where do we have to stand off.

Hydrogeological; I think Tom noted for you that most of the impacts that he identified were in systems where there was fracture dominated flow. Again, the conclusion Tom made was boiling and dryout enhanced fracture flow attenuation. But you need to consider the volume that's involved and the time that's involved. We have a 10,000 year time frame that we're dealing with right now for the total system performance.

The higher temperatures promoted drying, extended that resaturation time, which is important to both waste package lifetime and the dissolution, the release rate, and limits the fluids available that are able to carry those radionuclides away. So that's kind of a source term there as well.

Again, the question of the fractures promoting rapid condensate drainage if the thermal load could close those fractures. In fact, you know, it could be engineered to open those fractures just as well. I've continually said close those fractures, but the fractures, it may turn out that the proper way to orient the repository is not the conventional way that you would orient it for preclosure stability concerns, but you might want to orient it at an angle to that so that the heat in fact would open the fractures if you're trying to promote this condensate drainage. If that turns out to be a concern, you might want to go against conventional thinking, trade off to buy something in the postclosure.

We didn't talk much about this about this--we didn't talk about this at all, but there's that question of reliance on the saturated zone flow component. We really need to address that as well. There are uncertainties in that with respect to total system performance. Again, the questions of usable, flexibility and lateral diversion.

The geochemical concerns are more important pretty much all around for the source term. And we talked about the changes in that environment, sorption, deposition of minerals and so forth. Potential exists for near-field retardation enhancements if in fact the minerals change, as the evidence would suggest, to minerals that have retardation properties.

And, again, there's that region of altered permeability and porosity which has an impact on the source term.

Going off into the far-field, pretty much the same comments that were made with respect to the previous viewgraph. Dehydration and contraction of minerals is of concern. We need to understand what the effect of that is, and it's manifested as a potential enlargement of transport pathways, contraction of those pathways, or in fact clogging through deposition of those pathways.

Dave pointed out that some of these short term contractions may in fact be reversible, however, over the long term, there may be some irreversibilities in that. And then, again, the question Dave approached of the mineral alteration potential, he brought up the time that it takes for that to happen.

With respect to the waste form and materials, and again that's important as a source term in this diagram, these are the same points we've made before, the container materials seem to prefer being above boiling because there are advantages for corrosion rates and oxide formations. The spent fuel advantages seem to be between about 100 and 250 degrees C., and that's for the cladding rupture, the oxidation, the maintenance of the intact fuel pellets, fuel dissolution.

The borosilicate glass, on the other hand, seems to have its advantages when it's below boiling for the benign water/glass interactions.

And let me just turn to this diagram because I haven't mentioned it and point out that, in fact, the geomechanics basically is a repository horizon concern throughout the Topopah Spring and throughout the Calico Hills, I believe. I'm not terribly suspicious of this being a significant component, but I really think there are uncertainties with respect to that opening and closing of those fractures in that Topopah Spring horizon.

The hydrology is a question throughout the section from the repository horizon all the way down to the water table.

Mineralogy is really more of a Calico Hills concern because of where we believe our sorptive minerals are. There could be some sorptive minerals, there are some sorptive minerals in the Topopah Spring, but that could also affect that unit as well.

With the waste form and materials, worried about the repository and waste package interaction, as before. I don't see much for farther away from that as well. Probably could have colored the Topopah Spring in there as well; it would be more consistent with what I said.

Now, I swear that I did not know Dr. Cantlon would be the chairman of this particular section, but I've been expecting him to say all along that I have not put a box anywhere along there. Okay? You were going to notice that? Okay, that's because the biological resource concerns are not really addressed in the technical requirements of 10 CFR Part 60. They are addressed in the EIS process, and as you know, in fact the NRC has been directed to use our EIS to the extent that they possibly can. And so we do, because of that, address biological resource concerns in the repository design requirements. We've mentioned some of them to you before.

And I wanted to point out that the design calculations that I'm familiar with suggest a 1 degree temperature change at the ground surface, and that's well within the limits that Kent Ostler said would probably not have significant impact. Ben Ross seems to have walked out of the room. I meant to ask--oh, Ben, you might want to comment whether the model you showed this morning would show a significantly higher heat flow at the ground surface, which would make more uncertainty in this number.

DR. ROSS: I'm uncertain about that.

DR. VOEGELE: Thank you. The record should show that Dr. Ross is uncertain about that.

Okay, I'd like to wrap this up by going back to a theme of mine that I tried to work into Carl's talk and I tried to build my talk from that on Tuesday morning, and that is it is these repository design considerations that are most important to us in determining the appropriate repository design. We can't just shoot for an APD. We need to know what we want, how we want our design to perform, and I hope I've showed you this morning that what makes you determine how you want your design to perform is in fact what you have to do to meet the performance objectives.

So, again, I've reminded you of the kinds of design considerations that we're carrying with us in the program and hopefully I've showed you a relationship between the regulation, our goals to meet the performance objectives, and some of the uncertainties that we've talked about for the last couple of days.

So I do have a couple of concluding remarks, and in all likelihood, they'll be exactly the same as what I just said. The performance objectives do provide that framework for judging the suitability of the site. Those are our rules. We must meet the performance objectives. And as I said, the design considerations that we're talking about, the parts of 10 CFR 60.130 through 135 really need to address attributes of the system that we need to address to meet the performance objectives.

I think that the conclusion of my talk is that the ranges of the APD need to be examined during the design so that we can develop approaches to meet all the design considerations. And I probably could say that as well the other way.

We would very much appreciate dialogue with members of the Board, members of the technical community, the international community on these design considerations that we want to develop and strengthen to make them as defensible as they possibly can to meet the performance objectives.

And, finally, the point here, we tried to put this down in a bullet on viewgraphs, they tend to get a little bit terse, but the point here is we haven't begun to understand the system interactions that will allow us to make trade-offs in these component performance requirements. We have right now a relatively broad program. We have understandings of how the different pieces individually work, what some of the design considerations might be with respect to those individual pieces, but we haven't really begun to put all the pieces together.

Many of the uncertainties that we've talked about over the last two or three days may not be relevant once you begin to understand the trade-offs in the system interactions. It just may not be relevant.

So I think I would like to leave that as the concluding remark. No, I can't do that. I have to leave this as the concluding remark. We need to address the design considerations. That's very important to us, and I think that's why most of us have looked forward to this meeting.

So those conclude my formal remarks. I would like to take about five more minutes if we have them and show you a couple of figures and then do questions for a while.

What I'd like to show you are the results of, this one happens to be the first heated block test that was performed. I'll take the opportunity here to make a nasty little comment. You can tell from this, this is the title of this ONWI report where you can find this, I would urge you to go look in the 22nd Rock Mechanic's Symposium that was held at MIT in about 1982, because there the authors of this report are in the correct order. My point here is you can tell what month I left Terra Tek in. They left me on. Okay, that's important.

I wanted to make a little comment. You know, if this were a university publication--this is like an honorary position, the department chairman was a co-author, in a research organization, this is like the guy who knew how to turn the oscilloscope on. Okay? However, Ernie deserves a lot of credit for that work, too.

Okay, this is a couple of figures out of that document. I just want to show you what happens to a rock joint that's been isolated and very heavily instrumented and permeability measurements made in that as well.

This particular diagram shows a couple of excursions where we've loaded up the system and then unloaded it, and these were in fact uniaxial loads. Since I've already taken credit for this work, I also have to be responsible for something that was done incorrectly in this work. We did the uniaxial loading first and, of course, we sheared the joint before we ever had a chance to load it biaxially. But we fixed that, okay, a little later on.

These are the theoretical smooth wall apertures that are calculated as a function of an excursion in stress over just a few hundred psi, and it's easily a doubling, which is at--

DR. CANTLON: What's the rock type?

DR. VOEGELE: This is a granite gneiss. Granite gneiss; it's a hard crystalline granite. And calculated from a theoretical smooth wall aperture, we're talking about three orders of magnitude--or excuse me--a factor of eight, an order of magnitude change in permeabilities and flow kinds of concerns.

This is the comparable diagram of measured changes across that fracture. So in this case--I probably should have shown this one first--we actually had pretty detailed measurement devices across that aperture. It was a well isolated fracture. We were controlling the movement of that aperture. And you can see it goes all over the place as you load the block up because it was a fractured rock mass.

But what I wanted to show you was the difference in not only the behavior, but the amount of change, how more recoverable the permeability is in fact than the actual displacement across the joint. And so once you get comfortable with that, let me show you the more complex version. This is biaxial loading of that block under temperature, and in fact the block itself was just an isolated cube of granite where we had flatjacks, we had drilled holes along the side and put flatjacks into the rock mass, and we had a fracture across the block, and we had an injection hole and a couple of observation holes on either side of it.

Okay, this is what happens to a fracture when it gets cycled through temperature and pressure. Okay? It starts right here, and as we load the block, we see a decrease in the aperture of that fracture at a constant load. We heated the block, continued to decrease the aperture. Here we had an excursion where we unloaded part way and loaded it back up. We lost some of the aperture there. We could not recover it. Heated it up even more, and we were not that high, we were only about 75 degrees C. in this block and we, again, lost--got down to this point, lost a significant amount of it. In fact, I believe we gained aperture on this one when we got to here, unloaded, loaded back up.

And then when we came back up and cooled the block down, how much difference there is in there fracture aperture, and then went back to ambient and we still had a significant change. Now, as I've said, we've sheared that fracture first, and so a lot of people have been critical of us, you know, first time out, you can make a mistake, right? A lot of people were critical of the fact that we had sheared that fracture first, and expected the results to be totally different if we had done that block in a biaxial loading situation first and then sheared it.

This is the fracture permeability measurements in the heated block test that was run in G-Tunnel. Okay, so you can see what we did here is we tried to do permeability measurements before we cut the slots. Okay? And then we cut the slots, and so you can see increases in the fracture permeability. There are two paths in this one as well between a packed off interval and observation holes in either side of it. This is one of the paths. Gained quite a bit of permeability when we cut the slots. Tried to pressurize the slots back up to what we felt were the in situ conditions. We were trying to be very careful with this fracture. Couldn't gain it all back, but we gained quite a bit of it back.

Once there's some ambient temperature biaxial testing, and this is when we unloaded the block, and that's the behavior as well, and then we turned the heat on at this point in time and went through a couple of temperature cycles and so forth.

The point I wanted to make is this is where we sheared the block after we had done the biaxial loading, and we didn't do much damage to that fracture. Now, this fracture was much, much smoother and much more planar than the fracture at the Colorado School of Mines test. But we did not do much to the permeability in that block when we sheared that fracture.

Now, these are the kind of considerations I think that Larry showed you a model of a laboratory test on a joint where he knew what component of the deformation was attributable to the rock and what component that was attributable to the fracture itself. And here is some information approaching that same problem from another direction. This is actually what happens to the permeability when you heat and load these rock masses.

I firmly believe we have much more work in this area to do. I see that as one of the bigger uncertainties.

DR. CANTLON: Thank you, Mike. Questions? All right.

DR. VOEGELE: We have Tom Buscheck.

DR. CANTLON: Okay.

MR. BUSCHECK: For the net condensation rate, at the highest heat loads, we saw 30 centimeters a year. The reference data for bulk permeability in the rock is 365 meters per year as opposed to 30 centimeters per year. So what we see is about four, I don't know how many orders of magnitude, 30 centimeters per year is what the maximum would have been, and the rock, under its ambient conditions today, could conduct 365 meters per year. So I think there's probably excess capacity, so we'd have to see a lot of closing of fractures to significantly throttle flow.

The other thing is the maximum stresses are going to occur in close to the waste, and in the pillars furthest away from the waste, you will see less of that closure effect, I would believe. So where we were showing the hydrothermal umbrella between rooms, you would see less of that closure effect.

DR. VOEGELE: Okay, we're talking almost an order of magnitude permeability change between preconditions and after we went through the cycle, and we only went up to about a thousand psi. I think we did go to 10 megapascals, 1200 psi.

Larry's numbers, if memory serves me, went five times higher than that. Okay.

I don't know where the limit to closing a fracture is. We've never taken a fracture to the limit, to those kinds of limits. But I think that there are other concerns other than the drainage capacity of the system.

DR. CANTLON: All right.

DR. CORDING: One comment there in regard to the fractures. I certainly can see how you can open fractures when you excavate and change permeabilities by many orders of magnitude even, but in the closure of the fractures, in some of that earlier data of course you show that, the S-shape which is pretty typical, and a lot of that is already out of the material, or some of that is your testing. This was a lab test, wasn't it, on a big block?

DR. VOEGELE: This was a block in the field.

DR. CORDING: Oh, in the field?

DR. VOEGELE: This was underground, yes.

DR. CORDING: But had that boundary been disturbed or the stresses reduced from that surface before the test?

DR. VOEGELE: Sure. Both of these--well, actually, the CSM block was blasted. Okay?

DR. CORDING: Yeah. I guess my point is that a lot of that initial S-shape is the disturbance effect, and that you're really on the steeper portion of your curve in the field, so you're getting further away from the joint, further away from the excavation, let's say.

DR. VOEGELE: Okay, this particular joint was exposed with as much care as I believe it's probably possible to. We used an Alpine miner to bring this down to a reasonable distance below the blasted floor. And then, in fact, there was a lot of hand excavation that we did to really get down. There's no guarantee that we have, you know, a virgin fracture here, obviously, but we did take a lot of care to try not to disturb that any more than natural conditions. But the most obvious one, Ed, correct, I mean, this is disturbed by the opening.

DR. CORDING: And certainly even any type of excavation I'm sure you can get some significant opening locally at the edge.

DR. VOEGELE: Sure.

DR. CORDING: But I think more further away, the possibility of a closure is going to be, because there's stress already in the ground at that location, it's going to be on a steeper slope.

DR. VOEGELE: I understand.

DR. CORDING: And, in other words, there will be less opening, or less reclosure upon heating than you would get if you were looking at a disturbed joint.

DR. VOEGELE: That's very interesting. We did take this back to zero. Okay? We unloaded it before we started the heating cycle again to try to take it through a cycle and see if it was recoverable. We heated it at load. Okay? So we knew the stress on that fracture before we started it, so it had the stresses that we put on it, not the stresses induced by the excavation itself, which I believe is what you're talking about. Maybe not.

DR. CANTLON: Okay. Bob Shaw?

MR. SHAW: Bob Shaw from EPRI. Mike, I'm very impressed with your three dimensional array and the framework that you look at, you know, the technical uncertainties, and I'm quite serious about that. And you produced a fairly exhaustive list, you know, that comes under these various categories, and yet there were two areas that we looked at that didn't seem to be included. The first was the question of climate changes in the future and net infiltration and how that affects things, and the second is the general question of heat transfer and how that changes your temperature profiles with time.

DR. VOEGELE: This particular talk was intended to be a summary of the presentations that were given by the six gentlemen who spoke yesterday. So we did not address a lot of uncertainties that exist in the program, especially the climatological infiltration change types uncertainties. We dealt with geomechanics uncertainties, the hydrology. Tom did address that to some degree, and it was in the context of how well you can match the pre-existing moisture content profiles as a function of infiltration rate in his models. And so he dealt with that. I didn't bring that forward in my summary.

And your second point was the different heat transfer mechanisms. I guess that's probably more subtle than this overview summary was intended to be. The person to ask that question of would be Tom Buscheck.

DR. CANTLON: Okay. Mike, do you want to summarize?

DR. VOEGELE: One more question. Two more questions?

DR. CANTLON: Yes, three more questions.

MR. JARDINE: Jardine from Livermore. Could you put one of those blocks up, Mike?

DR. VOEGELE: One of the blocks?

MR. JARDINE: I think this relates to the angle Bob Shaw is coming from, and me too, and I'd like you to comment on the total system, the layers. You led me to believe that you started your thinking at the repository horizon and went down, and had no, let's say, considerations for the strata that were on the top. So I would say I think you may not be including the total system in your discussion, and then when you put up things like waste package, whatever you want to pick, lifetime controlled release, so could you kind of comment if that's--

DR. VOEGELE: The easiest comment is, actually it flips probably right about--this line flips up to the top. I think virtually everything that's in this column is applicable above it as well. Okay? I'm not going to stand up here and say for reasons of symmetry, I decided to only present the lower half.

But you're right, Les, we did have that discussion. We knew in fact that it would be more reasonable to address it above it. But that added four or five more components to the system, and the point I was trying to make I think could be made as I made it, with a concluding comment that the same applies to the horizons above it.

MR. JARDINE: Yeah, I think the only reason I wanted to bring it out was to--I see the same thing occurs in the SCP and I'd like to, you know, it looks like it goes down, but to get more people thinking, you know, it's a total system and starting at the top. We also saw that in Ben Ross's talk or even Tom's.

DR. VOEGELE: I can't defend what's in the SCP, of course I can't. I don't think that we were as smart when we put together that SCP as we are today. The strata above the repository horizon were viewed as something that got watered down to the repository horizon where something started happening. Okay? And in fairness to people like Parvis

Montezar and some of the other people who worked on that modeling, there was development at that time of some of these concepts, but the real focus I think in the SCP was probably the repository horizon and below.

MR. ROSEBOOM: Along those same lines, some of the words in your viewgraphs on 10 CFR 60 bring back memories of some old arguments with the NRC, and I would just like to point out that 10 CFR 60 came out for comment in 1981, and the letter that Max Blanchard referred to yesterday where the USGS first suggested to DOE a possibility of a repository in the unsaturated zone came out in February of '82.

Now, there had been almost no thinking on unsaturated zones--well, there had been no thinking on unsaturated zone repositories when the first version of 10 CFR 60 came out. There had been a number of generic models looked at of different rock types, but no thinking regarding what a repository in the unsaturated zone would be like.

The USGS and DOE suggested that NRC needed to consider this, and in fact I then wrote a USGS circular trying to explain how a repository in the unsaturated zone would be different, and I was writing that and funnelling drafts to the NRC people who were looking at the problem at the same time, but there was almost no thought other than that on how a repository would be different.

So that we have things like the problem of we objected in the letter to the groundwater travel time of 1000 years. We felt that was a problem because some of the water, as we've now seen, could travel very rapidly through fractures, if it didn't come in contact with the waste, it had no significance.

Other matters regarding fractures in the saturated zone, fractures are always bad, they open additional pathways. In the unsaturated zone, fractures may enable you to drain the repository down to, say, the Calico Hills, something like that.

The question of sealing openings, the NRC wants to tightly seal all openings. Well, it might be better in some cases to control the flow of water underground to specific places where, such as the Ghost Dance fault or other places, where the water might bypass the repository.

So I would just like to make the point that 10 CFR 60 still, because of the time frame when everything was done, contains a lot of thinking embedded in there that was locked into the saturated zone and very little thought and no idea of the kinds of things we've been dealing with lately.

DR. VOEGELE: This is very appropriate. I was involved in the DOE's position development at the time the unsaturated zone amendment was issued by the NRC. They did a rule making and asked for comments on what changes would be appropriate for the unsaturated zone, and we reissued basically the comments that the GS people worked on on the preparation of this. And, in fact, we proposed a flux based performance objective for the unsaturated zone. That proposal really never went forward to the NRC, and the reason was the DOE was dealing with having to make a decision between five sites at that time, and they did not want to have one set of performance objectives for one site and one set for another site, because they did not believe they would be able to make the trade-offs and pick a site.

And so the consensus was we could deal with the unsaturated zone in the context of 10 CFR 60 as it said, as a consequence. When 10 CFR 60 was amended for the unsaturated zone, very few changes were made in it, and most significantly there was no change made in the performance objectives.

MR. COSTIN: I have to defend myself a little bit.

DR. VOEGELE: It was not meant to be critical, Larry, just a different perspective.

MR. COSTIN: I tried at the end of my talk at least to give some of the spin that Mike Voegele repeated, in that yes, indeed, the two things, the two bullets that he kept mentioning, creation of new fractures and whether or not fractures, open or closed, is the key issue in dealing with those and the key uncertainty, I don't think anything I presented said that we had a very good handle on that. And, in fact, I tried specifically to make a point that we hope the ESF testing program, and we had specifically designed several tests in the ESF testing program to try to deal with that issue and to look at changes in permeability and the effects of thermal loading on a repository scale, or at least a room scale.

DR. VOEGELE: I will publicly apologize to Larry. I did not--let me tell you what's going on here. There was another viewgraph in my package. Okay? And it was the one that really said, well, you know what Larry was really talking about was preclosure stuff, keeping these excavations open, and I did not mean to imply he hadn't talked about the postclosure. I meant to say that the real focus of his talk was on stability. And I took that viewgraph out of my package because what it was, it was a viewgraph that said here are the things where high temperature reduces uncertainty or increases uncertainty, and here are the things where low temperatures increase or reduce uncertainty. And every time I looked at that viewgraph, I said something different, and I did not want to be held accountable for what I might say almost at random standing in front of the board, so the safest way out was to take that viewgraph out of that package and let you guys talk about that.

DR. CANTLON: Well, let's bring this one to closure then with Mike Cloninger.

MR. CLONINGER: Michael Cloninger, U. S. Department of Energy. I don't really think we could ask for a better summary than that just presented by Dr. Voegele. So with the board's concurrence, I'd like to donate my remaining time to lunch.

DR. CANTLON: Great suggestion. We'll be back here at 1:30.

(Whereupon, a luncheon recess was taken.)

AFTERNOON SESSION

DR. ALLEN: May we get underway, please?

I'm Clarence Allen, chairing the session this afternoon, a round-table. The table's not exactly round.

Let me outline what the game plan we propose for this afternoon is. Initially, we would like to have presentations by five, so-to-speak, invited--or six, I guess, invited speakers. Initially, I would like to ask our foreign guests--perhaps in the same order they presented their materials initially--to tell us whatever they would like to say in terms of their reactions to the past two and a half days or anything else.

I would like to follow that by asking Larry Ramspott to comment. After all, he kicked off the session on Tuesday morning, and I'm sort of interested in seeing what his reaction is after the last two and a half days, and finally, then, partly because the NRC was not represented on the program itself, due to their own wishes, we do, though, have Dick Codell, who would like to say a few words on behalf of the Nuclear Regulatory Commission.

At that point, then, we will turn it over into a true sort of round-table, and we are going to direct the discussion here by asking three specific questions in order. I will not give these questions now, but when the time comes. We will ask for reactions, and I do know that if no one in the audience has any comments in answer to these questions, there are certainly people on the Board who do.

So let us proceed, and if I might first ask Nils

Rydell, since he was the first of our foreign guests, to give any comments you might wish, and take however long you wish.

MR. RYDELL: Thank you, Mr. Chairman.

It seems almost preposterous to me to comment on what has been said during these three days. I come here with only very superficial knowledge about the Yucca Mountain site, the concept of dry storage. I come with experience from a cold, saturated condition, and we have been bombarded with considerations, statements, questions, and uncertainties and it's very difficult to sort out these things, so I can say I really don't envy the Technical Review Board their task to sort this out, because one of the impressions I get at close distance is that with so many things being brought up as uncertainties, and so on, it must be necessary to find some kind of priority in the coming work in the order of importance for the safety, whatever it is. Maybe there is such a priority and it has not come out in the presentations.

And then, of course, if I comment--and then, as I already said in my own presentation--I would comment from the experience we have, which is different, and which may not be applicable, and then I have two things I would like to comment on.

One is this concept that since you have a requirement to meet for the first 10,000 by regulation, it seems to be then a very clever way to try to load this repository with fuel which is aged so that the decay has evened out a bit, and then there may be a chance that you can keep it over the boiling point and keep it dry for these 10,000 years.

Now for us, 10,000 years is not a magic number, and not for the spent fuel, either. The radioactive source term will be almost the same, 20,000 years old; a little less, but still very much the same 30 years later on. Now, that's very distant in the future, difficult to grasp. We should feel concern for people even at that time, and equally much in that case for those who live 20,000 years from now on as those who live from 10,000 years on, and then this has a consequence.

I mean, if you want to fulfill the first requirement, I think it should not be done at the expense of safety after that period, and it was clear from Dr. Ramspott's first slides that a hot condition in 10,000 years calls for fuel about 250°C for, it could be a couple of thousand years, at least. Now it has come out from other presentations here, and it is also from our experience, that that may not be such a good idea.

We have studied dissolution of radionuclides in the fuel in an experimental program since some 12 years; in water, in cold, obviously. We think we have started to learn the mechanisms, and I won't go into detail about them, but the rate of dissolution has very much to do with the area of the fuel being exposed to water.

Now it's been stated in a presentation, and that's with our experience as well, if you come about 250°C and you have oxygen, in the presence of oxygen, then the fuel matrix will re-crystallize from uranium dioxide to U_3O_8, and in that process, it is kind of loosened up, and we find that it's by loosening it up that way that you expose radionuclides to solution and, in fact, what you may have achieved by this is perhaps a total safety for 10,000 years, which you pay for by some, or perhaps even considerably more release of radionuclides later on when the system may be saturated, and I don't know if that is the best way to approach this problem of satisfying the first 10,000 years of a functional repository.

Another observation is in the, more or less in the opposite direction, and in spite of all these many uncertainties, there is one area which no one really dwells very much upon. You call it the re-closure period, kind of like a design term. I have myself been responsible for the operation of one of our nuclear plants, and been project manager for another, and in that capacity, you evaluate very much flexibility of operation, accessibility, and room for contingencies.

Now, in a nuclear plant that is limited by space and backed by radiation, here you have radiation and we know how to handle that, and that's perhaps not a big problem, but in the hot concept, you also have to cope with temperatures. Now, that depends on how you arrange the system. From some of the slides, it was apparent that it's not going with lightning speed, the heat front, but over even some decades you will have--one or two decades--you will have pretty hot conditions in the rock and in the areas.

One of the things I didn't mention in my presentation as a design rationale, because it was not an initial design rationale, but it came out kind of ex post facto, our system with canister and compressed bentonite is good from the quality assurance point of view, because you can fabricate the engineered barriers under comfortable conditions in a workshop, well-fed people, no physical constraints or restraints or anything. They work under ideal conditions, and then you can hope for a good result.

Here, you may have to let people work in physically hard conditions, and I think that's a factor that should be observed, and on the whole, I would like to see here this period perhaps not called pre-closure but call it operational, and have that in mind all the time, that the most--the interesting period of the repository is now, when you have these kinds of interesting meetings. The important period is when you emplace the waste, because it is the way the waste is emplaced that will be the starting condition for the post-closure period.

And then to shift the perspective, also, I spoke before about 10,000 years, 20,000 years. That's very long in a distance. It's difficult to imagine, even. Perhaps we take too much concern about that, but the operational period is here and now at the place where people are concerned about this repository, and you wouldn't like to--you will meet with contingencies in this large operation. You wouldn't like to be kept standing in a situation you haven't foreseen, and in difficult conditions. You have to reassess. You have to wait, and things can become very complicated. So I think concern about the operational period should be in your minds, not that you study them in great detail now, but you have them with you all the time and think of how you want to run that period in a way that gives the operational crew as good possibilities as possible to fulfill their task to implement this repository operation.

They were two of the reactions I got immediately.

DR. ALLEN: Okay. Thank you, Nils. I think we'll forego any questions, since, hopefully, we'll have time later on if questions do arise, and let me turn to Klaus Kuhn.

DR. KUHN: Thank you, Mr. Chairman.

Being a mining engineer and being responsible for an underground research laboratory, I want to restrict my comments on some selected aspects. There are quite a number of underground research laboratories in the world for radioactive waste disposal in operation. I mentioned I'm responsible for the Asse Salt Mine, which is now in operation for more than 25 years. We have the STREPA Mine in Sweden. There is the underground rock research laboratory at Grimsel in Switzerland, and our Canadian colleagues are operating an underground research laboratory in Pinawa in Canada.

Let me call these laboratories those of the first generation. The French have created the expression that they are going to construct a research laboratory of the second generation. Whereas, in the laboratories which I just enumerated, it was clearly stated from the beginning that they only serve RD purposes, the French approach is now different. They are going to construct two underground research and development laboratories in two different geological media, doing or are going to do research and development in these laboratories, and at the second time they have another objective, to confirm, if possible, one of these two sites in order to turn it later on into a repository, and maybe I learned the last three days that you are going to construct a laboratory of the third generation, just going straight ahead for a repository at Yucca Mountain.

Having stated that I'm responsible for mainly underground research and development, I think there's no doubt about that it is very urgently necessary that the Yucca Mountain Project needs to go underground. I think that is necessary for mainly three objectives.

You have to generate your on-site data. All the models which you are handling and which you are calculating are only speculative. They fit in quite a number of data from literature, from other sites, from other experiments, but it is absolutely necessary to feed and to run the models with site specific data as a complete set of aspects there.

The second objective is that you can only validate your models on the site. You can develop your model. You can test your model. You can run your model, but you can validate it only at the site, which will be where it will be applied.

And the third objective is, at least in my opinion and due to my experience, you can reduce your uncertainties quite a bit with the site specific knowledge, and I think this will be true, also, for the site at Yucca Mountain, that if you go down and get the necessary data, many of the uncertainties which we have discussed the last three days will be reduced a high amount, and there is an example in your country available which was not mentioned very frequently. This is the Waste Isolation Pilot Plant in Carlsbad, New Mexico, also operated by DOE for the disposal of transuranic wastes, and I think they make extremely good progress going underground, doing the experiments underground, fitting the data into the models, and even in the site evaluation process, they could characterize the site extremely well using the data which were generated underground, and fitting into the model. So there is one example which you should look closely to.

And because the Technical Review Board is reporting to the President and to the Congress of the United States, maybe you can influence Mr. Bush and also the Congress in order to speed up the decision that the people on-site here get the permission to go underground. I hope this is very much so.

There were some critics this morning about the use of the application of models. I think there is also no dispute among the scientific community that we only can prove long-term safety, especially for 10,000 or more years, using models. So we have to rely on our models.

The best objective which we can follow and which we can achieve is to diminish the grade of uncertainties in our models as best as we can so that we can rely on our models. We will never be completely successful. We will never have the complete solution of all the uncertainties. There will be uncertainties for the future with which we have to live. The objective is that we can diminish these uncertainties, and that we can show, even if we take into consideration these uncertainties, there are no undue risks originating from the repository calculated with these models and these uncertainties.

One further item is more of a practical nature. We have had some bad experiences dealing with the licensing authorities and dealing with the public in our country with regard to the dimensions of the Gorleben exploratory shafts. It was finally decided that the shafts should already have the final diameter of 7.5 meter, which they are going to use for the repository purpose for the emplacement of waste. So there was immediately up-springing a strong discussion, "Are you going to investigate and to characterize your site, or are you already starting construction of the repository?"

So that is a very difficult question. I don't know--I'm not so familiar with the legal situation in your country, but of course, we had some very strong legal problems to solve this issue in our country. And also, I think it will enhance public acceptance if you make a clear distinction between a site characterization facility and a repository.

One further item is waste emplacement technique. We were told during the last three days the present concept of disposing of single canisters holding one fuel element at a time in one borehole drilled into the floor. I listened very careful, but I think I didn't hear a rationale standing behind this technical concept. I'm wondering if, also, alternatives like drift emplacement or horizontal emplacement in horizontally drilled boreholes are also under consideration or have been investigated.

Again, there are examples available in your country. The remote-handled waste in WIPP site will also be disposed of in boreholes drilled horizontally into the pillars in the salt for some specific horizontal reasons there for the salt thickness, but also, our Belgian colleagues consider horizontal emplacement of high active heat-generating wastes in horizontal boreholes drilled into the Boom clay formation underlying the site, where the former Eurochemic Reprocessing Plant is located.

And one final statement I want to make about retrievability. I am quite familiar that the 50 years period for retrievability is prescribed in your regulations, but I would like to ask if this is already a closed issue, or if it can be thought over again, for many of the problems--also of the thermal loading--can be at least ameliorated, if not even solved, when you are not looking for such a long time of retrievability. For instance, our German concept foresees that we will mine the repository to the very far end from the central shafts, then starting waste emplacement at the very far end, backfill immediately the filled areas, the filled panels, and then working backward from the outer boundary to the shafts, and then, finally, when the repository will be completely filled, filling up the shafts.

So in conclusion, I would ask if retrievability in this country is already a closed issue, or if you can open it again.

Thank you.

DR. ALLEN: Thank you, Klaus.

Let us turn to Gary Simmons.

MR. SIMMONS: Thank you, Dr. Allen. The advantage of coming third, is that there may be some repetition.

I'd like to compliment all the speakers during this meeting. I found it a very informative meeting and it provided me with a broad update on many of the engineering aspects in the U.S. program.

I'd also like to point out that as you receive my comments, please keep in mind that I'm not intimately familiar with your program, and much of what I say is based on what I've heard in the last two and a half days.

In the letter of invitation that we received from the Board, there were six questions asked that were to be addressed at this meeting. I think, in fact, most of them were addressed to a reasonable degree by the presenters within and without the DOE, and I think very much information was provided on the issues.

Prior to last night, though, when I re-read the letter I received very carefully, I was expecting to hear that there were thermal criteria that had been selected in this program, at least at a conceptual level, and that some repository designs had been developed based on those criteria. However, in the presentations that I heard, I got the distinct feeling that the DOE was reluctant to do that because of the recent change in the integrating contractor, and that that may, in fact, right now be a significant uncertainty in the U.S. program.

On a similar subject, I sensed from the presentations and from some of the responses to Board questions that coordination and integration within the program may not yet be effected completely. I must say, though, that this is one of the best integrated presentations I have heard on the U.S. program, but there were several presenters' remarks that indicated that communication and interactions might be further improved.

This isn't an issue that's unique to Yucca Mountain. I think we all suffer to some extent from this, but it's particular to Yucca Mountain in the sense that there's significant geographical separations between the site and the various program participants, and this, I think, represents a serious management challenge.

To some more specific points, I was expecting at least to see a simplified plan for establishing a preliminary set of thermal criteria for the Yucca Mountain repository. This was only really mentioned conceptually by Tom Blejwas during one of his presentations. Based on his comments, I gather, though, that this will have to await the integrating contractor participating in the development of such a plan.

Although it may have been raised more frequently than I noted in listening to the presentations, there was little mention that I heard of design of tests for the in situ work at the site to develop the link between laboratory properties and in situ properties, and it wasn't clear whether some of the lab data were based on confined or uniaxial tests and, in fact, how closely representative the lab tests were of the in situ condition.

I think the concept of alternative emplacements of waste packages, such as the horizontal placement in drifts, appears to offer some possible advantages in flexibility, and possibly cost, and it should be studied further. Conceptually, there may also be some benefits in the enhanced cooling concepts. From my perspective, though, issues to be considered would have to include layout and excavation methods, waste package placement method, retrievability, backfilling material and method, the safeguards aspects of tunnel emplacement, and the effect of the enhanced cooling system, other than ventilation on the placement and retrieval operations, the system chemistry, and the overall system performance, and as important as everything else, on the cost.

Based on the discussions, I sense that the project may be in a position where it's being forced into more and more complex modeling, and more and more complex conceptual assessment of the natural systems that exist or are perceived to exist at the site, and that's because there's currently no site access. I believe that a properly designed and well integrated in situ testing program will aid in bounding the issue of how detailed and complex a model must be to represent a sub-system's performance adequately.

Some of the systems are so complex, that the data cannot be gathered to calibrate the complex models that are being thought of, at least I don't think it can be gathered, and later, I don't think it can be gathered to be applied at a site in a site specific assessment.

It's important that we all remember one comment that I think Larry Costin made, that geo implies uncertainty, and although the models are essential to the work we're doing, they'll never be able to accurately simulate all the processes. In the in situ testing and studies of the natural system, efforts should be made to establish those processes that are important enough to warrant modeling, and how complex a model must be to adequately represent those systems.

I'd like to close by complimenting the Yucca Mountain Project and the non-Project speakers on the quality and content of their presentations. I have found this meeting very interesting and informative. I also would like to extend my thanks to Don Deere and the other Board members, and to the Board staff for their invitation to me to attend and for the hospitality they have shown the international visitors throughout the meeting.

Thank you.

DR. ALLEN: Thank you, Gary.

And our fourth foreign guest, Peter Stevens-Guille.

MR. STEVENS-GUILLE: Thank you, Mr. Chairman. I'd like to do a "me, too" to those last remarks that Gary Simmons made. We are very grateful, indeed. I didn't lose any money last night, so I've only got two comments to make today. I didn't win any, either.

One is about modeling. Modeling is going to be with us, I am sure, for the long distant future, and getting away from the sort of mathematical niceties of modeling, we've got, all of us have got a very difficult job to do--or some of us, anyway--a very difficult job to do eventually to convince the public about the truthfulness and the veracity of our models, let alone ourselves as technical people, and I don't want to make a comment either one way or the other about the hot repository, because I've only just been really exposed to it for the last two and a half days, but there are certain parts of it which are very compelling for the man in the street or the lady in her kitchen or, you know, the general public, and that is the idea that you keep things so hot for so long, that they're not in contact in water and they, therefore, don't corrode. Whether there's an internal rain in the repository after 70,000's of years, I'm sure will come up later this afternoon.

But we must be very careful, I would hope, in selling these, compelling that it's perhaps the simplified concepts to the public, if for only one reason: It's going to queer the pitch of other international programs which don't have the benefits of a dry repository.

I'd just like to make one other comment about the MRS. It came up today, but only really in connection with cost, but the aging of the fuel and the MRS are inextricably linked, of course, so is underground emplacement and ventilation, and so on, and I would just like to make the comment that when you do the arithmetic and you come up with these dreadfully crude numbers, they're not as bad as kW/acre, but dollars per kilogram, which is a consistent set of units, it's an extremely high cost for an MRS, and I would hope that you'd be able to bring that down, because you are ultimately responsible for taxpayers.

But anyway, that's a slight snide remark, but $2 billion for the MRS is certainly not very cheap by anybody's standards, and it is linked to this whole strategy that Nils was mentioning of how one emplaces the fuel, so that, I think, deserves a bit more discussion, possibly, at a future meeting.

Thank you.

DR. ALLEN: Thank you, Peter.

Let me now turn to Larry Ramspott. You gave us a somewhat philosophical and provocative introduction. We're certainly interested in seeing how much provoked you were during the meeting, provoked or soothed.

DR. RAMSPOTT: Well, before going to that, I would like to make an observation. I think Carl pointed out that I'm not representing the Department of Energy here today, and haven't in the talks that I've given so far, but I would like to point out that the draft mission plan amendment has been released by the Department of Energy, and it's going to be reviewed, in fact, later this month, and I think the latest time for comments is something like November 7th, or something like that, and on page 64, I'd like to note for the Board and for members of the audience, on this particular case, there are decisions related to design, and the first one of these questions is: "Should the heat load of the repository remain as currently conceived, or could an advantage be obtained from lower thermal loading?", and then goes on to several other questions. Nowhere in here is higher thermal loading mentioned, and I think a lot of that was discussed at this particular meeting, so that was the first observation.

The second one was: "Should the waste package be designed to exceed the regulatory requirements by a significant margin?" I'm not sure that that necessarily pertains to the subject today, but the last one is: "Should the waste packages be emplaced vertically into the floors of the disposal rooms, or horizontally into the walls of the rooms?", and yet, again, many, many times in this meeting we've heard that some form of emplacement in the drifts would be very desirable. So it appears that at the present time in the mission plan amendment, there isn't any consideration of some of the things that we are talking about today, and I just wanted to make that observation.

I only wanted to put this up for a moment, and to say that basically, a year ago, I wouldn't have been able to give the same type of talk, or do I think that some of the talks would have occurred today. I've found the fact that you can have this concept very exciting; in other words, what we have been talking since 1982, we've been arguing about whether or not we could keep the waste hot for 300 years or 1,000 years, but somewhere in that range, and not for the longer time period.

And the idea of keeping it hot and dry for at least the length of time that we are subject to the regulation, 40 CFR 191, is a very, very exciting concept to me, something that I think answers a number of problems that we've had earlier, and I think I'll come back to.

I only have two view graphs here. One of them was

I didn't get this one right when I went through the first time in the talk, and I'd like to go back to it and say a few more things about it, because I think it's a very key view graph.

I pointed out that only three concepts address the 10,000-year isolation from a simple viewpoint. What I'm talking about with a simple viewpoint is, that simple viewpoint is basically your licensing strategy. Now, down at the bottom here, I point out that the licensing strategy is really a testable hypothesis. It's your primary testable hypothesis, and I think having a simple licensing strategy--I understand the comment about let's not get carried away with things that are so very simple that people get turned off if we find that there are things about it that aren't quite right. What we need is a focus. We need a focus for the entire program from the top on down.

What I heard--and I've heard at every meeting I've ever gone to--is excellent technical work done by people who really aren't connected right up to the license, and I think having a testable hypothesis at the top that says: "Does this work have anything at all to do with what we're trying to prove in order to get a license?" This is what I really mean about focusing the whole program with one or more testable hypotheses. In fact, you'll have a large number, but the idea of having a primary testable hypothesis--the reason I only listed three of them when I first gave the talk was that I only saw three feasible ones for the United States in the Yucca Mountain Program, not that there aren't a series of others for other types of media and other places in the world.

I really felt that the partitioning, transmutation, super container, and the hot repository for 10,000 years were ones that I saw perhaps feasible for the U.S. program, but not necessarily feasible for other programs, but there are others in other places in the world.

What I tried again to do is to point out that these things are something that you can say to the person sitting next to you on the plane. They're things that Carl Gertz could talk to the people when he goes around and talks to various public meetings all the time. The partitioning and transmutation, you simply reduce or eliminate the hazard by reducing or eliminating the source, period. That's it. That's the idea.

The super container concept is: the waste package will hold the radionuclides until they become non-radioactive; or at least until a substantial proportion of them become non-radioactive.

The hot repository for 10,000 years: essentially, we're saying that, above boiling, there's no liquid water to corrode containers or dissolve and transport the waste.

Now, with respect to the international program, I think there are, in these other media, simple, testable hypotheses. With salt, basically, it's the idea that salt deposits have been there for millions of years and will remain there for millions of years, and I talked to Klaus at times during the meeting, and he agreed that the top hypothesis in the German program is the idea that the salt has been there a very long time, and it will remain there a very long time. Now, there are many, many other aspects to the program, but that's the top level hypothesis against which everything is judged.

I mentioned the unsaturated zone there. I wrote that down. With granite, basically, there's a very small quantity of water in granite, and it has a very low flow velocity through the granite, and that is a very fundamental feature which I think all of the countries that are working in granite work with, and I have also observed that, generally, the countries dealing in granite go back and use the super container concept and put the two of them together, so that they have two significant major hypotheses at the top level.

So I only will make one other comment, and I've been with the program for quite awhile, although lately, it's been off to the side rather than in the mainstream, but I've been, you know, working with it since early in 1976, and I wondered where we went wrong with the unsaturated zone. Why don't people really buy off on this? Why don't they think that's a great idea?

And I think I see a licensing strategy evolution for the unsaturated zone. When Ike Winograd and Gene Roseboom and others were thinking about this back in the late seventies, they weren't really thinking so much about heat, and basically, one of Ike's original papers--I think it was '78. I can't remember, but it was for low-level waste, and also for toxic and hazardous waste, pointing out how good the unsaturated zone is for things like that. You have a simple-- for the unsaturated zone without heat, water won't contact the waste, no water, no dissolution, no transport. That's the testable hypothesis.

The thing that you're going to challenge--and, of course, people will jump all over me about, what about this, what about that, what about this, but it's the hypothesis that counts. Well, what happened is, we got into heated unsaturated zone, what I described earlier in my talk as in the warm temperature regime, which is below boiling, when we looked at that, the elevated temperature introduces technical uncertainty, and the simplicity of the argument breaks down. So we've been churning, since about 1983 or '84, we've been churning up until the present time in this because we don't have a simple argument because of the technical uncertainty.

Now, we did look at the idea, well, let's boil it dry for--make it a thousand years hot. So we'll have a hot repository, but it'll be for a thousand years. That's only a partial solution for substantially complete containment. You still need to demonstrate release control for the 10,000 years, and you still have to meet EPA Table 1, or the overall systems performance objective, and still meet other things.

So this sounds nice, but it hasn't solved the whole problem yet as a overriding, top down solution; testable hypothesis. Now, if you go to the boiled-dry unsaturated zone that's 10,000 years hot, I submit that we're back to the water won't contact the waste, no water, no dissolution, no transport, so I think we may have evolved back down for that 10,000-year period.

Now, I realize the other issues about what about the fact that other countries look at two million years, and a whole series of things like that, but I think we still possibly have a top-level testable hypothesis for unsaturated zone, and I think the only other one that we have a possibility of in this country is the super waste package, something that's really very simple coming down from the top. But those, to me, would be--if you have to make a choice for Yucca Mountain among these simple licensing concepts, I think it's either the hot repository for 10,000 years, or the super container concept. I don't think, either politically or economically in this country that we're going to have partitioning and transmutation. That's a personal opinion, and I could be, you know, shown to be wrong in five or ten years, and obviously, we're not in a salt repository. We're not in a granite repository area.

So this may not be addressing directly the heat, but what I'm trying to say is, how does the heat fit into the fundamental, basic licensing strategy, which I think focuses the program, and then people can begin to say, "Where are we going? Why? What questions do we have to answer?"

So those are my comments. Thank you.

DR. ALLEN: Thank you, Larry.

And finally, among the sort of invited presentations, Dick Codell will say a few things. Dick is with the Nuclear Regulatory Commission.

MR. CODELL: I'm in the repository performance assessment section at NRC. One of the issues that we, as well as DOE and EPA are facing up to is the Carbon-14 release and transport. Because of that concern, we've been developing models for repository performance, and one of the areas is in Carbon-14 transport.

Since the effects of heat on repository performance is the topic of this meeting, I wanted to share with you some very preliminary results. They're so preliminary, that I hadn't prepared any presentation at all. I just put this together last night. It wasn't really an invited presentation, but I thought you'd be interested in hearing it, with the following caveats, that it is very preliminary, and this doesn't represent any NRC policy, nor is it presently a part of our performance assessment, and unlike Tom Buscheck's model, if someone had to ask me about the pedigree of the model, I'd say my dog ate it.

(Laughter.)

MR. CODELL: The model is a very simple, one-dimensional model; a finite difference with--which presently has 29 cells in it, and the idea is that from a regional model of gas flow in Yucca Mountain, you could take a flow rate of gas through the bottom, and for this I used the model that was based on Ben Ross's gas flow model for Yucca Mountain, generated a uniform flow through the column, that as it changed, was allowed to change with time, but it was uniform throughout the length of the column.

At the bottom boundary condition, which is close to the water table, you could put in a non-radioactive carbon dioxide as a source term. At the repository level, you could put in radioactive $^{14}CO_2$. These two figures show that the temperature varies along the length, and also as a function of time, and the saturation dose, too. I won't get into what the exact values are. They are just approximate values that were inputs for this demonstration.

The next slide talks a little bit about some of the simplifying assumptions of the model. The assumptions are you have instantaneous equilibrium between all of the chemical species you're likely to find in Yucca Mountain, and this is determined with a model, with a geochemical speciation model that we wrote specifically for the job.

Carbon-14 is carried through as a trace element in the general flow of all carbon, and it follows the dead carbon in the liquid and in the solid. There is no molecular diffusion. The gas flow is uniform, and only in the upward direction, so some of these assumptions are clearly not correct, because the gas flow around a hot repository is rather complicated, but for the purposes of this very preliminary analysis, I think it is okay to proceed.

Now, one interesting thing about the carbon cycle in the groundwater, I'm not a geochemist, but I understand that it's pretty well understood. Of all the geochemical systems, this is probably the one people understand best, and the interesting thing is with the solid calcite, is it behaves as--if you increase the temperature, it becomes less soluble. So this is a key factor in the model.

We ran this model, putting in approximate values for the chemistry from Yucca Mountain that we knew, and then in the model, used the values of temperature and saturation from other models, and this graph--which is a little busy, I apologize--shows the total carbon, the total calcite in the system as a function of distance from bottom to top. Maybe I should put it sideways to illustrate that better, but what happens is, as you start heating up the repository and drying it out, it favors the formation of calcite.

You initially start with a small amount of calcite, and as you heat it up and dry it out, the calcite increases for a time as it heats up and dries out, and then starts decreasing. So these are isotherms, if you will, of calcite, total calcite in the system.

This next figure shows the decrease in the total carbon in the liquid and gas phase in the column with time. As the carbon goes toward the calcite, as it heats up, it goes out of solution, and so the quantities of carbon in the liquid and gas drop. This has great implications for the transport of Carbon-14.

The next figure shows the transport of Carbon-14 and the general movement of carbon. Now, one of the key things we found from this model is that it depends, really, when you release the Carbon-14 into the model, what happens. If you release it very early on, at time equals zero, that's when things are happening fast. There's a lot of calcite forming, and the Carbon-14 will get trapped into the calcite, and this figure is for Carbon-14 in the calcite as a function of position, and for lines of constant time, showing here at 100 years, there's this much calcite, ^{14}C in the calcite, and it's increasing to 500 years. Then it starts coming down again as the calcite re-dissolves into the liquid and gas. So this ties up the calcite and gives you a large effective retardation.

Now, instead of releasing at a time equals zero, if you waited 1300 years, you get much less take up in the calcite, because most of the calcite has already precipitated. There isn't much available to take in the Carbon-14. This little tiny blip which you can hardly see, which is only about a factor of 20 less than what you get at time zero, is the amount of Carbon-14 that's trapped in the calcite for later times.

The next figure shows a breakthrough curve, not a breakthrough curve, but the concentrations of Carbon-14 as a function of time as it flows through the model. In this example, I've put in 10^{-6} Curies of C-14 at time zero, and if you look closely, you can see that it doesn't move very far for quite a few years. Even at a thousand years, it's not moving very far. This is because it's trapped in the calcite, and then as it re-dissolves, it starts moving down.

Now, the bottom line of this analysis is that you do get, with the simple model and with all the caveats understood, that you can predict with this model quite a bit of retardation, and it is very dependent on the temperature and saturation conditions, which is why I'm bring it up in this forum. This curve shows the cumulative release as a function of time and at different points along the column, this bottom line being at the end of the column, so if you put the Carbon-14 in it very soon, you get quite a bit of tie-up and very little will get out. This, I should say, is release for 10,000 years as a function of when you release it into the column. So if you release it at time zero, virtually all of it gets trapped and does not ever get out in 10,000 years, but even if you release it later on, you're still getting quite a bit of retardation, and so I think there's hope here, if the model is correct, that there could be some diminution in the releases of Carbon-14.

The last slide shows my conclusions, that you can get significant amounts of C-14 retardation, and that the process works best if the temperature increases and the saturation decreases. I was very interested to see Tom Buscheck's results there, because I didn't anticipate that the zone of drying would be so large and so persistent. If that is the case, then this would lead to the conclusion that, yes, you could have quite a bit of tie-up of Carbon-14 in the earth that would not get out in 10,000 years.

I have one slide that is just provocative, I threw in at the last minute, that shows an interesting effect. When I was watching the presentations on the biological effects of the increased heat loading, it occurred to me that everyone was thinking only of temperature, but with this model, if you have a large areal source of heat, and if you're getting large amounts of calcite forming and gas flowing through the mountain, it's a very interesting phenomenon and this, I hope, is a true reflection of the model, and not an artifact. But this shows the pH, actually, the log of the activity, of hydrogen, as a function of position along the column and time, and what this is showing is that well above the repository, you're getting an increase in pH.

What's happening here--this baffled me at first--is that the carbon dioxide that's in the mountain is getting stripped by becoming precipitated in the calcite near the repository. The air that's stripped of carbon dioxide, to attain equilibrium, is removing the carbon dioxide that's in the rock above the repository, causing the pH to drop, and I thought that this extended all the way to the surface and might be, in fact, a phenomenon that you'd have to deal with as an effect of repository heat.

Thank you for giving me this opportunity, and I wanted, also, to thank my DOE colleagues for helping me make these overheads at the last minute.

DR. ALLEN: Thank you, Dick.

MR. CODELL: I'm sorry. I should also say that my colleague, Bill Murphy at the Center for Nuclear Waste, Regulatory Analyses, was responsible for a large part of this modeling effort.

DR. ALLEN: Thank you very much.

I think we'll move directly on into the questions that we have posed and what reactions you people might have. Might I ask, in particular, that the people who are speaking go to one of the mikes. Well, there is one mike out there in the middle of the floor, and in particular, identify yourself.

The first question that we have is perhaps no great surprise. It's been touched upon by several people, but let me simply ask this, or let us ask this: The analyses to date have assumed a specific waste emplacement concept; that is, vertical borehole emplacement. Should not other emplacement concepts be analyzed; such as, drift emplacement, pre-closure ventilation, shielded waste containers?

Now, might I ask if someone would like to comment on that? Max?

MR. BLANCHARD: I'll be glad to, but I think the answer is obvious, Clarence. Yes, and we intend to do that, and this meeting comes at a very timely stage in the maturation of the program, because I think you've found, as a consequence of the talks that were presented by the team that represents the studies of Yucca Mountain and the conceptual repository designs, that we have been moving ahead in this area, perhaps at a faster rate than the rest of the repository concept system, and that the MRS and the transportation aspects aren't yet as mature, and we're groping for some answers that we can't yet achieve or acquire for two reasons: One, we haven't characterized the site enough to really know that we have mature models and are using meaningful data; and two, the rest of the parts of the design of the transportation and the MRS and the linkage to the utility dry storage haven't matured enough so that we can really make system tradeoff studies.

But there's every intent within the repository program to keep all of the waste emplacement alternatives viable from a conceptual standpoint, so as the rest of the components of the system go through maturation phase, that we will be prepared to feed that information in to produce viable tradeoff studies, and certainly, emplacement in a drift is one of the things that needs to occur in terms of design studies.

DR. ALLEN: I believe the mission plan amendment alternatives that Larry mentioned were--was it inadvertent that they were quite so restrictive?

MR. BLANCHARD: Yes, I believe so.

DR. BLEJWAS: Tom Blejwas from Sandia.

I just wanted to make sure that everyone realizes that we have spent quite a bit of time looking at two emplacement strategies, horizontal and vertical, and some of that's published and some of it isn't, and that we have considered the thermal in looking at both of those, thermal questions.

The other thing is, we have looked at ventilation systems to a fair degree of detail in our conceptual design for a repository, and among the kinds of things that we've looked at are the idea of blast-cooling areas, so that if we have to go in and retrieve, that we would be able to reduce the temperatures to an acceptable level for human beings in the retrieval process, and so we have considered some of the safety issues relative to ventilation and the temperatures that we would have in a repository. We just didn't go into the detail on that in this meeting because it had been covered at previous meetings.

MR. JARDINE: I think I'd like to make a comment along the lines to assure you that the Department does have a plan, you know, the waste package plan, which had been briefed to the Board, and had a process, and it was looking at a range of alternative and emplacement concepts, and I think as many as six of the Board members were at Denver in that workshop, and that process was in place and going on to look at systematically a range of emplacement alternatives, including the drift emplacement concept.

But due to the, you know, the budget situations of July 31st, and the reprogramming, that process has been put on a hold, from my perspective, with this fiscal year, and that there is a waste package plan. A process was implemented to look at the range of those concepts and bring them in, and because of the budget prioritization this year, the decision was made layers way above me that put that plan and the implementation on a hold pattern, and so I just wanted to amplify that that, indeed, was there and many of the Board members are a part of that process, and we did document the status of that in the focus meeting last week. There was a paper given that documented the progress of that system engineering approach, to look at a host of emplacement alternatives.

DR. PRICE: Is it a relatively fair statement to make that while certain parts of the program may go on hold because of budgetary constraints, that there are certain dates that do not necessarily go into slip, because these programs go on hold; such as a 1998 date, and that in the process of holding one part of the overall project and not allowing slip on other dates for whatever reasons that may be, the systems integration then gets confounded.

MR. BLANCHARD: You're quite right.

DR. PRICE: That comment whispered in my ear, which it does regularly, for similar reasons.

DR. ALLEN: Other comments from the front table?

(No audible response.)

DR. ALLEN: Okay. Another question: To what extent do thermal loading considerations enter into evaluation of site suitability? Tom?

MR. BUSCHECK: I'd like to comment how I think our continuing input will influence that. I think we've been pointing out over these last couple of years that fracture-dominated flow is the single most important repository performance issue.

If you're considering a cold repository site, fracture-dominated flow, or continuous preferential fracture pathways is obviously, you know, a very serious problem. We've been showing, through our dimensional analysis under a wide range of thermal loads, that under high thermal loads, continuous high conductivity fractures actually can improve performance. Therefore, in site suitability, there are no single-valued answers. I mean, I shouldn't say that, but on certain issues, the answer is not single-valued. So therefore, in terms of site suitability, the answers have to be answered with respect to what thermal scenarios and other considerations, because as we've been showing in certain scenarios, the hydrothermal system completely dominates the ambient hydrological system.

DR. DOMENICO: If you can theoretically design such a system to keep the temperatures high for a prolonged period of time, assuming that you find that's desirable, how about these heat pipe, natural heat pipe effects that might circumvent all your good intentions in the long run?

MR. BUSCHECK: Well, it wasn't our intention, but, in fact, the model calculations I presented yesterday had a very, very vigorous heat pipe occurring. I didn't point it out. It was occurring in some of the high thermal load cases. The heat pipe was extending over 100 meters in height. We had a very, very--and I could show you some examples if you would like to see right now, just to verify what I'm saying. Would you like those?

DR. DOMENICO: No, no. You could still maintain design temperatures, even though these--

MR. BUSCHECK: Well, our models, the model results you saw yesterday occurred with a very substantial heat pipe effect. The reason why it was so substantial is, as I was pointing out, the models that we use are equivalent continuum models, and the condensate flow, the vapor that was moving above the boiling region was condensing and staying within the matrix. That saturation above the boiling zone was approaching 100 per cent saturation almost continuously during the boiling process. It was continuously being conducted back in the fractures. We had a very vigorous gravity-driven heat pipe effect, which actually can be much more vigorous than a imbibition effect, imbibition-driven effect, but actually, we had both an imbibition and a gravity-driven heat pipe, which was actually greatly mitigating the net dryout rate.

DR. DOMENICO: Could you comment on what you would anticipate if you had representation of the fracture network instead of the continuum-based model?

MR. BUSCHECK: My feeling is that we would have a much larger dryout volume; that if, in fact--and we will in the future account for non-equilibrium fracture matrix flow. In non-isothermal situations, we will find that we can, I think, rigorously--also at invalidated models--show that much of that condensate will have shed off the boiling regions through cold spots between panels or between emplacement drifts, or off the edge of the repository. So what we'll find is that there's actually much less water available for heat pipes.

So, to date, our calculations account for a heat pipe effect to its maximum possible extent, and nonetheless, we see very persistent dryout.

MR. BLANCHARD: I'm glad you asked that question about site suitability. I'd first like to take the opportunity to make sure that our visitors understand that the Department has a iterative process, rather formal process for determining site suitability, with specific criteria in mind, and that, indeed, we've not made a decision to build a repository at Yucca Mountain, and that's one of the reasons why these repository design and the waste package design are very, very advanced, conceptual in nature at this stage, and indeed, we've not yet decided that Yucca Mountain is suitable.

As a part of John Bartlett's program, he has the engineers and the scientists in this program embarked right now on a site suitability analysis, one which is more advanced than the one that was published some five years ago in the environmental assessments, and it has been prepared and analyzed. It's been technically reviewed by a group of independent people, and it's now in the hands of some dozen or so university people around the country who have different disciplines in each one of the criteria, and I think that perhaps the manager of that effort, Jean Younker, could share with you all how thermal effects have been treated or analyzed in the site suitability assessment that's currently going on.

DR. ALLEN: Jean, would you be willing to do so?

MS. YOUNKER: Well, the DOE siting guidelines, 10 CFR, Part 960, do include several specific criteria that address thermal effects and I think the Board's already been briefed on this a couple of times fairly recently, but for everyone else, the siting guidelines include a couple of criteria where you're asked to look at, really, a combination of effects, where you look at the effects on the rock material in the pre-closure period in terms of thermomechanical response, stability effects, you know, the kind of the, will the rock handle this heat load, whatever heat load you assume you're going to put in, and that's the way we, in this current evaluation, we've used the conceptual design assumptions that Max mentioned.

So the pre-closure time frame, you ask, really, the questions that our people here presented and talked about in terms of: Can the rock accommodate the thermal stresses? And it's really asked in a coupled fashion in the criteria. For the post-closure, the same sort of question is asked: How will the rock respond to the long-term thermal impact that you're asking it to obtain, and there are some other guidelines or criteria where the thermal effect kind of must be considered, because the 960 criteria ask you to look at the total system response, so kind of in the same way that Mike Voegele laid it out for you this morning in his three-dimensional block diagram, we, on this team that Max just mentioned, had to go through that type of thought process in terms of what potential thermal effects would do to total system performance, and to any of the other performance objectives, because DOE's siting guidelines really adopt the performance objectives of 10 CFR, Part 60, NRC's criteria, as the overlying criteria that we have to meet, with the assumption, of course, that you don't want to make a decision that you have a suitable site if it isn't also very likely to be a licensable site.

I could answer questions, but that sort of gives you a summary of how we've looked at it in this current site suitability evaluation.

DR. ALLEN: There's a question or a comment back on the right there.

DR. ROSS: Ben Ross, Disposal Safety.

I wanted to make just a general cautionary comment about this. I think a lot of the issues that we've been debating are generic to unsaturated zone sites, and not specific to Yucca Mountain. I think you have to bear in mind that any unsaturated site is going to have a high permeability, for the very simple reason that if it has a low permeability, it won't drain and it won't be unsaturated, and a lot of these phenomena are going to be there in one form or another in any highly-permeable, thick unsaturated zone.

And in thinking about what implications you draw, if you see something here, you have to ask: How does this balance off? Is this specific to Yucca Mountain, or is this a general aspect of being unsaturated? And if it's a general aspect of being unsaturated, you have to balance it against the intrinsic advantages of an unsaturated site, many of which, I think, are not fully captured in the regulations.

So I think one should not be quick to leap to conclusions.

DR. DOMENICO: Ben, we don't have too many unsaturated sites, other than Yucca Mountain, that are unsaturated for several hundred meters. So the Yucca Mountain is almost--or at least this part of Nevada represents almost some unique conditions, I would say.

DR. ROSS: Well, I think one basis of comparison should be the suggestion that Ike Winograd made of burying it less deeply in one of the unsaturated, you know, one of the valleys in the basin and range, the closed basin and it has a deep water table in the alluvium, and I think if you do that --I haven't really looked at that from a heat point of view, but I've looked at it from a Carbon-14 point of view, and not in the sense of doing calculations, but just at a first glance, it looks like Yucca Mountain is terrific for Carbon-14 compared to putting it in a shallower alluvium.

DR. DOMENICO: Well, that's interesting. I'm glad you brought it up, because most people don't realize that Ike's first idea was to put it either in Yucca Flat or Frenchman Flat, and not Yucca Mountain. We have a very deep water table there, and I do recall that that site was thrown out because some genius put limiting criteria on the thermal conductivity of sites, and that just fell out. It just wasn't conductive enough, and it was gone, and so his ideas of unsaturated zone did not originate in Yucca Mountain. They originated out there in the flats.

DR. ALLEN: Steve Frishman wanted to say something. He had his hand up.

Steve?

MR. FRISHMAN: I think your current question about how thermal loading, or the issue of thermal loading affects site suitability is probably the key one that's going to last for quite awhile.

I can see that we have now a process going with the Department of Energy trying to figure out how to use its own regulation or guideline. We see 10 CFR 60 regarding its statements and non-statements about thermal loading, and then we see what has gone on here for the last three days, which we'll cynically call "tunnel fever," but I think there are some other things going on, too.

It points out that what's been happening between this discussion and the realities of moving towards determination of the suitability of a site for whatever reason, is a conflict that I don't see is resolvable, and that's that everything that has been discussed here for the last three days revolves around the concept of thermal loading being a design factor in a repository, and what good you can get out of it. It's almost like, you know, you hate to throw away the battery in the car until you've got the last electron out of it, and so how do you use that remaining heat to solve some of your other design problems, or to enhance design, and in concept, it's not bad. You've got a resource there, and you can treat whatever, you know, whatever your product is, which is disposable, you can treat portions of that as resources and do a net balance on it and have pluses and minuses and see how it works.

The regulatory world looks at it in an entirely different way, and I think Jean reflected that in her comments about what the site suitability evaluation is going through. The regulatory world generally, in 10 CFR 60 and DOE's guidelines, generally look at thermal loading as an impact to be dealt with. So now we have most of the people in the room saying it's a resource, but at the same time, the real decisions on whether you're going to be able to use that resource--say that it's an impact. Now, how do you reconcile the two of these?

And I'm not sure that it's entirely, as was suggested, something generic to an unsaturated site that has brought all this up. I have been taking part in EPRI's discussions about the EPA rule, and the extent to which it needs to be changed, and one of the things that I've--just an interesting bridge between the meeting two weeks ago and the meeting today is a good part of that meeting was taken up with potential problems of ^{14}C because of an unsaturated site, and is this a problem of the rule, or is it a problem of the site, or is it a problem just of design.

Well, this meeting is a real dichotomy compared to that. Here there has been a very obvious effort to ignore ^{14}C until about ten minutes ago, and you'll notice that essentially every model that you saw and every product of a model that you saw ignored ^{14}C.

What I'm leading to is that site suitability determinations are the way the administrative process has set out to determine whether you're going to go further with any site--whether it's Yucca Mountain or any other site that someone happens to either pull out of a hat, or maybe, ultimately, probably after my lifetime, bring out of a rational screening process. The question is, are we going to try to drive the regulation to meet the discussion that has been going on in this room about the use of the waste as a resource in disposal, or are we going to turn around in the other direction and try to live with the regulations that are out there, and this is the big question that's up right now as far as I'm concerned, relative to thermal loading.

If you, as erudite as the discussion has been in this room, if you take the concept of thermal loading, meaning just in general terms, keeping the repository horizon above the boiling point of water for over 1,000 years, if you take that concept to the first person on the street outside this room, I think you'll find you don't have any credibility, and regardless of whether you think it's right or not, whether the science is good, whether the engineering is good or not, you're going to have a real problem with that.

And I can give you another example of how that happens in terms of public credibility. When I worked in Texas, the concept was to look at a freezing method for shaft construction. We know it's been done before. We know relatively what the success rates are. We know that there has been at least one partial failure relative to the nuclear program with the freezing method.

The people heard, understood, DOE put on paper that they were intending to use a freezing method to get through the aquifer at the Deaf Smith County site, and credibility was gone immediately. So while there may be some value in looking at maybe a frontier area, or even, you know, the latest accepted technology on what you think you can do, you still, just as one of our speakers earlier said, you still have to make the public believe that it can be done that way, and that it's not just a way to be able to keep going and tell the people, "We know better than you. It'll be all right." Because the Department of Energy doesn't have that reputation, and can't afford to try it again.

So, now, let's go back to the real question. The real question is: Are you going to take a highly modeled, highly speculative, and, as someone said, you'll never be able to collect enough data to prove up these models that we've been looking at, just in the rudimentary forms, in the last few days. Are we going to use that as the explanation to the world that we are going forward with an even more complex system than we thought we had before, or are we going to live with the regulations, or are we going to change the regulations?

Somehow, it's got to fit together, and until it does fit together, this whole program is going nowhere, and I think we all understand it and I think, you know, you must recognize right now that the State of Nevada is, at least to some extent, a principal in the rate at which this program moves or doesn't move, and it's the people of the state who drive that. It's the people in the country who drive that, and if you want to change the regs, get out there and change the regs. Don't ignore them. If you don't think you need to change the regs, get the program into a shape that is at least understandable to people, believable to people, and is subject to validation, which right now, I think everyone in the room admits is probably not subject to validation, because none of us are going to live long enough to validate some of these codes that are being used just to get underground.

I hope I've upset everybody enough.

DR. ALLEN: Thank you, Steve.

Max was next in line.

MR. BLANCHARD: Well, I first wanted to point out, based on something that Pat mentioned about uniqueness of Yucca Mountain, and that is it may not be that in this part of the country, Yucca Mountain is all that unique.

As I recall, back in about 1983, the USGS started screening the United States for unsaturated zones that they thought offered potential with the concept that was brought forward by Ike Winograd and others, and I think Gene Roseboom can probably refresh ourselves better than I can, but they issued a Province 9 Screening Report, which was the southwestern United States, and found a very large number of localities that fit their screening requirements for low precipitation, high altitude above the water table, and appropriate rock characteristics that would be relatively freely draining, and high thermal properties, and Yucca Mountain was not identified as the best in that screening report, but it was identified as one among many that fit into that criteria.

And so it's not altogether a natural conclusion that if we decide Yucca Mountain doesn't have suitable attributes, that that would necessarily be the end of the concept of trying to place a repository in the unsaturated zone.

DR. DOMENICO: I didn't intend to mean that.

MR. BLANCHARD: Okay. I didn't think you did.

DR. VOEGELE: Without meaning to take exception to Mr. Frishman's discussion, I would like to make sure that the record includes at least three pieces of the regulation, relatively quickly following Mr. Frishman's discussion, that probably put a little bit different light on the way one might interpret what we're trying to do today.

This is 10 CFR 60.133(h). It's entitled, "Engineered Barriers. Engineered barriers shall be designed to assist the geological setting in meeting the performance objectives for the period following permanent closure." I don't believe that says you shouldn't use heat. I think it says you should use the engineered barrier system to the extent that you can to assist the natural barriers.

There are two others that are quite comparable. This is 133(i). "The underground facility shall be designed so that the performance objectives will be met, taking into account the predicted thermomechanical response of the host rock and surrounding strata, groundwater system." Once again, it does not say you can't use that heat to meet those performance objectives.

Finally, 133(a), the general criteria for the underground facility. "The orientation, geometry, layout, and depth of the underground facility, and the design of any engineered barriers that are part of the underground facility shall contribute to the containment and isolation of radionuclides." I'd just read that in as factual information.

DR. ALLEN: Jean?

MS. YOUNKER: Just a follow-up comment to what Mike Voegele just said. He was commenting about 10 CFR 60, the NRC's regulation. I'd like to go back to 960 with you for just a minute and mention that 960 really has that same flexibility that Mike is just talking about in that it allows you the tradeoff. It certainly never says that you must only look for negative impacts. They can, as well, be positive impacts.

So in the way that the team I've worked with has conducted the current evaluation, we have certainly considered both potential positive and potential negative benefits of the thermal aspect of the repository. So I think the Board probably would remember that in the post-closure evaluation for rock characteristics, we actually took a fairly aggressive and quite optimistic viewpoint of post-closure performance from a rock properties, rock characteristics viewpoint, and it was really precisely for some of the reasons that were discussed here in the session the last day and a half that we took that position.

MR. DANKO: George Danko, Mackay School of Mines, Mining Department.

I would like to make a general comment. Of course, I mean, the general question of site suitability and heat load, personally, I'm developing a feeling that many of the interesting programs are driven by budget constraints and moving into directions where work has to be phased and cannot be done in the right course and right time, and then I'm just wondering if the concerns, the interesting questions about ventilation, providing a healthy underground environment during site construction and waste emplacement will be appropriately investigated in this phase of the work, and the better that we can answer questions concerning climatization underground and enhancement of underground environment.

DR. ALLEN: Tom Buscheck?

MR. BUSCHECK: This is kind of in regards to a back to nature approach to model validation. This is a numerical experiment. It's not meant to represent what the travel time of the water table might be. I was hopefully getting across to most people that our feeling is that the high thermal loads mitigates against the impact that fracture-dominated flow has, in the fact that it's very spatially and temporally variable.

What we have here are three examples of a 100 micron fracture, where we have hypothetically assumed a direct pathway to a water table. It's, you know, a very extreme example, and the only point that I want to make here is the incremental impact that the upper Calico Hills has on the travel time of a particle through this numerical experiment.

In this case, the upper vitric Calico Hills--in other words, that which has not zeolitized--is 40 meters thick. Here it's 4.6 meters thick, and here it's not present, and all three conditions can apply at Yucca Mountain. In fact, I think it can be much greater than 100 meters thick.

You can see that because the matrix-dominated flow occurs within this horizon, that it tremendously attenuates the ability of the fracture pull, so we would require a continuous source of water for 44 years to break through to the water table. Where it's not present, it takes 52 hours in this numerical example. Now, again, this is just to show the incremental impact of the matrix properties. This is a point that I've been making, hopefully, continuously, that the single most uncertain, difficult to model feature of Yucca Mountain is the dis-equilibrium between fracture flow and matrix flow, and in many places in Yucca Mountain, we get very favorable interaction.

This condition, or much thicker than this, extends over a fair degree of the repository block. If we consider the fact that we can, under some thermal scenarios, go to a smaller footprint than where the repository lies, this is an example relative to the SCP-CDR, where we're using 100 per cent of that conceptual design. This is the proportion of that area that would be required for a variety of thermal loads, from 20 to 100 kW/acre, 10 down to 100-year-old fuel. You can see that in certain scenarios where you age the fuel and put it in under high APD's, you have reduced the areal requirements by 85 per cent.

So therefore, certain features of the mountain which we can show in site characterization to be more favorable, can be part, possibly, of the siting process, of placing where the repository is. But basically, the main point that I want to make is because what we've been finding, that boiling conditions and persistent dryout effects greatly attenuate the impact of fracture-dominated flow, and we feel these effects can be much more readily validated through the course of in situ testing by heating and boiling of large volumes of rock.

We feel, at least those of us that are familiar with the modeling outcomes of these, would like to see the opportunity to validate these in situ, and at least consider it as an option in what licensing strategy may ultimately be used. We're not suggesting that it be the only one. I think the best approach is to use whatever approach gives us the most certain and the least impact on the environment. But anyway, I just wanted to point out that we feel that thermally perturbing the environment can lead to models which are more validate-able, and therefore, less subject to uncertainty.

Thank you.

DR. ALLEN: Thank you.

There was a hand up over here. Larry?

DR. RAMSPOTT: I just wanted to make one comment to what Steve Frishman mentioned, and he sort of had the built-in assumption in his comment that Carbon-14 is going to get out of this hot repository concept, and although there wasn't a great deal of it mentioned in the last several days, I think that it's entirely possible that we could meet the regulation, either NRC or EPA, given the hot repository concept with respect to Carbon-14. It isn't a foregone conclusion that it's all going to get out and go to the surface.

DR. ALLEN: Other comments? Gene Roseboom.

DR. ROSEBOOM: I would just like to make an addendum to Tom's statement there. The concern over fracture-dominated flow, I would just like to remind you that this is not something that's going to go on continuously, but would occur only when you had major precipitation events, because the repository normally would be drained, and the water would move through rapidly, but they would be relatively short-lived events that presumably would pass most of the canisters fairly quickly.

MR. BUSCHECK: As I was pointing out yesterday, the rate of net condensate flux in some of the higher thermal loading scenarios was orders, several orders of magnitude higher than what's currently considered to be the average areal net infiltration rate at Yucca Mountain. One of the useful things we can do in site characterization is to get a reasonable estimate of that value, and to try to project it into the future.

My feeling is we're going to still find that under pluvial conditions, the net infiltration rate under extreme climate variation will be much less than the net condensate generation under certain high thermal loads. So, therefore, the impact of a climate variability will be almost lost in the noise relative to what the hydrothermal system can be doing to the hydrologic environment.

DR. ALLEN: Steve Frishman.

MR. FRISHMAN: Just one short comment.

In light of what Larry Ramspott just said regarding C-14, it may be advisable for the Board to go back into its previous reports and consider a revision of its statement about C-14 and the EPA rule.

DR. ALLEN: Other comments or questions?

In order to stay a little bit directed, let me ask if anyone has any specific questions to Dick Codell. We passed over his without asking questions after he spoke. Does anyone have any particular questions or comments on this?

DR. LANGMUIR: Don Langmuir for Dick Codell.

I guess I'd like to know a little more about the assumptions inherent in the model that Dick described; in particular, the interplay between the stable and the C-14 carbon isotopy and how that's dealt with in the model.

MR. CODELL: There's nothing much very magical about it.

The Carbon-14 is considered to be a trace; that is, that there isn't enough of it there to change the bulk chemistry in the rock, but it's assumed that it moves with the dead carbon into the calcite when that's precipitating, and out of the calcite, into the liquid and gas when it's coming out. So that's about the extent of the assumption.

DR. LANGMUIR: Another related question has to do with the amount of calcite, the attenuation will relate to the amount of calcite you can precipitate from the existing moisture or the recycling moisture. I presume that's part of what you calculated?

MR. CODELL: That's right. The biggest unknown, probably, is how much calcium is in the rock in Yucca Mountain, and that varies from place to place, I'm sure, but if you don't have enough calcium ions there, you're not going to precipitate any calcite. But the model takes care of that. It knows when it runs out of calcium ions.

I might point out that this model is the subject of two upcoming presentations. I'm scheduled to give one, if it was accepted, at the High-Level Waste International Meeting in Las Vegas in April.

DR. ALLEN: Thank you.

We've had a number of comments on this question of site suitability in relation to thermal loading. Let me just turn it absolutely wide open and ask whether anyone has anything further, any questions of any of the previous speakers, or any further comments on thermal problems in general.

Yes?

MR. WILDER: Dale Wilder of Lawrence Livermore Lab.

I'd like to respond to a couple of things, and I may use the view graph here in a minute, if you don't mind.

The first is talking about uncertainties related to things geo. I think that it was mentioned earlier, but perhaps I would like to reinforce that there's a couple of issues related to uncertainty with things related to geology. One is the natural variability that's just present in nature, and I think that there are many approaches that we've tried to use as a profession to describe this variability, and we'll never get away from it. I think that needs to be carefully distinguished from uncertainties in terms of processes and phenomenology and other uncertainties, even uncertainties in the measurements of those properties that we're trying to describe the variability of.

My second point--and this is the one that I may need to use the view graph on--has to do with kind of a follow up on Larry Ramspott's comment. Larry was talking about the changes in the licensing strategy or the testable hypotheses that developed over the years, and I think that you've heard a lot of descriptions of what would be the implications of various thermal loading. I think these implications also have to do with some of our site characterizations, so if I may, this is somewhat simple-minded, and I apologize for the crudeness of the sketch, but the point that I'm trying to make is that when you're looking at the characterization of the site, if you're looking for matrix properties, specifically, ambient conditions or conditions in which you have matrix-dominated flow--and I see that it got smeared trying to write in my lap there--vertical boreholes are sufficient, because you're looking for matrix properties.

What we've been talking about is a lot of processes that have to do with fracture-dominated systems. If you are looking for episodic fracture-dominated flow, or you're concerned about thermal perturbations, you then need to focus your site characterization on fracture properties, and so as a follow up to what Larry has said about the changing evolution in our licensing strategy, I would suggest that that also needs to be taken into account in the site characterization.

Thank you.

DR. DOMENICO: With regard to what Larry said and what

Steve said, you know, at one time DOE decided that Yucca Mountain was worthy of being investigated as a potential site, and they meant the Yucca Mountain that we've all come to love and honor, you know, the unsaturated Yucca Mountain, not exactly the dry Yucca Mountain, and our first day here I asked Larry a few questions, one of which: How much of the desire for a hot repository is driven by the sub-system 1,000 year performance, canister performance? And he admitted at that time that a considerable part of it was being driven by that concern.

Now, we find that there's a possibility we're looking at the 10,000-year requirement being driven by the very same concern, and good or bad, I'm not saying that's good, I'm not saying that's bad, but we find ourselves where we're letting the regulations drive what we're doing, perhaps--and like I said, I can't say whether it's good or bad--but I think Steve has a point, that there's a reporter here and we all wake up tomorrow and read the Las Vegas paper where they say that DOE plans to boil Yucca Mountain for 10,000 years, so I think these are all fine ideas, but I might also add that I have not yet heard DOE endorse this, and so that's very good, and we probably won't hear DOE. So these ideas, I think, are all right for discussion, but I don't think that this concept is certainly closed.

That's not a question, that's a statement.

DR. NORTH: I'd also like to commend Steve Frishman. I think he identified a crucial issue. I must confess, when I came into this meeting, I was thinking of the thermal loading issue primarily in terms of an impact to be dealt with, and I've since had my vision expanded to thinking about it as a design factor. But I think it's very important to avoid an either/or frame of reference, and especially doing that at an early stage now, where, really, our focus ought to be on the site suitability determination as DOE has said it wants to emphasize in the near term in this era of budgetary restriction, and identifying what information is really crucial to resolve the question: Is the site acceptable?

Now, in the draft mission plan statement, there is a paragraph beginning on the bottom of page 43, describing the results of the test priorities task force, describing how the task force adopted an approach to determining priorities, and then it concludes: "Consequently, our emphasis at the candidate site will be on two things: One, the information needed to determine the potential for gaseous releases over the long term; two, studies to resolve the geologic complexity of the site as related to radionuclide migration by groundwater transport."

I think what I've learned at this meeting is I can't really think about either of those issues as it needs to be dealt with, without bringing in thermal loading as well, and we ought to be thinking about it in both of Steve's dimension, an impact, and a design parameter; that, in particular, what Dick Codell has just shown us is that the potential for gaseous releases may be very strongly influenced by the thermal regime; that that may be a critical parameter and it might take things in a good direction as opposed to a bad direction. And then I think what we have heard about the issue of the thermal effects on groundwater transport may be quite critical in terms of the impact of a repository design on performance, and we clearly need to understand that better.

I come away thinking that a very crucial need in the near term that I would like to see met is a detailed discussion of what validation would be needed in order to convince skeptics--and I think the Board has at least one, perhaps a number--that one can carry out enough analysis to be assured that a warm or a hot thermal regime will, in fact, protect against wet continuous or wet drip scenarios, such as to reduce risks.

This means we have to be able to assure that the thermal loading will not simply pump water up that might then drip back down on the canisters, leading to corrosion and release. What scale does it take to validate the type of modeling that we have heard about and that we've seen in terms of frog-eye plots and the many visuals that Tom Buscheck has shown us? What does it take? Does it take a room-size experiment? Does it take an experiment extending over many acres?

And the Board has been to STREPA. We have seen the kind of validation that other countries are trying to obtain. What does it take to do that in tuff so that we understand Yucca Mountain well enough to be able to answer the questions that are going to need answers as part of the licensing process, or to determine that the site is unsuitable well before we get into the licensing process.

DR. ALLEN: Thank you.

There was a hand raised way in the back. Yeah, Steve, once more.

MR. FRISHMAN: This was the last point that I wanted to make. I tried to divide it up so that I had two or three points that could be made one at a time, and I think, Warner, after your comment and a couple others before that, it's time for the final point, and that's a point that was brought up by Dr. Price the first day, and that's: What's driving what decisions, and where do the decisions come from, based on the sort of amoebic movements of this program, where one part of it seems to reach out in one direction and it gets punched, so it just poofs out in another direction someplace else, and I've only been an observer and a participant in this since about 1976, so I really don't have all the scars on me that some of the people in this room do, but I think I have enough.

At this point, I think, as was mentioned, the thermal loading issue is a crucial one when you're looking at any site, given the constraints that the Congress laid into the system, and that's, get this stuff into geologic disposal as quick as you can. You're going to have the thermal loading issue as long as you don't have a site with an infinite plane or dimension where you can spread spent fuel out as far as you want to, to where the thermal effects from one emplacement are essentially unknown to the next emplacement. You don't have that luxury right now.

What you're stuck with is a schedule and a block where I noted the other day some structural features that used to be considered a benefit have now evaporated, or at least in some people's minds, because they're no longer a benefit, but you're in the position now where if you say that thermal loading is an integral part of determining suitability, because it is both a design feature and an impact, regardless of what you want to call it, it must be dealt with in a license application, and it must be dealt with first in a suitability determination by the Secretary, separate from a license application.

Where do the decisions get made that allow that to happen? Right now, we're sitting--well, let me just give you one sort of crucial example of where we are. Probably, either it has already been done or will be done within the next very few days, will be the official designation of a new reference ESF. If it hasn't been done, it will be. It's on schedule to be done. That new reference ESF is nothing like the other one. You know best why it isn't, and how it came to be.

All right. Now, the Department maintains, and you agree--and I think most people with practical thinking agree --that that ESF should be designed and constructed in a way where it would become part of a repository if a repository is to be built.

Now, what effect has any consideration of thermal loading had on that design? The answer is, essentially, none, and it can't, because you don't know that you can rely on thermal loading. You also don't know, if you can rely on it, what the parameters are, but at the same time, you're about to have a decision made at the highest level in the Department of Energy to build that thing roughly the way it has been designed.

And you have another piece that came up in the last couple days regarding, well, what testing should be done in the ESF, and maybe even surface-based testing, that would help to understand, validate, or maybe even improve some of the thoughts about how to use thermal loading as a resource, while keeping in mind the concept of impact. That's the test prioritization exercise that has gone on. What, in there, has been bounced directly against trying to figure out optimal thermal loading?

I'm not sure I know of very much in there that has. So, once again, we have a program where there are pieces running out there. Decisions are being made. Those decisions ultimately, one way or another, either get reinforced or shot by budgets, but somebody's going to dig a hole, and that hole is going to become part of the next thing if you're successful in getting that far.

So the real issue right now is what decisions are going to drive, and even if you think that thermal loading is the best thing in the world to do, are you going to be able to do it without having some other decision co-opted out of some other part of the program because that was the princess that year in the budget discussions that are going on, and next year, there will be some other one, and the year after there will be some other one.

So you end up with a situation where the program itself, whether anybody planned it to be or not, the program itself turns out to be a trial and error, just mish-mash of a whole set of sort of disconnected decisions, and it comes back to something I've been saying for years and years about this program, and that's that the real product of this program is not one of everybody trying to do the best that they can. The product of the program is much more a mix of sort of inconsistent bests on everybody's part that don't fit together, so it comes out to be one that is good enough, good enough in the minds of the people who are involved in it, and at some point in the process of becoming good enough, the public is going to say, "Good enough is not good enough."

DR. ALLEN: You're next, Max.

MR. DANKO: George Danko, Mining Department, Mackay School of Mines.

I would like to deflect back to this comment about thermal load using high temperature as an asset instead of a reliability to maintain a dry belt around the container, and intake rate, this dried out zone, into the engineered barrier system. I believe that can be considered as a spice, and if you use too much spice during cooking, you can ruin the dinner.

This thickness of this dry belt can be of a concern. If you turn Yucca Mountain into a volcano, with a lot of heat in the middle, that creates another public deception. Everyone can be convinced that a little bit of drying, or we don't need a lot of dried zone to prevent water from coming into the repository area. Even a few feet layer of dried bed would be quite enough, and if you think about the delicate balancing of the thickness of this layer, you might want to think again about those new techniques. You can use thermal heat pipes or other engineering elements where you can just do as much as you need to do and dry out only a relatively small area in the mountain.

Thank you.

DR. ALLEN: Max?

MR. BLANCHARD: Thank you, Clarence.

Everyone that comes to these meetings are fully entitled to their own opinions, and we're pleased to have them express their opinions. I feel that you may find some evidence that you'd care to cite, like you have, Steve, that the program is run on trial and error and is a mish-mash of illogical decisions. However, I, for one, feel just the opposite, and I don't think it would be fair to let that perception continue, either on the record, or for those who are at the meeting who are trying to positively work in a cooperative fashion to figure out how to get the correct data and to develop the appropriate models, and feed that information into developing meaningful designs so we can look at the system and make some calculations that will withstand a lot of criticism and debate for a number of years about radionuclide releases in the future.

Nothing could be further from the truth than what was said about not incorporating thermal effects into either the repository design concepts that were considered for the ESF alternative studies, or for the ESF itself. I assure you that they were there. They're involved in from a design standpoint, from a testing standpoint, and from a performance assessment standpoint. All of those are in the ESF alternative study. The alternative study, I understand that people can look at it and kind of miss things because there's a lot there. There's a tremendous volume of information, and in order to make sure that it's clear and it's not overlooked by those who may have, in a cursory view, thought they may have not been there, I'd like to ask Mike Voegele to explain how those were summarized for use in the decision process, where the viability of the program looked at all of the things that fit into making the picture for selecting alternative ESF's.

Could you point out some points, Mike?

DR. VOEGELE: This is Mike Voegele.

I guess I'd like to respond to both Steve's comment and Max's question simultaneously, because there's really a two-phase aspect to the answer that I'm about to give.

With respect to the regulatory requirements on a repository, all of them were considered in the development of the ESF alternative study. Many of them were found to be more significant discriminators with respect to the ESF alternative study than certain other ones. I think with respect to thermal loading-type questions, other than the potential for introduction of fractures, I would have to say that the thermal loading, per se, was not a significant discriminator in the ESF alternative study.

However, I would like to make abundantly clear that the decision that's about to be made by the Department of Energy is not a decision to start construction of the exploratory shaft facility. It is a decision to start Title II design for the exploratory shaft facility, and we have made it very clear in our plans for the Title II design activity--which is the design activity where you really flesh out the design--that all of the questions that have been discussed today, yesterday, and Tuesday with respect to things like ventilation of the repository, thermal loading of the repository, so forth, are on schedule as trade studies necessary to support the ESF Title II design.

So the point in time where the Department of Energy will be making a decision to start construction of an exploratory shaft facility follows the Title II design. The early phases of the Title II design will not necessarily address underground aspects of the facility in the repository, so the thermal loading question is not so important to the grading of the pads for the accesses, for instance.

However, as we told the NRC a couple of weeks ago, these types of issues--thermal loading, ventilation, numerous repository-based design tradeoff studies--will be done as necessary to support the Title II design of the ESF so that we will be able to say with confidence that we have considered things like thermal loading of the repository and repository ventilation before we start construction of the ESF. We believe that is necessary to meet the requirements of 10 CFR 60.21.

DR. ALLEN: Thank you. I'd like to sort of try to begin to wrap things up. I think we've had a good expression of opinion. Other members of the Board may have things to say, and Warner, in particular.

DR. NORTH: Well, I thought I'd jump into this because, as I hear Steve Frishman's last statement--which provoked response from Max and Mike--I get to thinking that I believe that Steve and I agree--I'm not sure I'd want to share his choice of words and examples, but I think the concerns he expressed are concerns that I feel very deeply, and I think they've been reflected in some of the reports that the Board has come out with; in particular, our emphasis on iterative performance assessment, and our emphasis in systems engineering.

Now, I'm very pleased in the draft mission plan amendment, which I heard Steve Frishman calling for a few years ago as something that DOE badly needed to do, the kind of emphasis I really want to see towards solving the problems that I believe Steve is pointing out. I'm looking specifically at page 56, and the paragraph that starts--the first new paragraph on that page that says: "We will use this iterative process of performance assessment to refine the design of the repository. As we complete more advanced designs, we use them in performance assessment models. We use the resulting estimates of system performance to determine what refinements are needed in the models and what aspects of design could possibly be modified to improve performance."

Now, a few minutes ago, in calling for work to validate some of the models we've heard discussed here today, what I wanted to do is see that theory put into practice. I haven't seen the update for the site characterization plan to figure out how to carry out the kinds of validation models that I think are needed to convince skeptics that we really understand thermal loading plus geohydrology well enough with respect to this site.

Likewise, on page 155, there is an extensive discussion of systems engineering, which I highly applaud. It's the paragraph directly above "Configuration Management," and I won't take the time to read it, but I'll commend it for attention, and I will also note that I think the kinds of tradeoff studies--not just of the repository, but the whole system--are badly needed. I haven't, as yet, seen them. I've heard discussions of some of the issues involved, but actually getting down to having some analysis of these issues and some numbers we can look at in terms of impacts on safety and performance, impacts on costs, and impacts on schedule, as far as I know, DOE is in the position of having some excellent theory which I thoroughly agree with, but putting the theory into practice and getting some insights is a job that needs doing.

Now, I'd like to encourage doing that job in a very positive way, rather than one that's critical of the program or intentions of the people involved in the program, but I think the need's a clear one.

DR. ALLEN: Any final comments from the audience? How about from the Board, or from our guests?

DR. PRICE: I'd like to just comment that there are two types of design phenomena that seem to me to appear in the processes that we observed, and seem to me, I'll say. I'd like to think that back behind the regulations that are in the program, there was an under-riding need to protect the public, and from this need came criteria, and then came regulations.

Then, in the attempt to implement the regulations, the regulations may have served to erect institutional barriers, where perhaps these barriers were not foreseen when the regulations were written. Also, a tendency to dictate design in ways that were not foreseen when the regulations were written, and maybe a degradation of the overall systems engineering integration in ways that were not seen as individual criteria were addressed in the formation of regulations.

So it seems apparent that there needs to be a feedback loop, that you now look again at performance and safety, and efficiency, and so forth, and apply them to the criteria and to another look at the regulations. Failure to do so, I would think, becomes design by regulation, where the comment was made that the licensing hypothesis is a testable hypothesis, which maybe I'm taking a little out of context here at this point, where the design becomes--the effort in design becomes one entirely dedicated to licenseability, and this tends to fragment the program, where the design repository looks at whether the repository is licensable, whether a cask is licensable, whether an MRS is licensable. And so it seems like we shouldn't be designing by regulation.

The other kind of design that appears to me to arise occurs when we have a desire to pursue system engineering and bring an integrated program into reality at the same time the schedule marches on, and there are certain indelible points which must be reached. There are contractual arrangements which must be satisfied, and we have to go on, and in the process, things happen that make the design a reality that was never intended, perhaps, to be a reality, and I think this is design by schedule default, and I'd like to suggest that both of these are contaminations to the proper design process, and sometime, somewhere, we need to be able to purge ourselves from these if it's at all humanly possible.

DR. ALLEN: Thank you.

Well, in that case, let me thank all the people who participated and the speakers this afternoon. Certainly, I found it very useful. I hope you have, and the Board is thankful to you.

At this point, let me turn the meeting back over to our chairman, Don Deere.

DR. DEERE: Thank you, and thank all of the participants of the meeting, those of you who sat through three days of interesting and, at times, rather laborious attempts to understand some of the graphs, but in the end, we think we all benefited. The idea was to have a comprehensive discussion of potential effects, both beneficial and non-beneficial effects, of thermal loading, and I think we have heard differences of opinions with respect to canister life, with respect to movements of the groundwater, precipitation. All of these things have come out and different people have come up with different results, but it has, I believe, broadened everyone's understanding of the issues that are being looked at, and this was essentially what we were interested in.

We wanted to make sure that the design concept that started really had a basis not only from the historical concept of what was known and what was done ten years ago and five years ago and two years ago, but right up to the current, and that the plans were sufficiently broad that they could handle some of these conflicting opinions that exist based on analysis to date with less than complete data.

So we want to keep the designs open. I think this was our interest. We've been very actively engaged in looking at the ESF studies, and we know that they tie into the repository design, and that's why we felt that the thermal loading and how it was going to affect the repository design was something that should be looked at right now, and obviously, it has been and will be during the Title II design.

So even though there have been differences of opinion and different emphasis, I believe that this has been a step forward to have this free exchange with the differences of opinion and different results all looked at, and hopefully, you'll be talking about these with some of your friends on the way home and it will have made a step forward in the process of doing the site characterization studies.

Again, special thanks for all the contributions of our foreign guests. We believe that they've brought to us an understanding of what they're doing, even though they have different geologies and, therefore, much different designs, it was very helpful, and we do appreciate that we put them on the spot and said, "Well, you've been here for two days. You don't know much about our program, but what do you think?", and we heard. They liked some of it and they didn't like some of it, and I think that's very good.

Again, thanks to all of you.

(Whereupon, the meeting was adjourned.)