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NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

THERMAL LOADING:

The Integration of Science and Engineering

July 14, 1993

Stouffer Concourse Hotel
3801 Quebec Street
Denver, Colorado 80207

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I N D E X

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I N D E X (Continued)

P R O C E E D I N G S

DR. VERINK: To those of you who have not, I would ask that you be sure to register back at the table so we have a record of those who attended.

This is the continuation of the summer Board meeting of the Nuclear Waste Technical Review Board, and we've had a very spirited session yesterday. We hope to have an equally spirited one today.

My name is Ellis Verink. I'm a Professor Emeritus in the Department of Material Science and Engineering at the University of Florida. I'm a metallurgist by training, and spend a good bit of my time being interested in questions of corrosion. I'm still active at the university. I still have five graduate students who are trying to get their Ph.D.s finished, and I chair for the Board the panel on engineered barrier systems. I'm also active on the transportation and systems panel, which is chaired by our friend from Virginia.

I might start this session on a perhaps unworthy historical perspective. I learned a piece of miscellaneous information which maybe some of you may not have been aware of; for example, I learned recently that General Custer was the first man to wear an Arrow shirt.

(Laughter.)

DR. VERINK: It may be that there are others of that category before the session is over today.

Yesterday, Don Langmuir gave us a splendid beginning for this activity regarding the science involved in thermal loading; natural analogues, and all kinds of thermal modeling. Today, we intend to follow this activity with continuing momentum, discussing the engineering that links and supports the science of thermal loading.

I'm starting off with a short session about thermal loading and the waste package, and I'll turn it over after the break to Ed Cording, who will lead the session on repository conceptual design and thermal loading, and this afternoon Garry Brewer will chair a session which has the understated title, "The Big Picture." We'll hear some of that, and then there will be a subsequent round-table discussion for which Don Langmuir did such a good job in setting the stage.

There will only be three talks in this session. The first will be Steve Saterlie, who you heard from yesterday, so he doesn't need further introduction to this audience, but he will be talking a just-completed two-month study looking into various thermal constraints in the site characterization plan, a question of where do they come from, and are they still valid, and do we need to change them, and add some more points that we'll want to consider.

Next, a long-time buddy of mine, Dan McCright, a fellow Ohio-Stater, a long time at Lawrence Livermore, will talk about corrosion in the context of the various strategies for thermal scenarios.

Then, Tom Doering, as many of you know, he's the Manager of the waste package design, the MGDS, and has some twenty years of background in nuclear power and design and construction, will be our anchor man, and will be talking about the compatibility of the current multi-purpose canister or container design with the various thermal scenarios.

Some of us will find ways of trying to see what connection this may have with the remark which Bill Simecka made yesterday about larger MPCs being perhaps incompatible with cooler thermal-loading strategies.

I asked the speakers in the session to err on the side of being too short rather than too long, to allow some time for questions, and with this in mind, I'd like to re-introduce Steve.

Would you take it, please?

DR. SATERLIE: Okay, I guess that's on, and I'll take your warning and start practicing my dodging and ducking here.

I'm going to talk to you a little bit about the SCP thermal goals and the reevaluation that we did. First of all, I want to introduce this subject with the fact, I want to recognize J.C. de la Garza and Dan Royer of the DOE, who really were instrumental in making sure that this effort got going.

This was a relatively short-term effort. It was something that we did to try to get some goals together, to reevaluate what we had there in the SCP to determine whether or not those were still valid goals, and what was the background that went into them, and I'll talk a little bit more about that.

I'm going to give you a brief introduction, the objectives of what we tried to do, the background of where these goals came from, talk about the SCP thermal goals themselves and the assessment we did, and then provide some recommendations.

All right. Well, why did we do this? First of all, the thermal goals in the SCP were really established based on information that was available back in 1986. They were put together in the 1988 document, and that primarily stressed vertical borehole emplacement. Since that time, of course, we've been looking at other emplacement strategies, and additional analysis has been available, additional data has been available, so it seemed appropriate to reevaluate these and determine whether or not they, in fact, were still appropriate. In many cases, they were. Let me say, a lot of work went into those original goals, so that there was some thought there.

The thermal goals themselves are really not directly derived. They are goals that it is believed that, if met, will provide overall performance that we want to achieve in a repository for waste isolation.

The objectives of the program, as Ellis indicated, was, first of all, to provide some thermal criteria to support the systems study that we talked about yesterday; to look at what testing and analysis might be needed to better evaluate these goals and help focus that; and it was believed these would be initial steps that might be taken to change the baseline, if it was believed necessary.

How we did that was, first of all, we did a fairly careful evaluation of what was the technical rational for establishing the goal. Why was it established in the first place? Is the goal still valid? Does it apply to all the emplacement modes; and, if not, should it be deleted or changed? Finally, are there any goals that we feel need to be added at this time, and what tests and analysis need to be done to better establish what those goals should be.

The background, the SCP criteria or goals, as we're calling them, were used to establish the performance of the repository. As I said, the SCP document was published in 1988 and, in many cases, it used 1986 data that was gathered in, I believe, Albuquerque, where an expert group got together and put all of this analysis into the SCP goals. As I said it's oriented towards, primarily, the vertical borehole, but also some in the horizontal.

The performance standards, what I mean by this statement is that the regulations are currently being re-promulgated. Also, there's not a direct link, in many cases, to the regulations, and, in many cases, we have an incomplete picture of how the mountain behaves. And so, therefore, we needed to provide derived criteria or surrogate criteria, which are what we call these goals. In this way, we believe that we can meet the performance that is needed if we meet those goals.

The strategy in the SCP was there were four functions that were identified in the regulations. The last one of those functions is what was focused on thermal loading, and the post-closure performance, and it is that one that we're going to concentrate on.

Based on that function, process steps were established, and we'll show you what those process steps were in the table. These process steps describe how the function will be accomplished.

Based on that, a performance measure, such as temperature, or relative motion of a layer were identified as the performance measures, and then a goal was developed, which it was believed would be adequate for the issue to be favorably resolved.

The reevaluation that we did, we formed a working group which was composed of several teams. We wanted to try to stress the fact that we understood that there was a couple processes going on, and so we tried to divide up the experts into different teams having to do with hydrological, geochemical, engineered barrier systems, operations and safety, and we had a couple of individuals in regulatory and licensing and performance assessment that were also involved.

As I said before, the duration of the effort was relatively short term. This effort was really planned to be a first cut at the effort, to try to help us identify where we need to go, and this is not going to be a one of a kind-type of process. This is going to have to be re-looked at as the thermal studies progress, as our modeling capability develops, and as the data starts coming in, because our ideas about what performance is and how we're to meet that performance are, in fact, going to change and mature, I'm convinced of that, as we proceed down this road. So this is really to be taken as a first cut.

We evaluated 15 goals, and I'll show you what those are. We documented the basis for each of the goals, and identified those that remained valid, and some of the uncertainties, and a draft report has been prepared. I believe Carl Di Bella has been given a copy of it.

Okay. Let's talk a little bit about the SCP thermal goals. The process, as you can see, one of the processes here was to limit the temperature changes in the selected barriers. These are primarily the natural barriers that were identified. Unfortunately, it doesn't come out very well. This is kind of a gray area, and it's in a different type to indicate changes that we have made to those goals. I think it comes out a little bit better in your presentation material.

The performance measure for this one was temperature, and the first goal was to limit the temperature to Calico Hills to less than 115°C. The basis for this goal, and, actually, the second one, was the concern that there would be mineralogical changes that would occur in these natural barriers which would degrade the ability to retard radionuclides.

There's also been a concern raised lately that the hydrological properties--and we've heard a little bit of that, and I think we'll hear a little bit more today--that the hydrological properties of these barriers can, indeed, be impacted if the temperatures change too radically. Zeolites can be producing water; as they dry out, give up their water.

So, these goals we looked at. In fact, we decided that they were still important goals, and we didn't change those at any time. We, however, did identify some additional tests that we thought should be done to help establish those.

We also--and you heard mentioned yesterday several times, the importance of the Paintbrush Tuff barrier. This has been identified as a critical natural barrier, and so we wanted to establish a goal that would help protect this. However, there was a great deal of discussion about what that goal should be, whether or not it should be to keep it below boiling, or what temperatures, should it be 115°C? We finally decided there wasn't enough technical basis at the time to really put a definitive goal in terms of temperature, so we at least identified as a basic goal that we wanted to protect this layer, and we identified some testing, which I'll talk about in a minute, to try to get at that.

The next several goals, this goal right here and this one were thermomechanical goals, which are primarily far-field type of goals, and the concern there was, again, for the natural barriers, that we not degrade the performance of those natural barriers.

The impact here on the surface environment, there was a goal to not have the surface temperature rise more than 6°C. As we looked back on this particular goal, we found out that there had been some original analysis that had indicated it should be 4°C, and that they had apparently inappropriately applied conservatism to it.

We then took some recent data by Kent Ostler--and I believe some of that was presented back at a meeting a couple of years ago. We've decided now that this goal should, in fact, be made more conservative, and we've come out with a less than 2°C at the surface as a recommendation.

The goal to provide the appropriate thermal loading, based on the strategy, was we re-wrote the process to make it more general in nature, so that it wouldn't be specific to boreholes.

This is a very general goal. It just says: "Design to whatever strategy you end up choosing."

The borehole wall temperature of 275°C was again very specific to the borehole, and it was a goal that was placed on there so that you would achieve the 350°C waste package temperature. However, what we determined was that, in fact, if we would meet the 200°C that we'll talk about in a minute, and the 350°C waste package, that this was a redundant goal, and since it was very specific to a particular emplacement mode, we recommended that it be deleted.

The borehole emplacement of 200°C was primarily established based on the fact that there was concern that the alpha-beta cristobalite phase change would occur, and this would cause stress in the rock. However, the one meter distance was somewhat arbitrary.

We re-looked at that, and there was some analysis that, in fact, was done that we used on this. There was some stress analysis that indicated that it was primarily the temperature gradient that was the important issue here. However, we did determine that, in fact, 200°C, if we kept the walls at one meter in either the borehole or the in-drift emplacement, that, in fact, we would not achieve significant stresses in the rock. So we felt this was still a good goal, and we have some analysis to back that up.

This last one was primarily just to ensure that the containers would survive, and we--

DR. CANTLON: Before you take them off--Cantlon, Board--the one meter, is that one meter depth?

DR. SATERLIE: One meter into the rock, yes; one meter.

DR. CANTLON: Okay.

DR. SATERLIE: Okay. There was a goal to limit the corrosiveness on the container, and it was felt that if you would keep the borehole walls above boiling temperature for greater than 300 years, that you could do this. Well, clearly, I think it's a number of things. It's keeping water away from the waste packages, it's possibly keeping the temperature of the waste package container higher than this, so we rephrased the goal a little bit to maximize the time that the waste package container stays above boiling, consistent with the thermal strategy that ends up being chosen.

There was concern for the degradation of the fuel matrix. This is still in many of our design studies, that we restrict the temperature of the waste package to 350°C. Some work is being done on that, and we're very interested in the results of that. There's Savannah and Hanford are all doing some work in that area. We're interested to see what the results of that are, so for the time being we kept the goal as is.

The same with the high-level waste glass temperature. Some work is being done at those organizations.

On this fuel cladding, we, in fact, do recommend some corrosion studies and some studies are being done there, and I'll identify those a little bit more. There are some additional studies that need to be done.

The access drift temperatures, the operations folks felt very strongly that this was an important goal so that they could operate in those tunnels, and there was one in the vertical borehole to keep the wall temperature to less than 50°C for the first 50 years. There was an additional one in the repository section for the horizontal borehole, and this actually, although it said wall temperatures in the emplacement drift, what they mean for horizontal boreholes is the emplacement drift is the drift that you drive into, and then there's offshoots. The horizontal boreholes come off that.

So, again, it's very similar to this goal. We probably should have combined them, but for the time being, we left them separate.

Finally, there was one other goal that had been identified as a goal in some other documentation, so we evaluated it. It was to keep the rock temperature midway between the drifts less than 100°C. However, as we looked back carefully through the SCP, we could not find that goal anywhere, nor could we find the rationale for such a goal. Therefore, at this time, we're recommending that it be dropped, and we'll further evaluate that based on the systems studies results.

Okay. Certain recommendations were made, and these are in more detail in the report, but, briefly--and, in many cases, these analyses or these tests have been identified before, but in some cases, the funding has not been available to do them, or we've had slow-backs, slowdowns because of the funding, so I think it's good to call them out.

We need to continue to investigate the dehydration and rehydration effects on the zeolites, and reversibility of such actions, and, actually, the enthalpy of those reactions we think is important, too.

We need to look at detailed hydrologic properties of not only the Topopah Spring member and the Calico Hills, but we need to now look at the Paintbrush Tuff member, and really try to establish the details of how those are going to perform as natural barriers.

We need to do a three-dimensional stress analysis. Now, this has been identified. I'm not sure that the funding is available to do it, but it's identified for next year, and we hope that we can do that. Once that is done, we need to incorporate that analysis back into the effort.

We need to do more corrosion tests on potential waste package materials over various temperature ranges. We need to look at the reactivity of the water, and we need to conduct some additional studies on the zircaloy cladding performance.

Let me summarize. This working group was put together. It evaluated the goals, and we've recommended some changes to those goals, recommended some testing and analysis, and what we'd like to see is that when this testing and analysis is done, and more data becomes available next year, that we revisit these goals and incorporate the results in this again. So I would suggest mid- to late '94 we look at these again, and that's basically the recommendation there, is that we continue to look at those.

Thank you. I'll entertain questions.

DR. VERINK: There will be time for a couple of questions.

John?

DR. CANTLON: Yes. In this process of reevaluating the base plan thermal strategy, was there any effort to look back into the total system and see what impacts the thermal strategy might have on the whole logistical flow of the waste systems storage, and that sort of thing?

DR. SATERLIE: For this particular two-month effort, no, we did not do that. However, that is one of the things that we plan to look at in this systems study, and we are going to be working with the system-wide issues in there, too, so that's something we're going to do.

DR. VERINK: Don Langmuir?

DR. LANGMUIR: A couple questions, Steve.

This is obviously--these are goals that clearly relate, or describe a perfect repository. If you could have everything working just the way you wanted it to, these are all the things you'd seek to have.

Two parts to this: Have you thought about how you could prioritize these if you were asked to do so; and have you thought about what the extended dry approach would do to any of them? Would it already violate some of those goals if we went extended dry?

DR. SATERLIE: Okay. In answer to the first question, no, we really didn't come to grips with how we would prioritize those. Each group had their own prioritization, obviously, that they felt were more important.

In response to the second question, these are goals, and as our knowledge matures, we may trade off some of these goals or change some of these goals because we find that we, in fact, can get better performance by doing certain things, and that's a little vague, I understand. What we want to ensure is that we meet the regulations and the requirements, and if we can do that by either changing or exceeding one of the goals, for example, then we will probably do that.

DR. DOMENICO: Domenico, Board.

A few of the goals deal with control of the temperature. Has there been any communication with the modeling people, doing temperature models to see whether or not and if, indeed, these goals can be achieved in either a perfect repository, or one that might be affected by a lot of the things we heard about yesterday?

DR. SATERLIE: Well, yes, we tried to do that because many of the people on the panels were, in fact, from the laboratories and were involved in those modeling activities, and so they were involved, yes.

DR. VERINK: I think that will be about all we have time for.

DR. SATERLIE: I don't know if there's a question back there.

MR. CODELL: I'm Richard Codell from the Nuclear Regulatory Commission.

I haven't seen anything on your recommendations for testing of the fuel itself at high temperature. It seems to me it would be especially important, considering the fact that uranium dioxide fuel has some bad properties at high temperatures in an oxidizing environment. Could you comment on that?

DR. SATERLIE: Well, yes. That's--and I didn't call it out very well in there, but there was some concern for that, and the question about that how that would, in fact--because the failure of the zircaloy cladding is primarily due to the oxidation of the fuel, and the fact that the UO_2 goes to U_3O_8, which then increases in size and puts stresses on the zircaloy cladding. So, you're right, that needs to be part of that whole study.

DR. VERINK: That was part of the assignment of the temperature, wasn't it; 350?

DR. SATERLIE: Yeah, so okay.

DR. VERINK: Well, thank you very much, Steve.

DR. SATERLIE: Thank you.

DR. VERINK: I guess our next one is Dan McCright. Dan, we will start you with no extra inroads on your time.

DR. McCRIGHT: Thank you very much, Ellis.

Someone once told me that in a technical presentation, it should be like a bride on her wedding day; in other words, you should have something old, something new, something borrowed, and something blue. I have all of those things, and most of the view graphs are going to be in blue.

I'm going to be talking about the corrosion aspects under various thermal scenarios, and first I'm going to talk about the thermal factors and how these broadly limit container materials performance, and then I'm going to go into the design considerations, particularly the configuration, because it's really hard to divorce the configuration from the thermal, and so then I'm going to talk a bit about the selection process we use to find candidates, and then as we winnow the candidates down to materials for advanced studies.

And then, I'm going to give two examples. One, again, was done in the recent past on the SCP conceptual design, and one is in the present, dealing with the overpacked multi-purpose canister, and then I'll summarize.

This slide is an attempt to try to put the response of the material into certain thermal and hydrological zones, and it's not easy to do because there aren't really hard lines. In fact, it's really hard to engineer this view graph and I really want to emphasize that these are not hard and fast lines. They really ought to be fuzzy, and they are just all kinds of caveats that come with them, but I just want to make something here that gives me something to talk from.

The rationale that went into trying to select where some of these lines should be, following from Steve's presentation, I'm just going to put approximately a 300°C upper temperature limit on the container surface, and all the temperatures I'm talking about are those at the container surface.

And the reason for this, well, first of all, is because of some of the thermal goals that he talked about; the contents of the waste package, and then outside the waste package. But then, from a metallurgical point of view, at temperatures much above 300°C, we can start to get some phased transformations and other kinds of metallurgical transformations in certain alloy systems. And, again, for instance, some of them were familiar ones, like the carbide formation in some of the austenitic materials, sigma phase, those are brittle phases, and so those would tend to make the packages less ductile, less tough with time.

Those are nucleation and growth processes that occur very readily at temperatures where metals are ordinarily processed, and they become slower and slower as temperature decreases.

Well, a lot of studies have been done, for instance, especially with the sensitization of stainless steel in light water reactor environments, and it seems to be that about approximately 300°C, at temperature, that below that temperature the sensitization doesn't occur over as long a time period as they can project.

So now we move, then, to another temperature, and I'm just going to say that it's 120°C to this bound, and the reason for that--and that's probably a high temperature--was that we talked a little bit yesterday that there were possibilities that there could be some localized pressurization around certain waste packages. Another factor that might add to essentially an increase in the boiling point of water would be if we have a concentration of solutes and the solutes raised the boiling point, so that sets that temperature right there.

60°C, again, a lot of studies have been done with different metallurgical systems. It's been found that, say, above 60° in aqueous environments, we start to get more serious stress corrosion, localized corrosion effects, and those are important, particularly for the corrosion-resistant materials, and a good example of that is chloride-induced stress corrosion cracking of stainless steels, which is one of the most technically limiting problems with those kinds of materials.

Well, it's usually been found that that occurs above 60°C, and below 60°C, it doesn't occur. Similarly, a lot of studies on pitting and crevice corrosion also point that those do have, also, critical temperatures above which they occur and below which they don't occur. And, again, I'd emphasize, these are studies that have been done in the months to few years, and coupled out with maybe a few decades of experience.

Then, 30° being the ambient temperature of the repository. The other line is the wet/dry line, and the rationale on that one--and I'll get to this a little bit later in the talk--was that observations on atmospheric corrosion of materials tends to indicate that above 70°, 70 per cent relative humidity, steel rusts; below that, it doesn't rust. So if one were to carry that kind of analysis or belief that there's some critical humidity that sets apart the oxidation phenomenon from the corrosion phenomenon, that lets us draw this horizontal line.

And speaking of the line above and below the water line, these are some pipes that were taken out of the USGS USW H-5 well. That well is at the periphery of the pork chop that Dave Bish showed in his talk yesterday. Anyway, that well was used for the USGS to monitor the water table levels, and, again, I'm sorry that Polaroid photography just doesn't do justice to what you really ought to see here, but some of these pipe strings--and these strings are each about 30 feet long--were in the unsaturated zone, and some were in the saturated zone, and this particular one is the one that transversed the unsaturated to the saturated zone.

What we've done is to go to the field, and we've sampled some of these tubes, taken sections out of them and brought them back to Livermore, and what we want to do is to characterize the nature of the corrosion products, and then to estimate what the depth of penetration was, and particularly for those that were below the water table, is the pattern of the track, whether it was localized or general.

I might add that the ones above the water table in the unsaturated zone looked very, very--almost new. You can still see the stencil marks that were on the pipe, the original surface.

The configuration of that particular well is as shown, and they had two strings; a large and a small diameter, and they traversed the unsaturated into the saturated zone, and at that particular location, the water table was 705 meters below the surface. The well was cased to 790 meters.

The water composition down here was very similar to that of the J-13 well water, which has been used so much in the past for different corrosion studies and other studies in the Yucca Mountain Project. It was at 36°C, neutral pH, low ionic strength. And, again, this was a ten-year exposure, so from our point of view, that's very interesting to have information for that long a period of time.

Now, let me switch just a little bit into some of the configurational issues, and in the advanced conceptual design part of the project, these are some of the issues that we plan to examine. There's to be a single metal barrier versus the multiple barrier; corrosion allowance versus corrosion-resistant, in other words, kind of a thick versus a thin approach; radiation shielded versus non-shielded, again, thick versus thin.

By radiation shielded, I'm mostly emphasizing the thickness that attenuates, again, in the field so that the radiolysis effects are minimal in the environment. An all metal package versus a metal/ceramic waste package; and then, packing materials placed around the waste package versus an air gap, and I'll come back to that one a little bit later.

Again, the borrowed slide here. This is from Bob Fish at the M&O, and this is to show you what goes on in the selection process, and this is a multi-stage and an iterative process. We start with the component function requirements, what we would like the component to do, and then to define the range of environmental variables, and these, of course, look easy on the block, but this is a quite large range.

Then we establish the selection criteria, how we want to weigh the different factors that would go into the selection process; identify candidates; and then collect relevant information. This is primarily done in what we call the degradation mode surveys, where we've compiled lots of information on different candidate materials of interest in environments that approach that of Yucca Mountain, and in those that might be, in a way you could extrapolate the results, there would be some applicability to Yucca Mountain. They might be kind of extreme chemical cases, but there's a possibility that that might be a factor here, and then we apply that information to each material.

And then this process goes around, because we start with a large candidate list, and then winnow that down to a smaller candidate list. All along at different stages we have project review, technical review, and peer review as appropriate, and the one that I'm going to talk about in just a minute or so is the one that we've talked a bit to the Board before, is where we get here an outside peer review that looked at the selection criteria that went into the selection for materials for thin-walled conceptual design.

Again, this is the old slide. Just to review for you what the conceptual design was, there was a single metal barrier, approximately 1 to 3 cm thick. It surrounded either the glass waste form, the stainless steel pore canister, or assemblies of spent fuel. It was thin. It wasn't radiation shielded. It was planned to be vertically emplaced in boreholes, no packing materials surrounding, and the approximate peak temperatures were about 220 for the spent fuel, 140 for the glass packages.

We went through the selection criteria, and to summarize those, we had approximately a 70/30 split, between 70 points given for performance considerations, heavy, of course, on the corrosion, but also mechanical properties, compatibility with other components in the waste package, and then 30 points for engineering considerations, the fabricability, the weldability, how to close it, material, the cost of the material, and then the experience base we had with each particular material.

We went through the process, and we looked at about 40 different alloys that represented a broad range of engineering families. In fact, all the important alloy families were represented, and the materials we recommended for more studies were nickel-rich Alloy 825, sometimes called Incoloy 825. That's about 40 per cent nickel and has a lot of chromium, some copper, some titanium to impart more corrosion resistance.

Nickel-base Alloy C-4, also called Hastelloy C-4. It's a high nickel alloy, about 60 percent nickel, has a great deal of chromium and molybdenum in there to give you very high resistance to localized corrosion.

And Titanium Grade 12, which is a dilute alloy that contains small amounts of nickel and molybdenum, and it's also up to now the most resistant of the titanium-based materials.

Now, switching a little bit to the multi-purpose--

well, in this talk it's called a multi-barrier design that's applicable to the multi-purpose canister proposal. It's also applicable to extended dry, and in this configuration, we would use, perhaps, an Alloy 825 as an inner barrier. That would be our corrosion-resistant material, and a carbon steel outer barrier. The principle here is that the outer barrier would slowly oxidize and corrode to protect the inner barrier. And, again, this is essentially the principle of cathodic protection, that the outer barrier is protecting the inner barrier. This is commonly done with lots of varied pipelines in the soil and in water, and perhaps more familiar and metaphorically for this audience is your trash can. If it's a metal trash can, it's steel and it's been dipped in zinc, and as long as the zinc coating's there, the zinc protects the steel.

Now, we have been examining the degradation of carbon steels, and other iron-based materials, such as cast irons and low alloy steels, and the oxidation of steel at the temperatures of interest to us is a very, very slow process, and again, we arrive at this mostly from extrapolations from the higher temperatures, higher temperatures being above 500°C, and extrapolating that down to the temperatures of interest; also allowing for the fact that at the higher temperatures, the rate of oxidation is governed primarily by the diffusion of oxygen in a Fe_3O_4 layer. Fe_3O_4 is the major oxidation product, and it may well be that at the lower temperatures, a process that would have less activation energy might dominate, or the surface diffusion or diffusion around defects in the metal oxide.

But even allowing for a lower activation process, we still project very low rates of oxidation. So, therefore--

DR. PRICE: Excuse me. Can I interrupt? Dennis Price, Board.

Our handouts show millimeters per year and yours is micro--

DR. McCRIGHT: This is the correct one, the one in the--thank you. So the analysis here is that the 10 centimeter or so thick overpack would endure for well over 10,000 years.

Now, the corrosion of carbon steel--and this is why this wet/dry line is so important to us, is much more rapid, and, again, these are actually measured rates because this is of great technological interest, this is generally not of technological interest, and we decided it would take a long time to try to generate the data at those temperatures.

DR. VERINK: Is that also micrometers, or is that millimeters?

DR. McCRIGHT: This is in millimeters here, and I want to emphasize the difference in the magnitudes here, and that's why the underlines.

And, also, if one were to add to that that in many different kinds of waters, not only do you get a general corrosion of carbon steel, but you get some localization, and one way of trying to quantify that is to take the depth of penetration at the deepest point, compare that to the general corrosion penetration, and then taking that ratio and describing that as a factor. And so, again, two to four has been the kinds of numbers that have been measured, and we've done some work some years ago in J-13 water, and also a lot of work has been done in other kinds of comparable waters.

And in this case, again, if we maximized, took the higher rates and the higher localization factor, the overpack could be penetrated in just several decades. Again, more realistically, they'll probably last a few hundred to several hundred years, again, assuming it was all wet, because many corrosion processes slow down with time, and, again, many of these factors would tend to be mitigated with time. But this was an attempt to do a kind of a mounting calculation on that.

Again, as we said, that the transition from wet to dry is very important to us. Let me just put this other view graph back on, so you keep it in mind, that, for instance, the steel overpack would start here, and then perhaps as temperature decreased and there was the possibility of water entry, we would go from this area down to here, and then down to this region.

Again, it's been observed in the atmospheric corrosion of metals, that at low humidities, we have a very low rate, and then as the humidity increases, we have a short transition as we go from the so-called dry condition to the wet condition, and this has been assumed that there are water films that become significant at this point, and that those water films are able to sustain the electrochemical reactions that are needed for corrosion. Corrosion is governed by having local anodes, local cathodes, and currents flowing back and forth between the two.

Again, at ambient conditions for metals like steel, this has been observed to occur at about 70 per cent relative humidity. Now, the difficulty in taking atmospheric corrosion data, of course, is that although it's good from the point of view that it's usually been done for several years, is that on a given specimen, you've got diurnal variation of temperatures, seasonal variation. Places where they may be located would be subjected to wind changes, and the winds could bring in a number of different contaminants.

So, in a larger sense, the factors that had to be considered would be the humidity for each temperature, and then what we don't really know is if that's--if the 70 per cent holds for other metals; copper, zinc, any other metals that might be under consideration.

Also, the surface conditions seem to play an important role, because if you have surface contaminants, those would, in many cases, tend to want to attract water, and those would then probably lead to the start of the corrosion process on the material surface.

What we plan to do in the laboratory, we're in the process of acquiring a thermogravimetric analysis system, and this is a very sensitive microbalance, if you will, that can measure small changes in the weight or mass of a specimen, and what we will do is to flow air through there, and we'll have one stream that's very dry air, and then the other stream of air that's been saturated with water, and then mix those two to establish different levels of humidity, and then we'll do this over a range of temperatures, and then we'll try to get those curves that I just showed to find out what the actual shapes of those are, and where the transition points are from wet to dry for steel and for other metals.

Again, the corrosion of carbon steel shows a maximum. It's not just a simple increase in the corrosion rate with temperature. Again, in neutral pH solutions, the important factor is the oxygen content of the water, and how the oxygen is transported to the metal surface, and at temperatures in this region, we have increased kinetics because the increased temperature influences, or it encourages the diffusion reaction, and so the corrosion rate increases as the diffusion of oxygen to the surface increases.

But, the oxygen solubility falls off with temperature, and so above this maximum, the solubility decrease is more important, and so then we get a decrease in the corrosion rate at the higher temperatures. So it's been observed at about 80°C that this is where the maximum occurs. This is work that we did at Livermore about 1984 or '85 on 1020 carbon steel and J-13 water, but it's also been observed in many other kinds of domestic waters and waters that would be similar in concentration to J-13 water.

Now, what this means for the configuration of the multi-purpose canister inside the steel overpack, is that the corrosion potential of carbon steel in J-13 water is at a relatively negative potential. The corrosion potential of Alloy-825 is at a higher potential. So we have the cathodic protection that is observed. This is a wide separation.

Sometimes you have to be careful with galvanic coupling, and a good case, again, is going back to the zinc/iron couple. At low temperatures, zinc is anodic to iron, it protects the steel, but at higher temperatures, the couple works the other way, and that is probably due to some of the retrograde solubility of some of the zinc corrosion products that set the seal up the other way, so that then zinc becomes the cathode and seals the anode, and this has led to some catastrophic failures of galvanized pipe at higher temperatures, temperatures being above 60°C in certain kinds of water. It doesn't occur in all kinds of water.

But it appears over the range of temperature that we measured, that the 825 will always be cathodic to the carbon steel.

Also, even more important is that the carbon steel would also protect the Alloy-825, and, in general, any corrosion-resistant material from localized corrosion effects, and, again, localized corrosion effects are again governed by potential, and they are observed at even higher potentials, so that there's even quite a very large range of protection offered.

The Board asked me to talk just a little bit about the corrosion-related aspects of packing materials, and particularly tailored packing materials that would be designed to protect the metal barrier, and these might work by a redox buffer, just the same principle as I just showed in that slide where I had the 825 and the carbon steel, that, in other words, you bring the potential of the couple down below the corrosion potential of the material you're trying to protect. They could also serve as pH buffers. Most metals perform very, very well under moderately alkaline conditions. It could also act as a diffusion barrier to keep oxygen or other corrodants away.

The real issues that we would have to discuss would be, first of all, are there any undesirable thermal effects? Because the packing materials normally don't have good conductivity, and so that the temperature of the surface of the waste package would be maintained at a higher rate. That might, again, hurt some of the contents that are inside.

And then, will the backfill itself undergo chemical changes, that, when it's needed, will it really be in the form that we want? For instance, it's been suggested that the lower oxides of iron, like Fe_3O_4, could protect, let's say, a copper container, because copper is relatively noble metal. It's possible if you could bring the potential of the copper just down a little bit, it would coexist with water and be immune to any kind of corrosion attack.

Then, will the backfill function as intended in the unsaturated environment? In other words, could it have a detrimental effect of perhaps attracting water when we want to avoid such a situation?

Another subject I was asked to mention was the cost aspects of thermal loading; and, again, the material related to factors that go into the cost of the waste package would obviously be the raw material costs, and then as we fabricate the material and any weld processes or any other kind of special processes, any quality control, quality assurance factors would add to that, so that the cost of the asfabricated package in, say, $/cm^3, then the dimensions of the waste package, multiply this by this, and then the number of waste packages.

This is, of course, the kind of statement that could engender a whole range of conversation and dialogue, but it may be more economical to make fewer, but more robust packages.

In summary, I'd like to say that the thermal strategies certainly have materials implication. They impact on the selection and the performance, as we've seen in this diagram of the zones, and there are many tradeoffs that are possible, between the material selection and the performance expectations, depending on the design and the thermal strategy that are selected.

It appears that the extended dry approach would have fewer materials performance considerations because we would try to confine ourselves to be mostly above the line, but the transition from dry oxidation to wet corrosion is a very key performance issue, just where this line is, and we plan to do some experimental work, as I talked about. We're also doing the characterization work with the samples that we got from the USGS well, and then I might add to this, we're going to do some field work in conjunction with the large block test, and Dale Wilder will be talking about that later on today.

DR. VERINK: Dan, just a question.

DR. McCRIGHT: Sure.

DR. VERINK: I gather from what you've said that your spectrum of choices that you're going to be looking at is broader than just 825 steel and titanium; is that right?

DR. McCRIGHT: That's right. That's correct, Ellis. The principle is, again, you have a corrosion allowance material, sacrificial material, if you will, and a corrosion resistant material on the inside.

DR. VERINK: Right.

DR. LANGMUIR: Dan, Langmuir; Board.

Several things that came to mind. I wondered if you had also looked at the radiation shielding ability of these materials? That was one question.

DR. McCRIGHT: Yes, we have. Again, the fellow that's most cognizant or familiar with that isn't here today, but, yes, it's pretty well known about the radiation shielding is proportional to the density and to the atomic number of the material, so very dense, very high Z materials are the best as far as a given thickness that will shield.

DR. LANGMUIR: But among the three metals or alloys you described, there's no major difference among those?

DR. McCRIGHT: You mean among the conceptual design?

DR. LANGMUIR: The two nickel alloys, and the titanium.

DR. McCRIGHT: Well, see, titanium would be because it's lower density than the--and so it would have a little bit less, but we were always talking of thin materials, anyway, so probably the net effect is not that great.

DR. LANGMUIR: Okay. You also showed a dry oxidation rate of about .3 millimeters per year at 100° in water. What kind of rates do you see? I know they're finite, apparently, also in steam, and have you taken those, our knowledge of those rates and projected what that might do within a thousand years, perhaps, to those materials?

DR. McCRIGHT: You mean as far as the amount of degradation or losses?

DR. LANGMUIR: Right; the amount of degradation or loss.

DR. McCRIGHT: Yes, we've done that. Again, assuming it would be continuously in that condition, that .3 mm/yr is a pretty high corrosion rate or oxidation rate, and so it would translate to something would only last maybe a few hundred years.

DR. LANGMUIR: Have you thought about the steam conditions under extended dry versus those materials?

DR. McCRIGHT: Well, in that case, if it's, again, where the saturation is so important, really, of whether we're at the saturation condition, or we're much less than that.

DR. LANGMUIR: Well, if it's a liquid vapor curve, even if it's for above 100°, you're still going to get significant rates of corrosion?

DR. McCRIGHT: No. Above 100°C, my understanding is that our--first of all, we're not pressurized, so I don't think we would be--

DR. LANGMUIR: Okay, you can't get above 100?

DR. McCRIGHT: We can't really get to saturation, so my understanding is that the rates ought to be very, very small above 100°C. They would be in the hundredths of a micrometer per year.

DR. LANGMUIR: I had one last thing. It would seem to me that it wouldn't make any difference what you put in a backfill in terms of trying to buffer to control the corrosion rates if you were under oxidizing conditions, which you're liable to be, because then it's the atmosphere that will control the redox condition. So whether you put iron or copper or anything else down, they won't make any difference in terms of improving the performance of the metals in the waste package. As long as air is present, that's going to define it; right?

DR. McCRIGHT: Well, the thought there, though, was that the, particularly if you've got immersed conditions--

DR. LANGMUIR: Yes.

DR. McCRIGHT: --because it's a little bit harder to talk about corrosion potentials, and so forth, when you don't really have water there as a medium. So, again, that's, again, dealing with something that would come way back when we got down to one of these areas.

DR. LANGMUIR: Thank you.

DR. VERINK: There's time for one very quick one. Carl Di Bella?

DR. DI BELLA: I hope it's--it'll be a quick question, anyway. Carl Di Bella, Board staff.

I'd like to hear your comments from a corrosion point of view on a multi-barrier container that had copper as the inner container, copper-based materials, the inner container material, rather than 825.

DR. McCRIGHT: The same principles that I've talked about will still apply. The carbon steel would protect the copper from general corrosion, would protect it against localized corrosion, and, again, my understanding is that I don't think there would be any possibility of reversing those potentials over any reasonable conditions.

The one thing, again, with copper, you'd have to be careful with--and I'm really stretching a lot of things--is a great change in the chemical environment, because copper complexes so readily and with so many different--if you had some organic ion there present that could change that coupling; maybe more so than the nickel-based materials.

DR. VERINK: Carl?

MR. GERTZ: Yeah. I just had one comment. Yesterday we spent a lot of time talking about natural analogues. It appears pulling that metal out of the USGS hole gave us an opportunity to look at an engineering analogue, at least for a borehole emplacement of carbon steel in contact with the existing rock, and you are looking at those, I assume?

DR. McCRIGHT: That's right; you bet.

DR. VERINK: Okay. Well, I think we're going to have to move along now, and our next speaker is, as advertised, Tom Doering.

MR. DOERING: Good morning. Again, as noted, I'm Tom Doering with B&W Fuel Company. We're going to talk about the compatibility of multi-purpose canisters and multi-purpose units with the thermal scenarios of the repository.

Just a brief outline. What I'd like to do is look at the impacts of the multi-purpose canister and the multi-purpose unit with respect to the repository, and then we're going to reverse it and look at the impacts upon the multi-purpose unit and the multi-purpose canister. Then I'm going to spend some time with the different weights of each device, material selection and their impacts, and then move into the thermal impacts directly, with some more detail of that area.

What I'd like to do now is spend some time, a few minutes, and go over the different design concepts. These three devices up here are all in the configuration as they would go into the repository itself; the multi-purpose canister, the multi-purpose unit, and the multi-barrier waste package.

The differences are the multi-purpose canister, inside the blue line, is the device that is loaded at the utilities in the spent fuel pool area. It is sealed. That is why there is a shield plug up here, to provide, then, the utilities some area or reduce the radiation from streaming out of the top, and then the outer shell. Now, that is, the utility is on the dry storage and has its own unique overpack for that.

In transportation, it is again overpacked again, and you need to overpack for transportation. And then for the repository, it is once again uniquely overpacked each time, so the internal, what we call the basket and the outer shell is the unit that goes throughout the whole system from start to finish, with unique overpacks designed specifically to meet the requirements of 10 CFR 71, 72, or 10 CFR 60.

The multi-purpose unit is a device that is designed initially, right away, to meet all requirements. It has a larger shield area to meet 10 CFR 71 requirements, and the basket material, and also the outer shell of the basket are designed to meet the most restrictive requirements of all three areas.

One of the areas I will talk in more detail on is why the difference in thicknesses. A quick brief on that is, it's the shielding requirements that require that to be a little bit different, since it must carry its shielding from start to finish. This is a device where the spent fuel goes into at the utilities and never comes back out. There is no inner sleeve that is transported. It is transported as a unit.

Again, we have a shield plug up here to allow the utility to do some hands-on, non-remote closure of the internal package and the external package. The multi-purpose unit, or multi-purpose canister compared to the multi-purpose or multi-barriered waste package is slightly different, since it is now designed specifically for the repository. There is no shield plug, as you can see, since all the handling of the fuel would be done remotely, and, therefore, there is no hands-on, nobody essentially climbing on top of the waste package and sealing it. So everything's done remotely, so the added weight to the shield plug is not required.

Very similar to these other devices, especially the multi-purpose canister, is that the inner barrier would have performance, and then it would be overpacked with an outer barrier similar to what Dan has talked about and the materials that we're reviewing.

The basket designs are slightly different, also, since these two basket designs must meet the transportation requirements, and this basket design only has to meet the repository requirements. That's a quick overview on that.

I'm going to leave that over here to give some bases of comparison.

The limitations on the multi-purpose canister and multi-purpose unit, they're actually derived from 10 CFR 60, and the performance under thermal loads, they must meet that throughout its time, and then the substantial complete containment, from 300 to 1,000 years, and then we have up to 10,000 years of release that sort of goes into the engineered barrier system. And so those are the two regulatory requirements that they have to meet, and this is only for the thermal area. Again, this is what we're speaking to today.

There are other requirements for the structural and other areas that we do have to meet, but this is specifically to the thermal areas, and then, as Steve has referred to, we have two thermal goals. The first one is the 350°C for the cladding, creep rupture, and we have the 200°C, one meter into the rock to maintain the overall stress concentration inside the repository.

Again, the 350°C is a time and temperature requirement, so depending on how long the cladding and the temperature with a certain strain rate, you get different failure rates, and we are evaluating that right now and we'll see what those evaluations should show.

Multi-purpose unit and canister impacts on the repository. It helped us out. Essentially, it allows us to handle fewer just spent nuclear fuel assemblies individually. It provides, in fact, a very clean outer surface that we can manipulate throughout the system without contaminating the system, and therefore, reducing the overall costs and maybe time inside the surface facility, and then underground. So that is a very beneficial area.

It does limit the emplacement modes right now, we feel. Looking at it from the first blush, and looking at it a little bit more with the system evaluations, the multi-purpose unit and multi-purpose canister, to be efficient for the utilities, tend to be larger, so borehole emplacement is questionable for this due to the weight that we have, putting something in that's 70 inches in diameter for the larger one, or closer to 50 inches in diameter for the small, going inside of a borehole that weighs quite a bit, as I'll show later, is going to be quite a challenge.

The multi-purpose unit and multi-purpose canister are not specifically designed for the 10 CFR 60 alone. They are designed for 10 CFR 71 and 10 CFR 72. Now, with the multi-purpose unit or our multi-purpose canister task force that's going on, we've put together a matrix looking at all the different requirements of all the three different requirements in the 10 CFRs and tried to map them out and see which ones are the most restrictive, and have looked at them and seen where the more rational design areas are. So we are taking a look at them over global, but they are more restrictive, such that now we're designing something that transcends from start all the way to finish.

So, essentially for the basket design specifically, 10 CFR 71 has a requirement of having a drop test of nine meters, and that is only seen in 10 CFR 71. 10 CFR 60, or 10 CFR 72 do not have those requirements, and there are different thermal goals for each one of them, also, which require either transportation or storage that might be more restrictive in the way we're going to design, and I can show you a little bit later on that.

And 10 CFR 71 doesn't require stricter shielding requirements than all three of them, and since the multi-purpose unit must carry its shielding from very beginning to very end, it will be essentially the heaviest because it has to carry it from start to finish, and the overall amount of spent fuel inside of it might be restricted due to the fact of the weight.

With the multi-purpose canister, it holds the same number of pressurized water reactors, of boiling water reactors as the multi-barrier waste package, so it's very similar to that. We're looking at different numbers from all the way up to 21 pressurized water reactors, 40 boiling water reactors, and simply overpacking that and sending it down to the repository, so that would be very compatible with that.

The multi-purpose unit, due to the different weight limits, not from the repository, but from other areas, there might be some limitations on the number of assemblies going into that. It could be as few as 12, or even 9, depending on the overall weight and the design of the packages, and both of the thermal outputs are very similar to the design that we're looking at right now. We're looking at a suite of waste package designs and both the multi-purpose unit and the multi-purpose canister fit right in there from the small to the large, so there's really not that much difference in the thermal output of them.

As mentioned earlier, all these larger designs do lend themselves nicely to a drift-emplaced and to the borehole-emplaced, and I think we've seen this slide before, but just reiterating, this would be the package. It could be a multi-purpose unit, multi-purpose canister overpacked for the repository, or it could be simply the multi-barrier waste package, and it would be inside the drift, allowing the package, now with a higher thermal load, to radiate to a larger area, and then spaced out. The spacing is not intended to be anything indicative to the design that we've been looking at. We're looking at different spacings and different drift spacings. This is only to provide an overall view of it.

As noted earlier, there would be some differences in weights just naturally occurring with the design, and I think your slide might have these two reversed, so you might want to note that.

What we see here is the multi-barrier waste package, the multi-purpose canister with a disposal overpack that is similar to the multi-barrier waste package simply for corrosion and long-life containment, and then we do have one that we say it meets 10 CFR, or it allows workers to be inside the repository longer periods of time, and then we have a multi-purpose unit that carries its shielding throughout the whole time, and what we're showing here is the different CFRs that they would have to meet.

This is meant to be a trend. These are not final numbers. The design is in process, is in work. The intent is to show the trend of valuation here due to the fact of different designs, the design of the internal basket, and the overpacking now gets larger each time. We gain some weight due to that. The internal basket in this design is simply designed for containment and for criticality, not for high loads as would have to be done for the multi-purpose canister and the multi-purpose unit. And, again, the multi-purpose unit right now must carry its shielding from start to finish, and that shielding must have performance. It must show performance, and the material must be compatible with the repository, which is one of the keys right here.

There are a lot of materials that are used right now for transportation, for storage that are really incompatible with a repository, as Dan has gone into. Most of them are using concretes, a great deal of polymers are used, and that really provides us more restrictive performance in a repository. Polymers have a great tendency of breaking down earlier.

There is actually some data out there from a spent fuel pool area, some low-rated polymers are breaking down very quickly, and, also, there is an ASTM specification for a corrosion test where you actually put a polymer on top of the steel that you're interest in, and put a water film on it, and that becomes the accelerated ASTM's corrosion test. So, with the repository, we are limiting the materials that we tend to look at.

With that, this is the MPC selection at this time. There is a great deal of effort going on with the multi-purpose canister in the material selection area, but what we provide are different materials, a primary and an alternate. Again, this goes right back into Dan's conversation, where we're looking at different materials in the studies we've done at Lawrence Livermore and sifting through it. We do plan to take a look at these materials in the thicker bases, again, bringing in the carbon steels and the other materials for multi-barrier.

But right now, what we suggested is using Alloy-825 for the outer shell. That way, we believe we can take credit for that material and essentially provide some containment for it.

The shield plug is really just a carry-along to provide the utilities and maybe the MRS some shielding, so some more hands-on manipulation of the packages can be done. So we felt an iron-base material there is just fine, and won't really affect corrosion or the overall performance of the package since it's in an isolated area, and it's simply there to do its own function.

The only restriction that we would have is that if you do weld it to any other material, we would like to have it sheathed so we have a similar material welds, and so that's why we have the sheathing over there, and if it's simply emplaced or held in with other mechanisms, angle or something like that, so it doesn't have a metallurgical bond to it, then we feel it could just be a simple iron base. We are looking at different--right now, since this slide, we are looking at depleted uranium to supplement that, so that's one additive.

The basket for the repository must show performance. It has criticality material in it, especially for the burnup credit design, such that we believe that the stainless steel, 316L or the Alloy 825 is the correct alloy for that to show performance to hold everything together in the configuration that we need for a thousand years and beyond.

Again, for the criticality material, we're looking at boron within the stainless steel matrix, or boron within the aluminum matrix. Both materials have been shown to be able to produce that, and they both go into good solution, so we're pleased with both of them so we're evaluating both of them.

Now, filler materials internally to the package is still something that's under review, and it's something that we believe, most likely, if we do implement it, would logically be done at the repository, not at the utilities or anything, but depending on the weight limitations and depending on what the filler material purposes are, from our understanding, there are two basic purposes for filler materials, and that would be a buffering device, essentially allowing this material inside the package to provide--to like be a sponge, or to actually buffer the material inside, so in case there's water or any kind of moisture or any kind of egress or ingress of water, that it simply slows it down, or it could be for thermal properties. If we find that we have a very highly-enriched or low-burn material, high-burn material inside, it could provide some thermal enhancements internally to the package to remove the heat.

The fill gas, what we're looking for here is a material that is, or a gas that is really not detrimental to the overall system. Argon is a very well-used or often-used gas at the utilities. Helium works just fine for us, and so we're not that sensitive to that one.

DR. VERINK: Tom, I assume from your quick answer, that there are other materials besides 825, for example, that are under consideration?

MR. DOERING: At this point in time, Alloy 825 for the outer shell of the multi-purpose canister is our preferred material.

DR. VERINK: I thought I understood the response that Dr. Di Bella got from his question to Dan, was that copper, among other things, is under consideration. Is that right?

MR. DOERING: For the first go around for the multi-purpose canister, copper has not been put up as an alternative material, but through the next year we may be bringing that in or something, if that would be requested for us to do. This is, again, compatible or consistent with the multi-purpose canister design activity going on right now.

Moving to thermal, and going a little bit into that now, what is really affecting the thermal behavior of the devices? I call the now devices, because all three of them will have similar thermal behaviors; just how much spent nuclear fuel is inside, how much heat is coming out.

The waste package is really dominated by what's the areal mass loading, what is the thermal pulse of the repository. In fact, the way the thermal evaluations are done is that we take the repository thermal load, go into the rock two or three meters, reverse that, use that as our boundary condition, and then we move straight into the package to take a look at, and see what the thermal response to the package is. So it is truly an animal of the repository, thermal loading.

The drift size, especially with drift emplacement how large is the radiation area that we can go to? There is some evaluation going on with the backfill, when should we backfill, at what time and what material we should choose. There is a great deal of effort going on right now to define what kind of thermal conductivities those materials have, so I won't show those areas because there's still too much fluctuation in the backfill area.

Canister size and drift. How large is the canister? How much fuel does it contain? How much surface area does it have to radiate to it? And then, what's the drift spacing? They all work together. The package spacing: when do the thermal pulses between the two packages see each other? When does the thermal pulse seep between one drift and the next drift? How do they interact? That's all important how that is.

The decay heat of spent nuclear fuel we haven't mentioned earlier; how old is the fuel, how much burnup it has on it. So all those are variables on that.

We have materials of fabrication. There are some materials that are very good at removing heat, have a good thermal conductivity, and there's other materials that have less optimum thermal conductivities. All that plays an effect on how the waste package sees its thermal pulse.

And then the design type. There are two design types. There is the design type that we call the flux trap design, where it does not take credit for burnup, such that we have, essentially, insulation space around each fuel assembly to essentially trap the neutrons in there, not allowing them to interact with another assembly. Therefore, you remove the criticality requirement.

Or we can take credit for the burnup and say that we do have so much burnup, so much energy has been removed from the waste package, and then simply allow that to be closer together, we pack them tighter inside there, thereby having better thermal conductivity, and also, we might be able to put some poison material in that.

Now, we've done a number of calculations in this area. I'm just going to show you one of them that we have available. The color one didn't come out that well in Xeroxing it, but what we're trying to show you here is that we have a lot of in-house capabilities to do these evaluations.

This is a 21 pressurized water reactor. It is a multi-barrier waste package design which would be similar to a multi-purpose canister with an Alloy 825 outer shell. If we go to a different design where the canister does not have an outer shell that's compatible with the repository, we would have to overpack that and we would see another material on here.

But what we see here is the outer shell at a relatively low temperature, and this is the hot point of it, and for a larger package, that's 320°C, which is below the 350° goal, and this is for a 33GWd/MTU burnup with, essentially 40 years old, but it's been in a repository for ten years, so we loaded it in when it was 30 years old, so that's sort of the last year's average burnup. This year's average burnup is a little different.

On your left side is a very interesting photo. It essentially shows the heat flux. It's not a temperature gradient, it's the heat flux. How does the heat come out of the waste package? And what we're seeing here is an interesting thing when we first saw that not all the heat goes out toward the outside first. Some of the heat has to come to this internal grid work, which is the basket, and then flow out to the outer shell to be rejected. These are similar to heat fins, and so what we're looking at here in this red area is that the flux through this one area is very heavy, and we would want to evaluate that more in detail, and maybe provide a different material there or provide a material throughout the structure that can carry that heat out very quickly.

The removal of the heat is the important thing in the waste package, removing the heat quickly so we do not get a pulse through here. If we choose materials with very poor thermal conductivity, the heat is actually held in here much longer, then maximum temperature comes up to significantly higher temperatures, then, if we would choose a good material, and if the basket design does not have a good thermal path outside.

We have actually done this evaluation with a flux trap design, and this temperature goes up to 420°. There is simply a 100° increment.

Okay. With that, we'd like to go a little bit into thermal loading, and we'd like to show you some differences in 12 and 25. This is a 12 pressurized water reactor design, and we're showing it at different levels of kW/acre.

What we see here is when we held the waste package spacing constant--we have to hold something constant, so we've held the waste package constant--we've essentially moved the drifts out. We essentially moved the drift out in space, and provided the same areal mass loading, or the kW/acre, and, therefore, you can see the difference. The come up similar in temperature, and they diverge later on, not only taken out to 50 years here, to show--because earlier today, I think, and yesterday, we've seen some long-term performance; again, we have slides on that, we have data on that, or not data, but results on that, that show the different temperature regions. And similarly, with the 21 pressurized water reactor and a 25-foot drift.

They trend similarly. The temperatures shift higher, and we are above the 100° for a longer period of time with the larger packages. Even at the 35 kW/acre, we're showing that we are hovering at 100° for some length of time over 50 years.

This is a curve that we put together to show the difference in maybe drift sizes. This width, with this average age of fuel, the 42 gW burnup of 22-year-old fuel, this is the behavior of it. If we would choose a different burnup of fuel and different heat output, these things would, of course, separate a little bit more and be different, but the overall form would be the same.

What we're seeing here between a 14-foot drift and 25-foot drift, for a 21 pressurized water reactor--and this is 114 kW/acre, or 100 MTUs, we're seeing about a 25°C difference in temperature both on the drift wall and in the internal temperature.

Now, if we use a younger fuel or higher burnup, this delta would increase, and we have that data here. We just, due to the time constraints, we didn't show it, so we do have trending evaluation on that.

Now, the thermal impacts are very important to us. The thermal response of this system, of the canister is, again, dependent on the repository, and depending how we load the repository, the waste package will add to it. What we're seeing, also, is the areal mass loading. It's truly the key of performance that we're looking at here. The kilowatts per acre is only an instantaneous view of it, so what we can do is, for the long term, the areal mass loading provides us a real view of how the heat is input into the repository, where does the integrated heat underneath occur.

The first 50 years or the first 100 years of the heat, if it would start at that outside, really, for the overall thermal input to the repository is very minimal. It's the 10,000-year heat input into the repository that's critical, and that's what we're trying to bring out, so aging for the thermal behavior of the repository is not as critical as thermally-managing the early years of the repository; infilling or doing some other activity such that you space the packages out if they have a lot of burn.

Now, if you do age the fuel, what we can do then is put the fuel closer together, thereby having an overall higher heat to the repository, because now we've removed the first pulse through it.

Now, one of the questions that keeps coming up is: What happens if we design something that's not compatible with the waste packages that we're designing in the repository? Well, the point of decision, when we have the thermal decision coming down, we've looked into it. Out of the 25,000, or 11-25,000 packages, we might be only having to reload or do something with 150 multi-purpose canisters or multi-purpose units, so, overall in the whole scheme of things, that's really a very minimum kind of activity, and so the multi-purpose unit and multi-purpose canister really do not impact the full system as much as we anticipated.

Now, the conclusion that we have, simply, the multi-purpose canister and multi-purpose unit, due to the thermal output, due to the amount of spent nuclear fuel inside of it, lend themselves more to the hot or extended hot slide or region, as what Steve referred to yesterday. They just simply have more thermal capacity to it, so they are more shifted that direction.

Spacing the packages out would only have an effect on the repository in a global sense, but in the near field, we'd still have the same thermal poles and you would still go through the same scenario of heating and cooling and wetting as if you would have a larger or a higher heat output.

Handling the devices would simply--you have to increase the weight. We're looking at greater weight, as we noted earlier, of the packages, and, again, the multi-purpose canister and multi-purpose unit really tend to deal with the drift emplacement, not the borehole emplacement.

With that, I'd like to say thank you.

DR. VERINK: Thank you very much, Tom.

I think we are supposed to be scheduled for a break now, and then start with Ed Cording in ten minutes, so let's reserve our questions, but thanks to all of the speakers. Thank you for maintaining the schedule.

(Whereupon, a brief recess was taken.)

DR. CORDING: We'll begin our session for the remainder of the morning. My name is Edward Cording. I'm a member of the Nuclear Waste Technical Review Board, and I'm also, in terms of my own work, I focus in the areas of engineering, geology, rock mechanics, geotechnical engineering applied to slopes, excavations, underground work. This is in my work at the University of Illinois.

I'm really looking forward to these next sessions. We have two more small mini sessions this morning, and we're going to be discussing how the thermal loading conditions, things we've been talking about for the past day and half, how they relate to the repository design and the exploratory studies facility.

It's very timely. In fact, things are moving ahead, obviously, very rapidly with the exploratory studies facility. Construction has started, and there are decisions that are being made that have implications for repository design, and there's a lot of work that is still being done in developing the thermal concepts and relating these things, so we're looking forward to this discussion today, and how these items of a repository design, exploratory studies facility testing, how they relate to the thermal loading, and how that is going to be integrated as things have to proceed with the construction and exploration at the same time, as concepts continue to be developed.

I'd like to comment just briefly on some of these concerns that we have, or some of these issues that are going to be discussed. Certainly, we are finding that with the start of the underground exploration in the exploratory studies facility this fiscal year, it is really also necessary to consider, at this time, the interface between the exploratory facility and the repository design, and that in particular related to the location and gradients for the ESF ramps and drifts in the potential repository block as they approached the potential repository block and entered it, and then those drifts also could ultimately become part of an access to a future repository.

It is apparent that this planning is being carried out well, while many of these strategies and concepts for waste emplacement and thermal loading are in transition, and these new concepts will affect both repository and ESF design.

One example is the possibility that large waste packages, the MPC/MPU-type of units that were being described in the previous presentation, will be emplaced in drifts, and that possibility is leading to a consideration of ramp and main drift gradients that we'll be hearing about, I think, during portions of the morning presentations, and reconsidering these gradients, actually lowering them to accommodate real transport of heavy packages, if this facility is ever used as an actual repository, and these adjustments, of course, would also be consistent with the use of rail transport during ESF construction and testing, and I'm pleased to see consideration of various-sized drifts for the waste emplacement, and I think the issues of stability and thermal temperatures on the boundaries are of interest, and it looks like going to the smaller diameter drifts for the waste emplacement off the main drifts, actually, there's not much penalty in terms of additional temperature from the presentation of the figures that were shown to us just previously, so I'm looking forward to learning more about that in the coming months as well.

Certainly, much was going to be discovered during exploration, and the ESF, of course, is an exploratory facility. Surprises may be encountered. The designers must deal with the conflicting requirements for locating potential repository ramps and drifts favorably with respect to anticipated geologic structures, and then, also, they must consider the fact that these structures still remain to be fully discovered and explored, and adjustments in the drifting for both the ESF and future repository, if it is built, would be required, or may be required.

Certainly, a program in which there is plan flexibility in, during construction, adjusting the locations of drifts and making decisions as conditions are encountered is going to be needed so that these drifts can be appropriately adjusted during the construction without requiring long delays to evaluate and make changes and slow the progress of the work.

I'm going to introduce our first speaker today in the area of the repository design. We'll be talking about that first, and then we'll go later, in about an hour from now, to the thermal relationships with respect to the testing, and I think it'd be interesting to look here today at how we can consider the process of developing the design and the exploration to consider this process as testing actually begins and continues.

So our first speaker is Dr. Larry Ramspott. He's quite familiar to most of us. He's project leader and analyst in the Energy Analysis Policy and Planning Section of the Energy Program at the Lawrence Livermore National Lab. He's been involved in the DOE high-level nuclear waste program since 1976. Prior to the nuclear waste program, he worked in the treaty verification nuclear test, containment and plowshare programs. He was an originator, in fact, of an AEC program to study radionuclide migration at the Nevada test site in 1972.

His topic is, "Designing a Mined Geologic Disposal System; When is a Thermal Loading Decision Needed?"

Larry?

DR. RAMSPOTT: Good morning, and I'm glad to be able to speak with you. I want to say a word about the origin of the talk and its subject, because it may appear to be a little bit out of context in light of some of what has been said in the previous day, and a little bit this morning.

At the time that the Board asked me to give this talk, it appeared that the DOE was looking for a very specific decision on thermal loading, explicit cold/hot SCP in the time frame of September or so this fall, and from what I've heard yesterday, I think that basically this is turning from a specific decision into a narrowing of options. However, the underlying focus of this talk was a specific decision and when it might be needed.

Now, I think it's necessary to recognize right up front in the talk that an up-to-date conceptual design is really necessary to conduct the program efficiently, and basically, there are a whole series of things that have happened since the 1978 Yucca Mountain SCP, which makes the current conceptual design out-of-date, and I'm not going to read through all of these new ideas, but many of them have been discussed or mentioned in the last day and early this morning.

So basically, we really need an up-to-date repository conceptual design. It's absolutely vital, and to whatever extent that a thermal-loading decision enters into that, then it's necessary to make some kind of a decision.

I'd like to mention the effect of thermal loading on repository behavior in unsaturated tuff. I believe this is almost a redundant view graph at this point in the meeting. A number of people have mentioned a variety of things, and this has been mentioned over and over, but basically, there are a few things I'd like to point out:

That under ambient conditions at Yucca Mountain, an opening into unsaturated tuff at Yucca Mountain is going to remain dry, despite the rock's containing water in the pores, and if you introduce heat into the rock, then that is going to mobilize that water, and, under some circumstances, that mobilized water can drip into rock, into openings. So this is basically one of the effects of thermal loading.

There are several other design input features if you're looking at spent fuel in unsaturated tuff, and what I'm talking about here is that the basic underlying philosophy or rationale for unsaturated tuff is a little different than from granite or salt, and the early thinking was dominated by reprocessing waste in granite or salt. But if you integrate the heat in spent fuel for more than 100 years, then the majority of the heat is going to come from actinides, and it will persist for thousands of years, and that's not the case for the reprocessing waste.

And, also, unsaturated tuff has one-third the thermal conductivity of salt, and it's got two-thirds that of granite, and so, therefore, the heat profiles that you're going to get in the unsaturated tuff are different.

Another thing is that ambient conditions at Yucca Mountain are going to be perturbed for on the order of 100,000 years under all of the thermal loading options that we have been discussing.

This view graph is one that I showed at the October, '91 meeting, and basically, what I'm just pointing out here, normally, you see these curves as log-log plots. I think it's better not to do that. You see here what you have is a fission product decay, and then the actinide decay.

It takes it into two separate stages, and what has happened over a period of time, we started our original concepts looking at this part of the curve, and now we're looking over at this part of the curve.

The thermal-loading concepts that we are discussing and talking about fall into three groups. There's basically the site characterization plan, the SCP-CD group, which has a series of characteristics that I'm not going to go through right now. Then there's the sub-boiling drift emplacement, and generally, one is speaking there of self-shielded casks containing about 30 YOC fuel, and if you had a maximum of 50 C, but if you wanted to impose that as a drift wall temperature, you'd have only 1 to 4 PWR per cask, and you'd be down at about 20 kW/acre; whereas, if you were willing to accept up to 90 C, you could have 8 to 12 PWR per cask and you could go about 40 kW/acre, and I think you've had a number of talks indicating to you how complex this is, and I'm generalizing, and I can't defend any specific number here. These are generalizations.

For an extended dry drift emplacement, again, you're talking about self-shielded casks that would contain about 30 YOC fuel, but if you went to a maximum of 205°C, you could have 21 to 24 PWRs at 114 kW/acre; whereas if you wanted to keep the temperatures down around 125 C, you have the same number of PWRs, but you'd have to spread them out to about 57 kW/acre.

The temperature history along the repository centerline is interesting here, and I just want to make one point on this view graph. What I've done is I've taken two of Tom Buscheck's view graphs and plotted them on the same scale for different things. The top four here are all drift emplacement, and this is the SCP reference design, which is borehole emplacement, and what you'll see is the various profiles that you get from the drift emplacement, and what you see is a very high temperature, very rapidly declining down and merging with this curve and going a little bit below it, I think, if we extended the calculations out, so you see that the SCP design is quite different from anything that you get in the drift design, and that's the main point that I wanted to make on that view graph.

There are some issues that are common to the three designs, the SCP, extended dry, and sub-boiling. One is that heat will affect the system. In all cases, heat is going to affect the system, and the real question, then: Is the effect of that heat going to be deleterious?

In all of those designs, water will be mobilized, and you have to predict the hydrologic behavior of the system. Now, Tom makes the point that it's less predicting of a hydrologic behavior with the extended dry.

Most of the water that affects the repository does not flow from the surface. I think several people noted that. It's already underground, so whether or not anything changes as far as the climate, you have the water there already.

There are going to be zones in all of these concepts where hot water is going to contact the rock for decades, and then, finally, the saturated zone is going to be heated, resulting in convective flow regardless of which that you pick.

The emergence of drift emplacement--and I use the word "emergence," because it seems to have emerged as a preferred design over the past several years. It's based on many features besides thermal loading. It's cheaper and simpler. It allows self-shielding, which makes retrievability more believable. It facilitates the use of a more robust waste package of the kind that Dan was just talking about, and Tom Doering just before the break. It makes the MPC/MPU concepts feasible, although it is possible to borehole-emplace them, but they're much more feasible.

It may reduce the risk from seismic activity, which I think was a point that was made in an earlier meeting this year with the Board. It eliminates the "bathtub" scenario around a single waste package, which is one of the main ways of getting radionuclides out into the environment, and it may lessen the consequences of human intrusion, so there's a whole series of reasons why one wants drift emplacement that have nothing to do with temperature.

However, drift emplacement facilitates both the extended dry and the sub-boiling repository concepts. I think there's a tendency among some people to see drift emplacement as connected with extended dry, but it also facilitates the sub-boiling repository concept. So if you want the same peak wall-rock temperature, drift emplacement will allow much greater loading density, and that, when combined with older fuel, will facilitate the extended dry concept. It helps the extended dry concept that way.

For the same loading density, you can get a lower peak wall-rock temperature, and if you combine that with older fuel, that will facilitate a sub-boiling repository. So drift emplacement will help either one.

The main distinction between the two drift-emplaced options, then, is thermal loading, and if you're looking at 30-year-old fuel, which is what people seem to be looking at as a standard now, because that, the average age of the fuel would be about 30 years for a 2010 repository.

Extended dry would range from 60 to 120 kW/acre, whereas sub-boiling ranges from 20 to 40 kW/acre. That's the main difference. So, therefore, the extended dry option is going to imply a smaller area. There'll be less miles of drift. You'll have fewer, but larger waste packages, which has the impact, of course, of cost. However, I think that there's an implication of the extended dry, because of the size of the waste packages, favoring rail haulage, and a greater challenge to designing the emplacement drift backfill because of the temperatures. Now, whether or not we need backfill hasn't been decided yet, but there is a greater challenge there.

The sub-boiling option, we have a larger area and more miles of drift, and many more smaller waste packages, and there's a possibility of non-rail haulage, and there's less difficulty to designing the emplacement drift backfill, so there are some differences there.

I think only a detailed study would show how much similarity could exist for the two options. One of the things the Board asked me to look at, or to comment on is, is a generic repository possible in the sense that "generic" would be that the same repository design could accommodate both very cool, sub-boiling, or the hottest extended dry, and there you would have a lot of things you would have to look at for drift diameter and spacing, ventilation requirements and handling equipment, and I really can't answer that question. I would say that I don't think it's impossible to have a single repository design that would accommodate all of that.

Then there's the question of thermal tests and a thermal-loading decision, and I think much of what I've given you up until now is background and summary of material. One of the arguments is that in order to make a thermal-loading decision, that you need thermal tests, and so I need to address that.

There wouldn't be any need for a thermal-loading decision except for its potential effect on licensing for isolation. That's the only reason that we would have a decision at all, because, otherwise, you would simply adopt the most cost-effective design automatically, and the most cost-effective design would be, I think, the extended dry design.

Now, you do need a specific thermal loading for a final licensing repository design. I think there's no question about that. You absolutely can't go into the NRC saying, "Well, we might do this, we might do that, but we haven't made up our mind yet." And at that point, you have to have a decision based on test data and an analysis. I don't think we can go in with calculations alone.

So, really, the question, then, is with respect to some earlier decision, and there's some questions there. Is a thermal-loading decision needed for repository conceptual design? And if the answer to that is yes, are thermal test data needed for the decision? And if the answer to that is no, then how can you proceed with a design of the storage and transportation subsystems if you don't make a decision?

Now, this talk has been a lot of assertions, because in the 20 minutes I have and what I want to cover, I don't have the time to back up all of this. I think, over a long period of time, I could back this up. I just make the assertion that the technical basis for a final thermal-loading decision at the present time does not exist, and I think you can think back to what you've heard up until the time of my talk yesterday and this morning, and I think the debate about the differences among all of these really misses the point that we don't understand enough about how heat is going to affect the mountain in order to make a decision at this time.

The calculations of both the cool and the SCP designs that show effects from heat that are very similar to those from extended dry. The SCP design has very high temperatures at the rock wall, and this is one point that's been made against extended dry. The so-called cool designs have long times with the rock in contact with hot water, and, again, that's been a point against extended dry.

Both of this, this SCP and the cool, will have a perturbation of water flow in the saturated zone, along with extended dry, and there will be mobilization of water in the unsaturated zone, so that's, again, for all of them, and I think some of these thermal-loading issues are not resolvable by more or better calculations. We simply can't send people back and say, "Go through and do some more calculations." We're going to have to get some field data.

Therefore, heater test results are needed to choose a final thermal loading strategy, and the question is how much testing, or what kind for how long, and I think we're going to hear more about that later in this session, and also, we need a formal analysis of several options for the EIS whether or not we adopt one.

I would argue that, fortunately, a thermal-loading decision is not needed for conceptual design. I believe what is needed for the conceptual design is understanding the constraints among the various subsystems. We have to understand those very well in order to be able to go on. When I say subsystems, I'm talking about repository, transportation, storage. We have to understand all of the constraints among those systems.

We also have to know what the bounds are for plausible thermal-loading strategies, not necessarily the details, but we have to know the bounds.

I would argue that neither a thermal-loading decision nor underground thermal tests at the repository are needed to do a conceptual design of the entire MGDS system, not is it needed to allow construction design of the storage and transportation subsystems. We don't have to make a specific thermal-loading decision.

However, there are going to be programmatic consequences from not making a thermal-loading decision for the conceptual design. We have to understand and accept that.

Here are some the consequences: The repository advanced conceptual design is going to accommodate thermal loads, have to accommodate thermal loads ranging from 20 to 140 kW/acre, or some figure in that range. It may be possible to do that with a single flexible design. I don't know whether a good designer can do that or not. It's possibly more likely that there will have to be multiple optimized designs for the extremes.

Again, the storage and transportation subsystems are going to have maintain flexibility to deal with thermal loads over that range from 20 to 40 kW/acre, and, therefore, loading of the MPU/MPC would be in the range of 1 to 4 PWR assemblies. Now, this is not what people are looking at at the present time.

Putting a single small unit together of 1 to 4 PWR assemblies would not prevent getting up to 24 PWR assemblies in the storage, transport, and disposal casks ultimately, but I think, having all of it in one cask, there may be a little bit of a problem in some of those concepts.

If you select 21 and 24 PWR MPC/MPU at the present time, you're going to pre-select for the extended dry option, and I think that would introduce risk into the MGDS program. I think it's essentially a pre-selection, and, of course, being an extended dry advocate, I'll take a step aside and say, I think that would be wonderful, but I'm not sure that from a program viewpoint that that's what you really want to do.

Cost projections for the MGDS may need to show a range rather than a single value, because there probably would be cost implications.

I think there are some options for advanced conceptual design without thermal tests in the repository block, and I listed four of them here. One could carry out early heater tests in an off-site test facility, but since we're already in conceptual design, I think we're really a little late for that one. I just listed it for completeness.

One could show that selected thermal loading options, or the option is acceptable even without field tests, and I think this is intellectually very difficult. I haven't been able to figure out how to do it, but I wouldn't deny that there's somebody here in the audience or in the program that could do that.

I think it's possible to adopt a repository design that is not sensitive to heat load of the unit capsules, and I think this puts it back on the repository designers, and I think that is certainly conceptually possible, but I don't know whether one can do that or not.

The thing that actually is possible and is certain is to carry multiple designs through advanced conceptual design, but, as I mentioned before, there are cost penalties to that.

I look at what are technically supportable approaches to selecting a thermal-loading option, and having said what I've said, this may surprise some of you, but I think that all three of these are technically supportable in one fashion or another. I think they are in order of technical desirability.

The first is to avoid selection until the heater test results are available. That would be the most technically desirable one, but one could identify a favored option, but assure that there aren't any irreversible steps taken that might preclude an alternate which relied on different technical mechanisms.

Now, what I'm saying there is that the technical mechanisms, I think, for the cool sub-boiling option versus the extended dry are quite different, so you can select one of those as the preferred, and you could carry the other, since it had a different mechanism, you could carry it as an alternate that you would check at all points in the conceptual design.

Or, one could identify and quantify the programmatic risk of each option, and then select the apparently most favorable, and proceed at risk, with all the risk factors that I listed earlier. I think that is a technically-feasible possibility.

So, in conclusion, I'll say that the basis of a technically-sound thermal-loading decision is underground test data. However, a thermal-loading decision can be made now by accepting the consequences; and that is of added risk and required flexibility for future changes.

I don't think a near-term thermal-loading decision is needed in the repository subsystem, because you can do advanced conceptual design without making a thermal-loading decision, and you don't really need a thermal-loading decision for the repository alone until license application design. However, design of the transport and storage subsystems would be affected by the absence of a thermal-loading decision, and also, some MPC designs are compatible only with an extended dry option.

So those are my conclusions. Thank you very much.

DR. CORDING: Thank you, Larry.

We've got time for one or two comments or questions.

DR. DOMENICO: Domenico, Board.

Larry, that is very good, but from what we heard yesterday, this may not be an ideal repository, and we may not be able to get into the extended dry situation because of those things, so don't you think that perhaps one of the early questions might be--and I think it's a modeling question--is can we actually achieve the temperatures that are required for the extended dry before we go any further into even contemplating such a decision? I don't think that's a field problem, I think that's a modeling problem.

Do you have any comment on that?

DR. RAMSPOTT: Well, I heard several things yesterday, and I'm still trying to put them together, but in the same talk I would hear that you can't possibly get things hot enough to boil anything because this set of things might happen; and, on the other hand, it's going to get so hot that the world will fall out on you, and that's a rather wide range of options and I'm not sure that we can't narrow that a little better in the next few months or few years, and even without testing.

I just have a very hard time seeing this immense range of things that are internally non-self-consistent, in my view.

DR. PRICE: Larry, you said very quickly that loading of the MPU would be in the range of 1 to 4, but would not prevent up to 24. Could you clarify that a little bit?

DR. RAMSPOTT: Well, I think what Tom was talking about, and I have trouble keeping them clear, but I think the MPC, as he talked about it, would be something which has not got the heavy shielding on the outside of it, and it's a small unit. Now, you could load a number of those small units in a larger cask, even for storage or transport. You could have separate sealed units, maybe up to seven of them, in a large cask.

MR. GERTZ: Excuse me, Larry. That's not the current concept, though, of MPCs.

DR. RAMSPOTT: That's true, that's not the current concept, but if you really want to go down to a repository which is about 50° max temperature, I think you're going to have to get down into the 1 to 4 PWRs per cask.

DR. CORDING: Thank you, Larry. We need to proceed with our next presentation.

It's by Dr. Kal Bhattacharyya. He has been with Morrison Knudsen Company for 19 years, and he's the current engineering manager for repository subsurface design group for the M&O. He's been involved with the waste projects in salt in previous years, he's been involved in underground work in mines and various hazardous waste site remediation projects over the years.

We're looking forward to his presentation, which is, "Repository Advanced Conceptual Design: Subsurface."

DR. BHATTACHARYYA: Thank you, Dr. Cording. I'm Kal

Bhattacharyya and I'm going to address some repository advanced conceptual design issues this morning. I'll try to focus them on their relation to thermal loading. It's not my intention to give a complete ACD status report at this time. Also, the speaker, Bob Sandifer, coming after me, will address the question of the ESF design enhancement, so you'll hear a lot more about it from him. I'll just touch upon it.

I'll give you a talk about what were our design objectives in fiscal year 1993, and then, as I say, we'll touch upon the status of the advanced conceptual design tasks which relate to thermal loading. Specifically, I'll touch upon waste-package handling, some subsurface layouts, and some ventilation concepts for the various thermal loadings that we are looking at.

These are the objectives for this year in advanced conceptual design. You have heard a lot about these thermal-loading studies. These are the system studies being done on thermal loading, as well as emplacement mode studies. We are looking at the subsurface ESF interface development. We're supporting the MPC design study. We are supporting the site characterization activities, primarily the surface-based testing and their effect on the repository, and we are performing some of the tasks in the ADF shaft design, and so forth, which are not directly related to these studies, but they are part of advanced conceptual design.

This is a list of the primary tasks we are doing. This is, of course, an M&O function. Since our design is driven by requirements, we are participating in the development of repository design requirements. We are preparing a plan and we are preparing a basis for design for this year, and then these are the rest of the tasks. I'll talk about these.

Shafts and ramps concepts, we are simply looking currently at a mechanical excavation of shafts and their location. This is part of our subsurface design area. This is the task where we're looking at the subsurface layouts. The underground service system, we're looking at some ventilation concepts, and operations and maintenance is a task where we're looking at how to emplace the packages and retrieve them.

These are some of the things we are doing for the system studies that Dr. Saterlie talked about yesterday. We are looking at a range of thermal loading, as you know, from 20 to 114, and so forth; three emplacement modes for the horizontal in drift; and some of this waste-package design concepts from 2 PWR to 21 PWR, some of the stuff that Tom has talked about.

We are looking at the operability issues, personnel safety, how to handle these big packages if we get to select some of these, and how the retrievability is affected by some of these emplacement modes and waste package design.

We are providing some preliminary comparative cost analysis to the system studies group so that they can fit that into their evaluation in their attempt to narrow the range of this thermal loading.

This is one of the tasks that I was going to touch upon, due to some interest expressed from the Board and others, is waste package handling concepts.

Again, we are looking at these MPC packages, which are big, in the 125-ton ranges, and so forth. We are looking at the different way of placing them, and to support these studies, here is the list here. There are containers from 2 to 21 PWR and weighing from 29,000 to 360,000 pounds. They are much larger. To put this in context, the typical SCP packages are in the range of 14,000 pounds. And we are, again, looking at all these emplacement modes.

To handle these varieties of packages, we are looking at wheeled, tracked, rails and monorails, and even some other mechanical devices, and I'll touch upon a couple of these devices.

Typically, these are some of the--not an exhaustive list, but some of the things we are looking at in the process of selection of the waste package transport or handling equipment. Gradients, as have been mentioned by Dr. Cording and others, that the gradient can be made to the use of rail, for example. Drift size, again, rail probably tends to lead to a smaller drift size.

Waste package size and weight, when you are looking at probably 100 tons and up, you probably are looking at rail haulage. Manufacturers get a little bit concerned about wheeled packages of that weight.

Operating environment, you are looking at a, you know, fairly high temperature environment and radiation. Some sort of equipment would work better in high temperature atmosphere than others.

Emission requirement talks about diesel emission. If diesel emission is found to be a problem, then we may be forced to look at electric equipment, and electric equipment, in time, gets into use of certain type of equipment, for example, for in-haulage of some battery equipment.

Ease of automation is another concern, or a consideration, I should say. Automation in the railroad is pretty much state of art, whereas, automation in truck haulage is not, although they are being done in mining.

Power sources, again, they will provide us with electric, battery, or diesel, trucks probably related to electric and diesel.

Compatibility with emplacement mode, some emplacement modes lend themselves to easier, or rather, I should say, some transportation equipment lend themselves to some emplacement mode. For example, obviously, some rail haulage may not be so easy to deal with in a vertical emplacement as a matter of fact.

Requirement for relocation. This is a concept where you could start a repository out at the widely-placed packages and low thermal density, and then when they get cooler, we can probably either push them all closer, or the waste packages in between emplaced packages. We call that relocation, and we are looking at equipment that can do that.

Retrievability, of course, it's required that we plan for it, and whatever emplacement or the retrieval equipment that we use would allow us to perform retrieval.

This is a list, again, of some of the limitations. As I say, they can be diesel, electric, and battery, and rubber-tired trucks are probably confined to diesel and electric.

Typical operating limits. Standard rail is 4 per cent. Mines have used 8-9 per cent, as a matter of fact. We'll probably limit it to less than 3 per cent. Cog rail, I've used up to 48 per cent, as a matter of fact. Adhesion rail, about 10 per cent is the limit. We are trying not to look at these, primarily focusing on the standard rail.

Operating environment, 50°C for this type of equipment. This is primarily from the manufacturer, that this is the temperature, air temperature of the intake manifold. Now, we have talked to manufacturers who have talked about pre-cooling this air before intake to the engine, and looking at temperatures of up to 90-95°C, but there is really no equipment that does that right now. We will have to develop that.

Some of the concepts lend to different drift sizes, as was mentioned earlier by Dr. Cording. In-drift emplacement, using a rail, 14 foot is probably a minimum that you can look at, as a matter of fact, allowing you to use a much smaller emplacement drift. Rubber-tired equipment for the same type of heavy packages, and we are looking at, you know, the upper limit of the packages at this time, almost 7 feet in diameter packages. Typically, these trucks tend to be much higher for the same weight, and you are looking at 6 meters, around 20 feet in diameter tunnels.

For vertical and horizontal emplacement, of course, the equipment is the fact that you have to actually stand up the package, which is typically high. It automatically gets you up to this 23 feet range, and has really not much to do with the equipment.

We've got some very preliminary concepts on various manufacturers, and so forth. This is a truck with three axles, with the gear in front articulated. The concept here is that this truck can raise the waste package up and down, which can be seen in the cross-section, and actually, it travels in the raised position and can travel over other emplaced, in-drift emplaced canisters, as a matter of fact, and then when it comes to its designated place to put it down, it can then put it down and travel over the emplaced canisters. A concept like that could be used if we are talking about infilling or relocating some of these packages.

This you don't have in your handout because it probably wouldn't Xerox very well, but this is an artist's conception of the same equipment, as a matter of fact. It's a truck being shown, with the canister in the raised position, and traveling over in a tunnel that's got emplaced packages.

The question about bearing pressure of some of the tunnel floors, we have talked about--typically talked about filling the tunnel floors with crushed tuff, which may not be such a good bearing material, as a matter of fact, so we might want to look at some tracked vehicle, which will put more pressure. These are nylon-type tracked vehicles, and this is a concept from Caterpillar Company with the same idea of raising and going over other emplaced packages to its site.

This is a sketch of a rail transport. What we are looking at here is simply a pre-engineered dolly or a low-boy of some sort. You put the packages here, and then, by some remote means, using a locomotive, just push it into the emplacement place and leave it there at this location in this manner. Of course, we have to look at the compatibility of the rest of this rail with the waste package, and make sure that that is okay with the waste package life. This allows, as you can see, a fairly small diameter tunnel.

This is the second task that I was going to talk about a little bit, subsurface layout concepts. Again, we are doing this for two reasons: One is to support the system studies that Dr. Saterlie is conducting. We are looking at, again, a wide variety of packages, a variety of emplacement modes. For these studies, we are primarily preparing a generic-type of concepts for all these so that some thermal analysis could be done on them to make sure that these are feasible ways of doing it. So we are providing some concepts, and our performance assessment people and the Sandia National Laboratory people are looking at the thermal analysis of these. This is--Bob Sandifer is going to talk a lot about it, but I'll touch upon it briefly.

This is a very familiar picture. This is the point of departure. This is the SCP, as modified by the TBM excavation, 57 kW, areal power density, and it's basically a point design providing us with the familiar--this is where we start out from. Just some things to point out are this north ramp is 6.9 per cent gradient. This north-south drift is around 4.6, 4.7 per cent gradient, and this whole thing is tilted towards the east, making, basically, a conventional rail usage really--it's infeasible, is the way I want to put it. So this was a truck haulage system.

This is simply a illustrative idea to get across. Again, this is our 57 kW pork chop that we've talked about, the SCP-CD layout, the north ramp and south ramp, just to put it in perspective the way this design looks like. And if we are to look at something that's as low as 20 kW and as high as 120 kW, you can see the variety of area on here, where we have listed the areal requirement for the areal power density, roughly from 700 acres to as high as 4,000 acres. The primary area available in this is about 1850 acres.

I'm just going to show you a concept that we have developed, which is a step-block concept. It does not have any development going through in Ghost Dance, if you recall the other picture. This had all the emplacement drift crossing the Ghost Dance Fault, which lays about in this fashion. This is all TBM excavation.

This has integrated rail transport throughout the repository from the point we pick it up from the surface, all the way to emplacement. We are looking at a virtually flat emplacement drift, just enough to drain water, and gradients of less than 3 per cent elsewhere in the mains and in the ramps. And this particular one uses the in-drift emplacement, using approximately a 4 meter diameter emplacement.

This is a picture that is slightly different than you have seen in the past, and Bob Sandifer is going to talk about this ESF that this interfaces with, but, briefly, the waste ramp in this case is down to about 2 per cent. It is extended farther west, south ramp is extended farther west, and the north ramp drift runs in this concept parallel to the Ghost Dance Fault, and drawing the gradient around this thing around 2.7 per cent, as a matter of fact, so in this entire repository, we have all gradients less than 3 per cent, and this is laid out so that these are virtually flat, going east/west on an upper block. If you were to put all the inventory of the waste packages here, you probably would achieve something around a local area power density of 75 kW/acre.

Just as a point of reference, that local area power density in the SCP-CD design that you are familiar with was at 70 kW/acre, so that's not too different, whereas all the waste is confined to this.

If you were to use both sides of block, this is going to be the lower block, this is going to be the upper block. You will then develop this to reach the lower block, as a matter of fact, and, again, these are going to be in a lower position and I'll show you a cross-section to show you how it look like, but they are going to be virtually flat, also, and, in this case, you will achieve a local power density of 60 kW/acre.

These are strictly derived numbers. We didn't set out to start as a initial power density or anything like that. We took the total inventory, the total acreage available, and we've got about 800 acres on this side, about 300 acres on this side. You divide that into the 63,000 MTU of waste that we have. That's approximately what you're going to come up with.

This is a cross-section of the A and A'. As I said, the repository is going to be included from block step, both virtually flat, and a distance of about 205 feet at this point. This is going to be the proposed main drift in this case, which will utilize the existing drift. We will maintain a pretty good distance from the water table.

There are some questions or interest to talk about the ventilation studies, especially since we are looking at some higher areal power density than SCP. In our fiscal '93 work, we have primarily started looking at the drift length, maximum drift length that we ought to look at, because we want to make sure that we can cool them for retrievability, within ages and over a period of time. We are looking primarily from the previous work at the effect of continuously ventilating all this emplacement drift, or ventilating them, cooling them as required, and I'll show you some pictures on this work.

Again, we have looked at some past work just to see whether you can remove a large amount of heat from the waste package during the pre-closure period of time, we are looking at the concept of ventilating, for example, the concept that I showed you; how many shafts and how you can ventilate them. So that's a typical conventional ventilation concept.

This is all done from previous work, as a matter of fact, primarily from Danko, and so forth. You can actually control air temperature by continuously ventilating, but heat may require a very large amount of air and I'll show you a picture of how much air we are talking about. And if you didn't want to do that, continuously maintain ventilation, then you could actually ventilate something that has been closed previously and within a reasonable time. This is done by pushing a large amount of air.

You could cool it off in a reasonable time, and this simply talks about ventilation air flow is capable of removing a fairly large amount of waste heat, and these next three view graphs illustrate that.

This is some of Danko's work that he did for some earlier thermal studies that the M&O is conducting. In this case, this is in-drift emplacement mode, areal power density at 114 kW, and a drift length of about 900 meters. If you were to put 3,000 cfm of air through this drift, you really would not cool it down any measurable way from the 140° temperature, as a matter of fact.

By keeping it at 10,000 cfm flowing through the drift, you will probably reach temperatures around 90°C. By putting 25,000 cfm, you will now bring it down to around 65 or so. He did not put his periscope vision for a higher number, but he can surmise that by putting something higher than that, in the range of 35-40,000 cfm through this tube, you probably could maintain a 50°C in an emplacement--air temperature in an emplacement drift for all time, as a matter of fact.

This again is part of Danko's work. This is a case where if you use the sealed drift, you won't open it up for material or whatever, the initial temperature is 140°C, or 45°C, and by putting a substantial amount of air in there, in the order of 200,000 cfm, you could actually bring it down to the temperature of about 50°C in about three months period of time. This calculation does not consider any withdrawal of water from the drift, which will hasten this time to a matter of weeks, as a matter of fact. But this is a conservative approach where there is no cooling effect of moisture.

Now, there was a question yesterday from the Board about the feasibility of ventilation one of these drifts at a high temperature. I'm not sure exactly; was it you? What was your question again, exactly?

DR. LANGMUIR: Langmuir, Board.

It was just depending on how the packages were emplaced, whether we could ventilate around them. It was a question of their proximity to each other within the repository, and the issue of how far apart they'd have to be, and under what conditions, and how much ventilation you have to provide to bring them to a monitoring kind of status for people to be down there.

DR. BHATTACHARYYA: As you can see, this, at 114 kW is basically our upper limit in an in-drift emplacement, the same scenario you're talking about. If you were to push 200,000 cfm, which is really not much, you are looking at a usable velocity in a drift. This will amount to about a 400-500 per minute velocity. It could easily cool down that drift using the 200,000 cfm. We are typically looking at maintaining somewhere around 100,000 cfm through a TBM operation for a drift, so it's not a large amount from that point of view.

This is a fairly old work by St. John. It's a Sandia report, and he looked at a concept of removing a substance, how much amount of heat could be removed by ventilation. This scenario is a 66 kW/acre scenario, and his paper assumed that if we were to maintain a 30°C drift wall temperature as a boundary, then if you just put the 66 kW/acre, then add watts per meter drift length, this is the amount of heat that will come out from this waste package.

And this is the same amount shown here, but if you are to maintain that 30°C temperature at the drift wall, you could actually remove about 60 per cent of the heat that has been generated by the waste packages. This paper did not mention waters, just maintained the boundary heat. It just showed the feasibility that if you wanted to put some large amount of--initially keep the emplacement drift open for a period of time, ventilation could play a good part, an important part in removing heat.

Just to summarize what I have covered, we have started repository ACD in October of last year, as a matter of fact. We are not making a decision about thermal loading and waste package size. Everything is open, and we are looking at it, and these are the tasks, primary tasks: We are doing system studies. I have mentioned the repository layouts, transportation system, and ventilation schemes. These I have not covered very well, because that's not directly related to the system studies.

We have also looked at a concept for ESF repository interface. This is going to be covered a little more by Bob Sandifer, as a matter of fact, but I have shown you how a concept of repository could work with the ESF design enhancement that we are looking at.

At this time, I would like to answer any questions you have.

DR. CORDING: Questions from the Board. Yes, Don?

MR. LANGMUIR: Kal, one of the other things that George Danko has talked about and tried to sell to us in the past was installing heat pipes, as well as using traditional ventilation. Did you assess that, because obviously it's going to compromise the integrity of the system. It's going to open up possible pathways for movement of radionuclides at a later date. Aside from that, were there cost aspects of it, as well, that you considered?

DR. BHATTACHARYYA: No. We are not there yet. We are aware of Mr. Danko's work, you know. We worry about putting any water in there, or put a number of heat pipes will put some holes in the wall. We are just not there yet. Once we move on a little bit on the thermal loading, we will look at that, with all the possibilities.

DR. CORDING: I had a question; Ed Cording, Board.

Regarding the planning that you have for the advanced conceptual design, one, what sort of effort are you anticipating in the next year; and I guess the other question, at what point are you going to be making the decision or recommendations on some of these items such as drift size, gradient, thermal loading options?

DR. BHATTACHARYYA: To answer your first question, before this time--and I don't see Mr. Gertz here--but for next year is basically at the level of what we have this year, so we are going to be able to support the ESF interface work and some of the system studies, but we are not going to do a whole lot of advanced conceptual design at this level of funding, as a matter of fact.

DR. CORDING: So your present level is what, in terms of--

DR. BHATTACHARYYA: In terms of money?

DR. CORDING: And of your manpower.

DR. BHATTACHARYYA: Oh, manpower; subsurface design effort is around 9 FTE for this year. It includes all the M&I and all this stuff, and we'll probably be slightly higher next year, you know, around 12 to 15 FTEs.

To answer your second question as to the drift size, and so forth, I think these all have to come forward together, the system studies, waste package design, all of them have to come together and we will then be able to make a combined decision, as a matter of fact, on things strictly related to both waste package size, areal power density, and we can only advance together and we're slightly behind in the curve right now, so all of the studies have to be coming up and the decisions will be made as the studies advance. So it's a kind of a overall effort. It depends on what the system studies do, how waste package design is advancing, all depends together, as a matter of fact.

DR. CORDING: At this point, you don't have a date for a decision or recommendation on any of these items?

DR. BHATTACHARYYA: No, sir. The only date we have, really, is completing the advanced conceptual design in June of '96. That depends, again, on the funding that we get.

DR. CORDING: Clarence Allen?

DR. ALLEN: Clarence Allen, Board.

Just an observation here. In pushing the western boundary of the repository very close to the Solitario Canyon Fault, I think we have to bear in mind that the recent studies of the complexity of the Ghost Dance Fault suggest that probably the Solitario Canyon Fault is going to be equally if not more complex, since it has greater displacement, and from what we know, it's a fault with high degree of activity, so we just have to be prepared, perhaps, for some flexibility and how close we get to that boundary of the Solitario Canyon Fault.

DR. BHATTACHARYYA: I don't think we have--I may be wrong here, but I don't think we have pushed the boundary of the Solitario Canyon Fault, and I don't have the pork chop on it, but it's pretty much--the only thing we have shown here is a fairly large stand back from the Ghost Dance Fault, as a matter of fact. Maybe there's some information that might be given that it could be wider, and so forth, so this is simply maintaining that option at this time. We are not advancing farther west than that.

DR. ALLEN: No, my only comment is that setback may, in fact, have to be even greater than we show here. We'll have to remain flexible on it.

DR. CORDING: Thank you very much.

DR. BHATTACHARYYA: All right.

DR. CORDING: Well, continuing now with--I'm going to make it a very brief session introduction for our next session on thermal loading, and the testing level for thermal loading, and I'd like to pick on comments that Dale Wilder will be making to you, according to the briefing book, and it was noted in his discussion of the testing, thermal testing, that there were some surprises in the heater tests conducted in G-tunnel, and I think, in fact, the observations of the drying and wetting around the heaters that they observed there really was a major aid and help to investigators, people that we've been talking with yesterday as well, who had been focused on the thermal hydraulic problem, and I believe this work has been of much assistance in giving some impetus to better concepts in regard to the thermal hydraulic problem and improvement of the models.

I think this is an example of how exploration testing provides much more than the input data to existing codes. It can provide new perspectives into both the boundary conditions and phenomenon that are related to thermal behavior and other problems. It can result in better models and more appropriately applied models.

It was over four years ago that G-tunnel testing was terminated. There's a lot of catching up to be done. We look forward very much to discussing both the immediate efforts needed to bring thermal testing methods and studies of the phenomena up to speed, even before the work begins within the ESF, and then the continuing program, short and long-term testing that would extend to licensing and perhaps well beyond that.

So our first speaker, then, is Dale Wilder. Dr. Dale Wilder's with Lawrence Livermore National Lab. His background is a masters in civil engineering, bachelors work in geological engineering, and he's been with them for 14 years. He's currently the Technical Area Leader, Near-Field Environment, Yucca Mountain Project, and Acting Group Leader for the Nuclear Waste Group of the Earth Sciences Division.

He's had much experience with projects even prior to that time, in siting studies for power projects, environmental assessment, seismic work, groundwater studies, so, Dale, we look forward to your presentation.

DR. WILDER: Thank you, Dr. Cording.

Well, as the title of my paper indicates, I will be talking about the thermal tests that are planned for the waste package environment. I don't think that I need to call to your attention the need for these tests. They've been discussed several times over the last couple days, and I think that the Board very succinctly had indicated the need for thermal testing in the Sixth Report, in which they outlined or called to our attention again that DOE's understanding is, to a large extent, untested, and what I want to do, then, is talk about the test strategy that we have for trying to address these thermal models, if you will, conceptualizations.

I'm going to start by talking briefly about laboratory scale, although I'm not really going to spend much time talking about laboratory scale testing, except to point out that laboratory scale testing is normally done both on small core size as well as short duration. Usually, we're talking about tests that last a few days. In the case of geochemistry, they may be as long as a year, but, in general, these are short duration, as well as small-sized tests which allow us to look at basic properties, perhaps at single fractures, but certainly not looking at some of the complex interaction where we have interconnecting fractures, and usually, we're not able to look too much with core at multiple fracture and, therefore, coupling processes.

Those tests, of course, are ongoing, and all that is required to be able to do the lab tests is the facilities and the rock availability.

I'm going to be spending some time today talking about what we call block scale tests. These are relatively small scale, up to approximately five meters in size, with durations of approximately a year, and the advantage of a block test is it allows us to look at some of the interconnectivity issues, it lets us look at the coupling of processes between the matrix and the fractures, and certainly will allow us to look at the characterization and testing techniques that will be used later in the in situ tests.

As a note, the block test will support some of the early decisions that have been talked about several times, as well as to allow us to plan the in situ tests.

I will also be talking this morning a little bit more about some our large-scale in situ tests. These are scales of on the order of hundreds of feet, with durations anywhere from one year to perhaps as much as five to seven years at heating, a total duration of approximately a decade. These tests will allow us to characterize the response of Yucca Mountain to the emplacement of waste, and of course, we're doing that by emplacing heaters, we're not actually emplacing waste, and they also allow us to look at the in situ block mass, large-scale, if you will, property characterization and the interaction, the coupling.

There are shorter duration tests, which I think Dave Stahl had introduced you to yesterday, which are planned which will support the license application; longer duration tests which will be necessary in order to defend the license, and I will talk about that a little bit later.

And then the final category of testing is what I would call performance confirmation monitoring. We've always recognized that with the length of time that we're trying to evaluate performance over, that we just don't have the opportunity to really test our models over those kind of durations of time. However, we do have a fairly long, in terms of human history, period of time that should be available to us, and that is from the time of emplacement of waste until the repository is actually closed, and so we have always maintained that that is an opportunity to do testing of the early portion, at least, of our long-term predicted modeling.

The other thing that it does is it allows us to look at the large scale characterization, and it gets at some of the heterogeneities, and you've heard the discussion about features that are perhaps 100-meter type spacing. Bo gave me an indication awhile back that if we did have something like a heat pipe developing, that we would see that across the entire region between these 100-meter spaced features in about 100 years. So if we have retrievability, a period of on the order of 100 years, that would allow us to now look at some those larger-scale heterogeneity issues.

Now, as was mentioned, we have done some work earlier, about four years ago. I apologize that I have not updated this slide, but I intentionally wanted to use the slide as you saw it approximately four or five years ago. Obviously, everything's changed; different logo, and so forth, but this is the conclusions that came out of the work at G-tunnel, and this was a testing, if you will, of our conceptualization, our modeling, and as was pointed out, many things which we expected did occur at G-tunnel.

Now, I'm not trying propose G-tunnel as a complete test of what we expect at the repository. It was very small scale, but we did see the drying out and developing of the saturation "halo," and so forth. There were some surprises, as I think that term is the term that David used. I need to point out that when I say surprises, it's not that we didn't understand that gravity is present, or that capillary condensation may occur.

What we learned from this test, however, was that some of the simplifications that we were making in our models were inappropriate, and I think you've heard a lot of discussion yesterday that models are going to have to, by their very nature, include some simplifications, some assumptions, and I think that this was a good example of the value that's gained out of the heater test. It allows us to calibrate, if you will, our simplifications, our assumptions, and our conceptualization.

And so, we did see that we should have, in our scoping calculations, included explicitly the effect of fracture flow as dominated by gravity, and also, early on, capillary condensation for the first 10 to 20 per cent of the re-wetting process.

You also heard from Tom Buscheck that there are about five major hypotheses that we feel we can address through heater tests. I'm going to talk about two different types of heater test. One is the large-block test, and we feel that three of these five hypotheses can at least be addressed to some extent, not entirely, but to some extent in the large-block test; that is, whether or not we can remove the mobile water if we are above boiling temperatures; whether there is sufficient density and connectivity of fracture to allow dry out. In this case, we're not characterizing Yucca Mountain, per se, what we're looking at is what is the impact of fracture density on that dry out, whether the re-wetting significantly lags the end of boiling.

Now, we're not going to be able to see the re-wetting come back very much, but there are some things that we feel that we can do to address that issue in the large-block test.

The other two issues: Conditions where conduction dominates convection and vice versa, and looking at the large-scale, buoyant gas-phase convection will require a larger in situ test.

Let me start, perhaps, in a little bit backward order to set the stage, and tell you what the plan is for ESF testing itself. This is just a conceptualization of one possible layout. There are many others that are currently being discussed, but the layout does include a series of three parallel drifts for abbreviated testing, which I will explain in a minute, and then a series of five tests, or five drifts to allow the larger, longer duration in situ testing, and one of the reasons for five versus three for the longer duration is not only to get at a larger area, but it allows us to look at things like condensate shedding in the pillar zones between these parallel drifts without worrying about the edge effects.

I'm going to focus on the three-drift arrangement, and I want to talk a little bit about the criteria that was used to try to determine some of the design features; and, specifically, I'm going to be talking about duration of testing.

There are five criteria, and I guess if you'll excuse me, I'm going to put this over here. I think you can remember three-drift arrangement, because I want to talk about each of these criteria.

The first criteria was the volume of dryout. There are a couple of things that have driven this criteria. First, we recognize that at G-tunnel we had a fairly small-scale in situ test; that is, we heated up to a bubble boiling point about three-quarters of a meter radius, and we recognize that that was really inadequate to look at things like the interaction of fractures and to look at some of these processes, and so our feeling was we needed to get certainly enough rock that we'd incorporate a number of fractures.

And secondly, as has been pointed out many times, we recognize that not all fractures are equal when it comes to hydrology, and some very good quantitative work that came out of the Stripa Project would indicate that we may need to be looking at as small a percentage as perhaps 10 per cent of the total number of fractures if we really want to describe the hydrology.

Therefore, it was our judgment that we wanted to incorporated approximately 100 fractures within our dryout zone, so that we had the chance of seeing somewhere in the neighborhood of five to ten fractures that we could expect to be the major fractures that would carry water.

On that basis, using the three-drift arrangement, we looked at a number of different scenarios for total power within the drift. This is the kilowatts per drift, and we said we wanted a 20 meter total thickness of dryout; that is, 10 meters above, 10 meters below the heater, which, based on the best information we could get on the fracture density for Topopah Springs, should include a set of approximately 100 fractures. And so, on that basis, we defined a duration of heating, depending on which power was used, between four to six years.

The second criteria was that of the temperatures. While this is, perhaps, a little less defensible in some respects; that is, trying to maintain below 200°, we really felt that we wanted to avoid some of the phase transition problems, and so we wanted, to the extent possible, to try to stay below 200°C in our testing.

Well, as you can see, that goal really is not consistent with the earlier goal of trying to dry out approximately ten meters. These are calculations for the central drift midpoint. As you can see, for each of the three heater cases--and I should have indicated I wasn't going to talk too much about the 12 kilowatt, it was just too high in temperature. As you can see, it gets up to almost 500°C.

With that kind of a criteria, we are not able to satisfy the stay below 200°C if we go with durations of four to six years with the respective power densities. However, if you look at the entire test array, we do have the opportunity of seeing some areas that we can do testing that will be below the 200°. For instance, on the inside drift wall of the outside drift--so what we're saying is the inside rib of the drift on the outside of the three-drift arrangement, also the pillar between drifts will, in the approximately four and a half years to five and a half year duration meet the criteria of staying below 200°C.

The third criteria was one driven by our concern over geochemistry. One of the biggest challenges that we've had--and we were not able to address it at G-tunnel, and we were hoping to do so in a subsequent prototype test, but we knew that we just didn't know how to do the sampling of the water chemistry without impacting our testing, and that issue was brought up yesterday, and I should have mentioned a couple of other methods that we are looking at. It was pointed out to me that I had overlooked those.

We're looking at things like microelectronics, and also very small absorptive sample techniques to try to sample the water, but regardless, one of the problems that we have is how do you sample the chemistry and keep that chemistry within the range that's going to be appropriate?

One of the problems we have is that we're moving the boiling front, you know, if that really does occur, we're moving a boiling front through the rock, and if we are moving it too fast, we don't have sufficient resident time to allow the chemical reactions. We noted in the lab, for instance, that there were cases in which we had to run rock water interaction tests for up to as much as a year before we got adequate information, and so if we're moving the front too fast, we're concerned that we may be doing some violence to the conclusions on geochemistry.

What I've got here is a plot of the rates that we expect for the dryout front to move in the repository itself; two different cases, 57 kW, then 114 kW for 30-year-old fuel. As you can see, the rate of dryout front movement is in the order of perhaps a half of a meter to a little over a meter to begin with, and drops down into the range of two-tenths to somewhere around five-tenths after about 100 years. Then it becomes almost linear, dropping down to about .3 meters per year out into the thousand-year time frame.

Well, we would like to, as much as possible, match those rates. We looked at how fast the boiling front would be moving in a heater test, and while we cannot match the rates, we felt that we can at least come close to seeing rates that will not be totally out of the ball park.

What I'm showing here first is an abbreviated test, a year and a half heater duration, and it'll become more obvious why that's a concern later when I get into the abbreviated test discussions. And in that case, we're about 200 times the thousand-year repository rate. We're probably --well, you recall that the rates at 100 years were like .3

meters, whatever, and so we're about 20 times the rates at the 100-year time frame.

When we go to the longer-term test, somewhere between the four to six-year duration, we drop down to about 100 times the repository rate, but as you can see, we don't gain a whole lot by going in tests that are longer duration. We've got about as much as we can in terms of dropping down the rates. So, once again, we're kind of honing in on the four to six-year time duration for those in situ tests.

The other criteria was that we wanted a large enough zone that we could actually sample, and that is the size and duration of the condensate zone. These are studies --and I should have indicated, that last study was for 6.3 kW. We kind of focused in on that as a compromise. This is also calculations for 6.3 kW heaters. This is the central drift, using symmetry, the outer drift. As you can see, there are temperatures above 50°C and below 200°C in the pillar area for about three years. Unfortunately, we don't have saturation conditions to where we could really sample for geochemistry. We dry out rather quickly.

However, if you look just outside of this three-drift arrangement, you'll see that there's a zone of about five meters in size which maintains temperatures between 50 to 100°C for approximately three years, which should be adequate for us to look at the geochemistry processes and to do the sampling, and in that zone, we anticipate saturations at least at ambient.

The reason I say at least at ambient, early calculations done at G-tunnel also showed this same kind of saturation buildup, but we didn't see it at G-tunnel because of the condensate shedding, and so we're not sure what the saturation conditions will be, but they certainly will be ambient or more.

Well, on that basis, then, we looked a number of different approaches for trying to do the testing. This first one is what I call the ideal strategy; that is, the ideal from a scientist's standpoint. It has no schedule constraints. I think, however, if you look at this, you'll see that it's probably not a very satisfying schedule from the standpoint of trying to get the project moving forward.

And I've got to explain that these are kind of wraparounds. I didn't have enough room to have a big, long view graph, so the way this is laid out is that there's about three and a half years of prototype testing which precede the planning for the ESF testing, and so you'll see that this little line comes down and connects the test planning, the ordering of equipment, and so forth, which is about a two and a half year cycle, with an approximately ten-year cycle of testing, followed by a year and a half of analysis, and that gets us to 18-20-year time frames.

Well, while that may be great from a scientific standpoint, it certainly would not move us forward to try to meet any sort of a license, so there's another strategy which was developed. Now, I recognize that these charts will talk about a 2001 license application, and you heard yesterday that that may not be the case anymore, but the general thinking, I think, would still apply; and that is, if we could go to an off-lot, prototype facility, we could do two different types of tests.

One is what we call an abbreviated test, in which we have about a year and a half of heating, six months of cool-down, six months for coring to look at the geochemical processes, and then the analyses, and that that data would be available to then compare with a longer duration test with approximately four years of heating, to where we could evaluate the scale issues.

That would then be compared with abbreviated testing that's done at the ESF, at Yucca Mountain itself. In this case, we've got an abbreviated test that's very similar to the one that was done off block, which would be, essentially, the data submitted to the license application, and at the time this was done, the license application PA data brief was in the end of '99 or the beginning of 2000.

We would also incorporate a longer duration test starting concurrent with the abbreviated test in the ESF, and there was a decision point made, and we recognized that we were proceeding at some risk, and the decision was that we would compare the data from the abbreviated test with the data--the abbreviated off block test with the abbreviated ESF test, as well as with the longer duration block test, and from that would be able to make the decision, can we proceed with the license application? If no, there was a decision that had to be made.

We also called for confirmation testing, which would be done that would continue on at a longer time frame, but that the long duration test was really that which would be used in the defense of the license.

Without a off-block test facility, the strategy--and this is the strategy which we currently are following--was to go to a facility where we could create a larger block, where we could do tests which would look at the coupling and the interaction of fractures, but would be a short duration test which could be completed in sufficient time to help us in the planning for the ESF testing, to allow us to order the equipment, and so forth, and still begin the ESF test in the '96 time frame, as planned.

The ESF test would consist of three basic tests: a abbreviated test, as described before; a cool-down test--and the reason for this is that there's real concern that if we're cooling down too fast, or we don't go through the entire cool-down cycle, we may not really get the information we need, both from geomechanics, as well as hydrology, and I might just mention to those of you that are familiar with the spent fuel test at Climax, one of the problems we had was we did not continue monitoring the geomechanics. It was not a hydrology test, but we were looking at geomechanics. We did not continue monitoring after the temperatures had decayed, and we were not able to resolve the data because we were still recovering on this, and so we really feel that we need a cool-down test in which we have a long duration of cool-down in comparison to the heating, and so there are two abbreviated tests, if you will.

The abbreviated test that I've described earlier would be the one that goes to the license application data, but this cool-down test would then be available before the license was actually submitted, to justify that there was nothing that we'd overlooked in the cool-down, with a constant heat, long duration test--and I would call this a license defense test--meeting the criteria that we've just gone through; about a six-year duration heating, six or twelve years of cool-down which would be available to give us additional confidence as the process continued.

And one of the things I was trying to indicate in my comment yesterday was that this is not a one-step process. We are not going to know everything at one stage at the license application, and so we recognize that we will continue to do tests to increase our confidence beyond what is done at the time of license application submission.

I show this merely to give you an example of the kinds of things that we feel we can do in heater tests. The question was asked: Can we really see things during heater tests? This specific plot is to look at convection versus conduction-dominated responses, and we feel within a two- to four-year period of time, we will see sufficient difference in the temperature profiles if it's conduction-dominated versus convection-dominated, that we can make that kind of distinction. There are others, but I don't have time to go through all those.

Let me then move to a discussion of the large-block test. There are a number of issues that require testing prior to ESF testing. Some of these may not be quite as critical as others; that is, we talk about early decisions being based on models, and we really would like to increase our confidence in the models, but as you heard from Larry Ramspott, there may be some things that can be done to where it's not as critical to have that information.

But we feel it's really a problem to try to validate tests and, to a large extent--excuse me for using the validation word, but we feel that we're going to have to be building some confidence in those models at Yucca

Mountain, and it's very difficult to do that if you're using the same tests that you're using to characterize and to develop and test your models, and there's a couple reasons for that.

If you're developing and testing models, you are, by definition, tweaking some knobs, and we really don't feel you can do that in the ESF test if you're trying to use ESF for validation. And, secondly, any validation test is going to have to rely on scoping calculations and plan. Therefore, we need to have some confidence that the physics or that the conceptual model is correct.

And so this is almost an overriding reason for large-block tests, in addition to which it provides a great deal of help to us in planning the ESF test. It allows us to evaluate some of these instruments and techniques which are not well-developed right now, or at least not well-proven, and will give us the confidence in those models for the scoping calculations.

So let me just review very quickly the status of the large-block test. The large-block test is designed to be excavated from an outcrop at Fran Ridge. What we plan to do is essentially excavate the rock away from the block, leaving a pedestal in place of about 3 meters on a side, to 4½ meters high, which we will then do heater tests in.

The current analyses and the test layout is not determined for sure. We're still talking about this, but it appears that we'll probably be going with the five-heater array, approximately 500 kW, at least that's what this calculation is for, and if we do that, as you can see, in 120 days, we will be able to get coalescence of the boiling point isotherms. We will also, by controlling the boundary conditions, be able to develop a zone of refluxing, in which we will--I'm going to turn this sideways. I apologize that the label is off to the side, but we're looking at a vertical direction.

Above the heaters, we'll be able to maintain saturations of 100 per cent, because we're not allowing the water to escape, and in this case, we can look at the refluxing issues, the fracture healing, the geochemical processes; in addition to which, below the heaters there is a zone in which we have elevated saturations and elevated temperatures in which we can place metal coupons for the waste package material test, and so we are planning to put coupons below the heaters, as well as within the heater holes themselves to look at some of the corrosion.

We feel that we've got a good test site. Just as a point of reference, this is a fracture map at Fran Ridge. As you can see, there is certainly an adequate number of fractures. There is some question of whether they're perhaps too filled with carbonate materials, but we do have a number of different fracture types available. I was hoping that the shadow of the hard hat would be a little more plain here for scale, but that is a hard hat, to give you an idea of the scale.

As you can see, we've got some very linear and throughgoing fractures. We also have some smaller fractures which terminate on other fractures, in addition to which we have areas where the fracturing results in very intense shattering of the rock, and so we feel that we've got a good sampling of fracture characteristics at Fran Ridge.

The intention is to excavate the block by the use of a belt saw, pretty much a Sandia chain saw, except a belt, and Sandia's going to be assisting us with a lot of the excavation there, and that work is scheduled to start essentially the end of this month, early next month, and we will be putting a large test frame over the block. This is a cutaway view, this being the block, which will allow us, then, to control our boundary conditions. It does give us some problems, of course, with conduction of heat through that metal test frame, but it does allow us to apply stresses if we feel that we need to do that, and we can control the moisture conditions.

And I guess just as a final view graph, just to let you know that the conceptualization of the instrumentation is that we will have--and this is not all, but we will have access from three dimensions, so that we will be able to have a complete array of geophysical sensors. We will have ERT networks--this is only a single one showing the array--where we can do tomography. We'll have the thermocouples, and so forth, very similar to what we plan to do in the ESF and what was done in G-tunnel.

Thank you.

DR. CORDING: Questions?

DR. DOMENICO: Dale, in your experience in the G-tunnel or any other heater tests that you may have conducted, has the hole in which the heater was emplaced made water?

DR. WILDER: We tried to monitor whether the hole at G-tunnel would make water. We did have a moisture collection or monitor and a moisture collection system. The moisture collection system did collect some water, and what that was was basically a tube coming out of a packer, and we had a catchment basin and we condensed the water. We didn't get as much water coming back as the calculations would have shown that we would expect.

We also had a humidity gauge in the chamber. Unfortunately, that was one where we had serious corrosion problems, and I know Bill Clark's really fascinated with this. It was gold-coated, perhaps not as well as it should have been, but we lost our instruments, so we don't have any good information. We did get indication that water does come back in, though.

DR. DOMENICO: Do you have any idea whether it's fracture-induced, or, I mean, is it possible that you fractured the rock?

DR. WILDER: We didn't see any indication of fracturing of the rock. We have some direct--I should say indirect evidence from the spent fuel test. It was not at the same temperatures, and so it's not the best test, but we've looked for microfracturing at Climax and could never find any.

What we did after G-tunnel test was to go back and do permeability testing in all the boreholes that we had done testing prior to the test, and we saw in areas where we had existing fractures, there was an increase of permeability. We did not see any increase in permeability in the unfractured areas or the less-fractured zones.

DR. DOMENICO: Thank you.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: Dale, my sense is that what you're doing with the heater tests in blocks and in situ is trying to establish average conditions and average behavior. My appreciation from yesterday, or one of the things that I gained from yesterday was the concern that the heat pipes could be the fatal flaw in the whole system in terms of performance.

I would suspect, or I would feel--and I'd like your reaction to this--that putting heater tests at wet zones in fracture zones in the ESF would be more productive than putting it in average materials, because that's where the problems are going to be. Trying to identify zones of highest conductivity, and putting the test there in the mountain might be very constructive.

DR. WILDER: Okay, I appreciate that comment, and one thing that I didn't mention that I should have, our plan at the ESF was to do a suite of tests in the main test level, but we've also discussed and have some recognition on the part of the project that we may need to put small tests throughout the access drift to make sure that we are not in different hydrologic regimes, if you will.

The problem with trying to select a fracture, if we could find one that we knew was going to be the major conductive fracture, we could do the test there, but the problem is we haven't been very good or very successful as a profession in identifying which fracture is really going to be the one of concern.

And, therefore, I think that what we're really going to have to do is rely on these long-term performance confirmation tests or monitoring, where we've actually got the waste in place, and we're watching how things develop over a 100-year time frame to look at those 100-meter type scale processes, and that coupled with the natural analogues

Bo talked about, I think, are probably going to have to be our real primary way of addressing things like heat pipe.

We can address it on a local scale, but to really address whether or not we've got those largely-spaced features that could give us heat pipes, I don't think we can do with the heater test, although we will try if we can see a feature that we can put a heater test on.

DR. LANGMUIR: Isn't the most obvious place for them to develop going to be in fracture zones and fault zones that you can identify in the ESF?

DR. WILDER: I would agree, and we would certainly attempt to put a small heater test in one of the fault zones.

DR. CORDING: Board staff; Russ McFarland?

MR. McFARLAND: Russ McFarland, Board staff.

Dale, on your ESF test layout you indicate three parallel drifts and five parallel drifts. Your presentation was primarily on three drifts. Three drifts you feel would provide a minimum representation of the repository?

DR. WILDER: Once again, I was trying to go too fast, I can see. The three drifts on the outside were designed for the abbreviated tests, for the year and a half heater test and the cool-down test.

We feel that, yes, with some caveats, that that's probably adequate for the license application, and the caveat is we're going to have to compare with what we're getting out of the five-drift test in the early heating stages, and we'll certainly compare it with what we've observed at the large-block tests, but the intention is to separate the three drifts from the five-drift test so that we don't get interference, and we can use that for the license application data.

MR. McFARLAND: Now, in your layout of your drift and your diameter, you would be aiming at a high thermal loading. How would you handle the variations in thermal loading?

DR. WILDER: Actually, we've talked about a couple of variations, and the details aren't worked out yet, Russ, but one variation was to load at the high thermal at one end, and do lower thermal loading up at the other end so that we could look at both cases. The other would be to actually duplicate the tests and to do both high and low.

Right now, we don't know if we're going to be able to focus on a single thermal loading, and so it's kind of like the ESF testing was years ago, where we were looking at both vertical and horizontal emplacement modes. We're having to do the same thing with thermal loading, but the details aren't worked out, and so I can't really tell you what it'll end up being.

DR. CORDING: One last question. Leon?

DR. REITER: Dale, you got to answer a little bit of my concerns in the last few comments, but the concern is the question that I raised yesterday, was have you methodically thought out what are the critical hypotheses that needed to be looked at vis-a-vis the low thermal loading, and how long did that take?

I see the list of five hypotheses here. Yesterday I asked Dave, and Dave Stahl, I think, seemed to indicate he thought the main issues were geochemical. Some people in the hall seemed to say that DOE's ready to go at this point if they want to do a low thermal loading.

What are the critical issues about low thermal loading, and how long would it take to establish the issues? Is this just a duplicate of what you've said here?

DR. WILDER: It's pretty much the same kind of issues. There is one major difference. That is this buoyant gas convection, sub-boiling. That is a difficult one to monitor in the heater test, and Tom Buscheck probably can help me out on--I know he's been looking at the durations, but we feel that if we go to a below, sub-boiling case, we may actually have longer duration testing required in order to look at those buoyant convective conditions.

DR. REITER: Excuse me, but in the below boiling, are you concerned about drying out the condensation? Is that an issue in below boiling, also?

DR. WILDER: The dryout, per se, will still occur perhaps in local areas, but the real concern is that if we are below boiling, you can bring moisture from the saturated zone up above the repository and bring that rock to a higher saturation than you can even with the above boiling, because there the thermal breaks down the convective cells, so you don't get the water coming up from below.

And so, we've got to evaluate that process, and it's a slow process. That's why I say it is probably a longer test in the case of below boiling.

DR. REITER: So are these tests being planned?

DR. WILDER: The issues are being looked at. I can't say the tests are currently on the planning board.

MR. McFARLAND: Russ McFarland.

Kal, may I ask you a question?

DR. BHATTACHARYYA: Yes.

MR. McFARLAND: At the last meeting and at previous meetings on thermal loading, it's been brought out by various speakers that one of the issues of concern, edge effects on the repository, and that packages internal to the orthogonal layout or a different environment than was on the edge.

It's been postulated by several people that one way of addressing this would be in the design of the repository and the geometric layout of the arrays, and there was some comment said at the last meeting that perhaps an orthogonal layout is not in the best interest of trying to get uniform thermal--uniform temperatures across the block.

Are you doing anything in looking at this?

DR. BHATTACHARYYA: Yes, I'm cognizant of this concept. The best way to look at it would be, you know, I guess that you could come up with a form of a disc, as postulated by Lawrence Livermore, for example. It's kind of defeating the geometry that's available. One of the ways you could handle that is maybe put the waste packages first through the entry of the repository, and then push them inside once the outer edge is heated up, allowing enough heat there to cut the edge effect.

We recognize that, and we are looking at a concept that the repository is going to be either an ovaloid or an elliptical shape to cut down the major effect.

DR. CORDING: Thank you. We need to move on.

MR. GERTZ: Excuse me, Ed. I just need to answer one of Leon's questions, which was a little more programmatic, probably, and it also was to Dave Stahl.

I don't want to give anyone false impressions about the low thermal-loading concept. It is equal partner in all our thinking processes, but our tests are not laid out to do low thermal loading. Our tests are based upon the SCP, which was approved, commented on, and accepted by everybody, and if we change that, we will, but our entire test program is essentially based on the SCP at this time. While our thought process gives equal merit to all thermal loadings, 20 to 114.

the physical test program hasn't been changed at this time. If it has merit, we will change it in the future, but I wanted to make sure the equal partner thing didn't get any false expectations.

DR. CORDING: We have to move on. I said at some point I had hoped--perhaps it'll have to be after the meeting, but hope to talk with Dale more about disturbance around block tests and the sampling problem, and off-block tests, and disturbance around drifts in the underground. Those are the questions, I think, that are important in terms of the way this program will be set up, and I'd like to discuss those further.

But let's continue now with Dr. John Pott, who is at Sandia National Labs. He's been there with them for nine years. He's currently in the Yucca Mountain Project Performance Assessments Department and principal investigator and task leader. He's been working on 14 of the experiments for the in situ mechanical properties, thermomechanical properties, and the in situ design verification studies.

John?

DR. POTT: In contrast to what Dale has just talked about, I'm going to be talking about thermomechanical instead of the thermohydrological experiments, and I'm going to discuss these experiments and try to show how they tie into the different thermal issues.

I want to start out by discussing why we're doing these experiments in the exploratory studies facility, and there are several reasons. The first reason I have is to verify the fundamental model assumptions that have been made about the behavior of the rock, and that would be things like heat conduction, heat transfer is conduction-dominated.

Another purpose of these tests is to actually measure thermomechanical properties of the rock mass, so that would include things like thermal conductivity or deformation modules at the rock mass.

These tests are also coupled strongly with computer models, in that the data generated from these tests will be used to validate computer models, and then computer models can then be used to extend the results that we determine here to other geometries and configurations.

This does tie into hydrology somewhat, in that we want to look at stress-induced changes in fracture apertures, and so as the fracture aperture changes, the water that would flow through the fracture would also change.

Some additional objectives of the experiments I'm going to talk about, one is just to demonstrate the effects of the high temperature on rock, actually see that we know that what happens is what we expect. We also want to look at stability issues, and here I'm going to talk both about the stability of emplacement boreholes, if they're used, as well as stability of the drifts.

We can also use these tests to evaluate the effects of the thermal loads on ground support systems, and the ground support systems are things like rock bolts and wire mesh used to support the tunnel.

And, finally, if a repository is built, these tests can be used to confirm design concepts, because the ESF drifts will most likely look like repository drifts.

The approach to meeting these information needs is through a series of experiments, and these experiments increased both in complexity and size of the experiment, and what I'd like to do is talk about each of these four experiments in order in the following:

The first tests we've called the heated-block tests, and this is the simplest and the smallest of the tests, and what will be done is, in the floor of a drift, a block of rock will be isolated with these two-meter slots. Instrumentation will be then installed in the block. Heaters will be installed in two lines on opposite sides or the opposite faces of the block, and then, in addition, these flatjacks will be inserted into the slots in order to impose a mechanical load. So once the heaters and the flatjacks are in, they'll be cycled, pressures and temperatures will be imposed on the block, and the resulting temperatures, stresses, and displacements will be measured.

The reasons for doing this test: One is it gives us a chance to measure thermal and mechanical properties fairly straightforward; the particular ones would be things like the deformation modules and Poisson's Ratio, in addition to thermal properties. Because this is a fairly simple geometry, but still large enough to contain several joints, it will give us a chance to do some code validation work. Again, we'll also try to verify the basic physical models of how we think things will work, and because we can see whether these fractures open or close, it will give us some idea of how the fracture aperture changes due to stress and temperature.

The second test, increasing a little bit in size and complexity, we've called the canister-scale heated borehole experiment, and what I have--this is a schematic of the test, and what we have here is the drift, which we've drawn here as circular. It would be whatever the repository drift would look like. A borehole is then drilled downward and an electric heater is emplaced that would simulate the heat that would be generated from a waste canister. The rock surrounding this borehole would be instrumented to measure temperature, stress, displacement. The heater would be turned on, and then we'd look at the response of the rock surrounding the borehole due to this thermal load.

I have a plot, as planned currently. This shows the kind of temperatures we expect in the test. We have a line of symmetry here. In this analysis, the drift was modeled as a square, rectangular region. The heater lies in this region, and you can see that around the heater, at least, the boreholes would see 300°C, and in addition, there's a fairly large Region D here, large region here that's above 100°C. It extends about four meters away at the center of the heater.

Some notes on this experiment, the objectives, again, will give us another chance to measure some thermomechanical properties on a larger scale than in the previous test. It will give us a different geometry to use to validate the computer models. It will give us a direct evaluation of borehole stability; in other words, we'll have elevated temperatures and elevated stresses, and we'll be able to see directly how the borehole is stable, and if borehole emplacement were to be used, this would provide some design confirmation on the borehole.

A couple more comments. If, before this test is run, drift emplacement is chosen, in-drift emplacement is chosen as the method to be used, then we would seriously consider dropping this test. Because it has additional objectives, we would reconsider it. It's only objective is not borehole stability, but we would consider dropping it.

If, however, the decision is not made before this test is underway, this would give us a chance to evaluate borehole emplacement as a waste emplacement scheme.

This is sort of an interesting test. It looks pretty ugly. Let me try to describe what it is. This is the thermal stress test, the third experiment, and sort of this region in here, either circular or--because we recognize, maybe, that the tunnels in the ESF won't be circular--that shows the drift, and then there's a series of instrumentation which is installed, predominantly in the roof of the drift, and as well as lines of heaters will be installed, also, in the roof of the drift.

So what will be done in this experiment is the heaters will be turned on, and then, again, as you can see, we have temperature stress and different types of displacement measuring devices, and we'll monitor the response of the rock mass to that heat.

First of all, I'll show you some of the temperatures. As planned, these are the temperatures that are projected, and you can see that there right around the heaters themselves, temperatures exceed 400°C. Sort of in the crown here, above the crown of the drift, we have a large area that's exceeding 300°; in fact, the 280° isotherm, letter H, includes most of this area above the crown of the drift.

Some notes on this experiment, again, it will give us a chance to measure the thermal and mechanical properties. It will give us a different geometry and a bigger scale against which to validate computer models. Because we're heating the roof of the drift, it will allow us to evaluate drift stability. We're going to get very high temperatures and stresses up there where failure would occur.

It will also give us a chance to evaluate the effect of heat on the ground support, and seeing how well the ground support behaves in a heated drift. Because the temperatures and stresses we're trying to achieve in this test are so high, we're actually going up to a level where we think rock mass failure will occur, so this will give us a chance to investigate rock mass strength. And I make a note there, obviously, if that's true, the temperatures and the stresses will exceed what's expected in the repository.

The final test, the largest experiment that we have planned, we've called the heated-room experiment. This is a schematic of that. You can see the basic layout. There's a central drift, and then two parallel drifts on either side of it, and the rock mass in the region between the two--in between these drifts will be instrumented; temperature, stress, displacement, and then heaters will be installed, again, in the rock mass surrounding this central drift. So the heaters will be turned on and the response of the rock mass to this heat will be measured.

I have a plot of expected temperatures. Again, we have symmetry. This sort of first box in here is the drift. You can see where the high temperatures are the location of the heaters. Here's one of the two drifts parallel to the central drift, and you can see that, actually, if you look at the 300° isotherm, the entire region surrounding the central drift reaches a temperature of 300°C.

So, some of the notes on this experiment: First of all, again, the objectives. This will give us a chance, again, to evaluate drift stability. This drift will be modeled to look like an actual repository drift, so we'll get a chance to directly evaluate stability. We can also measure some rock mass thermomechanical properties. It gives us the biggest scale and the most complex test in which to compare against code predictions to do some computer code validation, and also, again, it will give us a second chance to evaluate how well ground support behaves under these elevated temperatures and stresses.

And a last note on this experiment is, as you may have noticed, it looks similar to one of Dale's experiments, where he had three parallel drifts, and we are working with him right now to integrate his tests and our tests; in other words, seeing whether we can't satisfy his objectives and our objectives both just using the single test.

This is just a quick summary, and what I've listed is the columns show each of the four tests, and then the rows, then, would be the different objectives that we're trying to accomplish, and you can see that all the objectives are met and that, in fact, each of the tests satisfies multiple objectives.

And then, just some conclusions to try to tie this into thermal issues. One of the things is that these tests will support evaluations of retrievability; first of all, by a direct demonstration or evaluation of drift and borehole stability, but also will validate computer codes, and then that can be used to predict stability and other geometries, other qualities of rock, things like that.

The information we will obtain as planned would be suitable for all potential thermal-loading scenarios. In other words, we're going up to high enough temperatures, that whatever scenario is chosen, we've included that in our set of data. And also, it will support either in-drift emplacement or borehole emplacement, whichever scheme is chosen, and it could actually help make a decision on which scheme to use.

That's all I have.

DR. CORDING: Thank you.

Your comment on integrating that with Lawrence Livermore's work is, it seems to me, very important, and you're doing some very similar things. You might have some differences in what you're looking at, but it seems to me the more you can integrate those tests together, the better off the project would be, and you can really measure this in both conditions, both the hydraulic and the mechanical conditions in the same tests, I would think.

DR. POTT: Yeah, I would strongly agree with that. I think Dale and us at Sandia very strongly realize that, so as we pointed out, we are involved in the, for example, cutting the block, so we keep apprised of each other's plans, so even if, you know, we want to get whatever data comes out of Livermore and then try to use it to our benefit, even if all the tests can't be directly integrated; in other words, piggyback on ones that are the same tests.

DR. CORDING: I think as much as possible, that would be desirable.

One question I do have is the surface of the rock is going to influence some of the stress conditions around the opening in the fracture, and the failure of the rock is going to be very much influenced by the way it's excavated, and it seems to me that consideration should be given to the TBM-type excavation of these test drifts. I know that takes some different types of layouts and planning, but that's something I think ought to be thought about.

MR. GERTZ: Yeah. I think, Ed, we are considering some type of mechanical excavation for all these activities right now.

DR. POTT: The idea has always been brought up, yes.

DR. CORDING: Other questions?

Don Langmuir.

DR. LANGMUIR: Langmuir, Board.

I'm just concerned, you're proceeding with the borehole approach to the heater tests, and how difficult would it be, and costly, just to reevaluate and start over again and go with a drift approach, which is really more likely to happen? It'd be more relevant. I realize it's an extrapolation to this, but still...

DR. POTT: Well, I think that's what we're trying to really accomplish with the heated-room experiment. In other words, in order to efficiently heat this rock mass, instead of putting the heater here, we're putting the heaters off to the side. I don't want you to think that we're real far along on these tests. I mean, we are proceeding along with them, but we're not in danger now of spending a lot of money to design a test that won't be fulfilled.

DR. CORDING: Other questions?

(No audible response.)

DR. CORDING: Thank you.

I'd like to go to our last presenter. He has two presentations to make, Mr. Robert Sandifer. He's with the M&O. He's currently Manager of the MGDS Development. He's been with Duke Power for 25 years as a design engineer, principal engineer, engineering manager.

His initial presentation is on ESF, Repository, Waste Package Design Integration, and then he'll continue with ESF changes under consideration.

MR. SANDIFER: Thank you, sir.

First, I'll briefly talk about the integration of the repository ACD, the waste package ACD, and the Title II ESF design, and then I'll spend most of my time talking about two changes that we have proposed which we feel, number one, demonstrates that we are integrating the three activities. Of course, we're interested in your reaction to these two changes, neither of which has been approved baseline. In fact, we don't have all of the data to present to the Yucca Mountain Project Office at this point.

We would view integration pretty much as we've drawn in this chart. You will notice that it clearly shows that lab testing and surface-based testing, and over on this side, the same thing interfaces directly with the ESF Title II design, the repository ACD and the waste package ACD. System studies, in a similar way, also interfaces with these designs, and then we've shown the interfaces between the three of these, and we plugged in the MPC design, because that also is just part of our effort.

The elements of how we integrate, if you will:

Communication and teamwork. We're co-located, if you will. We work together day-to-day. We share requirements. Technical baseline and change control, we certainly share those controls. We have design reviews and system studies that impact all three, and we certainly solicit each other's impact as we progress with our work.

The last chart on integration is difficult to read, and I apologize for that, but the idea here is to show you that we developed, some time ago, an integrated schedule, in this case, for the repository ACD, and it shows the elements, the system studies, the waste package elements, the repository elements, and the ESF elements, and it shows where the formal interfaces are.

I'd certainly hasten to add that there are interfaces on a daily basis, and I think you'll see, when I discuss these changes, that these did occur because we do have the right kind of communication and understanding on why we've got to integrate.

Again, the changes that I'm going to discuss are changes that are under consideration. Carl Gertz and his DOE office teammates were briefed with this about three weeks ago. He will, at the end of this, when I conclude, he will discuss some considerations that he has in view of these proposed changes.

I'll discuss these changes. First of all, I'll spend just a moment talking about design control. I think that's important, again, reminding all of us of the relationship between Title I, Title II, and the ACDs. I'll talk about an ESF reconfiguration that we have proposed, which Kal has already touched on. I will briefly talk about surface-based testing adjustments to support this reconfiguration, and finally, I'll talk about a north portal entrance redesign which we have proposed.

Managing design change. We've started on the left with the SCP, the conceptual design report, and we've shown the progression, at least in the case of the repository and the waste package designs, further study phases which interfaced, if you will, with Title I of the ESF design. Note that surface-based testing is continuously feeding information as we progress with the ESF design.

In October, we commenced design, if you will, on Package 1B and Package 2. Package 1A is the one that's currently being constructed in the field, and the ACDs commenced at the same time that, actually, the M&O commenced the Title II work on the 1B and Package 2. During that time period, there has been interfaces with the ACDs, and again, with the surface-based testing.

As I mentioned, Title I and Title II, by its own definition and nature, you would expect that Title II is a better version, it's a more refined, a more mature version of

Title I. The point is, you would expect change to occur between Title I and Title II as you get better information.

Well, what drives these changes that occur between Title I and Title II, or during Title II? Well, in the instance of the two changes we're talking about, or I'm about to talk about, the ACDs provided some of this information. Some of the information came from the surface-based testing program. In general, however, you may get comments from oversight groups, you may get new design information, or, for example, another instance would be a vendor may change your design. But at any rate, design change certainly occurs and is normal, and should not alarm anyone.

I will talk next about the ESF reconfiguration that we are proposing. Why do we need to adjust the ESF reconfiguration? I've given two reasons. Anything that occurs, any new information that's identified through studies or information from ACDs, whatever, we must incorporate, for example, if it enhances safety, or, for example, if it provides a big cost savings.

I've given two examples here which is specific to the instance that I'm going to talk about. We found that we could maximize the distance above the water table, that the recent drilling results confirmed that the Topopah Spring contact is higher at the north end of the block--and I'll talk about that further--that the Ghost Dance Fault is more significant, is wider than we had first thought, at least we feel like the potential is there, and, therefore, crossing the Ghost Dance Fault with emplacement drifts seems to be at higher risk than it was earlier.

And, second, anything that we can do to preserve the repository design flexibility, assuming the site is found suitable, we certainly are charged with that, if we come across new data and new information that can suggest we can do that.

Briefly, there is a link to previous work that's recognized. The end of the alternative studies was accounted for and the document provided the bridge between the selection of Option 30 and the slightly-modified reference design. It's understood that the evolution is going to take place is the point of that view graph.

Taking you back to a summary chart from the alternative studies, we certainly clearly selected Option 30 because we ranked it the highest. We could not check these two columns; that is, maximize distance from the emplacement level to the water table, and avoiding emplacement drifts crossing Ghost Dance Fault, we could not check this, either. What we are proposing here will provide the checks in both of these columns.

What does the new information provide? As I mentioned earlier, briefly, a higher TSw1-TSw2 contact in the north allows the development of a flatter layout, provides for, because of that, conventional rail haulage is certainly feasible, and it allows the distance from the emplacement area to the water table to be increased.

Clearly, there are concepts on the table in the waste package area that are going to be heavier than was first thought. Rail haulage, certainly, if we're going to go to those options, we need to have rail haulage as an option, and this certainly provides that opportunity.

The design provides a better opportunity for flexibility to deal with the characterization of the Ghost Dance Fault. Again, the latest data would suggest it is wider than we first thought.

How do we preserve the repository design flexibility? Well, ideally, you would develop an ESF configuration that can accommodate any underground repository layouts under consideration and transportation concepts, while accomplishing the objective of properly characterizing the site.

Obviously, the reason we're designing an ESF at this point is to build an underground laboratory to characterize the site. At the same time, we must consider repository requirements if the site is found suitable.

At any rate, we have developed a layout that, first of all, maintains the current portal location and the horizontal direction of the north ramp. It results in no grade in excess of 2.7 per cent. You will recall the current north ramp rate is 6.9 per cent. It maintains the full scope of site suitability and characterization provided by the current Option 30 design, and significantly enhances the characterization of the Ghost Dance Fault, without affecting the repository layout flexibility.

We believe that it preserves the repository design flexibility to a much greater extent than the current configuration, including concepts which increase the distance from the emplacement drift to the water table. It better accommodates repository layouts having flat emplacement drifts and layouts that seek to avoid having emplacement drifts crossing the Ghost Dance Fault.

The first view graph here is the current ESF layout. Of course, the Topopah Springs main drift, the Calico Hills main drift are superimposed on each other, with the Calico Hills drift below, and as I have said, there's a 6.9 per cent negative slope on the Topopah Springs north ramp.

One point I should make is that Topopah Springs main drift crosses the Ghost Dance Fault at an acute angle, and you can see in what we are proposing, the Topopah Springs main drift parallels, if you will, the Ghost Dance Fault, so this is the Topopah Springs main drift. We've deliberately not shown the Calico Hills drift for clarity. The 2.1 per cent slope at the north ramp, 2.6 per cent on the south ramp, it's essentially flat through here, and beginning to go up here.

And, for clarity purposes, we show here the Calico Hills ramp, which is basically in the same position it is in the current baseline concept. Again, we've shown the MTA here.

This shows the two superimposed on one another. The difference here is the approach on the Calico Hill. Ramp 1 is different because of the difference in the Topopah Springs drifts, but, again, this is the proposal, then this is the current baseline.

I'll briefly mention this. The point that we're trying to make here is that you are going to, with emplacement drifts, you're crossing the Ghost Dance Fault many times. If you look at, in this case, it's 75 kW/acre, where you'd only use the upper block. The lower block would not be required. Clearly, that's not the case now. We have the illuminated the problem with crossing here, as you'll see with the next view graph.

DR. ALLEN: Clarence Allen.

You have, in actuality, pressed that west border closer to the Solitario Canyon Fault, the repository itself, than in the current plan; just comparing this with the current plan?

MR. SANDIFER: Of course, the main purpose in our design effort here is to design an ESF that will maximize our characterization efforts. Certainly, this does that, and when we show you the lower level, or the upper level, the lower level, you will note that we would have a higher capacity there. If the point is that you could benefit from crossing here in some way, I guess the answer to that is yes.

DR. ALLEN: No, I'm speaking to the Solitario Canyon Fault.

MR. GERTZ: On the west side, where we come closer to Solitario Canyon than we did with our existing conceptual design, because that's all we've done, is got conceptual designs for both of them.

DR. ALLEN: And I see no reason why the Solitario Canyon Fault should be any less complicated than the Ghost Dance Fault.

MR. SANDIFER: I guess the point that we would make is it's due to the new information on the Ghost Dance Fault. We think, certainly, this concept at least ought to be considered. We think it makes a lot more sense. Again, if you go to the 60 kW/acre, it requires a lower block.

I will now show you a cross-section, A-A', which Kal, I believe, showed this. This is the upper block emplacement drift and the lower block emplacement drift with the Ghost Dance Fault here.

This, for clarification, shows the pork chop superimposed on the step block concept that we're talking about, which I believe if I understood your comments, certainly confirms what you were saying.

Leaving that one on, we'll take a cross-section of that particular layout, of the superimposed layout. What this does, the added information is that there's about 150-foot increase above the water table with this concept, at least on this end, and pretty much the same as far as the lower block is concerned. This, of course, is the current baseline for rock.

The advantages that we see with the enhanced layout, it enhances, in our opinion, site characterization ability. We can make Ghost Dance Fault crossings with relative ease. Two Solitario Canyon Fault crossings are planned instead of one, and the ramp extensions give a good look at a large percentage of the vertical extent of the TSw2 interval.

On this next view graph, I think the point we're making is if your Topopah Springs main drift is here, then access to the Ghost Dance Fault, you can access it wherever you like with relative ease.

It enhances repository design flexibility. With the new proposal, we preserve the option for conventional rail haulage, and I might add, conventional rail haulage is certainly helpful in the ESF, also, in the construction of it. It preserves the option to increase distance from emplacement drifts to water table, and it preserves the option to avoid the multiple crossings we talked about and it, in our judgment, allows consideration of more potential repository layouts.

As I've just mentioned, it enhances ESF constructability, flatter slopes improves the safety of our underground operations. The flatter slopes allow servicing the TBM with conventional rail haulage, and there's, in our judgment, minimal impact on the amount of ESF excavation, and, therefore, we could conclude, minimum impact on cost and schedule. This, by the way, is what we're currently refining and finalizing. We do not have that information in definitive enough form to make a final recommendation.

Technical and programmatic impacts, or disadvantages, whatever you choose to call them. It requires limited re-sequencing of surface-based testing program. It delays gathering of drillhole data regarding water table gradient and unsaturated zone conditions, and it requires a definitive understanding of Ghost Dance Fault prior to excavation of the Topopah Springs main drift. I will show you that in a view graph later.

Where are we with this particular proposal? Our intent at this point is to proceed with construction of the start tunnel at the reduced gradient, Package 2A; proceed with normal design review process. And back to this one, what this has done is given us a window of opportunity to allow a proper evaluation of this concept, and we can obviously--or, not obviously--we can reverse our course back to the current baseline if that's required as we do the evaluation.

Prepare impact analysis that defines changes to baseline cost and schedule. As I mentioned, we're doing that now. Present to the CCB, if approved by the CCB, proceed with the change to the technical baseline using normal change control processes, and DOE will report the changes to the program in the SCP semi-annual progress reports.

I'm going to talk next about the adjustments to the surface-based testing program. I have a chart which shows, again, the proposed layout, and, of course, a concentration of where we need the information is along the proposed Topopah Springs main drift. There's one hole that we propose to move. There are others where we are combining. Tom Statton is in the audience. If there's a need for a lot of questions concerning this, he can certainly make himself available.

Finally, I'm going to talk about a much less significant change, but we think, again, an indication that we are integrating the three efforts. We have taken a hard look at the north portal entrance design. We've done that because it is simply part of preparation to construct the north portal entrance. We have an opportunity, as we progress, to always look at cost and, if you will, safety, and in this instance we're saying that we think we have identified an opportunity to save some money, and not impact safety.

This particular view graph is not in your package, but I think it may be helpful to explain what we were proposing or what was in Package 1A. Package 1A had a multi-plate steel arch which we refer to as an Onco, because of the vendor relationship, and the idea was to backfill it so that you had a very natural, smooth, even slope down to the entrance, eliminating any safety hazards whatsoever. As a secondary benefit, it looked nicer.

The reason RSN did this in their design was because they did not have data on this high wall section. They could not satisfy themselves that it would be safe if they did not do this. We now know what the rock conditions are here, and we would propose something different.

Basically, what we're proposing is instead of, if you will--I'll put this back up over here--instead of installing this and backfilling, we simply reinforce the high wall, if you will, with shotcrete. We put in a concrete portal. We do the necessary seismic analysis to show that that's okay, and the end result is we would save money. It would certainly be less schedule-intensive.

The reason this is on the table and being considered is because we now have the information we need on the rock characteristics in the high wall. As I mentioned earlier, Carl is going to discuss some considerations that he has put on the table. Some of those, we feel like we've already addressed, but certainly I think it's beneficial to hear those.

That's all I have.

DR. CORDING: Thank you.

MR. GERTZ: I think more than once over the years that I've talked to the Board, I've reminded you that we operate in a very exacting regulatory environment, and while I've been a project manager on the civil works and just general construction projects, this is certainly different, and we do have to pay extensive attention to the regulatory environment. We operate it, so I'm going to offer you some thoughts that need to be considered before we proceed with this change.

On the whole, we think it's the right thing to do and it's the right change, and we're heading to do it. We just have to make sure we've met all the regulatory requirements before we move forward, because the past changes, the existing design have met most of those.

One, the orientation of the main drifts. We've turned them about 30°. Well, that's a little bit different orientation to the main stresses and the fracture and in situ stresses, so we have to address that, make sure we're okay.

Secondly, the upper boundary of the potential repository. We've talked about it before. One of the regulatory requirements is you're going to need 200 meters cover at the repository area. We have to make sure that we have 200 meters everywhere the repository is; fairly simple, but we have to do it.

One that's not quite as simple, difficult to analyze and establish, but is do waste isolation performance calculations. We must, in accordance with 10 CFR 60.21, and on and on, pay attention to those features that enhance waste isolation. We have to have good justification if we're going to use a feature that isn't better from a waste isolation point of view.

What I've pointed out, also--well, we pointed out some of the advantages in the 200 meters about being above the water table. One of the disadvantages, and you will see in our PA, is if you're looking at gaseous releases, we're closer to the surface, and that makes the Carbon-14 pathway a little shorter, so we have to make sure we understand that that's the right thing to do. Of course, we're addressing the Carbon-14 in many other areas, too.

One of our big issues with our regulator was with the minimum number of accesses. Our current concept only has four accesses, two ramps and two shafts in the conceptual design. When we look at alternate design features, if this would be used as a repository, do we add potential pathways or shafts?

We must assure that there's adequate east-west exploration. As you recall, current design had an east-west drift through the block. That was part of the discussions we had with you all. Now, we do not have an east-west drift through the block. We could add one, but we have east-west drifts on the edges of the block.

Relationship to the Ghost Dance Fault, we want to make sure that we have proper offset. We don't want to run a TBM down the fault. We want to run it parallel to it, obviously. We have nice opportunity, though, for adits to look at it in any amount of areas that we need, and in the southeast quadrant, we may need some more data before we develop that as a repository.

Most importantly, we need to know about test program implementations. Do the extensions make up for the east-west ramp? Is the core area access going to be difficult or costly? The test area now will be essentially in the lower block. We're going to have to run our first drift in the upper block, and how do we get to the lower block? We'll do it with ramps, and we'll do it with appropriate grading, but we need to look at it.

And the drifting in Topopah Springs may not be representative of the proposed emplacement profile in that we don't look at the entire emplacement profile, as we talked about. However, talking about representativeness, the other representativeness is with our test area here, even though that's in the same relative position in this strata of rock, it is lower than our emplacement area. We think we make a case that's not much different than the original one, but we need to sit down, analyze that, and make sure our regulator agrees with us.

East-west step. We've talked about thermal loading. Will that change anything? Will this create any thermal-loading perturbations on the lower block? We need to understand it before we rush to a change, and all changes to the concepts should be managed and reflect an effective design control process.

In this project, our design control process not only includes cost and schedule, but it also includes the effect of the changes on waste isolation and test-to-test interference, which are not usually done in any other civil works project.

So, we think we're heading in the right direction, it's the right thing to do. We just want to make sure our i's are dotted and the t's are crossed before we bring it into the baseline.

That's it for Bob and I.

DR. CORDING: Okay, thanks. I'm pleased to see these changes that are you considering, I think, that there's some real obvious advantages to lowering the gradients, and I think one question, one comment on the offset from that Ghost Dance is that there will probably be other joints and features parallel to it, the offset from that is going to be a question as to how you deal with that, and perhaps might even require some adjustments as you tunnel through there, so I think that it generally reduced the parallel structures to these faults, and I think that's an issue that we need to be looking at for the flexibility of the excavation process as you get down there and as you try to do any exploration before you finish your turn and start parallel to it.

MR. GERTZ: Ed, I'd like to point out that many of those considerations were discussed as the design process was going on, but I just wanted to make sure you on the Board knew that we had to get this all well-documented from a regulatory point of view before we brought it into our baseline.

DR. CORDING: One question, I guess, would be: Is the plan to have this in place before the TBM starts down? I noticed you were indicating that you presently would go with the starter tunnel. Would you have this in place before you start the TBM?

MR. GERTZ: Yes. Correct me if I'm wrong. The question is, yes, but I think the other day I might have went through it too fast, but Package 2A is only going to take us to the proximity of the Ghost Dance Fault, and it will be--Bow Ridge, excuse me, Ghost Dance over here; too many Ghost Dance--would take us to the proximity, maybe 375 feet additionally, and then I think my schedule I gave you yesterday was that the first of the year, we'd have the design review for 2C, which is all the way down here, which is the TBM activity.

And assuming we get the TBM in April, if we're 90 per cent done with the package in January, we should be ready to implement it with the TBM in the April-May time frame. So the simple answer is yes, and that's some of the details.

DR. CORDING: Other questions? Board staff?

DR. BARNARD: Bill Barnard, Board staff.

I have a question for Bob Sandifer. In the original, or I guess in the existing ESF design, the Topopah Springs main drift and the Calico Hills main drift run in parallel; in other words, the Calico Hills drift is directly below the Topopah Springs. Now, in the proposed it's changed.

Was there a reason why they were running in parallel in the current design?

MR. SANDIFER: The current design was a preference by both the program and the regulator, that that was the most appropriate place for Calico Hills drift, is directly under Topopah Springs drift. We have not looked at it in the new layout. We've simply not done any effort there as to whether it's still in the right place or not. We have merely accommodated where it was before.

DR. BARNARD: Okay. Thank you.

DR. CORDING: Russ McFarland?

MR. McFARLAND: Bob, another question. In your new layout, you're running at an extremely small angle to the drill hole wash, whatever it may be, the structure. If that should turn out to be a zone, a fault zone, aren't you in a rather difficult situation? You're almost parallel as you cross it.

MR. SANDIFER: Well, obviously, the main purpose for us getting in underground and tunneling is to find out what's down there. Based on the information we have today, this seems the best choice. Now, if the scenario is like you projected, we would certainly agree. We would have to make adjustments, just as we would expect to have to make adjustments as we go into the main drift, as was discussed earlier.

We will only have limited drillhole bore data available. When we do the Topopah Springs main drift, we would expect to change, the same with this one.

MR. McFARLAND: My point is you've laid it right on top, almost parallel, in your new realignment. Has that been taken into consideration in your new realignment?

MR. SANDIFER: Dana, can you address that?

MR. ROGERS: Dana Rogers with MK.

All we've done is extended the north ramp at the same azimuth that it was originally, and yes, the drill hole wash structure is a concern. It's something we've got to deal with, hopefully, before we start the TBM, but, if not, we could stop before we get to the structure, and probe ahead to see if we do have a potential concern.

MR. McFARLAND: You stop and probe ahead in lieu of perhaps swinging out slightly across at a more right angle?

MR. ROGERS: Well, at one point, you've got to cross it at any rate. I mean, if you're going to extend that north ramp, you're going to have to cross it at a bad angle unless you swung way off to the north to do it, so it's an issue that we've got to deal with, and we don't have the answer at this time, but it's something that's under consideration. Right now, we don't know anything about that structure.

DR. CORDING: Thank you.

Dr. Barnard?

DR. BARNARD: Bill Barnard, Board staff. I have a question for Carl.

If you decide to go with this enhanced ESF design, how long would you anticipate it will take to get the Change Control Board to approve it, and any other approvals that you'll have to get from NRC or anybody else?

MR. GERTZ: I think it'll meet the current schedules that we have planned. Our change control process, at the project level, we've processed, as you saw the other day, 300 field changes in the last nine months. This is a little more significant, but the process works pretty well as long as all the analyses are done and the backup's there.

So I anticipate after the 90 per cent design review in January, if this is the design we choose, we fine-tune that design, and between January and May there'd be plenty of time to get it implemented.

We will have interactions with the NRC. I don't anticipate any major issues with them, but there may be some that we haven't identified.

Once again, as Bob pointed out, this, we appear to be, to us, this is a natural evolution. We have not designed any part of the ESF except the first 200 feet. Ninety-eight per cent of the ESF is only in a conceptual or a Title I phase. What we're doing is evolving the rest of the design to build it by, so we look at this as more of a design evolution, and not as any major change to the activities we're doing. So, therefore, I don't anticipate major approval process stumbling blocks, but you never know.

DR. CORDING: Thanks very much.

We're behind schedule, but we're going to have a break for an hour for lunch, maybe a little less than an hour, one-thirty, and we'll be continuing the next part of the session.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. BREWER: As everyone is taking their seat, I have a couple of information items before we get on with the regular presentation.

Someone lost their room key.

The second informational item is to introduce members of the Yucca Mountain Project and others in the audience to our newest staff member on the Nuclear Waste Technical Review Board.

Let's say a few things about Dan Fehringer.

Dan, would you stand up or raise your hand? There he is.

Dan has long experience in the NRC and his specialty is in radiation protection, repository performance assessment, regulation development and the use of expert judgment in licensing. As you know, one of the working groups or panels of the Nuclear Waste Board has to do with environment and public health. If somebody noticed, this is my first year anniversary. Indeed, it was the Denver meeting one year ago that I showed up on the board. I'm almost to the point where I know maybe 10% of the acronyms and will be moving into action in years to come.

Dan, on the other hand, has a lifetime of knowing the acronyms related to nuclear waste and other nuclear programs. He's a fine addition to the staff and a guy that we're all looking forward to providing a great deal of good help, and welcome to the board and welcome to everyone for Dan.

The afternoon session departs to a certain extent from the norm for board meetings in that the subject matter is technical, because that's what we're supposed to be doing, but largely at my not insistence, but I keep asking the question so I guess it's insistence, I often ask of my esteemed colleagues on the board, other members, what does it all mean? I should note by way of background that that's an appropriate question for someone like myself because I'm not a hard rock person. I'm not a geochemist, I'm not an earthquake person, I'm not a tunnel boring person. I'm none of the above. In fact, until I met Linda Smith yesterday, I felt maybe like I didn't belong in this group. But now I found a soul mate of sorts.

I am primarily trained in business, in economics and mathematics and modeling, surprisingly enough, but of social and environmental systems as opposed to rocks. I was sitting there listening to a lot of things that were familiar yesterday but just a different noun in the subject place. As opposed to people, it was rocks and other perversions.

I am the dean of the School of Natural Resources and Environment at the University of Michigan. I've been there about two years. And prior to that I was in the School of Organization and Management and the School of Forestry and Environmental Studies at Yale for a period of about sixteen years. And when I had a real job, as opposed to being a university faculty member, I was at the Rand Corporation for about six working in strategic studies, model simulations and games. So my background is one that really is I think appropriate to ask what does it all mean and what is the big picture. And really, the format for this afternoon is to elicit kind of the larger setting or context in which the Yucca Mountain Project is proceeding. And I was interested to note, by way of just the subject matters of the presentations that will go on between now and the break, that we're going to be talking about basically the question of retrieval. There is an interesting discussion just given the materials in the handouts on the environment. And although the environmental impact statement process is not part of site characterization, which is something that I'm constantly reminded, it's hard to decouple the two because ultimately and somewhere down the road we got to be worried about the environmental impact statement, regulatory consequences and requirements just as much as we are about transport of nuclides through rocks.

We're also going to be concerned with total systems performance, and, by definition, this should be how the pieces all relate to the big picture. And then finally, some consideration for human health, human health and risk, and experts and expertise.

We are running late and let me stop at this point and introduce our first speaker for the afternoon. If you'll note, everything is basically accelerated by thirty minutes. We are supposedly starting at 1:15. We're starting at 1:45 and that's how the schedule will be adjusted.

Eugene Roseboom is our first speaker, speaking on extended retrievability. Dr. Roseboom has a bachelor's and master's degree in geology from Ohio State. They have occasionally a good football team, not a great football team, and he has his Ph.D. from Harvard, which is a good university but a guy who's a Yale, it's questionable. He has had good employment, however. He's been with the USGS his entire career, as well as I could tell, starting in 1959, and he is currently on the staff of the director of the USGS for whom he has oversight and overview for the surveys work for the DOE.

Eugene Roseboom, if you would, sir.

DR. ROSEBOOM: The topic, as Garry just mentioned, is extended retrievability. In this program, of course, you're going to need an acronym, so here's a new one for Garry. Fortunately, Larry Ramspott coined the term underground retrievable storage a few years ago. So we have a very useful acronym, URS, which is handy because it's the same as MRS except you substitute a U for underground. So it makes it relatively easy to remember.

What do I mean by extended retrievability? And I would say that basically it's keeping the repository open almost indefinitely instead of as usually the case is site--plan, the assumption that the repository will be finally closed sometime in the next century after all the waste has been in place and suitable tests have been done to show we're performing satisfactorily. At that point it would be closed. But there's a real question, I think, as to why should it be closed, particularly if it's not going to particularly affect the performance of the repository? I think maybe this is a subject that may at least at last have reached a timely point since the Technical Review Board mentioned the desirability of further examining retrievability in its special report, and also DOE and the recent strategic plan also mentioned retrievability.

So what I would like to do would be to start with the broad picture of what are the benefits of a URS, underground retrievable storage, then look at some background, some history and some regulatory aspects, the subject of backfill, which is rather critical because if you shove backfill in all your tunnels, you end retrievability right there, unless you're prepared to go in and mine it back out. And then finally, the subject we're looking at today, being relatively high thermal loadings, how could that be accommodated in URS.

Starting with the really big picture, at the present time we really have a relatively limited choice between either interim surface storage either at reactors or in monitored retrievable storage facilities, or in a repository. The other options like deep sea disposal are postponed indefinitely and so forth. So we really have a choice between those two.

Now, some of the arguments that one hears against a repository, the one that most common is, we got an awful lot of uncertainties, what with all the testing and so forth. If something goes wrong, it's going to be difficult to remove the waste. And that, of course, assumes that you're going to have some kind of final closure with backfilling and sealing of the repository. And as a URS doesn't require backfill, then the waste remains retrievable and you've done nothing that cannot be reversed.

The second argument is that surface storage will do fine until better solutions are found. Of course, this is a very optimistic viewpoint. It assumes that better solutions will be developed, and it also assumes that if such solutions develop, society and resources will be there to carry out those solutions. And if one starts looking fifty to a hundred years ahead, that could be a fairly questionable assumption. So that surface storage could become disposal by default, and a number of people have commented on this recently, especially in states where the waste is being stored.

Okay. So comparing underground retrievable storage with surface storage in case of some sort of societal breakdown, which of course we hope won't happen, but this could be--well, it's only a short time ago we were worried about nuclear war, which would certainly have been a major breakdown. There are other things that can happen; revolutions, civil wars. Hopefully none of those, but still, abandoning surface storage could be a very serious matter in the long run for the environment.

On the other hand, abandonment of a URS would have little in the way of consequences. It essentially provides a fail-safe storage. And so if it were abandoned, you would be in a far better situation than the first case. And also, material placed in the URS is certainly safe from bombs or missiles and terrorist attacks, of which we've had a few examples of late in this country.

Now then, compare the URS with essentially a conventional repository, or what we've usually been thinking in the past. The benefits are, of course, you can run the monitoring for as long a period as necessary to see whether the models predicting the performance of the repository are really being born out by facts. You can change the canisters if better ones develop, or if after say a couple hundred years it looks like the original ones were not doing as well as we thought or maybe shorter periods, they could be replaced. If something new with the site develops or a part of the site that was not considered, again you remove the waste. And, of course, you retain the option if things proceed and our technological solutions develop in the future, then those could be employed because the waste can be readily removed.

And finally, of course, as frequently mentioned, you could use the spent fuel in reactors in the future.

So, I think this although essentially sums up the view, and that was of course thinking nuclear waste disposal. So those are the real choices that we face at the present time.

Now, one thing that needs to be remembered is that a URS is possible only in the unsaturated zone. The whole assortment of repository sites that were looked at previously would not really be suitable for being kept open indefinitely. And the reason it's possible is that basically the tunnels will remain dry without pumping. Now, many tunnels in perhaps tight rock, like granite, would require minimal pumping. But still, if you abandon them, eventually they would flood.

Backfill to reduce contact with ground water is essentially in saturated zone sites, but in the unsaturated zone it's not needed, and we'll look at that further.

Sealing of shafts is not really necessary. The amount of water that would come in from the surface in a desert region is very limited. If the shafts extend locally at their base below the level of the tunnel, the water will simply bypass the tunnels and drain on out. This is commonly done in mines, to extend the shafts that way.

The tunnels should in an unsaturated zone repository should remain open pretty much indefinitely, whereas, for instance, in the case of salt, you're relying on the slow flow of salt to close the tunnels. In fact, at WIPP we may see which creeps faster, the legal process or the movement of salt.

Okay. So briefly let's look at the history of repositories and the appearance of unsaturated zone and ideas on retrievable storage, and this is rather like the kind of evolutionary diagrams that paleontologists draw up with the origin over on that side and then the certain species becoming extinct as you get over here. The salt deposits, of course, were the earliest and most primitive species of this variety, and they were all descended from Lyons, Kansas. And, of course, they became very prolific at one point there when the Nuclear Waste Policy Act was passed. We had seven possible salt sites that were under consideration. At the same time that the DOE reservations at Hanford began being considered, the Basalt being post-rock at Hanford and the Nevada test site there were several different hosts available. It was only after Yucca Maintain, which was the tuff site, had turned out was not succeeding and was, in fact, the last site on the NTS. In fact, it's half off NTS now if you look at the map. It was only then that the unsaturated zone was considered.

The crystalline rock repositories in the east of course were the creation of the Nuclear Waste Policy Act which occurred right about in here also. We'll look at some details of that in a minute. And that was a new species of site. Then there was the great extinction event in 1987 when a congressionally mandated asteroid destroyed all of the sites in those states that had large congressional congregations. So it's only through a couple fortunate circumstances that we've ended up down here with an unsaturated zone site and persisting to the present.

Now, the ideas on retrievable disposal go back to Ike Winograd's 1974 paper in EOS where he first proposed use of the unsaturated zone. Most of that paper is considered shallow burial in pits or in shallow boreholes, and then we'll look at some of the others as we come along.

So basically, at this point, just to summarize, Yucca Mountain, as I say, was the last saturated zone site and NTS, it was failing in that case and it happened to have potential as an unsaturated site. So that's why it succeeded. But there never has been a screening for unsaturated sites, so there undoubtedly are more of them around should it become necessary to look for additional ones. The closest to such a screening was a study that the USGS carried out in the early 80s evaluating the entire basin range for potential areas, and that included all types of repositories; saturated and unsaturated. And most of the basin range contains fair thicknesses of unsaturated zones. And also adjacent areas probably contain such sites. This is something that needs to be considered in the event we start worrying about backup or contingency plans, I think.

This is essentially just a summary of the papers discussing extended retrievability and those are there in your handout as reference.

Perhaps the most interesting one was the proposal by Phillip Hammond that was made in the American Scientist, and basically the idea was simply a tunnel into the mountain with a shaft as a chimney. Of course, this is to try to divert rainwater. And the canisters were handled remotely and it was intended as a permanently facility. Now, if you take that design and simply change those canisters and racks into multi-purpose canisters in appropriate conveyances, you have a simplified model of essentially what we're looking at today. So that this is really the forerunner of the URS and needs only that kind of modification but in long-term canisters that could be used for final disposal and make the transition.

It's interesting to look at the details of some of the chronology that we looked at on our extinction diagram in that a lot of things happen in a very short time period, and in fact, the purpose of this is that there's a lot of saturated zone thinking that is carried over into the regulations and I think needs to be considered. In mid-1981, NRC proposed technical criteria for saturated zone repositories, and it was only six months after that that the

USGS proposed the first unsaturated site, Yucca Mountain, and shortly after that, six months later, DOE shifted to the unsaturated zone. Another six months and the Nuclear Waste Policy Act was signed, and that set up nine sites in competition with one another, only one of them being an unsaturated site, and that one being a recent conversion. So at the same time and shortly after that, the guidelines of course came out, so that emphasized features in saturated zone sites and retrievability never even made the list.

The final version of the technical criteria for 10 CFR 60 came out shortly later, and at that point they acknowledged that unsaturated zones might exist and so we'll need to modify them. To try to help them in their modifications, I wrote a circular 903 which was primarily intended to explain to the NRC and anyone else who might be interested how an unsaturated zone site might be different or would be different from a saturated zone site and therefore considerations one might want to look at in revising the regulations. And very soon after that came out, I passed the draft under the table to appropriate people. The final rules for the unsaturated zone came out and then they proposed the further ones and the final ones came out.

But even though there were a number of changes made, there's still a lot of saturated zone thinking that remain in the regulations because most of the changes that were made were those that would be considered in looking at an unsaturated site, not in changes that would be made with respect to building a repository or other aspects. So, they still contain in various parts of 10 CFR 60 an assumption of final closure of the repository, an assumption of backfilling keeps appearing, concerns over carefully sealing shafts and boreholes, which of course is very important in the saturated zone but not particularly important in the unsaturated zone. And also reference to the containment period lasting for the first few hundred years. So, they were locked into a particular view at that time and we'll look at some of this in a moment.

On the other hand, the NRC liked retrievability in the NRC's first version of 10 CFR 60. In fact, NRC proposed a fifty year period of retrievability beyond final waste emplacement. But after comments came in, they realized that this wasn't practical and so they had to--a lot of this is simply because you have to recognize the realities of putting waste in salt and the possibility of keeping it over. So that while they like the idea of retrievability, they had to recognize that parts of a salt repository might have to be backfilled well in advance of any final closure. So that to some extent, retrievability became a theoretical matter and would simply have to be treated as a possibility.

On the other hand, here's an example. As you can see in their definitions, permanent closure means final backfilling of the underground facility. Now, that doesn't allow for omission of the backfilling if it's in the unsaturated zone. It's a pretty straight forward statement. If you were looking at it, you would figure, okay, they mean backfill it.

Also permanent closure. There are sections devoted to permanent closure. Of course, we could put off permanent closure indefinitely, would be one way to handle that.

Looking at backfill, they did recognize in the NUREG 1046, which summarizes the changes to be made in the unsaturated zone and the regulations for the unsaturated zone, that a repository in the unsaturated zone would more likely be more accessible. They wouldn't quite come out and say backfill might be omitted, so they cited my circular and said that a plan similar to that discussed by Roseboom might be easier to gain access to the waste packages. Well, what they're referring to is this. They wouldn't quite come out and say backfill is omitted.

The NRC requirements for backfill are quite general and so the argument can be made that, okay, if no backfill assists the geologic setting in meeting long-term performance objectives as well as it might with air circulating through it and removing moisture, then air backfill is sufficient.

Let's just look at backfill in an unsaturated zone repository. It has some favorable aspects and some unfavorable. It, of course, tends to protect the canisters from rock falls from the roof of tunnels; helps to support the tunnels and if you want to keep the heat in emplacement tunnels, it certainly will do that. On the other hand, it pretty much ends any easy retrievable of waste and monitoring at least until the sensors burn out or some such, you could continue briefly, and so forth.

In terms of immediate protection to the canisters, this is sort of repository 101 design. If you don't have backfill and a fault occurs, you might tilt the canisters around a bit but you're not going to cut them in the cutters generated by the fault and until you have several feet of displacement. On the other hand, if you have it tightly backfilled, some of that displacement might be transmitted directly to the canisters. Rockfall, on the other hand, presents a more serious problem.

Let's look at 10 CFR 60 and its general view of engineered barriers versus natural barriers. I think you're all familiar with this. The engineered barriers are essentially the canister and the buffer. They're concerned about the near field environment. It meant to be boiling less than 1000 years, but my computer screwed it up. And a relatively small disturbed zone. I believe this was the conventional thinking at the time. And the natural barriers, sorptive minerals, far field, boiling wouldn't reach out that far, and relatively undisturbed, so that you could, from studying the natural situation at the present time, you could have a pretty good idea of maybe how it would perform in the future.

Okay. Now we have this new beast, the extended dry concept, which comes in. And all of a sudden, instead of a nice material barrier, we have a thermal barrier out there, essentially an energy barrier rather than a canister. Certainly the examples we've seen run far beyond the near field and well out into affecting the rest of the mountain. Affects may run for thousands of years and you're clearly going to have a very large thermally disturbed zone at least. So I would feel in view of these considerations, that maybe while the National Academy is looking at EPA's regulations, it might be a good idea if they look at 10 CFR 60 on some of these. I think it may have gotten out of date with some of the concepts that we're considering here.

Okay. Of course if you want to keep heat in emplacement tunnels and away from the access tunnels, bulkheads of some kind could well do the job. Also hearing the discussion of the thermal activity, they could also serve as safety valves, in which case they would blowout before anything appeared on the top of the mountain. One could put pressure gauges in them and see how much pressure developed.

And this particular slide we've already had the discussion of Danko and this was a very interesting paper in the last international meeting, and the conclusions are that even though you keep an emplacement sealed up for fifty years, you could open it up and within a few months get complete accessibility to the waste, which is very encouraging.

And some final thoughts here on thinking--and basically the changes would be more in the way of goals. You're thinking for the very long term instead of, say, for eighty years. So a transportation system; do you leave the containers on the carriages? It certainly is appealing to have a repository that would be essentially an underground railroad switching yard. It would certainly maximize the retrievability, but then there would be questions; how long would rails and ties last under the drift conditions? You'd have to take special consideration there because it's going to be even worse than Washington over the last couple of weeks.

There's a certain appeal I think in the very long term to a rubber tired system because if you still had the appropriate equipment, you could get in there even though the rail system had deteriorated.

And, of course, with tunnels, the general rule is the smaller diameter of the opening, the more stable it is. So that certainly needs to be considered. And you also reduce the distance for rocks to fall on the canisters.

Bulkheads would raise new questions. How long will they last? And then you have to look at the performance assessment after if the bulkheads go at sometime in the future, and so forth.

So anyway, those are some thoughts along the lines that need to be considered if you're looking for a really long-term retrieval.

DR. BREWER: Okay. Thank you very much.

Are there questions from the board?

(No response.)

Our next presenter is Kent Ostler. Kent is process and plan ecologist. He is with EG&G Energy Measurements currently managing the reclamation and litigation section for the Environmental Studies Department at EG&G. He's involved in site characterization of the Yucca Mountain. He has long experience in the area of environmental impact statements and basically restoration remediation.

Kent? His presentation is Desert Ecosystem Water Dynamics Under Various Thermal Scenarios.

DR. OSTLER: I'm very grateful to be here today to talk to you about a subject that I think is very important and one that's often overlooked when we consider thermal issues. I know in the last day and a half you've heard a lot about the subsurface impacts of thermal loading, what it's going to do to the geology, redesign those kind of components. But what I want to talk today about is those that relate to the surface, particularly the impacts related to the biological resources that exist on that surface.

To me, that's particularly important because we're in an area, Yucca Mountain, that's already thermally stressed, the ecosystems that exist there, and increase in temperatures, a significant amount may cause serious impacts upon those ecosystems.

Let me just start with the present outline of what I want to present today, talking about kind of putting borders, eliminate the characteristic that we anticipate that will occur at the surface, talk about the significance of those impacts, talk a little bit about what we know about the magnitudes of change on existing systems, and some of the uncertainties, things that we don't know. And then present some research ideas that may resolve some of those uncertainties and provide some conclusion.

Again, this talk was taken from one that I gave about two years to the Technical Review Board. Since many of you are new here, they asked me to do it again.

The figures that are presented here are really based on the models developed at that time from Sandia. What we were looking at as the most probable increase in the surface soil temperature was from a 1.0 to 1.5°C increase. Maximum temperatures would be anticipated to be less than 6°C. And then those temperature increases would be over a surface area of about 2.3 to 3 square miles, roughly 1,500 to 1,700 acres. That temperature increase would begin about a thousand years after initial emplacement, would peak between two and three thousand years, and then would decline after that.

Now, those simple models predict--they're developed based on a uniform surface and kind of a uniform substrata which model the heat flow through it. I think we all know that that isn't what exists at Yucca Mountain, that there's going to be fractures. So I think we can reasonably anticipate that there will be areas where there's canyons that run through that block where we have fissures, where we may see temperatures certainly up around 6°C.

Then talk about what is the significance of those increases. I think from a biological standpoint one would see very minimal impacts at anything less than 2°C. I think we see that kind of variability that exists just on a yearly basis, and I'll show you some data to demonstrate that.

I think we would anticipate either moderate or light impacts when you get from 2° to 6°C or above. And what those problems areas would be principally would be related to altering the water balance that exists at Yucca Mountain. They could also alter the timing of the biological processes. I'm going to discuss each one of those in a little more detail.

Finally, those two could certainly lead to destabilization of the ecosystem that exists.

I want to just talk first then about altering the water-mass balance. If any of you have been to Yucca Mountain, you know, we're in a very dry area to begin with. We have average around four inches of precipitation a year, some years we don't get any. So the plants are already under water stress through a major part of the year. By increasing the soil temperatures, we anticipate that we would see increased evaporation from those soils. We'd also see increased transpiration from the plants trying to cool themselves. What that would lead to then would be less available water for the plants to complete their biological processes.

One of the other things that certainly could happen with an increase in thermal loading is altering the timing of some biological processes. Many species use environmental cues in order to initiate certain phases of their life cycle. For example, desert tortoise use burrow temperatures as a key to emerge from hibernation. Many plants will use soil temperatures as well as moisture to initiate the germination and growth. And what you do when you change those soil temperatures then is you'll cause those processes to be initiated too early and the air temperatures are not tracking the same as the soil temperature would be. So you could get a plant that may grow and may initiate germination, and once it gets up to the surface, it'll find out that it's still winter when it gets there. Some of the data that we have indicates that, for example, pocket mice, for every degree centigrade increase in soil temperature, they will emerge from hibernation a week earlier. So with the 6° increase in temperature then, you're talking about instead of emerging from hibernation on April 1st, they're emerging in the middle of February. Well, conditions there may not be suitable for that species to survive.

There may also not be sufficient time to complete their life cycle processes. I think that we anticipate happening is that the plant and animals will tend to shift their life cycle more towards a winter time period. What that will do is create a longer period of summer dormancy or aestivation where the plants and animals will have to be using resources to get through that critical summer period. And if you extend that period too long, there certainly will be species that will not be able to survive it.

Also by pushing those cycles toward the winter period, you also will be experiencing a darker photo period, thus there's less light available for photosynthesis and bio-mass production. What that will tend to do then is to reduce the resources that are available for animals that are consuming those plants.

What this tends to do then is to destabilize the ecosystem that exists there, especially when you're already in an area where many of the species are on the edge. They've made some tremendous adaptations to survive that very harsh environment. By increasing temperatures, decreasing water, we may see many of those species unable to survive that.

I think we have data to show that the drought in the late 80s on Yucca Mountain did cause some very significant losses of certainly individuals from the vegetation associations that exist. We saw anywhere from 70%

to 90% of the individuals of certain species completely die out, leaving a lowered vegetation association as a result of that drought period.

In addition to temperature increases, we may also see other detrimental processes enhance, one of which with increased temperatures we're going to see a more rapid cycling or decomposition of organic matter which is a very important component of water holding capacity in soil. We also may experience soil micro organisms that cannot tolerate temperature increases, particularly the mycorrhizae which many of the native shrubs out there are dependent upon this relationship that exists between the mycorrhizae and the vascular plants. We also may see enhanced soil pathogens or insect pests result from that.

So what do we currently know about the environment and about increases in temperature and their affects on plants and animals? I think there is certainly abundant literature available as it relates to increased soil temperatures. Most of that relates to agricultural or horticultural crops. And there's numerous studies done as well on the effects of reduced soil moisture and the decrease in bio-mass production from less soil moisture available to plants. There is less information out there available on the interaction between temperature and soil moisture.

I think the most important thing is that there really is almost no site specific information on the species at Yucca Mountain themselves, so while we have this great bank of knowledge on how we anticipate species are going to change, we don't know what those absolute changes will be for the species in question on our side.

What we do know, we look at the current environment that exists at Yucca Mountain. We can get certainly some clues to assess that natural variability and the impacts of increase in temperature. Certainly from a regional standpoint we see significant variability, and let me just show you one chart that is very typical of this kind of data available. Here's a chart for six years in the Great Basin that looks at soil temperature at three different depths. You can see that generally the curves are very similar but there's a real seasonal difference, seasonal fluctuation in temperature. There's also differences in temperatures from year to year, from '67 to '69 for example, that are in the range of 5°C easily.

Let me provide you some information that comes from Yucca Mountain from the work that we're doing on the terrestrial ecosystems program there. We've identified four vegetation associations that occur at Yucca Mountain and we have soil temperature data from those, and these figures, January and August temperatures, represent needs of twelve ecological study plots for each one of those. And you can see within the four vegetation associations that there is for January '91 anyway, there is a 1½° difference that exists naturally between those. You can also see that the range of temperatures here, and these ranges are different ecological study plots, and there's anywhere from a 2° to 4° difference between those study plots. And again, those have the same vegetation association that exists there. So we know that within that system there is flexibility for easily a 2° to 3° temperature change and doesn't even change the vegetation association.

Here again, the very last column compares September '90 versus September '91. Again, within those two years. So on a yearly basis we can see that there's a fluctuation of 1° to almost 2.8°.

There's some new data that we just put together this year and it's not in your handout, but it provides us three years worth of data for January temperatures. Again, these are means from those same twelve ESPs in each one of the vegetation associations.

Now, what it provides you, if you look at the differences then between the two maximum, the two years that are farthest apart, '91 and '92, you can see that for winter temperatures there's anywhere from a 2° to little over 3° temperature difference.

I think the more critical thing to evaluate is the summer temperatures here. Those are lot more uniform. We're only looking at, you know, .3 to a 1, and that is really going to be the time period that will be more critical to the species.

The other thing that we can do is to look at natural analogues that exist out there, areas where we have geothermal heating. Unfortunately there's not many of those that occur near the Yucca Mountain area. Often when they do occur, they're often associated with additional soil moisture.

I present here some data from White in 1978, work that he did up on the Yellowstone looking at impacts of thermal loading on Lodgepole Pine. And he identified three different zones and those refer to the visual impact that he could assess on the Lodgepole Pine itself. So under normal--what he classified normal Lodgepole Pine, the increase in surface temperatures went from 2 to 8 lots per meter square. And then as you got above that, from 9 to 14, you started seeing some--they call it mixing, some stunting of a not normal growth, and final some extreme stunting, anything above 20.

What is more important is his analysis there of the real critical factor that was influencing plant stunting in that environment. Again, this is a colder environment than what we have at Yucca Mountain, but he concludes that it's not so much the actual heat flow as the seasonal maximum soil temperatures that exist. On Yucca Mountain, we're already at a very high seasonable maximum temperature and increasing that certainly could have an impact on species.

So I think we do have a fair knowledge of the systems out there and the direction that they may take with increased thermal loading. Some of the uncertainties, things that we don't know, are really the magnitude of those changes as they relate to species processes and even more so in ecosystem processes. We suspect that there's going to be a change in phenology or the activity periods. We don't know what the magnitude of that change would be. We think there will be a decrease in bio-mass production, food resources available to the animals. We don't know what exactly that magnitude of that change would be.

And as far as ecosystem processes go, we suspect that there will be species lost from that ecosystem. I think we have some data now that we can start looking at what impact that has on the ecosystem, but we really at this point in time don't have a good feel for what that interaction would be or the significance of the loss of one or five species from an ecosystem. Whether you'll have new species come in or whether the other species will just expand their niches to fill up those roles.

And probably the biggest uncertainty is our limited information that is site specific for Yucca Mountain.

Now, there are many things that we can do to resolve some of those uncertainties. We can go out and measure ecosystems along latitudinal or elevational gradients to try to simulate what an increase in 6° would have. We could look at geothermal areas and try to restrict those to more desert areas that are typical of Yucca Mountain. We could initiate greenhouse studies on the species that exist at Yucca Mountain or put in even small field trials and then artificially increase temperatures underneath those. And then finally, we can take that information and feed it into models that currently exist or improve those models.

I think what's really unique here is we see a natural variability and certainly the temperature out there, but what's going to be different is that the heat source is now going to be coming from the bottom of the soil rather from the top, so that is certainly something that is very unique and most animals that get through the summer period at Yucca Mountain often times do so by using the soil as a shield and they will burrow into it and stay underground during the day and come out at night. Now if you increase that soil temperature, they may have to use greater resources to survive that period or not be able to survive.

Finally, some residual uncertainties that probably can't be addressed would be the secondary impacts of loss of species, some of the evolutionary scale affects. And then to climate change which couldn't be addressed. Climate changes within that next three thousand years: heat may also increase causing even greater stress.

Let me just conclude then with these statements. I think that high thermal loading certainly would have an impact on biological resources. The significance of that impact certainly depends upon what that surface temperature increase is going to be. We think that 1° to 2°C would have minimal impacts, something that already exists within the natural variability of the system. As I said, it's going to be kind of a different heat source this time, so that may surprise us. High levels above 2°C certainly may influence or may cause some species to drop out of that ecosystem. We may see either a loss of vegetation types or even a shift in vegetation associations, and perhaps a destabilization of the system. But my experience has shown that biological systems really do have a tolerance for change, that's the reason they've existed and able to get through the climate changes that's occurred on this earth. It's the speed at which this one will occur is something that is unknown.

There certainly are some uncertainties that exist that we don't know the magnitude of those changes that will occur and we can answer many of those questions, I think, with the research program, and that's it.

DR. BREWER: Thank you very much.

Are there questions from members of the board?

John Cantlon.

DR. CANTLON: John Cantlon. You mentioned and really put most of the emphasis on the surface temperature difference, but you mentioned earlier in your discussion that it is really the root profile and the below ground temperatures for germination and mycorrhizael relationships and so on occur, and in that environment you have many of the species that are fractured roots that will in fact have roots down in what are apt to be the heat place where we can almost anticipate the bigger temperature--are you doing anything or planning any experiments to begin to look at that set of issues?

DR. OSTLER: We are not doing anything currently on that. We just have not directed the program for that. We have really been looking at site characterization impacts at this point in time in our program. So here's the data that relates to soil temperatures here are part of site characterization program rather than, you know, thermal-loading issues. But you're right, and we expect to see much higher temperatures in those cracks and I think those are going to be the hardest hit areas.

DR. CANTLON: So the two ways to get in, one of course would be design of a set of experiments that could be set up either in the greenhouse or more preferably the field. The other one would be to really look more carefully at some of those natural analogues. White also did some studies in the Hot Springs, Steamboat Springs area just outside of Reno where you are in a somewhat nearer environmental set of issues, environmental similarities. You might want to look up and see if there are any data floating around from that, or even maybe do some looks yourself.

Do you have any idea what the watt, square meter you're anticipating there? What are the numbers?

DR. OSTLER: I don't know. I think we're running around six to ten, but that's a guess.

DR. CANTLON: Then it's right at that critical point; 20 you're in real serious trouble, and 6 to 10--

DR. OSTLER: Yeah. I don't think we're near that. That's going to depend upon what your initial loading is as well.

DR. CANTLON: And you put your finger on the other thing that I think is important, and that is it is temperature rhythms that are the environmental cues that most of these species work on. And that's particularly important in the mycorrhizae relationships of desert shrubs, where they are obligatory mycorrhizae species.

DR. BREWER: Other questions from the board?

(No response.)

I had one. You mentioned climate change. Is there any consideration to elevate the CO_2s with respect to the temperature ranges that you're thinking about? Because elevated CO_2s, it's happening and it certainly happened within the period of time that we're talking about here in terms of either greenhouse studies or field studies. Is there any consideration given to that?

DR. OSTLER: We haven't looked at it at this point in time. That'd be something that we certainly would want to consider, you know, if we get into a research program on what those impacts would be.

DR. BREWER: Because in some of the field studies that are now being done and published, elevated CO_2s, a lot of it is directed toward root zones. And if the energies are being directed toward the roots at the same time you have a concern about temperature differentials in the root zone, I would think that that would be an area that you should at least give some consideration to.

DR. BREWER: Any other questions? We have some from the staff.

DR. BARNARD: Bill Barnard from the staff. Just a picky point. This morning Steve Saterlie talked about thermal goals and he mentioned a rise in surface temperature previously less than 6°C being changed to less than 2°. I don't know whether that's consistent with what you've got here or not.

DR. OSTLER: I was successful in getting that changed.

DR. BARNARD: Okay. Well, my question was if the maximum temperature expected is less than 6, then do you want to change your criteria to less than 2, or is the less than 2 an average, regional as opposed to local, or something like that? Just to confuse the point.

DR. OSTLER: Well, I think I'll address it. Even if you have temperatures of less than 2° over this uniform surface, which is what the models predict, I think we're going to see in certain areas, long fissures, we will see temperature increases higher than that. And that's just a guideline, isn't it?

DR. BARNARD: Well, maybe the thermal goal should be a regional rise in temperature as opposed to looking at an absolute that would include both regional and local.

DR. BREWER: Okay. We have time for one more question.

Dan?

DR. FEHRINGER: Dan Fehringer of the staff.

In your conclusions you said that many of the uncertainties could be addressed through a research program but you didn't say anything about the magnitude of resources that would be necessary or the time period involved. Is that a relatively short term research or take decades to unravel?

DR. OSTLER: No, it will not be a short term research project. I don't think you could get at those interrelationships and look at a system in a short period of time. I think it would have to be a four to five year minimum period we're looking at.

DR. BREWER: Thank you very much, Kent.

DR. OSTLER: Thank you.

DR. BREWER: We're going to shift gears and we have three presentations from various points of view on total systems performance assessment, TSPA II. Our first presenter is Jeremy Boak who will provide an overview of TSPA II.

Jeremy is a Harvard trained geologist with his Ph.D., six years working for ARCO in Alaska and other places where there used to be oil, and has spent three years with the Department of Energy at Yucca Mountain. He's currently the Acting Chief of the Technical Analysis Branch.

DR. BOAK: Thank you. I guess we probably need to revise that actually. I'm now the Branch Chief for Technical Analysis.

DR. BREWER: Congratulations.

DR. BOAK: And since Alaska is still providing approximately 25% of the U.S.'s oil, we'd say that there's still oil there.

Dr. Roseboom took the usual latitude of Harvard men when getting ripped by Yalies and he said nothing. But I've always enjoyed the byplay between Harvard and Yale people and so I will point out something that's not on my biography, is that when I was at Harvard I was on the lightweight crew, and one of the conventions of rowing at the time was that after a race, the losers gave up their shirts to the winners. And I can say that I never gave up my shirt to a Yalie but it didn't make the race any easier.

DR. BREWER: You want my shirt?

DR. BOAK: I was about to say that I'm grateful for the fact that not only in the rowing community but elsewhere this convention no longer applies.

DR. BREWER: Get to work, would you, Boak?

DR. BOAK: I want to give an overview of our section generation in total system performance assessment and then turn it over to the people who are in charge of the efforts at our national laboratories and our M&O contract, talk about the particular aspects they'll be bringing to this next generation of total system performance assessment. But before I talk about the TSPA II, I'd like to talk about some of the other things that performance assessment has to do.

We have quite a menagerie of priorities, many of which take up a good deal of our time and quickly say I want to talk about the priorities, talk about the TSPA II, mention the major participants and their roles and give you a quick idea of the schedule.

The top priority that we have at this point is to provide continued support to the ESF design and to the surface-based testing program. As I said, it's quite a menagerie. This up here is the 500 pound gorilla and he gets what he wants.

We are expending quite a bit of effort looking at the waste isolation impacts of the tests and looking at how the design might affect the waste isolation capacity of the site. We've discovered that in fact answering all of the questions that come up about that is a much bigger animal than we had anticipated.

The second major priority we have is to look at alternative regulatory standards in order to provide some DOE perspective to the National Academy of Sciences, provide technical support to the National Academy in its effort to provide standards for Yucca Mountain. Someone suggested this was a dinosaur because it was something we thought was extinct long ago and was only recently revived in the movies. And I would suggest, therefore, that because our major problem is to avoid being ambushed by it later, it's the velociraptor.

We've also been trying to support the system studies that are looking at questions of alternative thermal loads, the issue we're addressing here today, alternative waste-package designs and alternative waste-emplacement modes. And this is sort of like a small herd of elephants. They take up a lot of space and make a certain amount of noise. They leave behind muddy footprints.

The next priority in here is something that we often ignore, which is the long-term development, verification, validation of flow and transport codes. This is the camel with his nose under the tent and year after year we stomp on that nose to keep it from coming further into the tent, but we know--here's another one of those things about my undergraduate experience. I once took a class devoted entirely to the camel and so I know that the camel has his haughty stare because he knows 100 names for Allah and humans only know 99. Well, we need that last name to get into heaven. I've often likened the debate over thermal loads which has to do with whether we're going to reach regulatory compliance by prodigious acts of engineering, or by informed reliance on the natural barriers as being comparable to the old medieval debate over whether it's going to be faith or good works that gets you into heaven. This battlefield for this debate has often been centered on performance assessment and that's why those of you who have been involved in this for a long time of course will recognize the validity of this analogy if you've ever sat through one of those meetings where people sit across the table and lob citations from the Code of Federal Regulations at each other. But you can see the total system performance assessment, this TSPA II, ends up being the mouse trying to find a clear space amongst the elephants velociraptors and 500 pound gorillas, hoping to scarf up a few of the crumbs.

Our objectives, based on some of those programmatic priorities included looking at these alternative thermal regimes and emplacement modes and waste-package designs to try and see how those might affect the total system performance. And that reflects the programmatic priority to move beyond simple evaluations like TSPA 1991 in which we didn't really even look at the thermal effects of the repository itself. We had certain aspects that reflected thermal perturbations. But for the most part, if you really want a low thermal-loading evaluation of the performance of Yucca Mountain, TSPA 1991 is it.

We wanted to incorporate new site information, benefit from some of the insights we're gaining through the characterization activities that are going on now. We wanted, again, to understand what might happen if our standard changed, if the target we were shooting for was different. So we're hoping to expand the amount of dose calculation we do. And, of course, we wanted to have the sensitivity and uncertainty analyses that form a critical part of the value of a performance assessment rolled up into the actual performance assessment itself. In TSPA 1991, we felt a little bit under the gun. We rushed out with a document that presented in rather heavy form all of the results we had. Subsequently, we've done a fair amount of sensitivity studies, but those were not part of the original document. We want to improve it. We want to include those next time.

So we will be looking at vertical emplacement and in-drift emplacement. We'll be looking at the SCP design with thin wall borehole emplaced canister. We'll be looking at something that looks a little like an MPC. Our models, of course, are not sophisticated enough to look at a multitude of large waste packages, so one large waste package will have to do. And we'll be doing analyses of 28, 57, 78 and 114 kilowatts per acre, so spanning the range of cold to cool to hot to whatever. As many of you sitting in this room has found out, what your view of cool and hot is varies depending on your body makeup. It's probably true for repositories as well.

The shading on this is a little bit light but I want to show here on the diagram that we've used repeatedly in the past the relationship of the work that the site characterization and design groups do in developing the site and design data and providing the first level mechanistic process models to the performance assessment task of connecting to these mechanistic process models and rolling those up by means of a process of abstraction into progressively higher level subsystem and system models until we come out with something at the top of the pyramid that gives us some kind of an answer against performance measures, either our own or the regulatory performance measures that determine whether we have suitability and whether we get a license. But in the course of TSPA II, the top of the pyramid will be largely covered by the M&O running the repository integration program that was developed by Golder, and they will be hopefully covering a somewhat wide range of repository performance aspects in essentially a descriptive load. RIP is a descriptive model. You have to teach it everything, otherwise it knows nothing.

The modeling done by Sandia is a little more directly connected to the actual processes and carries on down through and feeds on results from the mechanistic process models, and in fact incorporates some process modeling within it. The work of Livermore which covers both the details of the engineered barrier system components, but also to some extent rolling them up together in order to feed into the total system modeling. And, of course, some support in developing the parameters and the ranges of values and the models that we use for various things; Los Alamos for geochemical and vulcanism models, LBL for hydrologic conceptual models, USGS for hydrology as well as for ultimately tectonics. So we do have a range of participants in this effort.

I'm going to skip over a couple of these in the interest of time. There are a couple of slides in your package that talk about some comparisons of alternative TSPA approaches and some common features of all TSPA approaches.

I want to talk about the significant conceptual differences between this current iteration of total system performance assessment and our previous attempt in 1991. First of all, we really are going to be involving coupled thermal and hydrologic processes for aqueous flow. In essence, we used ambient temperatures or ambient flow fields for modeling in the solubility for the transport calculations.

Especially in RIP where we can do a great deal of correlation of variables and put in a wide range of distribution types, we'll be enhancing the statistical correlations of parameters within the engineered barrier system and the natural systems, and also in the Sandia effort we'll be looking at some of the geostatistical variations. Bob and Holly will talk about this more in detail in their talks.

We hope to be testing some of the significance of fracture of degree and fracture-matrix coupling, a very simple approach to that last time trying to enhance each of our models to represent that a little more realistically.

There will be a dependence of the water-contact mode on the amount of flux. In fact, in many of our previous exercises that was a relatively loose connection. We had source terms that in many cases were the result of fluxes much higher than what was being put in to the hydrologic system.

And finally, we'll be considering multiple engineered barrier systems. In the past we have had a simple distribution for container failure. We hope to be looking at and modeling to some extent the performance of the cladding, the waste form itself as barriers to radionuclide migration.

We've had a fairly energetic schedule for this, scoped it out during the course of the time following the completion of TSPA 1991 and developed and designed who was going to be responsible for how to parcel out the various parts of this, finishing up in April of this year.

The M&O did a test of RIP in which they tried to duplicate the effort at Sandia in TSPA 1991 without contacting the Sandia folks. They essentially did it blind, came up with reasonably similar results. I think it's a reasonable demonstration that the TSPA document that Sandia put out does in fact give sufficient information to duplicate their results, therefore to examine the validity of assumptions and models that went into that.

Define the revised site characteristics, the waste package, emplacement designs, revise and upgrade some of the models that's still ongoing. In fact, there's a great deal of flux in what we actually expect to be modeling. We're just at the point of really diving into the major parts of the calculations for this total system performance assessment, including defining these thermally dependent parameter distributions and the thermal-hydrologic regime. I think that's beginning finally to settle down.

Once those are in place, we can more or less complete the definition of the source term. We should be finishing up at this point and then completing the calculations. We would like to have those done before the end of the fiscal year. It's our intention to have a meeting in which we then get together and present our results to each other, figure out where the inconsistencies lay, where the gaps are, what else we need to have in order to have a comprehensive total system performance assessment. Possibly lay out some additional sensitivities and uncertainties that we'd like to have in-hand before we go public with the document. It's our hope to have the documents from the M&O and from Sandia completed, at least into us for review in November. And this may be a little ambitious to suggest that we'll have a summary DOE document describing our total system performance assessment by the time the board meets in January. We certainly hope to be able to present the results to you, but I think it might be premature to suggest the document will be ready at that time. We'd like to finish that up in the first quarter of 1994, do our own internal technical review of it and put that report out for some kind of external peer review.

One possible avenue that we've considered is to have the OECD's NEA, which has a performance assessment advisory group, to review that document because there are some of the leading world experts in performance assessment there. At the same time, we'd also like to see some kind of review that's domestic. It's just hard to find a lot of performance assessors outside the field. So the scoping of that peer review will be a little bit tricky.

Then I'd like to give up the rest of the time to Holly Dockery and Bob Andrews to talk in a little more detail about the total system performance assessment, unless there's some over-arching questions that need to get asked at this point.

DR. BREWER: Anybody on the board have a question?

(No response.)

Okay. Let me, by way of introduction, Holly Dockery is Ph.D. in structural geology from Rice. Her professional career has been in the National Labs, Los Alamos, Lawrence Livermore. She's been at Sandia for the last two years where she is a senior member of the technical staff involved directly in total performance assessment, particularly with respect to Yucca Mountain.

DR. DOCKERY: As Jerry mentioned, I will be talking about primarily the Sandia total system performance assessment effort that is, as he stated, very much a work-in progress. I look at some of my view graphs now that were generated almost two weeks ago, and I could throw them out and say they're not really completely true anymore, and I could probably throw them out again at the end of the week if I generated them today. So we're really in a very dynamic phase of trying to determine the specifics of how exactly we're going to calculate the total system performance assessment. However, the general over-arching theme is still going to be the same. It's the details that we're really not sure of at this point.

The Sandia's primary contribution to the TSPA effort will be to iterate on TSPA 1991. We advertise that as our first in a series of iterative performance assessment when we build on what had gone before. And so, in this iteration we're trying to incorporate as much new information, site information, or any information we can get our hands on into this calculation and also trying to make our models more sophisticated where we think it's appropriate and where we think we have enough information to make any kind of a difference.

The biggest part of our effort this year has been directed at trying to perform our first non-isothermal calculations for the Yucca Mountain. We try to stay very closely tied to the thermal goals working group and that's caused a few iterations within self, trying to keep up with the waste-stream and the different thermal loadings that they're trying to address. But I think, like Jerry said, it's beginning to settle down. It looks like we know where we're going to be going at this point.

We've worked very extensively with Lawrence Livermore on the source term, so a few of the things that I will say you may have heard Dan McCright say or Bill Halsey's been very much involved in this. So a lot of this work is Lawrence Livermore's and we're being fed the results of their efforts.

Sandia's also organized and led efforts to obtain information on infiltration that was primarily with the USGS and with WIPP project, and we're also trying to get a better understanding of the geochemical information, and that's been primarily with the Los Alamos folks.

So I'm going to try to hit a few of the highlights of what we did in this TSPA realizing that we're also going to have to leave out a lot of the details in a twenty minute, fifteen minute talk on what will probably end up to be a two hundred page document. So the way I structured the talk is to talk about the effects of alternative thermal regimes, emplacement modes, waste-package designs, give you a couple of instances of how we're incorporating new site information, show you how we're trying to evaluate the effects of alternative performance measures, and then a very brief foray into the types of uncertainty and sensitivity analyses we expect will go for both TSPA 1991 and some of those that will grow out of the calculations that should be finished sometime in the August/September time frame.

The source term for TSPA II will probably see some real close similarities to what I'm going to say and what Bob Andrews will say, because we're both being fed by the same source. We're trying very hard to couple hydrologic, thermal and chemical effects for the first time with the source term. It's been not as well coupled in the past as we would like. We're going to have some alternative emplacement and thermal-loading strategies incorporated. We're going to have an inventory that's based on the current waste-stream estimates, and we chose this inventory primarily for release but also for dose effects.

Here's one of the ones that I said I could probably throw out now because it's already changing as we speak. This is the repository area that's been modeled for the alternative emplacements and for the thermal loads. This porkchop here with the peas and the beans, this is the area that would actually be modeled for 57 kW/Ac thermal loading. And you can see that we'll have slightly different layouts given the different thermal loadings and the different waste-package designs. And the difference that this makes to us is that for aqueous calculations and our gaseous calculations, how we represent the repository is based on where we choose our stratigraphic columns, and in this case we'll have to be more really dispersed in where we choose our columns, and in this area we will have the same number of columns but they'll be bunched up closer together.

On the other hand, we've just found out from the new waste-stream estimates that probably these will have to be increased on the order of maybe 25% or more. So those particular layouts are changing.

The current waste-stream, the spent fuel is a 25-year decay. You may remember from the TSPA 1991 we had a 10-year burn. We have approximately the same percentage of PWR, BWR but we have slightly different numbers because we've included the glassified high-level waste as 10% of the total now. And so, there's some slight differences in what we're doing for this go-round.

The source term module that's being incorporated into both TSPA IIs will have some aspects of coupled thermal and hydrological processes; that's the boiling front, dryout and reflux. And this is a very rough schematic of what we're expecting will happen. At Sandia what we're using is a combination of the discrete emplacement source models that Eric Ryder talked about yesterday. We were trying to get the best of both possible worlds, and in that case what we see here is the waste-packages with the water being driven off by the heat, condensation cap forming, and then reflux being driven down the sides. And what we see is kind of a nice traveler's umbrella affect here. But in actuality, there will be some typography on this so you will get reflux that's focused on certain areas in the repository, and that will change with time.

For the first time we're not going to be using the mission impossible waste-package, the one that instantaneously disappears at time to be determined by the code. We are actually incorporating the multiple barriers and we have the waste-package degradation processes, both the pitting corrosion and general corrosion. And we are incorporating some waste-form degradation. So we're finally taking advantage of the cladding and we have high-temperature oxidation, aqueous alteration and congruent leach that will be included in the calculation of the source term.

This is a curve that was convolved from a number of the different models that Livermore had provided and it's simulated in their little source module YMIM that we now have up and running. What we have here is we have the juvenile failures, which is somewhat arbitrarily defined; we have the time that the waste is essentially dry, and so we have the oxidation occurring during this time period; and at some point in time, depending on the thermal loading and the place that the container is situated in the repository, you'll get a transition in here of whether you go from the dry to a wet localized; in other words, where the reflux starts to drip on the waste-packages and you get localized corrosion effects, and then gradually you go back to the normal system where you get a generalized wet scenario.

Just to talk very briefly about how we're trying to incorporate these effects into the models, you may recall that we used the Weeps model to simulate fracture flow as opposed to matrix flow in TSPA 1991, so all of your water is moving down through fractures instantaneously to the water table; and the way we're incorporating this information in a very simplistic manner is we're looking at how far the boiling isotherm, how it moves out from the repository and how much reflux will be moving down along the edges, and that will change with time. Obviously the number of containers outside of the boiling isotherm will go down and then back up with time. You'll also have the actual volume that's incorporated by the boiling isotherm, or if you look down in this kind of a circle here which again is not truly a circle, there will be some inlets and outlets in the circle. But the volume of the boiling isotherm will determine how much water is being driven off given the different saturations that we would choose. And the amount of water will then flow down into the weeps or the fractures that are on the edge of the boiling isotherm. And the source term in this particular instance will have some number of containers outside the boiling isotherm and some inside, and so your container wall temperature will be used to determine what mechanisms are affecting your source term.

If I go into the next section to talk very briefly about the new site information, a couple of the things that we were asked specifically about is how are you going to treat the saturated zone, or are you going to treat it any differently? What we're going to do with the saturated zone, what we're planning at this point, is we're going to use a 3D representation of saturation zone that more closely approximates the actual geology that occurs underneath a repository, and in this case it occurs approximately in this region right here. Before we simply dumped the radionuclides down into some sort of an averaged tuff aquifer and then it's moved out at some sort of averaged porosity. This time we were actually trying to figure out how velocities and conductivities would change given the different units. And so, we have a little bit more realism in our saturated zone module at this point.

The other thing Jerry mentioned briefly was that we were going to include some geostatistical correlations. This is a picture of some variograms that have been generated for frosting. You may recall that you saw something similar to this with Eric Ryder yesterday, and if we're going to look at the effects of the uncertainty and stratigraphy, we have to use some sort of geostatistical correlations. The way these particular variagrams were constructed was by using conditioning data from boreholes. We know that certain stratigraphic unit breaks occur in certain places, and so those are held constant. And then as you move away in three dimensions in any direction, you have some sort of a correlation length beyond--you have a certain degree of confidence that that contact stays the same or that parameter stays the same and it varies differently in different directions. Obviously when you think about a ashflow tuff, it probably is a fairly long correlation length in the horizontal direction, but in the vertical direction you can change very rapidly. So we used these kinds of simulations to help us understand, since we're not going to be able to put a borehole down every foot or so, how are we going to simulate the differences in the stratigraphies that we modeled. And so, we generated ten simulations. So we had ten different possible stratigraphies and in each stratigraphy just to--this was in Eric's package yesterday. I just borrowed it so we could have an intermediate step. You overlay a grid and simplify that variagram, pick out your individual units based on porosity, and then you come up with something--obviously we made some steps in here--you come up with the stratigraphic realizations. So now for one point in the repository, column 2, we now have ten possibilities for that stratigraphic cross-section. And then within each individual unit, we have distributions of hydrogeologic parameters. So first we sampled to find out which one of these columns we're going to use, and then we sample the different hydrologic parameters within those units.

We're hoping that this will address one of the questions that the board had last year on what kind of effect will a geostatistical correlation have on your total system performance assessment. At the time we weren't really sure that it was going to have a large effect, and until we run the calculations, we still aren't going to be sure.

The effects of alternative performance measures is something that we're also trying to look at. I showed this one in your opening. It's kind of a joke slide. We're finally putting dose into our calculations. We're doing it very simply. We're simply putting an ingestion calculation in right at the five kilometer boundary. So we're looking at how the contaminate plume moves out from the repository and then we're taking a sample and running it through a very simple dose module. But this will give us a chance to check our release calculations against a dose calculation and get some sort of an idea of what a change in any particular performance parameter, or in this case release versus dose, what kind of impacts that might have for this site.

The last thing I'll talk about is the conducting sensitivity and uncertainty analyses. The uncertainty and sensitivity analyses have already been done for the aqueous releases in TSPA-91. We did a number sensitivity analyses on human intrusion and vulcanism; however, we did not have time to do the aqueous and gaseous. So those have been completed and we found, surprise, surprise, the sensitivities are mostly in the conceptual models that we used. I don't think that really caught anybody completely flat-footed. We knew that that was the case. But in the case of the composite porosity model, we had some very different sensitivities than we did within the Weeps model.

The composite porosity model, percolation flux was by far the most important parameter for us to dedicate our time and to investigate. As much time as we dedicated to investing it, we found there wasn't much to find out yet. So these other things like gaseous transport, container lifetime and fuel matrix alteration rates all had some sensitivity but they were very much overwhelmed by the percolation flux.

In the Weeps model, because it doesn't really have any coupling between the matrix and the fractures, obviously the fractured properties were the most important. And so fracture aperture was extremely important. How wide is your fracture? But the surprising thing was that before TSPA-91, we all assumed the bigger the fracture, the worse the problem. Not true. The bigger the fracture, the better it is. The wider the fracture, the more water will move down that one fracture rather than across the entire repository, and so the fewer containers are impacted and the less the release. So there was some point in doing some of these sensitivity analyses.

This is a plot to show you the sensitivity that occurred to the percolation flux for the composite-porosity model, and you can see there's a strong correlation but you'll also notice that it varies over forty orders of magnitude. And this is the normalized releases versus the amount of flux in millimeters per year, from 10^{-6} to 10^{-2}, but you go from the 10^{0} to 10^{-40} and so you can see that there's an extremely strong correlation in the scatter plot. In other scatter plots you could see a very slight trend but not a strong trend. And you're also seeing that it's not a completely linear fit because in the higher flux values, you're mostly in an extremely advective response, and so that essentially the flux value is what's determining how much transport is going on. But in the lower areas, the diffusion and dispersion is becoming more important, and so these competing processes you can see the effects of those within the sensitivity model itself.

So the summary of our improvements in the first iteration is that we're going to have coupled thermal and hydrologic processes. They're not going to be extremely sophisticated but I think we will be able to start to see some of the sensitivity in some of the parameters. We have a much more sophisticated source term and the saturated zone module is much more pleasing to the USGS folks who have been trying to give us information for a long time and we're now finally going to use it. And we have the dose module and sensitivity studies. The sensitivity studies will hopefully be conducted for a number of additional parameters that we have not yet identified because we have not run our calculations. But there are in addition to these things that I talked about here, we have done a study on the appropriateness of using 1-D calculations to simulate 3-D processes, and the good news is it looks like they're much more rigorous than we had thought in the past. And hopefully in January, when we talk to you, we can talk in some detail about that because it was a very heartening response.

We're doing an expansion on the volcanism analysis where we're going to try to investigate the effects of aggressive volatiles and also of the thermal response that's added by a dike that includes the repository. We are adding extensively to our suite of hydrologic parameters, particularly in the fracture properties regime. We're incorporating disposal safeties gas flow calculations, which is based on the geostatistically correlated columns that were generated by Sandia, and Ben Ross says that they're Federal Expressed and on my desk now. And the human intrusion drilling scenario is also going to be more sophisticated because of--the source term is essentially the issue in human intrusion, and with a more sophisticated source term, we have a better human intrusion calculation. So when we have our calculations run, we expect to have a significantly different and enhanced version of the total system performance assessment over what we had last time around, and hopefully with the help of the board in January and with the technical review that we get, then the next iteration will be more sophisticated yet.

DR. BREWER: Thank you very much.

Are there questions from the board?

(No response.)

Staff? Leon?

DR. REITER: Holly, you mentioned the USGS and how happy they were with the information on the saturated zone. At the last board meeting some of the USGS people, hydrologists, expressed either very little knowledge of or ignorance of the Weeps model. This time around have you decided to get together with the people in the field and see whether or not how they might want to alter or suggest improvements to that model?

DR. DOCKERY: Luckily we've already had several of those meetings. I don't know if you've heard, but we had a TSPA road show. We took TSPA-91 on the road here to Denver and we had an all day meeting where Larry Hayes was kind enough to invite the entire Survey there to listen to what performance assessment is and what it needs and what it wants, and we also had a chance to hear what they're expecting to produce from their field studies and from their conceptual modeling efforts. And then, in addition, we had a meeting with Alan

Flint and Lorraine Flint and several other USGS connected people at Sandia to elicit some information on the infiltration and how to handle some of the aspects of the Weeps model and the composite-porosity. So I can say yes, we have had a number of interactions and we're very happy to have their help in trying to make this a more realistic, if I can use that word, total system performance assessment.

We also did the same thing at Los Alamos. We went on the road to Los Alamos and learned about geochemistry and natural analogues. The direct analogue for that was to have the elicitation for sorption and retardation properties in Sandia, and we have a much better suite and a larger suite to use for this time around.

DR. REITER: I would hope that interaction between the models with field people would not be a one-time road show, but would be an ongoing process.

DR. DOCKERY: And I think that's what happened as a result of the road show, because now we understand what each other is doing. We've had a number of follow-up meetings where individuals have gotten together and worked on specific problems.

DR. BOAK: Holly, could I add to that? The Weeps model was added a little bit late in TSPA-1991, but the aqueous models that we had for the most important parts of TSPA-1991 were the result of field interaction with Alan Flint over the course of several years. The Weeps, however, was an attempt to respond to a need that we perceived somewhat later in the completion of TSPA-1991 and did not have as much opportunity. Alan Flint had actually been at the TSPA road show before he expressed his ignorance in Reno. It's just a matter that he had not had time to sit down and read our document again before you asked him his question.

DR. BREWER: Okay. Other questions? Board, staff?

(No response.)

Thank you very much.

The third presenter in the TSPA suite is Robert Andrews, who has a Ph.D. in geology and hydrogeology from the University of Illinois at Urbana. He spent the last twelve years with INTERA, is the Division Manager of INTERA responsible for high level waste performance assessment. Previous experience with the Swiss Nuclear Waste Disposal Agency and experience also in salt deposits, Robert Andrews.

DR. ANDREWS: Thank you. Gene was showing his slide of the 1987 cut that reminded me definitely of having worked on the SALT program and 1987 was a nice time to go to Europe where they still were having radioactive waste problems that were non-salt.

What I'm going to present today is sort of the, as

Jerry presented it, the top of the pyramid, if you will, total system performance assessment using the Repository Integration Program developed by Golder. Golder developed this for headquarters in FY '91 and '92. This fiscal year they've been working as a subcontractor to the M&O in making small revisions to that particular program that will enable us to better abstract some of the process correlations and parameter correlations that we feel might be important to the overall system performance. We won't actually know if they are important until we do the analyses.

This slide you've seen now three times, which is very good, I think. That we're all shooting for this ultimate objective, and that is in the first quarter of calendar year '94 to have a DOE document that details a baseline, if you will, total system performance assessment that's composed of two separate parts, that Jerry pointed out; the Sandia component, which is a little more process oriented in its approach, and the RIP approach, which the M&O is responsible for. But the overall objectives of that TSPA are the same. First, to evaluate the effects of alternate thermal loads; second, to incorporate those new site information that are available to evaluate alternative performance measures, i.e. dose and cumulative release; and finally, to conduct a series of sensitivity and uncertainty analyses to try to identify what is important and what is not important in terms of total system performance assessment, post closure.

In my presentation I'm only going to focus on the thermal effects part that we're trying to incorporate into RIP in this TSPA II. I think that's sort of germane to the discussions over the last day and a half, and it may be worthwhile to refocus on them as it effects or could effect total system performance. So the main goal of evaluating the thermal effects is to look at some of these thermal dependencies as they impact release and ultimately dose. And those I'm breaking into those that impact the failure itself, so that the waste-package lifetime, if you will, the EBS release and ultimately radionuclide transport to the five kilometer accessible environment, the EPA remanded standard. And then ultimately dose.

General approach in evaluating thermal effects with RIP is first and foremost to abstract some of the stuff we've been hearing over the last day and a half, the sort of primary functional relationships between thermal load and temperature, thermal load and aqueous flux, thermal load and gaseous flux, and thermal load and water saturation, water saturation or water content. It's those primary parameters, if you will, the effects of the thermal load that will ultimately have an impact on performance.

The second step is to define the secondary, what I call secondary functional relationships between these primary factors; temperature, saturations, aqueous fluxes, et cetera, on some of the processes in the waste-package EBS area and then also in the far field area. The important step, of course, is the third and fourth bullet, and that is to incorporate these functional relationships, both the primary and secondary, into RIP. And finally, to evaluate the total system performance.

Because it's probably been a while since the board--maybe the board has never heard about RIP. I'm not sure, but I think Ian Miller of Boulder presented some initial thoughts on RIP a year ago or a year and a half ago. I thought it was worthwhile to throw in a slide on RIP. The board has been provided a user's manual and the description of the basic philosophy behind RIP and has also been presented our kind of comparison of RIP versus TSPA-1991.

As I was reading this first bullet, I realized I had as many P's in there as I probably could get, but basically what RIP does, it's an overall shell that drives from uncertain processes and uncertain parameters, samples off of those uncertain processes and parameters, and drives through a prediction of performance. Now, most of us think of prediction and performance as total system area, as cumulative release to the five kilometer accessible environment. It just as easily could be some other alternate performance measure; concentration or dose or health effects or whatever.

It describes, and I think the operative word in that second bullet is describes, the waste-package behavior, the transport and disruptive events. And I want to emphasize the word describes because RIP is essentially a very glorified spreadsheet. It's only as good as the input information to it is, and that input information comes from, the bases for it, is the detailed process modeling which underlie some elicitation for parameter correlations and correlation effects on other properties. But it just describes that.

It incorporates all the relevant processes that you would want to have in the total system performance; from the rewetting phase to the container failure, descriptions of that, to the exposure of the waste-form itself, to the alteration and dissolution of it, and finally the mass transfer from the package that's essentially released from the EBS, if you will, or if there is a backfill around the package in terms of the drift emplacement. Maybe it's the edge of the backfill. And then allows for two methods of doing one-dimensional advective-dispersive transport in the far field to the accessible environment. One is simple, a 1-D analytical solution. That's one option. That's used when it's more or less matrix dominated flow and transported. The second option is a much Markovian transition which allows you to transfer the nuclides in this case from the matrix to the fracture and the fracture to the matrix. So in one slide, that's the essentials of RIP.

Primary functional relationships, and these are the backbone of what's going into the thermal dependencies in this TSPA II. First is the temporal and spatial temperature distribution, and this is primarily going to be at the repository level and by spatial I mean radially, or out from the center of the repository to the edge of the repository. These primary functional relationships almost entirely are coming from the analyses that Tom Buscheck has done at the different thermal loads, and we are essentially just taking very small portions of his humongous output files and using those directly as reads essentially in the TSPA input to RIP.

Second thing is the temporal and spatial water saturation for water content distributions as a function of thermal load. And finally is the temporal and spatial aqueous flux distributions. Those we are getting from Livermore. The gaseous flux distribution we're not getting these directly from Ben Ross because he is actually doing a travel time distribution of carbon-14 migration, and we will just use that carbon-14 distribution directly rather than a flux, per se.

So these are the primary factors; temperature, saturation, and flux. Now, what are their effects that we want to try to incorporate in this iteration of TSPA within RIP?

One is simply the effect of the aqueous flux distribution which, remember now, is spatially and temporally variable on the number of packages that are in different--what have been termed in the past in PA water contact modes. Those essentially define different release modes and will also define, as we'll see in just a second probably in the next bullet, will effect the cutoff between dry oxidation and aqueous corrosion. That limit between when you have very, very slow corrosion oxidation rates and much greater rates as presented by Dan McCright this morning.

The nominal one is the "dry" case. The moist continuous is a diffusive sort of release only, and the wet drip, if there is such a thing in this mode, is just an advective released.

Second bullet, fairly straight forward, either aqueous flux or saturation. I think we found some back and forth on this and we ourselves are in a state of flux on the best way to incorporate this into the assessment. But there's either a flux or a saturation water content dependency on that transition between dry oxidation and aqueous corrosion. Somehow we need to try to capture that into this iteration performance assessment.

Finally is the effect of temperature on maybe not so much the dry oxidation rate. As you saw this morning, it's a very small number. But the effect of temperature on aqueous corrosion rates can be for general corrosion. Maybe it's not such a big deal, but for heat corrosion it might be several orders of magnitude. So we'd like to incorporate that temperature dependency on a rate.

What are the sort of indirect effects, if you will, of alternate thermal loads? The effect of temperature on the fuel matrix alteration rate. That might be important and would like to incorporate that in this iteration of total system performance. The effect of either flux again or saturation on the fraction of the waste matrix that's wet. This is essentially the volume of water, if you will, in contact with the waste matrix. Now clearly that's a function of space and time also, and saturation, but that volume which will effect the alteration rate is relatively important and we'd like to incorporate that functionally dependency into this TSPA. It is a temperature effect, it is a thermal load effect.

The effect of temperature on radionuclide solubility. Holly already alluded to that one. There's been elicitation from the Los Alamos folks.

I mentioned this a little bit earlier, the water volume in contact with the waste matrix effecting the non-solubility-limited releases. The solubility limited ones would not really need to care how much water was there. They should just come up to their solubility limit and be released at that.

The effect of temperature and flux on the liquid saturation along the diffusive pathway. So for those passages that do get wet at some period of time in the future, the amount of water contacting the waste matrix allowing for a continuous diffusive pathway could be very important in terms of the total diffusive release. Finally, the stuff I think the board has seen from Jean Younker on the effect of particular more the water saturation, or water content I think was his curve, versus diffusion coefficient, very non-linear and at the lower water content modes the diffusion coefficient itself, the effect of the diffusion coefficient is reduced several orders of magnitude, which is clearly going to effect the release. Whether it effects the performance, that's what we're trying to evaluate. But it will effect release.

Finally, the thermal hydrologic effects on the transport itself, the effects of temperature on the gaseous phase flow-paths. That's coming from Ben Ross. The effects of temperature on the matrix flux properties. The Survey has done a little bit of information here. There's other information in the literature on this. And finally, the effect of temperature on retardation itself. That information is coming from Los Alamos.

So in summary, one of the objectives and maybe the principle objective of this iteration of TSPA is to evaluate the effects of these alternate thermal loads. The effects now are on total system release performance of the engineered and natural barriers as they work in conjunction with each other.

I want to point out though, and it's very important, that the abstraction--the goodness of the results, if you will, of any total system performance is only as good as the abstraction of the detail process modeling that we have available to us. The abstraction for the process models is in progress. The stuff's coming primarily from Livermore but with some support from Sandia, Berkeley and the M&O itself. There are a number of laboratory measurements of some of the thermally dependent properties, so if the temperature is raised X amount, the property, i.e., retardation or solubility or corrosion rate or alteration rate changes X amount. A sort of the strength of RIP is you can incorporate that dependency into the analyses and does that dependency make any difference or not.

But finally, I want to be also clear that there are some thermally dependent processes and properties and also some non-thermally dependent processes and parameters that are still uncertain and that will still impact performance and that we don't have maybe a very good handle on. But we can analyze and one of the purposes of TSPA is to evaluate the importance of those other things that are also uncertain.

One, water saturation itself in the emplacement drift. There has been no detail modeling at the actual drift scale within or without backfill.

This one we mentioned earlier, that transition between dry oxidation and aqueous corrosion is still going to be somewhat uncertain. It might be something that we actually sample over, acknowledge that it's uncertain. Try to get a best estimate from the people who know the most about this, the folks at Livermore primarily, and say let's see if we sample off of that parameter where that transition occurs. Does it make any difference.

That transition from diffusive to advective release, clearly that would also be for when you get advective release.

The behavior of the cladding, whether we include it or not. I mean, I think we'll make the realization where it is included as a barrier. We'll make other realizations where it is not included. Essentially it breaks down at the same time that the secondary or the secondary barrier, inner barrier from Tom Doering's talk, breaks down.

The fraction of waste matrix wet, that will probably be just a sample value, you know, from zero to one. And the water volume and contact with the waste matrix, that will also be highly uncertain but not a real good handle on it as this time.

So, with that, I will close and entertain any questions on mine, or maybe all three of us at once.

DR. BREWER: Are there any questions from the board for either Bob or the other two presenters for TSPA?

Pat Domenico.

DR. DOMENICO: How do you get it from the unsaturated zone to the saturated zone? I didn't see any information on the unsaturated transport to get it down to the water table.

DR. ANDREWS: What we're sampling is sampling off of the advective flux that's coming off of the thermal hydrologic models. That advective flux is dominately a one-dimensional flux. So we're taking a QZ, if you will, that's coming straight from a thermal hydrologic analyses. That clearly changes with space as you go radially out and it changes with time. So we're taking both that spatial and temporal variability. We're generating a few columns to represent the repository itself or repository panels. We're essentially taking Eric Ryder's approach and representing it in panel spill. Then we're using that flux and also the saturation, the water saturations and porosities. The porosities are being sampled off layers. The water saturations themselves are also time dependent in this case. So those are being sampled. Then it was taken down one-dimensionally into the saturated zone.

DR. DOMENICO: And then out?

DR. ANDREWS: And then out.

DR. DOMENICO: The transport across the unsaturated zone is an entirely different process than in the saturated zone. That is in terms of unsaturated flow. Is that in there?

DR. ANDREWS: In one case we had the velocity and the other case we have this possibility that if the aqueous flux is high enough and exceeds the matrix saturation, then it goes into fracture dominated transport. Then you have the possibility for a transition between the fracture and matrix.

DR. DOMENICO: My last question on that. Does that

include daughter elements? You don't have that incorporated?
DR. ANDREWS: Yes. All the daughters are in. The entire inventory is in there.
DR. BREWER: Don Langmuir.
DR. LANGMUIR: A related question. This sounds extremely complex to me but you're also apportioning. If you've got an umbrella type of an effect, then you're going to have an apportionment of fractions of the water that were in the original block which varies across that area from the sides to the middle. And somehow you've got to figure out how to do that.
DR. ANDREWS: And how you validate that.
DR. LANGMUIR: And in some cases, of course, you're liable, given the situations we were describing yesterday, have water going in the middle in different places. Then you've got decide how much of that's going to hit the packages if you're going to release radionuclides. I don't see how one could possibly validate any of that.
DR. ANDREWS: The validation issue is a very important issue and generally our thinking on the totally system sort of assessment is that validation efforts have to take place at that detailed sort of process level. You know, we can only abstract the results from the detailed process models and then evaluate the impact given that curve as

conceptualized and parameterized from the detailed process models. The impact of that on performance and performance now I mean, you know, cumulative release performance, not how much did the temperature change or how much did the flux change. We are trying to account for that spatial flux, spatial and temporal flux variability but the bases for that variability is coming from the detailed process modeling results. It's not something that we are dreaming up that this is how the flux looks like. We're taking it from the best analyses that the project has today.

DR. LANGMUIR: One more related question. Are you then trying to as realistically as you can describe a system as you think it would occur? Are you also including bounding or extremes to give some sense of what the uncertainties might be?

DR. ANDREWS: You know, for some of the parameters we're trying to give extremes as much as the expert elicitation that has taken place allows us to give extremes. One example would be corrosion rate. There are quite a range of corrosion rates even given water exists. In other cases we've tried to bound it based on observations. Porosities is like the bounding. We're not going to dramatic extremes to things that would be unrealistic. So to answer your question, if we try to be as realistic as possible but acknowledge that that uncertainty exists because of lack of information or spatial variability, we try to capture that to the best that we can and ultimately evaluate, you know, doesn't make a difference given that range. I mean, I think Holly showed a real good example of one particular parameter and its importance. The objective would there would be a whole suite of parameters that you'll have essentially correlations of output, i.e., you know, cumulative release to input. And you can determine for the conceptualizations and parameterizations tested and for those ranges did it make a difference or not.

DR. BREWER: Okay. We have time for one more question.

Leon?

DR. REITER: One question to Jerry. Jerry, the board has repeatedly urged the DOE to increase the cooperation of expert judgment outside the DOE as contractors in its assessments. How are you going to accomplish this at this time?

DR. BOAK: At this point most of our judgments are still internal on the input to this, but we are hoping in our next --in this iteration they'll be relatively little opportunity to go for external elicitations. But we are looking forward in a future time when we instead of having steadily decreasing funding, we get some small increases to have the time to expand that realm of expert judgment. We also hope to incorporate the results of an external peer review and rather than revising the TSPA II, we'll take the results of that peer review and incorporate it into the next iteration.

DR. BREWER: All right. Thank you all very much, the TSPA trio.

We're shifting here somewhat to take into account again from the big picture some changes in the external environment related to the calculation of exposures and dose, and in this case the discussion is the performance assessment studies and support of the National Academy of Sciences Committee on the Technical Bases for Yucca Mountain, the so-called FRI committee.

James Duguid is currently a senior scientist with INTERA, responsible for scientific models and performance assessments. His long career is a variety of positions related to high level waste modeling, performance assessment and so on.

Jim, would you go ahead.

DR. DUGUID: Thank you.

Before I start, I'd like to preface this with the fact that there is a DOE panel that is looking into the issues related to standard development. That panel is chaired by Steve Brochum and what I show you here today is in support of that panel and ultimately some of it will make it to the Academy of Sciences.

DR. ALLEN: Excuse me. Clarence Allen. I don't quite understand. Is this being done at the request of the National Academy of Sciences?

DR. DUGUID: No. It's being done in support.

DR. ALLEN: What do you mean in support?

DR. DUGUID: Support calculations of various sensitivity analyses. I think if you let me continue, I'll answer your question.

A bit of background. The National Academy of Sciences Committee on Technical Bases for Yucca Mountain Standards will examine whether a standard based on dose to an individual is reasonable, whether a system of post-closure oversight will prevent intrusion, whether it's possible to predict human intrusion over 10,000 years. This, in very short brief status, is their charge under the 1992 Energy Act.

The objective of our performance assessment analyses are to provide sensitivity analyses on alternative performance measures for use as background information to the NAS committee on Yucca Mountain standards and to compare alternative approaches to developing environmental standards.

Our approach is to start with a basis of a prior

NAS panel, the waste isolation systems panel, which concluded in a report in 1983 and showed the doses resulting from high level waste repositories in tuff, salt, basalt and granite. We want to update these calculations to a current understanding of Yucca Mountain, conduct sensitivity and uncertainty analyses to define potential dose limits and time periods. We will compare the sensitivity analyses using different models. We're starting out at the bottom of this list using UCBNE-41, which was a model developed by Tom Pigford and his students, and it was used as the basis of the calculations for the waste isolation systems panel. We also want to compare the results using RIP, which you've just heard about which is the basis for the M&O TSPA II, and we also want to check these results using NEFTRAN-S, which is the model that EPA used for 40 CFR 191. We want to briefly examine alternative approaches and population constraints.

The possible performance measures are release, which 40 CFR 191 would release standard with the exception of the ground water and individual protection requirements. Concentration, here looking at peak concentrations. This would be similar to values in the drinking water standards. Individual dose. This would be maximum dose or the maximally exposed individual. Dose to a critical population. Here you need to define that population. Average dose to a population. The basis for 40 CFR 191 was based in health effects to worldwide population and then worked backwards into a release standard. We can also look at a standard based on health effects or on risk. One could have all of these standards related to the same bases so they were equivalent, and in doing that you would find that some of them are easier to demonstrate compliance with. And this is what we want to investigate, which ones of these are more robust.

Our alternative approaches are to look at uranium ore body, which has been looked at numerous times, and initially in the development of 40 CFR 191 EPA started out looking at uranium ore bodies. That got lost in the machinations over the standard over the years, but we need to look at that again. And comparison of standards for other radioactive materials. A standard for a repository should be equitable with standards for other radioactive materials.

Now, I want to show you by shifting gears a couple of results. First is to define the size of a critical group based on the available ground water down gradient from Yucca Mountain.

Here we looked at the water budget for the three subbasins around Yucca Mountain. We said that the available ground water was between the annual safe yield, 300 acre-feet per year, and the annual recharge of these three subbasins. Annual recharge being 2,300 acre-feet. For household use, we used 150 gallons per day per capita. This is probably a bit low for an environment. I think Tucson is well over 200. Farming requires 20,000 square meters per capita per year, 150 liters per square meter per month, and we assumed the growing season of six months. These data are similar and come from data from Hanford, the PNL has worked out and I don't think Hanford's quite as dry as Yucca Mountain.

Using these values, you define a critical group for household use that is between 1,800 and 13,000 persons. Based on the farming scenario, you're talking twenty to 160 persons. So we have a very small population down gradient from Yucca Mountain that could be directly exposed.

Shifting gears again, I want to show you one of our results from using Pigford's model on Yucca Mountain, and this model is an analytical solution to the one-dimensional transport equation with chain decay and retardation dispersion. So basically to do an analysis, you need a spreadsheet model on the front end to calculate your source term, then you need a spreadsheet model on the tail end to calculate the doses.

For the results that I'm going to show you, we assume the ground-water travel time of 25,000 years, an infiltration rate which is the same as Holly called the percolation rate of one millimeter per year past the waste-package, porosity of 10%, the aquifer thickness, the saturated aquifer thickness, we assume that 2,400 meters. Now one would say that this is a relatively large number. There was a method in my madness here. This is the number that EPA used in NEFTRAN-S and I was wondering how with NEFTRAN-S they had gotten their concentrations down as low as they did. So we tried this number just to see what it looked like. This gave us a dilution factor of 1.15 times 10^{-4}. We used a dispersion coefficient of 50 square meters per year. We assumed that iodine, carbon, technesium, selenium and cesium were alteration-controlled, and the other radionuclides that you see in the analysis are solubility-limited.

And this is the result that we obtained from this run. You can see that carbon-14 and iodine start to just discharge just prior to 10,000 years, and the discharge is almost constant in dose. The site is rem per year. This site is severts per year, this line is ten millirems and the repository peaks stays above or near ten millirem for over a million years. The uncertainty in this calculation is very large. As one could expect, somewhere in here you start having to say that the geology is changing but this is the nature of the beast. It doesn't make too much sense to regulate in here say at a thousand years before anything's happening. Neither does it make too much sense to regulate over the entire period unless you know how to take the uncertainty into account. This is something that will be a real challenge for the Academy now. But these results are useful in that we assume no waste-package, we have had advective release. If we assume a waste-package, let's a priori take a 10,000 year waste-package, the neptunium peak we simply hit over 10,000 years because it's a two-million year half-life. The carbon-14 would decay somewhat, has about a 5,000 half-life. And by the way, the carbon-14 is higher than it would be in the TSPA modeling because we have allowed no gaseous release and this is only aqueous. Also, there is one peak that I haven't shown, and that is the selenium-79 peak which sets right in here. The reason I didn't show it, it was a function of the plotting program I used and that was all the curve that I could plot. It doesn't effect the total dose.

Very quickly in doing the sensitivity analyses with running different travel times, anywhere from a thousand to 100,000 years, different infiltration rates, different mixing in the aquifer, different dispersions, dispersion makes very little different in the calculation, and different solubility limits, you find out that it's really, as Holly showed, the source term that makes all the difference. The release from that waste-package that you need to control. To draft these peaks down to some realistic level, you need a package that when it fails it's diffusion controlled after failure. If we can get these controlled releases, we can drop these down an order of magnitude or more. Maybe several orders of magnitude. So these are the types of calculations we're doing and we have probably 25 or 30 different runs with different assume values, but I only showed you that one to give you an idea of what we're doing.

The status of our calculation, we have completed the sensitivity analyses using UCBNE-41. We're well along with the analyses using RIP. We are at the point of just making a comparison between UCBNE-41 and RIP to see if using the two codes we can get the same results. The advantage of going into RIP and NEFTRAN-S is that we can start to investigate the uncertainty because these are codes we can run in probabilistic mode and take into account some of the uncertainty. Uranium ore bodies is pretty well underway. One thing why we're looking at uranium ore bodies is you don't want the repository to serve as a remediation program for natural uranium ore bodies. There's a considerable amount of dose associated with these and one wouldn't think you would want to mine the ore then put it in the repository and make it better.

Thank you.

DR. BREWER: Thank you very much.

Any questions from the board?

(No response.)

Staff? Leon Reiter.

DR. REITER: Jim, last year PNL did some studies on individual dose. They concluded that pollution was the key factor. I was wondering if you did some sensitivity studies in lowering your infiltration rate?

DR. DUGUID: Yes.

DR. REITER: Did DOE's claim or--

DR. DUGUID: Right.

DR. REITER: --infiltration rates a lot lower than a millimeter per year.

DR. REITER: What would happen then?

DR. DUGUID: We've taken it down to about a tenth of a millimeter and it's a linear effect. It doesn't change it much. One thing that I should do is--

DR. REITER: What do you mean by linear effect? Multiplies it by a factor of ten? What do you mean by that?

DR. DUGUID: Yeah, it's just a multiple of ten. It's a little more than that because of--

DR. REITER: Does that mean increase the dose by ten?

DR. DUGUID: About. About. Because you're going to have less water contacting the waste by a factor of ten. The dilution factor's going to stay the same. The concentration you start out with would be a factor of ten lower. Consequently, the concentration you wind up with, the whole thing, all other things being equal, would be a bit lower. The only thing that would change is your travel time giving you a little more time for decay of some of these nuclides. So they would be somewhat lower than the factor of ten if you do a problem that's consistent all the way through. If you say I'm just going to change the infiltration rate but keep the travel time the same, then it'd just be an order of magnitude change.

DR. BREWER: Thank you very much.

We are at that point in the very intense two days where we have one more wrap-up panel. We'll have about a fifteen minute break. If everyone could try to be back as close to 4:30 as possible, I think the chances are excellent that we'll be out of here by 5:30.

(Whereupon, a recess was taken.)

DR. BREWER: I see Jim Duguid in the back. There are a couple of loose ends in terms of procedures. There has been a request, Jim Duguid, to re-ask you a question, and that's sort of unfinished business from the last session before we get going here. You could probably take the microphone here and it was Clarence Allen who had the question for you.

DR. ALLEN: I didn't get an answer and I'm curious, that's all. What is the context of this DOE study in terms of this relationship to the National Academy of Sciences? Why is this study being done?

DR. DUGUID: This is to do enough sensitivity analyses that we can present them to the Academy to show them which parameters the repository is sensitive to. As background information, if you were setting a standard, it would be nice to know what the time frame of release was, what orders of magnitude those releases were, when they occur, what nuclides were involved, what the uncertainties were and how it compares with other nuclear standards.

DR. ALLEN: Yes, but I still don't understand. We didn't ask for this. Is this something--

DR. DUGUID: No, this was on our own that we started doing this and we started down this track as soon as the 1992 Energy Policy Act was passed because there are a good bit of these sensitivity analyses that don't exist in the literature, and we're kind of completing the record so you have some basis for setting a standard. At least you're looking at the entire picture when you set the standard.

DR. BREWER: Okay. John Cantlon?

DR. CANTLON: Yes. Jim, you may also want to correct the statement you made. I think you made a misstatement right at the end.

DR. DUGUID: Yes, I did and I'm aware of that. You asked the question if you reduce the infiltration by an order of magnitude factor of ten what does that do with the dose. It decreases the dose by the same factor unless you alter the travel time commensurate with that infiltration, and then it will reduce it slightly more.

DR. BREWER: This is Jerry Boak.

DR. BOAK: I wanted to amplify some of Jim's response on our program to look at potential standards, and probably I will also defer to Steve after I do. But Edward Demming, the father of quality, has stated that quality suffers when standards are set in the absence of operational experience. And we can't have operational experience on an actual repository operation but we do have some operational experience in trying to comply with standards. And so we feel that we have something to contribute to the efforts of the National Academy to come up with an appropriate standard for Yucca Mountain. And when the act went into effect telling the EPA to contract with the NAS, we began to look around and see what kind of efforts we needed to do to look at what kinds of standards there might be and how performance assessment would be effected by those standards and a variety of other issues. So it was in fact on the basis of Dr. Demming's criticism of standards set too early in the program that we criticized the pre-existing EPA standard. We thought it would behoove us to know something about what sort of standards we might get. That's sort of why I likened it to a velociraptor so that when the final result comes out it doesn't ambush us.

DR. BREWER: Steve Brochum, DOE.

MR. BROCHUM: I think the question was why are we doing this because NAS panel hasn't asked us, and I just walked in the room. But they haven't asked us specifically for any specific information yet, but we are kind of in a sense trying to prepare our technical basis for any positions we may take. And this work that Jim described is just a small part of this whole effort we're putting together to come out with what we think is a frame work of the whole issue and specific issues that we're trying to address. And we've made some presentations to the National Academy and I think Bill Barnard was there. He got all the presentations. I suggest you might want to look at those.

DR. BREWER: Thank you, Steve.

Now we turn to a traditional part of regular board meetings, and that is to open up for public comment, and I understand that there's at least one member of the public who would like this opportunity to speak before we get on with the panel, and that's Steve Frishman from the State of Nevada.

MR. FRISHMAN: There's a lot to say out of the last few days but I'll spare you all of that. I do have just one observation that I'd like to make and it goes all the way back to Don Langmuir's opening statement yesterday. And that's that in talking about natural analogues, what Don pointed out was that because heater test data may not be complete by the time it's needed for a thermal load decision there may have to be some reliance on geothermal analogues, and that's why the board is interested in the study of geothermal analogues. That concerns me a little because it seems to be totally inconsistent with the board's position that science should not be put aside on the basis of DOE's schedule. I'd like to hear whether I'm correct in seeing an inconsistency there or whether there's some explanation that I'm just totally missing.

DR. LANGMUIR: Don Langmuir. Steve, obviously I don't think the board would endorse such a decision if it was made at a time when the tests were not made. I think I'm saying at least is if the decision is made in the absence of the board's support, it may be made on the basis that would require or we would encourage the DOE to at least look at analogues as the only long-term information that was available to them. But I don't think the board is going to endorse it at that point. We would simply have to say, okay, you made it, we don't agree with it.

MR. FRISHMAN: It seems to me as if by making a statement such as I think I've faithfully reproduced here, what you're doing is essentially reversing what has been a pretty firm position of the board and that's that rather than acknowledging that schedules should drive the scientific investigation, seems to me that there may be other good reason to look at geothermal analogues rather than DOE's schedule. And it would be much more consistent with the board's position about not having schedules drive the program.

DR. LANGMUIR: Let me say this. That was my statement. It wasn't approved by the board as a whole. Nobody saw my script.

MR. FRISHMAN: Well, I just wanted to point out that at least I observed an inconsistency there and I wanted to point out that I think it's an important inconsistency that maybe somehow should be resolved. I think there may be other good reasons for looking at geothermal analogues and I would have much preferred to hear it in that context.

DR. BREWER: Are there any other members of the public who would like to take this opportunity to speak? Yes, please? Would you come and identify yourself, if you would, sir.

MR. FRAZIER: My name is Gerry Frazier. I've worked on this project for half a dozen years or so and I think it would be delinquent if I didn't make a statement here. A lot of people in the audience probably know ahead of time what I'm about to say. Takes me about two minutes to make my statement. Let me just read it here.

I note that the interactions that are being considered between the repository and the natural environment are being considered with an underlying premise, that the natural environment remains essentially unchanged. I think that that underlying premise or assumption needs to be looked at carefully. I personally, along with many scientists, would disagree with that assumption. We have through years of looking at this problem have painstakingly come to the conclusion that the site at Yucca Mountain has been recurrently--the proposed repository horizon has been recurrently flooded from water from below at intervals apparently at about tens of thousands of years.

Yucca Mountain is located in a geodynamically unstable portion of the earth's crust and this is manifested by several lines of evidence; the abundant local active faults at the site, volcanic cones around the periphery of the mountain, high heat flow at the site and thermal anomalies revealed in the lower crust and upper mantel from seismic tomography. There's abundant springs in the region and having reactivated springs at Yucca Mountain would not be a particular anomaly for the region.

I suspect that site conditions will continue to change in the future and that flooding of the repository can be anticipated. If this world-class, deep water table were to merely adjust itself and become normal for the region, the repository would be flooded. Considering the possibilities that the repository might be flooded, I pose two questions that I think are relevant to the subject matter of this meeting.

The first question is obvious. What is the probability that it will be flooded? Perhaps the probability during time period important for waste isolation perhaps is something like one in ten, but it is not zero. And yet, I see the premise of this meeting being based as though the probability were zero.

The second question that I think needs to be addressed, and I appeal to people working on the project to at least consider these things, what would be the consequence if it were flooded? I don't know the answer to that. But I see that the arduous work and the important work going on here is being done as though the site is unchanging. We have limited calcium being discussed. There's a lot of calcium supplied from below. A lot of the basis assumptions that are being made throughout the meeting seem to be based on the idea that the existing natural environment remains unchanged and I simply wish to challenge the assumption.

DR. BREWER: Fine, thank you very much.

Are there other members of the public who wish to speak now? Yes, please; and stand and identify yourself, if you would.

MR. JOHNSON: I'm Cady Johnson from Woodward-Clyde and M&C, and this is a comment related to data needs related to gaseous flow and heat transport, and I guess I'm hoping that the comment might stimulate a response from one or more of the board members to either focus attention on it or not.

Really there were two data needs mentioned, one yesterday, one today, that I think I'd like to offer an approach to resolving. The first was the difficulty that Tom mentioned in dealing with the large scale buoyant convective gas flow and a time scale of site characterization, and the other was the fairly big reference to research needs related to the surface ecosystems study. And so what I'd like to suggest, and I hope that I'll be able to generate a comment or two, is that by comparing the temperatures on out-crops during times when the barometer tells you air should be exhausting with--this would be looking at the diurnal cooling. Compare the cooling during the time when air should be exhausting with the time when air should not be exhausting during static barometric conditions. If the out-crop is affected by the air flow, then you should have areas that cool more slowly when it's exhausting than when it's not. So what that allows you to do is to basically have identified features that would be the focus of both the ecosystem studies and also they would represent the gas out-pool locations. That should be useful in looking at the importance of heat pipes and direct gas exchange with the ground surface.

So the board did make a recommendation and it was following Ed Weeks' presentation in the June '91 meeting that thermal imaging be looked at to address this problem and in the little bit of work since then, basically non-Yucca Mountain project work, it looks like that's feasible. It's not as simple as just going out there and putting a thermal scanner on the out-crop, but you probably would need to have comparison between times when you would expect air exhaust, with times when you would not expect air exhaust and by differences you find your out-flow locations. So really, that's the suggestion. The project, as far as I know, hasn't been able to implement that recommendation. There are so many competing priorities and I think probably the reason is just that it hasn't been a focus of attention, you know, do we really need to know these gas out-flow locations. I think if we really do need to know those, there's a fairly straight forward way to go get them.

So I guess what I'm hoping for is whether any of you are willing to provide any feedback on whether that's something we need to know.

DR. BREWER: Is this something that can be answered on the spot, or should we take this up as a panel?

Don?

DR. LANGMUIR: Don Langmuir. I was going to suggest that I could pass the buck because I see Bill Dudley's back there in the audience and he talks every day with Ed Weeks, and maybe he could comment on it.

DR. BREWER: If he would care to, yes.

Identify yourself please, sir.

MR. DUDLEY: I'm Bill Dudley with the USGS. I'm trying to remember how many weeks it's been since I saw Ed Weeks.

Quite some time.

Temperature measurements of course that Ed has dealt with so far have been basically those of air discharging from boreholes that penetrate the rock. I think Cady is talking about measurements over a wider area of out-crop and in that sense I would expect shallow soil temperature measurements. The possible approach to that might be a fairly widespread uniform network over areas where the fracture tuffs exist where nearly continuous records could be obtained or at least periodic records. To my knowledge, Ed has not instituted anything of that sort, and if anyone here knows his plans better than I, having seen him more recently than I, I invite them to go ahead and tell you about that.

DR. BREWER: Good. Thank you very much.

I think it's time now to move on to the panel portion of this, and let me remind everyone in the audience that the use of a panel at the conclusion of a day's session is a relatively innovation for the board. The idea in this particular case is to offer an opportunity with a representative sample of those who have presented and several who have not to talk about major issues that came up in the course of the two days discussion. In this case, the general topic and theme was the consequences of thermal loading. And basically because I am not a thermal-loading person, my admission up front, Don Langmuir who is the co-conspirator in all of this, has agreed to help chair the panel. I would like quickly to remind everyone that several members of the panel have already performed and have been introduced. And these include Bill Halsey of Lawrence Livermore, Carl Johnson from Nevada, and Larry Ramspott who presented earlier today.

We have three new members of the panel and I would like to identify them and then have them spend just a minute or two explaining who they are, and then we'll get right to it.

DR. LANGMUIR: Garry, by the way, Bill is new. Bill Halsey is new.

DR. BREWER: Oh, I'm sorry. I thought you presented yesterday. My mistake. Excuse me. All right. Since you're new, you get to start. Bill Halsey. Please, if you would, sir; just identify yourself quickly for the purposes of the audience and your leader.

MR. HALSEY: For any of you that don't know me, I'm Bill Halsey. I'm at Lawrence Livermore National Lab. I've been involved in the program for a number of years in the areas of waste-package materials interfaced with the design effort, and in the performance assessment and interface with some of the systems engineering.

So now we have a few minutes for comments or just introduction?

DR. BREWER: Just introductions, then we'll get to it in a moment. We're just trying to identify the cast of characters.

Paul Gnirk of Table Top Consultants. Paul?

MR. GNIRK: Yes. My name is Paul Gnirk and my career began strangely enough in April, I think it was, of 1971 when I was on a panel that was formed to reviewed Project Salt Wall and site characterization results in the repository design and all the rest, and I was probably perhaps the only person in this room that's ever been in Lyons, Kansas in the salt mine, except perhaps Bill Dudley or Gene Roseboom. But since that time I've had the opportunity to work I think in every project and every capacity as a consultant. I was involved in the competence rule making testimony, development and promulgation of 10 CFR Part 960. It was in the front end of the comparative evaluation of the repository sites, of the ESF designs a couple years ago, and I spent twelve or thirteen years as DOE's representative to the International STRIPA project in Sweden.

Thank you.

DR. BREWER: Thank you, Paul.

Our next newcomer is Rosa Yang of EPRI.

MS. YANG: I'm Rosa Yang, EPRI. I have the responsibility at EPRI of fuel performance, storage and high level waste repository. I'm totally new in this game. This is the first time I attend the TRB meeting. I have the responsibility of high level waste since March of this year. I'm trained as a nuclear engineer with nuclear material. UO_2s, zircaloy is my background. I know very little about a lot of the stuff here, but I'm interested to learn.

DR. BREWER: Good. Thank you, Rosa.

And our last panelist newcomer is Tom Cotton of JK Associates.

MR. COTTON: Right. I'm actually a member of the M&O team. I've been with the M&O since it came on board. I've been working on strategic planning and contingency planning with DOE in that capacity. Before that, I was with the Congressional Office of Technology Assessment for eleven years, and in 1978 I got stuck to the nuclear waste tar baby when I inherited the directorship of a study on high level waste management. So that occupied me for eight years at OTA and I've not been able to escape it since.

DR. BREWER: Good. Thank you very much.

Now, to remind everyone, the basic point of the panel is to serve as a summary of the two days events and it's not strictly limited to the matters discussed since noon.

Let me return to my place and become a panelist. Just a moment.

(Pause.)

DR. LANGMUIR: I'm going to start this off with a question or two that kind of bridged the days. At least that's the hope. I'm hoping to get some attention, a little controversy perhaps.

On one of the board's trips, I don't recall which one it was, we learned about the born loser which was a very sorry kind of a fellow who happened to live on top of a repository and put his well down into it with which he watered all his plants and fed his kids. And I'm suggesting I'd like to hear someone talk about the unlikelihood of the born loser heat pipe into the repository. Namely, the worst case would be, I would think at least, packages in fracture systems which leaked or open and porous or got that way readily and with heat pipes never got above 100°, the system above it stayed at boiling so you're on the liquid vapor curve, and periodically, maybe 10% of the time, the water dripped onto the packages at 100°, and on that basis you're going to corrode about three millimeters or three centimeters, I believe, in a thousand years of carbon steel. This is certainly to me the worst case. I guess the question is, how does one address this and by study of the site heater test perhaps discount it. I view it as kind of a bounding worst case condition that one might have to defend against. I'd like to have some comments. Bill Halsey and I talked about this and he doesn't buy it, but I'd like to hear what he thinks.

DR. BREWER: This individual that Don's talking about also drives a Yugo.

Would Bill Halsey like to take a whack at this?

MR. HALSEY: That is the kind of worst case scenarios that you have to look for and see how credible they are. The problem is having enough water to keep the waste-package wet and yet not so much that it becomes cool. Having enough water that it drips on but not so much that it rinses off the ionic species that you're concerned about getting aggressive water. And you have to have above it a geologic system, a hydrogeologic system which stays at this precarious balance point of just putting the right amount of water on it for very long times. If you have not enough water for it to be wet most of the time, then you don't have aqueous corrosion processes most of the time. If you have a lot more water than you don't have concentrated ionic species and a very aggressive environment. You can have a very aggressive environment but it's very difficult to maintain that critical balance for very long time frames, both on the waste-package surface and in the thermally perturbed hydrogeologic system above it. Yes, you can have these worst case conditions, but they're really on parametric boarders between processes, either having enough heat to drive the water away or not enough heat to have the waste-package hot and now you're down to lower temperature processes. That's a quick summary. Yes, you can have very aggressive conditions but maintaining them for very long times is trying to maintain a delicate balance.

DR. BREWER: The lower temperature process is where you end up. That's even worse, isn't it? Because then you have wet conditions perhaps coming down your fractured system and in contact with the package and you're looking at this corrosion, like we heard this morning, of .3 millimeters per year.

MR. HALSEY: Yes, but you don't then have the combination of evaporation and a very aggressive water on the surface that we were discussing. You can have a variety of bad affects but trying to keep them all in operation at the same time is actually less likely than having all the good effects happening at the same time.

DR. BREWER: Anyone like to follow-up?

MR. GNIRK: Paul Gnirk. I think you can probably estimate the probability of that, Bill. What could happen. But I'm more interested--let's assume it has a probability of one chance in a thousand or something like that. What's the consequence because you have to still weight the consequences by the probability to get the risk of what's going to happen. So what do you think the consequences are?

MR. HALSEY: Well, that gets into the subsystem analysis and ultimately how that couples into the total system analysis. We heard this afternoon about the first efforts to put the temperature dependent processes into the total system performance assessment. And some of those are these good and bad effects. Can you keep the waste-packages dry and what are the corrosion processes of the waste-package failure, the mobilization and transport mechanisms as a function of temperature. And by putting in distributions of those, the parameters of which come from best estimates from the experts, we're just getting the first estimates as to the results of those distributions; and I think part of the things that were described by the performance assessment people is the sensitivity studies that will be done after the tool is completed and that will show you to a first cut, a very crude level, the probabilities of these things adding up or occurring and how long they persist and allow you to figure out the sensitivities of which ones are most important and do you need better descriptions of.

DR. LANGMUIR: Bill, what kinds of on-site tests or heater tests could you propose that would give us some information, some data that would allow us to discount this or statistically address this question?

MR. HALSEY: I feel like I have a lot of people feeding me questions to--

DR. LANGMUIR: You can always pass.

MR. HALSEY: --address the issues that I had listed down here.

DR. LANGMUIR: You can pass the buck or to someone in the audience or a speaker from the day.

MR. HALSEY: We heard a variety of different processes in both the engineered system and the natural system and the testing that's going on to try and understand those. And I think we heard a lot of good plans for those tests. There's a few that we didn't hear the plans for and that may be what you want to hear. One of the critical issues that was identified by Bob Andrews is the water contact mode on the waste package, and that's what you're alluding to. Is it a moist continuous pathway which is diffusive? Is it dripping? Is it continuously wet? Is it trickling past? And yesterday we heard the likelihood of producing water fluxes due to the hydrothermal processes. The connection between those two, there's still a gap and that is how does hydrothermally driven water turn into water contact? And it has a lot to do with the design of the engineer barrier system, your backfill, how does the water diffuse through the backfill. If it flows down a crack and gets to crushed tuff backfill, what happens to it? And I think there's some general plans to do those tests, but they have not been planned in any detail and I think they're very important. And they would help answer the probability question that you asked, Paul. How likely are you to be wet for what fraction of the time? Right now, in the total system performance assessment, those questions, what is the probability distribution and the time distribution, are estimates.

DR. BREWER: Yes, Larry Ramspott.

DR. RAMSPOTT: Yeah, I have a little bit of a problem with the heat pipe scenario that you just raised because by definition a heat pipe is a closed system, it's sealed. Otherwise it won't operate as a heat pipe. In the geothermal fields that we were having described to us are sealed. They have a caprock and they're basically sealed systems. At Yucca Mountain, to start with at least, the mountain is open at the top and it's open into the drifts. The whole system is open. You can't have water drifting or flowing out of these cracks into the drifts out of essentially what is a sealed system. So I'm having a problem with having the concept that is essential that you have a sealed system versus this openness which lets the water flow out on the drifts. The Weeps evidence that we've had discussed suggests in private conversations at least that Tom Buscheck has told me that we're talking about 50 darcy types of permeabilities for the mountain, and those are very large. I don't see how we get heat pipes unless we have some form of ceiling before that. And if you do have a heat pipe there, then all you're going to do is use that as a heat transfer mechanism, just going to have the water boiling, coming down and boiling. It isn't going to get back down in to the drifts. So I'm having a problem coming up with the same scenario you do.

DR. LANGMUIR: I see Bo Bodvarsson back there and Tom also. These are heat pipe experts and let's get them involved.

MR. BODVARSSON: What are heat pipes? I have one response to your question. The systems as I see them when they evolve, they may not have any caprock at all. The caprock is the result of the hydrothermal activity, a lot of it being chemical sealing due to the temperature. That's one way to seal off your Yucca Mountain. Second one is that if you take a look at some of the results Alan Flint has and some of the USGS people, you have various stratigraphy within the mountain. This is not a very homogenous body. You have confining layers with very, very small permeabilities in the mountain where we don't know if the permeabilities are continuous or not. And those might also provide the caprocks to the system, so it wouldn't have to be developed chemically.

DR. LANGMUIR: Tom Buscheck.

MR. BUSCHECK: Tom Buscheck, Lawrence Livermore. I wanted to highlight something that Dale's taught. He showed a five drift heater test which heats on the order of 100 meters by 100 meters, and I had failed to remember that. In fact, that was our "reference" case that we had right now, though it's subject to change. So the scale that he described yesterday would almost be accommodated by that heater test design. It's something that's in its very preliminary stages, something I referred to yesterday, is we're developing geostatistical means to take calculations of average condensate flow and put them through the geostatistical filter as you might call it, and utilizing what data we have now, we're utilizing--and this is kind of a test mode--the STRIPA data and finding that it does take a tremendous amount of focusing of flow in order to maintain two-phase conditions, a liquid phase flow at the waste-package. And as a result of that, the probability of an individual package receiving this condensate flow is relatively low. You have to take a large volume condensate from a larger region and hit an individual waste-package and it's rather preliminary right now to quote the probabilities we're calculating, though we looked at a variety of thermal loads and there are quantitative differences and the differences seem to be effected by how much of the rock you have been able to boil.

Another point that wasn't mentioned in Dale's talk is that we're planning to have an alcove sitting underneath our heater test where we're going to be collecting condensate flow that's generated by this boiling process. That's going to be more readily accomplished than collecting fracture flow within the heater drift. So that would be at least something that we would like to attempt. So we will be getting data similar to what they collected in STRIPA looking at the variability of the return flow condensate and hopefully better able to get some better definition of statistical parameters that are relevant to this process at Yucca Mountain.

DR. BREWER: Thank you very much.

Let's stick to the panel, then we'll go to the audience.

MS. YANG: Well, mine is going to be a change in direction. As I said, it has been a very stimulating two days for me to learn the many new issues and I'm trained as a nuclear engineer with nuclear material background. My expertise is in designing of the fuel elements, so during two days I kept thinking boy, those guys really miss all the important things. There's a cladding there. The important thing is to see how cladding would survive during all those years and how fuel would perform. So I would actually come out with a totally different list of things like solubility, diffusivity, creeps and all those things. And by mentioning that, I'm not trying to add to the list. On the contrary, I'm thinking about we need to prioritize and focus more on what things to do because a lot of the very interesting thought are interesting things to do but they are not necessarily necessary for the repository system.

As you all know, the livelihood of the nuclear industry really hinge a lot on the success of the repository and if you have followed the on-site storage problem closely with the nuclear industry, several of the reactors face being shut down prematurely because of lack of progress of repository. So we're quite concerned from the industry point of view about the lack of progress of the repository system.

And now, okay, I said we need to prioritize and I hear a lot of prioritize, a lot of trade-off, compromise being made. But from my personal point of view, I think that's not the best way to prioritize. I think the best way to prioritize, I'd like to submit, is to use total system performance analysis. And the reason I say that is because designing of a geological system from what I hear in the last two days certainly confirms it's a very, very complex engineering system and it requires many multi-disciplines of engineering signs and as I just illustrated with my example of what I think is important is totally different from what many of you think are important. And none of us is wrong and the important thing is to use a scientific way to decide not what is right, what is wrong, but what needs to be done, not what is interesting to do. And the way to do it is not from peer review. I think peer review and design review are very efficient processes when you are within the same discipline. When you have multi-disciplines I think each one of us are unfortunately trapped by our own expertise. We see what's most important for us and we see what could be improved to make these uncertainties, discrepancies smaller. And all these, like I said, are good to do but not necessarily necessary to do.

And again, I want to come back to total system performance assessment. The importance of that I don't think

I could over-emphasize. I like to disagree quite with the importance of it with Jerry Boak. I think he's being extremely modest about the usefulness of it. I think it shouldn't be a mouse in a zoo. It really should be the brain of a system. You know, I say this not just because my own bias--well, maybe it's because of my own bias. Let me share my own bias with you. Before joining EPRI I had been working in General Electric for ten years and in there my job is mainly design of the fuel element, and the way we design things is to use a fuel behavior code which I think is the same terminology you use called total system performance assessment. And we don't do experiments because it would reduce uncertainty or it would increase the knowledge of certain aspects of it. We actually do a more rigorous way. We use the fuel behavior model, we evaluate the uncertainty and the impact of that on the whole system. And if it turns out the temperature uncertainty is X at a certain position, we don't do experiments just to shorten that. We ask ourself how much design margin there is at that particular point and if there's plenty of design margin, we live with that imperfection. So this is what I would like to propose, the more engineering approach to this whole thing. A lot of things will reduce uncertainty, would improve our understanding, but they may not be necessary for the designing of the repository.

I'd like in closing to quote one of my favorite professors, Professor Pigford of UC Berkeley. That's where I get my training in nuc engineering from. And the reason I like to quote it is because he summarized what I just said much more eloquently than I can. Here we go. "Challenges of what is important and necessary as determined from objective performance analyses are sometimes contrary with the claim that we must fully characterize the technical features of a repository to develop sufficient understanding of what we are doing. This is rhetoric without logic. For the mission of the repository program, sufficiency of understanding is met when a suitably reliable assessment of successful performance has been made. Not perfect performance. Complete understanding and characterization are not necessary, nor can they ever be achieved."

Thank you.

DR. BREWER: Is there anyone on the TSP crew who would like to agree, disagree or maybe comment? Jerry?

DR. BOAK: I'm Jerry Boak, Technical Analysis Branch Chief for the Yucca Mountain project. It's an exciting perspective and much of what I've learned about total system performance came from a bunch of delightful meetings with Dr. Pigford. Many of us have had that sensation of going through orals all over again in front of him.

I think that the difficulty with using total system performance assessment to answer all questions about priority is best addressed by referring to the viewpoint of Felton Bingham, another one of those delightful gray hairs of the performance assessment field who for many years resisted the idea of rolling up of our knowledge into a CCDF because he really felt that the product gave us no insight that was not already available from the lower level models. He was delightfully surprised when he actually did participate in doing so to find that in fact there were insights that were to be gained and there also was a great advantage in communication from doing that exercise. So to the extent that it's possible, I would love to be using the total system performance assessments to indicate why we think certain things are so. But because engineers and scientists often have a difficult time talking to each other, there are other parts of the problem that have to be addressed. And, for me, a total system performance assessment involves exercising multiple levels of the pyramid.

With respect to Don Langmuir's question, I did want to say that one of the things that you do from a total system perspective when you are faced with the possibility of a born loser heat pipe victim is that you decide it must be there and you put them in there and that's why you may have heard Holly Dockery refer to the mission impossible waste-package for years when we've done any kind of assessment of the performance of the waste-package that's involved, having a waste-package that once breached vanishes instantaneously. And that is, of course, the source of Rosa's assertion, that we need to be looking at the cladding and we need to be looking at the solubilities. We have looked at solubilities. I think what we hope to get out of our next iteration TSPA is to have a little better understanding by looking at the thermal affects and the thermal coupling of the sensitivity of the system performance to the born loser fraction.

DR. BREWER: Thank you very much.

MR. SATERLIE: Very briefly. I'm Steve Saterlie with M&O. To just answer the question about trying to prioritize those elements that are the most important, although not clearly brought out in my presentation, that's one of the things that in the system studies that we are trying to get a handle on and do. So how successful we'll be I think remains to be seen and we'll take another look at that in a few months.

DR. BREWER: Thank you.

The whole question of wise heads was one that struck me on and on in here and what I'd like to do is to warn three of the colleagues on the panel, Paul and Mary and Tom, who have been in this game for a long, long time, the conceptual model that one sort of starts out with often determines everything that follows. The assumptions that you agree to either agree to accept or not or seldom do we come back to it. And I was really struck particularly in Gene Roseboom's presentation today with how powerful the assumptions are in arriving at certain decisions about loading and certain decisions about a range of things, and in this case talking about our capacity to retrieve, because maybe we want to keep that option open. And I'm wondering to just sort of alert you, are there classes of assumption or other kinds of assumptions of this sort bearing on thermal loading or anything else that in your opinion or view or experience as wise heads really have important impact on where we are right at the moment. Long sort of statement but I hope you get the drift of what I'm up to and just sort of Paul and Tom, let you bat clean-up.

MR. GNIRK: Well, Larry, Tom, the conceptual model of the site as you see your diagrams on the board are basically the same almost for the last ten or twelve years. It's been upgraded with additional drill holes now and then and some data from off-site and everything else, but we're still dealing essentially with the data that we had ten years ago.

And the calculations that were made for thermal loading back in the early 80s were made on a basis of that conceptual model, on the basis of that stratigraphy, on the basis of performance constraints that were very similar to those described by Steven Saterlie. In fact, they were more conservative in many regards than what Steven has shown. And, in fact, we did the calculations for all four horizons. I don't if people know that or remember that, but the calculations were done for the Topopah Springs, Calico Hills, Bullfrog and Fran, and all four horizons were considered as potential repository emplacement zones and they were sent through a screening based on all the different performance constraints and I can tell you what finally drove this magical 57 kilowatts per acre, which I'm one of the parties responsible for that, was the conditions for operation of the repository. The operational aspects of retrieving waste out of rooms that were subjected to high heat loads over periods of 20, 30, 40 years. It was not necessarily the temperatures in the far field, it was not the rates of uplift, it was not the damage to the rock and the far field or the near field. It was in the very near field, around the canister and in the rooms, and that's essentially what drove the 57 kilowatts per acre in the Topopah Springs and similar considerations in the other three horizons. The model that we use, and I think it's the same thing that Keith Johnstone and Ralph Peters and I have in the Decision Framework Report on picking the Topopah Springs or our recommendation is what you see these days. There's some more detail here and there but the conceptual model is essentially the same.

There's been improvements certainly in the hydrology and additional information. People have developed a lot better computer calculations to give us insight and so forth. I don't know, that's how I view it.

DR. BREWER: Any other assumptions we ought to know about? Tom?

MR. COTTON: Yeah. I was going to raise one. It's an assumption in a different level and it's one that I became pretty aware of recently. I worked with a small DOE task force coming up with alternative program strategy you may have seen. And in that we looked at an alternative model for how one goes about developing the repository and I became aware that there's really been a conceptual model that we're going to design the repository. And all these design things that we're talking about, thermal load and all that, are essentially it's a one shot design and that's in and then we go do it. And that brings in a whole lot of issues and concerns into deciding what is my initial thermal load and so forth. If you look at it differently and think about a step-wise process of what can I do to get started and establish proof of principle that we have a disposal system that works and then perhaps change things as we get better information and data and do some of these longer term tests and maybe increase thermal load or whatever, you might come up with a very different answer about how one proceeds. And that's an almost unconscious model I think a lot of people have in their heads.

DR. BREWER: Larry?

DR. RAMSPOTT: Well, I have a viewpoint that the safety argument sets the priorities, not the total system performance assessment. The total systems performance assessment is very generalized, highly assumption dependent. But the safety argument is basically how at least the priorities have been set in the past. For example, my view of what the safety argument for the Yucca Mountain repository under the present SCP is that everything happens in the Calico Hills. Basically there's an assumption of matrix flow in the Calico Hills. The waste-package really only meets the NRC regulations. It'll last for a thousand years for substantially complete containment and meet the 1 and 10 to the fifth release rate. After that, it simply releases into the Calico at one to the fifth release rate assumption of a millimeter per year downward flux, and then everything proceeds from there. So basically the priorities of the present site characterization program are largely set based on that. Now if you made a different safety argument, for example, if you made the safety argument that you wanted to go to prevention of anything ever getting out other than mitigation after it has essentially been forced to get out, you could say we don't ever want any water come down the surface and get to the waste. We want to prevent any water that's in the rock from ever getting to the waste, and then you can analyze things. And what you would do probably there is focus most of your site characterization effort between the repository horizon and the surface, whereas now much of it, the main focus of it is underneath in the Calico. So basically I think the problem that needs to be done is there needs to be a very clear understanding of what the safety argument is. It either has to be one that's in the current TSPA or some modified version of it, and then the priorities will fall into place.

DR. BREWER: Anyone care to follow on that?

Rosa?

DR. YANG: Can I just clarify what I said about total system performance? I'm not arguing that the model right now is adequate to the point to drive the whole system. But my concept is that ought to be the case. If the model is not there, let's improve on it. After all, we're talking about designing a repository which would be able to fit certain criteria, and what to me, very new in this game, is surprising to know the criteria really haven't quite been set yet. We're in the process of setting the EPA criteria. But nevertheless, whatever criteria is set, the whole program should be designed to that criteria. And the PA model is not there. Let's improve on it. But again, I'm not seeing that, I'm not seeing a more system approach. I'm seeing, well, because of this, therefore this is what we are doing. So that's my whole criticism about that and again based on my own experience that the nuclear industry has designed millions of fuel rod and most of them perform perfectly. And the whole process of what to do, what is important is based on using a fuel performance code, because that's the only way that I know of logically to really prioritize various scientific disciplines, you know. Otherwise, there is no scientific way to really do things quantitatively even.

DR. BREWER: Larry, did you want to follow or go to Carl?. Carl

MR. JOHNSON: Yeah, Carl Johnson. Rosa, I want to take exception to some of your statements you just made and it's mainly because I think I've become very sensitive to phrases and the way things are. I don't think that the purpose of the phase of this program we're in is to design a repository. We're in the phase to do site characterization to determine whether we have a site that we can design a repository. So I think we need to be looking and studying the attributes and the conditions of the site and we should be prioritizing those particular studies to make sure we are focusing on the key issues of characterization.

DR. BREWER: Rosa, did you want to respond?

DR. YANG: No.

DR. BREWER: Okay. Don Langmuir has got sort of the next line.

DR. LANGMUIR: I'm the rocks person up here, according to Garry. It just occurred to me that we had our first introduction today to the planned heater test in some detail, at least where they've gotten in this juncture. And I was curious how the modelers who are going to use the information from those tests to enhance their models and their function and parameterize and then validate them in a sense, how they feel about that and whether they have suggestions as to perhaps how the tests could be done differently, could be enhanced, could be emplaced differently. I had an opinion this morning which was perhaps--we didn't really talk about it--but why don't we stick some heater tests right in fracture zones where you might expect to see pipe effects?

But if I can get the modelers from yesterday to react to this question and then have the heater test folks react to them, that would be constructive.

Bo is sitting back there. Here's Bill Murphy.

MR. MURPHY: Bill Murphy, Center for Nuclear Waste Regulatory Analyses. I had one observation during today's presentations that there were very interesting studies being designed by different groups, one emphasizing the mechanical affects and another the hydrological and hydrogeochemical affects. And it seemed to me that in some instances with a relatively small additional effort, there could be synthesis of these and that was addressed with the largest scale case certainly. But maybe with a little extra effort, a great deal more information could be gained by integrating these studies.

DR. LANGMUIR: Any suggestions how to do the integration?

MR. GNIRK: I just had a complimentary question on what you asked. I'd like to find out how the DOE plans to use the results of those heater tests in selecting a thermal loading for the site.

DR. BREWER: Is there anyone from the DOE who would like to respond?

MR. HALSEY: I can respond some but not for DOE.

DR. BREWER: Paul, would you ask the question again? Maybe he didn't hear us.

MR. GNIRK: My question was, how does the DOE or the M&O or whoever plan to use the results of the heater test to select a thermal loading? Because as I gather from the timing of the decision frame work, that very first heater test that had to be done very quickly was an integral part of picking a thermal loading for the site, unless I misunderstood the presentations. And I'm curious how they're going to use those results to arrive at the thermal loading for the repository.

MR. SIMECKA: This is Bill Simecka, Yucca Mountain. We're not going to necessarily limit ourself to that one test. It's a combination of the large scale test, the laboratory tests that I talked about yesterday, and the accelerated ESF tests and the long-term in-situ tests. And based on all of those, we will be assessing as they go along when we think we know enough to determine what's going to happen to the near field environment and the rest of the site based on these tests. And so, we don't have an algorhythm that says we're going to use the results of tests one, two and part of three and automatically say that's what we're going to use. Because as I said earlier, we weren't schedule driven. We were going to assess it as we go along until we have enough information.

DR. BREWER: Paul?

MR. GNIRK: May I ask one more question while you're there please?

MR. SIMECKA: Sure.

MR. GNIRK: I'm curious as to how you're going to develop a design that you submit with the license application, whenever that is, around the year 2000, without having selected a baseline thermal loading to gear that design to early. Because as you I'm certain are well aware of and understand, the design process for this is going to be extremely complicated, it's going to be very detailed. And if people are going to be designing for a range of thermal loadings over a factor of two or three, if I was on the regulation side I have a hard time buying into that type of thing. And I'm curious as how you're going to, as I asked before, you're going to select the thermal loading on a basis of all these results, but at the same time you're undergoing a design that has to go with the license application, and I find that a very complicated set of circumstances.

MR. SIMECKA: I agree. The issue is, though, that as we find the results of the analysis and the tests that we're using to check the analysis, when it becomes such that we are comfortable in initiating the license application design, we will then go forward with one thermal loading. That may be a narrow range but we'll go forward because we've got enough confidence. We will continue to test to validate the decision that we have just made, but we're not going to initiate the license application in my design, in my mind, until we have a pretty good idea of what the thermal loading is.

DR. BREWER: Yes. Bill Halsey.

MR. HALSEY: That is one of the questions that is a real constraint on the schedule and the license application design, as DOE indicated before, they will be carrying the multiple options to some extent. They may have a preferred and backups along to the extent necessary. The timing that we saw from Dale Wilder's presentation for the accelerated PSF test corresponds to obtaining the hypothesis validation for invalidation information that Tom Buscheck showed in his modeling in time for the license application design decision. And that's the critical linkage between the hypothesis testings hierarchy that Tom Buscheck, the accelerated portion of the testing Dale Wilder showed. If we had more time, you didn't have to do the accelerated test. You could do the long test. And then you proceed at risk into the license application design. If the long-term test disagrees with the accelerated test, you are then going to have to change your schedule back up and do something over I think.

DR. BREWER: Yes, Rosa? Did you want to follow up?

DR. YANG: Yeah. I just want to kind of maybe repeat myself again. The purpose is to maybe not now but eventually design a repository and the importance is the leakage rate to the health of the public, and I consider that the most important thing. And from total system performance based on our calculation, based on IMARC code, that there is no difference in terms of hot versus cold in terms of releases. So from our point of view, that from an engineering point of view, from the public health point of view, there is no difference. We would prefer the hot repository because it's compatible with MPC and compatible with a lot of other reasons. So unless our understanding of the system changed, which in the same time would modify our model, but based on our current understanding, the IMARC code that shows there is no difference. So just to illustrate my point from our point of view, we wouldn't put a lot of resources in that area.

DR. BREWER: Tom Buscheck wanted to say something here.

MR. BUSCHECK: Our total systems performance assessment from--Tom Buscheck, Lawrence Livermore--from a hydrological perspective, is it's only as good as the process models feeding it. And only in the last four or five months have we been identifying new potential sources of liquid water which may pertain more to the subwetting repository. And so, I don't see how one could incorporate those mechanisms and any analysis which would differentiate between hot versus cold. So I think that since we haven't identified even in a gross sense until recently and perhaps have a lot more work to do in that regard some of the major ways that heat can drive liquid flow to the repository. I think it's very premature to say that there aren't quantitative or qualitative differences between them.

DR. YANG: Can I respond?

DR. BREWER: Please do.

DR. YANG: It may be premature but my whole point is not we have a perfect model. My whole point is a systematic approach.

DR. BREWER: Bo had his hand up.

MR. BODVARRSON: Bo Bodvarrson, Lawrence Berkeley Laboratory. I have a couple of comments, one with respect to your comments, Rosa, about using the engineering approach. I think we all have to recognize that we are faced with a problem we've never faced before. We have to predict something for 10,000 years or longer, and that's very difficult to use some kind of standard methodology to do that because we don't know what to expect over the next 100 years. So what DOE is doing, and I think is a very good approach, is to use the broad approach in trying to understand the system as much as possible before we start any kind of design work and kind of try to form a methodology for doing that.

So another comment I have about the heater test then, I think all of us agree that heater tests are very essential and I think the comments Don made about the heater tests and how they're going to be used in the models is very relevant. As you heard yesterday from my talk, I'm all in favor of heater tests but I'm a little concerned about the scale of the heater tests. We can never test all over the mountain and my geothermal experience indicates that the features that dominate the heat transferral are on the order of 100 meters. That's a concern to me but the heater tests are very essential and very important, and it's very important to design them properly, to put a lot of thought into where we are going to do the heater tests and how we are going to do them and what we hope to get out of them. And so, the comments that Don made about testing of specific features I think might be very important too. If we don't see heat pipes from the heater tests, in my view, it does not mean we're not going to see it in the mountains because we haven't tested maybe sufficient volume. If we see them, on the other hand, it's very likely we'll see them in the mountain. So I think a lot depends on what we see from the heater tests. So the only thing I urge is that we really spend a lot of time, Livermore of course, and some of the other participants, and really think carefully about the heater test, because they are very essential.

Final comment. I thought it was kind of funny. After my talk yesterday, after all of my talks about heat pipes and geotherm analogues, one fellow came to me after the talk and said that was a good talk, but how much is it going to cost to put all these heat pipes in place.

DR. BREWER: That's a very good line.

MR. CHESTNUT: I'd just like to comment a little bit on some of this.

DR. BREWER: Please identify--

MR. CHESTNUT: I'm sorry. Duane Chestnut, Lawrence Livermore. This discussion about the use of total systems performance and a more engineering oriented approach, I have absolutely no quarrel with an engineering approach nor with a scientific approach. I'm a little bit of each. I'm a registered professional engineer and have a Ph.D. in physical chemistry. So I think I can look at both sides of this issue. I have a problem with relying too heavily on performance analysis for this problem. We have no measurable performance of a repository that we can go out and make a measurement and compare it with the model prediction. Every connection between what we can measure and the performance of the repository over the regulatory is indirect. So we have to rely on fundamentally a scientific approach, mechanistically based and it has to be tied in through long-term performance models, but it still doesn't give us the same kind of feedback that you get in designing fuel rods because you can measure the performance of a fuel rod. You can set certain measurable standards that you can go out and measure the strength of the cladding or whatever. But we simply don't have that kind of a situation and I think that's something we need to keep in mind. Just isn't an analogous problem. And I'd also like to suggest that it's too easy to get into the mode of thinking that this is an engineering project because we're going to dig a bunch of holes in the ground. As an engineering problem, I don't think this is really all that difficult. We've got lots of experience with mining, carrying nuclear materials around, all this kind of stuff. What is difficult is to make people believe this thing works when we get through. So our real job is to construct the confidence in the public that this repository actually does its job of containing waste, and that's a different problem altogether.

DR. BREWER: Thank you very much.

Mick Apted had his hand up.

MR. APTED: Mick Apted with INTERA Sciences. All this engineered and natural barriers is reminiscent of the taste great less filling debate, and the aspect of both sides are true. I mean, the whole purpose and basis worldwide is multiple redundant barriers. But I guess the point I want to make, we heard a lot in the last few days about the uncertainties and the variability in the far field, and I think that's a given and inherent and there's a certain amount of irreducableness to that. One of the things, getting back to the thermal issue, wondering about the near field, is that I believe that Pigford or some of his students have done some calculations where they've looked at how much water will vaporize just again below 100°, how rapidly will the water vaporize coming into contact with the fuel. And I believe that their estimates were that for several hundred thousands of years, even when fuel surfaces below 100°, that the rate of water infiltrating then is insufficient to sustain or keep liquid water on that surface. And I was wondering perhaps if Bob or Holly were going to be looking at that in their next year's analysis in terms of trying to do a balance between--they're doing a lot of studies on water coming in and that's sensible. But it seems to be one of the basic fundamental intrinsic parameters that might be available and very limiting in this case would be that the water coming in cannot come in fast enough to sustain a liquid water film on that surface. And a lot of the time we're making the transition that from Tom Buscheck's model of water coming in and Don's episodic fracture, to assume that water is going to be able to contact the fuel. And I think maybe right there's a basic intrinsic. We know we're going to have spent fuel and looking at the intrinsic property of the fuel itself may give us some guidance. It may be even if we have early container failure that there will be no water contacting the fuel, sustained contact with the fuel for hundreds of thousands of years.

Bill is sort of nodding his head. Maybe you can respond to that.

DR. BREWER: Bill, would you like to try?

MR. HALSEY: I think that's correct and I think that is some of the process I was discussing earlier. We need to go from the hydrothermal flux to water contact. We're starting to do that and put it into the performance assessment models. The TSPA presenters today didn't have time to go into the details of that but it is a first effort to incorporate--and it goes back to what you said, Paul--what is the probability of these things happening and what is the probability distribution over the repository as a function of time. Because these things are all changing and that goes back to the original question you asked, Don; can you maintain these adverse conditions for a very long time when everything is changing.

DR. BREWER: I'm willing to take one more question from the floor and then I think we've got oral comment and then I think we've got to wrap this thing up.

MR. MELSON: Bill Melson, the Smithsonian. In speaking of these things, Gene Roseboom proposed a situation of extended retrievability, indefinite retrievability, and yet I still hear coming forth the old thinking about let's fill the tunnel up and let's predict a thousand years and two thousand years into the future, and that isn't really possible. I mean, you all see that regarding volcanism, some of our models. No matter how carefully you look at it, you can't do it. So maybe we have to conclude we can't seal it up but we can watch it and we change things as we learn through the years.

MR. JOHNSON: Let me respond to what--Carl Johnson, I'm sorry--respond to what Bill just said and I go back to I guess my reaction Gene Roseboom's presentation and I thought to myself is what Gene is proposing is we go back and revisit the philosophy behind the Nuclear Waste Policy Act, and that the original philosophy was that we deal with this environmental issue right now so we don't burn future generations. And what Gene is telling us is he wants to burn future generations and if that's what we want to do to watch and baby-sit this thing, that's fine. But if so, then let's go back and revisit the Nuclear Waste Policy Act and we start over again.

DR. BREWER: Tom Cotton has one last comment.

MR. COTTON: Yes, I'd like to comment on that because I think there were a couple of concepts running around in Gene's paper that are very different. Some of the concepts were open in an extended retrievability underground storage, particularly the Hammond one, the early concept really was based on no argument about long-term performance, and it was purely, totally dependent. It was essentially an underground storage system with no selection of a site that was designed to provide long-term isolation. I think what Gene was suggesting is you can take the repository we have now but design it to allow extended access and retrievability which is not burdening the future. If they want to close it up, they can close it up at some point. You can design it so you are giving them more options rather than putting an additional burden on them.

MR. JOHNSON: Well, it certainly didn't come across that way. Let me add one more thing and kind of wrap this thing up and maybe bring some of this discussion back to what I consider being reality after two days, is I think that this project has actually made a thermal-loading decision. They have made a decision they are going to thermally load Yucca Mountain. Now, what they haven't decided yet is exactly what the kilowatts per acre is. I think we saw in the changed ESF configuration that it's going to be somewhere on the order of 60 to 70 kilowatts per acre. So we got a ball park of what it's going to be, we just don't have the exact number. What we're concerned about is what has totally fallen through the cracks, the other alternative, and that is the below boiling point option. And we see no plans to look at that and look at that in the same extent that one is now looking at the thermal option, which a decision has already been made.

MR. HALSEY: Bill Halsey. Just to respond to that, we're considering the suitability of Yucca Mountain for a repository which congress has mandated will be thermally loaded. You're right, we haven't decided what kilowatts per acre but when you put in heat producing waste, you have thermally-loaded a repository. And I do believe that the program appears to be addressing the concerns; modeling, testing and design issues, total system operation of transportation and storage, and repository design for a wide range at this point.

MR. JOHNSON: I think the definition that everybody has been working with for the last two days is a thermally-loaded repository is a repository above the boiling point of water.

[Chorus of "Nos!" from the audience.]

DR. BREWER: There is obviously not a consensus on that. There is consensus, however, on the fact that we've had a very intense two days with an incredible amount of information being delivered. Some of it is being absorbed. I would like to thank all of the members of the panel for coming and providing I think a very useful summary of the two days events.

I'm now about to turn this over for the benediction to our chairman, John Cantlon.

DR. CANTLON: This may be the shortest benediction on record, but I think the important point here is this has been a substantial departure from the board's typical session and we deliberately set up this kind of an exchange in which we took a particular subject area, thermal loading, and used it as an organizing theme to look at the total array from essentially the systems performance assessment at the large systems level all the way down to the minute scientific area to examine a fundamental question, and that is, how well are we taking solid science and using it as the basis for the decision? Not surprisingly, some members of the audience are skeptical. Some have even stated that the decisions are all made. I think most of the people don't really believe that, although the need to really demonstrate momentum to the people who have to pay for these projects requires that real progress be documented. And that, of course, really gives us that sort of schizophrenic feeling about these kinds of projects. But this sort of a session I think is particularly useful to look at that interplay between the quality of the science undergirding the model-making, undergirding the performance assessment, undergirding a look at how the whole system fits together. That is really the core that will lead to the public confidence which is, after all, the critical element. Is congress comfortable with it? Are the regulatory bodies going to be comfortable with it? Are the people of Nevada not going to be comfortable with it, but can they tolerate it?

Thank you very much for coming.

(Whereupon, the above-entitled matter was adjourned.)