UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

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

October 8, 1991

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

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman
Dr. Clarence R. Allen
Dr. John E. Cantlon
Dr. D. Warner North
Dr. Donald Langmuir
Dr. Patrick Domenico
Dr. Ellis D. Verink

NWTRB STAFF PRESENT

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

FOREIGN GUESTS

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

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

Nils Rydell, National Board for Spent Nuclear Fuel (SKN) Sweden

Peter Stevens-Guille, Head, Radioactive Materials Management Ontario Hydro, Canada

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P R O C E E D I N G S

DR. DEERE: Good morning, ladies and gentlemen.

It's just three weeks ago tomorrow that I had a chance to give a welcome address to those attending our meeting of one of the panels. I'm glad to be back here today. I am Don Deere, Chairman of the Nuclear Waste Technical Review Board, and it is a pleasure to welcome you to this meeting of the full Board.

The next few days will be devoted to a comprehensive discussion of the potential effects of thermal loading on the design and construction of a high-level radioactive waste repository, and I should remind all of those at the head table, as well as those who speak from the audience and use the mikes, to please speak into the mike. Speak up. Speak slowly. Identify yourself and your affiliation so that we may have a complete record.

As most of you know, the Nuclear Waste Technical Review Board was created by Congress in 1987 to review the technical and scientific validity of activities undertaken by the Department of Energy as part of its program to manage civilian spent fuel and high-level radioactive waste. In the same Act that created the Board, Congress directed the DOE to characterize one site, a candidate site at Yucca Mountain, Nevada for the possible development of a repository for the permanent disposal of the nation's high-level radioactive waste.

A question the Board has been considering as it evaluates the DOE program is: What effects will high thermal loading have on repository performance? We have designed this meeting to look at all aspects of thermal loading, and to attempt to reach some conclusions about problems and uncertainties associated with this issue. Because of its importance, we are devoting three full days to a discussion of thermal loading.

When 20-year-old spent nuclear fuel is disposed of in a geologic repository, the heat given off by the spent fuel will increase the temperature of the surrounding rock formations for a few hundred years, after which the temperatures slowly decrease, and this is known as the thermal pulse. Uncertainty about a repository's long-term performance is related, in part, to this thermal pulse. In its First Report to the U.S. Congress and the U.S. Secretary of Energy, the Board called attention to the importance of this issue and suggested that uncertainties relating to repository performance might be decreased by reducing the thermal loading of the repository. The Board also indicated its intention to explore with the DOE the benefits of reduced thermal loading, balanced against the potential cost or extra cost of waste storage and the possible needs for more repository capacity. This meeting will provide an excellent opportunity for doing just that.

The timing of this meeting is particularly fortuitous, because the Office of Civilian Radioactive Waste Management has recently issued its "Draft Mission Plan Amendment," in which thermal loading is identified as a key technical question. Quoting from page 64 of the mission plan, the question: "Should the heat load of the repository remain as currently conceived, or could an advantage be gained from lower thermal loading? If lower thermal loading is desirable, should it be achieved by cooling spent fuel for a longer period of time, or by changing the spacing or the design of waste packages in the underground repository?"

Although, as the draft plan states, "the actual heat loading to be used as a basis for the next phase of repository design has not been selected," so we're talking today about an open question. The DOE's current repository and waste package design calls for maintaining the near-field temperature of the waste package above the boiling point of water for 300 years or longer. We believe that it is important to understand thoroughly the underlying assumptions used by the DOE to support its rationale for the conceptual design of the repository, and we look forward to exploring these important issues over the next three days.

We begin today with an overview of the various repository design concepts proposed for use by several other nations. The potential benefits of aging waste before disposal are issues of mutual concern in many countries, as well as in the United States. Many European countries plan to reduce uncertainties associated with geologic disposal by reducing the thermal loading of the repository by (1) allowing the radioactive material in spent nuclear fuel to decay and cool prior to disposal; (2) putting less spent fuel in each canister; or (3) increasing the spacing among waste canisters as they are emplaced in the repository.

We are extremely fortunate and gratified to have with us today distinguished representatives from international high-level radioactive waste management programs: Mr. Gary Simmons and Mr. Peter Stevens-Guille from Canada; Dr. Klaus Kuhn from Germany; and Mr. Nils Rydell from Sweden, who have taken time from their very busy schedules to share with us how thermal loading considerations affect repository design in their respective countries. Gentlemen, please accept our heartiest welcome and thanks for coming so far to be with us today, and tomorrow and the next day. I know your presentations and contributions will be very important for this meeting.

Following the discussion of international programs, we will hear a presentation of the U.S. program, its history and evolution, and the rationale for the U.S. approach. Tomorrow, we will focus on the technical uncertainties associated with thermal loading concepts. We look forward to having our presenters respond to the following six questions about low and high thermal loading concepts as they discuss their respective technical areas:

(1) What are the potential problems? (2) What is the significance of each of the potential problems? (3) What uncertainties are associated with each potential problem? (4) Can these uncertainties be resolved? (5) What time and costs are associated with resolving these uncertainties? And (6), will there be residual uncertainties? These are not new questions posed at this meeting. They have been sent out to all participants well in advance.

Our afternoon session tomorrow will be devoted to a discussion of aging of the waste and potential enhancements to the repository and waste package designs that could help reduce uncertainties associated with the various thermal loading concepts.

On Thursday morning, we will hear presentations on the implications of alternative thermal loading, and after lunch, the presenters and the Board will participate in a round table discussion of thermal loading issues.

I would again like to thank our distinguished guests from Canada, Germany, and Sweden, as well as the representatives from the DOE, the national laboratories, the Electrical Power Research Institute, and others for participating in this meeting. We look forward to what we anticipate will be a very substantive and productive discussion.

Before we begin, I'd like to make a local announcement, that the Board has extended its deadline until November 1st, 1991, for applications for the two senior professional positions open on the Board's staff. Notices describing the positions and outlining the requirements are available at the registration table for those of you who may know somebody that could be interested.

At this point, I would like to introduce Mr. Carl Gertz, who will favor us with some introductory remarks. As most of you know, Carl is Associate Director for Geologic Disposal at the Office of Civilian Radioactive Waste Management. He is and has been Project Manager for the Yucca Mountain Site Characterization Project Office since 1987. Following his remarks, Carl will introduce Dr. Larry Ramspott with the Lawrence Livermore National Laboratory, who will discuss the strategic implications of heat in a high-level radioactive waste repository.

Carl, thank you for being with us today.

MR. GERTZ: I think I'm all set. Thanks, Don, for the invitation. Once again, for those who don't know me, I am Carl Gertz. I live and work here in Las Vegas, Nevada. It's a pleasure to talk to you today about the Yucca Mountain Project in some opening remarks.

We thought this was an opportune time, as part of my opening remarks, to provide the Board--it's the first time the Board has met this fiscal year--with an overview of the Project so you can know what my scientists and engineers will be doing over the year. I talked to Bill about this and he thought this was appropriate, so I'm going to take about ten minutes or so and briefly outline our program over the year. I was going to go over the agenda for the meeting, but Don did such an eloquent job, I won't have to do that, so I'll take a couple more minutes talking about the project.

I'm going to talk very briefly about our '91 accomplishments, to set the stage for our plans and priorities in '92, talk to you about the status of some lawsuits that we've had ongoing, and then talk to you about the status of the permits and what work we are going to be doing at Yucca Mountain.

Many of you have been involved over the last year in looking at these activities. Number one, we did start some new site characterization work at Yucca Mountain on July 8th. We have been working on our site suitability methodology; in fact, that's out for independent peer review at this point. We've continued our non-surface disturbing activities, the monitoring, the seismic station monitoring, all the other things we need to do. We've completed four major studies: test prioritization, the ESF alternatives which we've discussed with you, Calico Hills risk benefit analysis which was folded into the ESF alternatives, alternate license application, brainstorming strategy, and we have completed our revised ESF Title I design summary report. In fact, Dr. John Bartlett just last week accepted that on behalf of the Department, so that is our point of departure for continued design in ESF, and I'll talk about that in a little bit.

This is really a little bit outdated. Not only were we ready to start major new site characterization activities, we have started new activities. In fact, we're drilling out there today as we talk in the unsaturated zone infiltration holes of principal investigator, Dr. Alan Flint's area, so we are doing some things right now.

But let me tell you where we're going in '92, and our priorities do reflect limited funding. We're going to work on the initial site suitability evaluation. We're going to make a report available for the public shortly after the first of the year. We're going to initiate new surface disturbing drilling and other type activities, including we'll do some major drilling with the LM-300, which you saw. We'll do a monitoring hole. We'll do Alan Flint's unsaturated zone holes. We'll do some geologic investigation holes, field trenching and test pits. We are, in effect, emphasizing surface-based testing.

We'll do, of course, our ongoing activities, and we'll begin--and I'll talk about this in a bit--limited Title II design for the ESF, including updating any repository designs as we go through the process.

In order to do work, though, we have to maintain a sound environmental program and provide support to new field activities, both for archeological and environmental studies. We want to do our performance assessment to support not only our project priorities and activities, but to do a total systems performance assessment, as we had promised we would do just after the first of the year. We should have that.

We want to continue to implement our quality assurance program and our project control program, and conduct a minimal waste package near-field environment waste form characterization program.

In addition to that, we have to maintain our fixed cost items, be it roads, buildings, records centers, conduct our institutional/outreach program in accordance with the mandate of the law, and transition the M&O, the TRW team into our project activities.

Just a Gant chart to show you where we're heading, this happens to be Dr. Flint's activities. It's a drill operation with an ODEX rig, 50 to 200-feet holes, working eight hours a day. We are, of course, doing ongoing trenching and test pits, be it Dr. Crowe's volcanic studies, Dr. Gibson's Midway Valley trenching studies, and other things, and that'll be working throughout the year.

Along about this time, we'll bring down the LM-300, and we'll have two drill rigs working on daylight shifts, and this will first do a monitoring well, and then do an unsaturated zone deep hole, 2800 feet deep.

Later in the year, we'll work on some geologic holes or some unsaturated zones. We haven't decided which one this will be. We will be working on some boreholes to identify the physical properties for potential ramp locations. We have completed our prototype holes at the HRF facility at our geologic lab, and we do all the ongoing things.

That kind of lays out where we're going with the surface-based program. Some of this will be 24-hour a day work when we get into this part of the program.

The other aspect or focus of the program has been slightly de-emphasized in 1992; that is, working on the ESF. Because we only had so much money, we decided to concentrate on the surface-based program, but we continued an aggressive program for designing the roads and pads for our first portal location, and that'll be designed during '92. In '93, we will start the comprehensive design of the entire ESF and the construction of the roads and pads, and choosing a contractor to get ready to do the big stuff out in '94. We had hoped all this stuff could have been done in '92, but unfortunately, we didn't have enough funds to do that, which brings us to the question, you may wonder how currently our funds are distributed.

That's our work breakdown structure. You're familiar with our accounting system. Here is our distribution of funds. In '91, I'll point to the bottom line, we spent about $205 million. We're only going to have $170 million, almost a 20 per cent reduction, in '92 to allocate to the program.

In addition to reduced funding, we have certain things we need to do, and we have to transition an M&O, so we have a lot of challenges this year in accomplishing our work. As you can see, what we have emphasized is the site work, the drilling, trenching, and physical work ongoing at the site. We've, in effect, de-emphasized the ESF for awhile, but we've made major decisions that the concept that we presented to all is our departure point for starting Title II design. We certainly appreciate your comments and enhancements. They will be very seriously considered. As you're well aware, we've considered many of them in the process of getting to where we are, as we move forward with this design. But that's our spread of funds.

Very briefly, I'll just point out we are still in litigation activities with the state. One suit we consider has been closed. That was the state suit which the Supreme Court refused to hear, and this suit, which enables us to get some permits which, under Court order, the state did issue two permits. A third permit is being considered by the state. We've just concluded nine days of hearings on this water appropriations permit. We expect two more days of hearings on this the end of the week, and then the state engineer will be charged with making a decision.

But in the interim, the state did issue a permit allowing us to use water from VH1 to conduct site characterization studies. It's a well that we had--hole we had built in 1982, but we've not used it, and so we won't have to haul water from California to work on these activities that I've pointed out, but it is still 45 miles from our area of interest, and it does expire in May, '92. So we need the major permit that the hearings are considering, but this does allow us to continue work throughout the early part of this year.

In summary, to move forward, DOE continually needs assistance. We are working through the litigation process, and that has enabled us to start work. We are supporting legislation in both houses of Congress, and they have passed bills that would separate the science from any political posturing and will allow the science to move forward while eliminating any political interference over the ten years of the program, ten years of study in the mountain.

We need administration and departmental support to obtain resources. Candidly, this year we're resource-limited. We could be doing more drilling, more design, more whatever, but due to the uncertainties in the budgets and because of permits and whatever, we were not provided sufficient funds this year. We're certainly working on '93 right now with the OMB, and once we do propose funds to Congress, we'll need Congressional support of that funding, because without all three, this program will, in effect, become stalled and we think that it's too important of a program to become stalled, too important for the nation.

So we hope to be working our way through this tough year and hopefully, in '93 we'll be doing some expanded things, but I thought it was important, Don, to provide to the Board an idea where my team will be focusing this year.

DR. DEERE: Thank you.

MR. GERTZ: With that, I'm going to get back on schedule, I hope, or Larry is going to get us on schedule, and I understand Larry has the first briefing. I would like to point out Larry is not speaking for the project or for the program. He's speaking, I guess, as an individual, Larry, and you will hear a lot of program talks a little bit later in the day. As I said, I won't go through the agenda because Don did such a fine job of that.

Larry, it's all yours.

DR. RAMSPOTT: Good morning, members of the Board and ladies and gentlemen. The subject I'd like to talk to you about today is the strategic implications of heat in a high-level nuclear waste repository.

As Carl pointed out, it's not an official talk from DOE. As many of you know, I did give a talk at the last spring's meeting on the constructive use of heat in an unsaturated tuff repository, and that's published in the proceedings of that, and that was an advocacy talk. When I got the invitation to talk here today, I thought that I would like to step back and look at it from a somewhat broader viewpoint. I really am not trying to advocate any specific approach today, but really to sit back and look, if I can, at some of the strategic implications. As I looked at it last night, it may seem to be an advocacy talk, but I didn't start out to do that.

The main points I wanted to do with the talk was to look at the strategic implication of three temperature regimes; those where it's greater than the boiling point of water, between the boiling point of water, and ambient temperatures and those that are near ambient. I also wanted then to follow up with looking at the idea of strategic implications of heat on the selection of Yucca Mountain as a repository site, and on the need for long-term surface storage; in other words, what I'm concerned about in this talk is with the idea of who cares about any of this, what difference does it make.

I have some definitions for the talk, because I want to look at three ideas: hot, warm, and ambient. Now, last spring when I talked at the Las Vegas meeting, Lief Eriksson was talking about a cold or a cool repository, and I was talking about a hot repository. As I began to put this talk together, I was thinking to myself that we're talking about for the so-called "cool" repository concept, temperatures of 90°C, and I've worked in restricted spaces at up to 55°C and I can absolutely assure you that 90°C is not cool, or certainly not cold, and so I thought that I would use the word "warm."

Also, I think we need to point out that "hot" means the distribution in space and time of rock temperatures which are greater than the boiling point of water, and then they're sufficient to produce significant dryout benefits to repository performance, and I kind of worked those words out with some of the technical people. What it really means is hot enough long enough over a large enough volume to do some good. That's what we're talking about, because we think that there is a beneficial effect from the heat, but it's not just being above the boiling point of water.

Also, warm, I think, doesn't mean just being below the boiling point of water, but it means a repository is designed to some upper temperature limit. 90°C has been suggested by some. I think in the Swedish program, at one point they were talking about 100°C centerline temperature on the glass waste form, or perhaps--I don't remember what the spent fuel, but basically take some component of the repository and set some upper temperature limit.

Now, in this talk, when I talk about "ambient," we're meaning within a few degrees, actually within a few degrees of the ambient rock temperature prior to emplacement, and I think we can achieve that, and I'll talk more about that later. So there are really three. These are not continual. They're discontinuous. The hot and the warm and the ambient are discontinuous. It's not a continuum in temperature.

Just for a moment, it's a concept that goes along with this, but I think there are really only three concepts in the U.S. program that address 10,000-year isolation from what I would say is a simple viewpoint. One of those is partitioning and transmutation, which I'll say more about, but it's basically--I think you've heard the words "actinide burning." It's removal of some of the radioactivity from the waste and transmuting it in a neutron generator. Another is what might be called the super container concept, a very, very stout container, and that's fairly simple and easy to explain. One that I think hasn't been talked about before is the hot repository for 10,000 years. This is a new concept, and I'm going to talk about that a little bit more today.

Now, as I went over the talk last night a little bit and was thinking about it and saying, "Have I been really fair about this?," I had a thought that there are really a couple of geologic concepts which I think are fairly simple to explain.

If you go back to the 1957 National Academy recommendation for a salt repository, the idea there was that these salt deposits have been there for 200 million, 500 million, 70 million. Various deposits have been there for a very long time, and if any significant amount of water had gotten to those deposits over that time period, they would have dissolved, and so I think the salt repository concept as originally put forth had a very, very simple, easy to explain, powerful viewpoint behind it.

I think, also, that one could--and I'm not sure exactly that it has been put together--construct a simple series of arguments for why an unsaturated zone repository would really work well, but those are the kinds of things we're looking for, I think, from a strategic viewpoint. And why would one want a simple viewpoint? I think basically when you go into licensing, it doesn't matter whether you're trying to explain this to somebody next to you on the airplane, or whether you're trying to explain it to a licensing board, I think you need a simple licensing approach, a very simple viewpoint in order to bound the problem, to constrain things.

Now, I have a little bit of perspective. I'm going to get out of the background fairly soon here, but I want to give a good bit of it. I've already pointed out that I've given an advocacy talk in the past for constructive use of heat at Yucca Mountain, and I'm mainly directing this talk today on the effect of heat at a potential repository site at Yucca Mountain, so I'm just trying to give you some background about the perspective that I have.

Obviously, I believe that drying is going to limit the container corrosion and prevent dissolution and aqueous transport of the radionuclides. That's something that I take as a given in the talk. I also believe that the repository can be designed to optimize the effects of heat. I also am coming from a perspective that the Engineered Barrier System and the natural barrier system work together, and that they cannot be assessed independently. We can't just go out and look at the ambient conditions in the site and ignore totally what's going to happen when we put the waste in there. They have to work together. They have to be assessed together.

I also believe that Yucca Mountain is a fractured open system, and this is unlike the case over, say, a salt repository or in some of the granite sites where it's very, very tight. With Yucca Mountain, you just can't fracture it anymore, or if you do, it doesn't matter, and so therefore, the heat or any of the construction that we're going to do is not really likely to be significant to isolation.

Now, you'll see in this talk an emphasis on 1,000 and 10,000 years, and that's based on the need for meeting the EPA and NRC sub-system constraints. We're not really dealing directly with safety in the United States. We have a series of constraints put on us in the regulations, for which there is no real nexus to the dose that gets to an individual, but what we have to meet are these 1,000 and 10,000-year constraints, whereas, in other countries, they're looking perhaps at times out to a million or two million years.

Now, in the hot repository concept, there are really two time frames. One is 10,000 years, and one is 1,000 years. I'm going to talk a little bit about this a little more later. If we're above the boiling point of water for 10,000 years, then I'm submitting the proposition that you only have to show that the repository horizon is not going to flood for this 10,000-year period in order to meet the EPA standards and the NRC sub-system requirements for the EBS, and when I talk about flood, what I'm talking about is that the actual water level, the groundwater level will rise above the repository horizon, and it will become saturated in that time period through something such as a Szymanski Hypothesis or something like that. But I mean, actually, it would flood and become saturated.

If it's above the boiling point of water for 1,000 years, what you gain out of that is that you can demonstrate the substantially complete containment sub-system performance objective. However, you must also show compliance with the EPA standard and with the other NRC sub-system requirements, and you're also going to have to model sub-boiling processes post-1,000 years. So there's a very large advantage to being able to keep it dry for 10,000 years.

There are implications of the methods of keeping the repository hot. Don Deere went through a number of these in his introductory remarks. If you have closer borehole/drift spacing, you can do that with young fuel, you can do it with old fuel. You can put very young fuel in the repository, and by that I'm talking about maybe two-year-old, three-year-old fuel. You can age the fuel and pack more in individual containers. You can have a rock with low thermal conductivity, and you can also have drift emplacement. Now, all of these things are possible, but each of them have certain implications.

You do get cost savings if you get closer borehole and drift spacing, but you're going to be limited by some facility constraints. In other words, you just can't pack the boreholes and the drifts much closer together. Well, the boreholes don't go closer together than our current design. The drifts can come a little bit, but then you get into stability issues.

You're also going to have temperature limits on components. You're going to be exceeding the 350° limit on the spent fuel cladding. If you try to do that with old fuel, you're going to require possibly a long-term central storage facility if that's all you do. I realize I'll say later that you don't have to have that facility, but just for the current design, with old fuel you'd have to have some form of storage facility.

If you wanted to put very young fuel in the repository, you simply limit it by system constraints. A lot of the fuel is already 20-years-old. You're not going to be able to put two or two-and-a-half-year-old fuel in the repository. If you try to age fuel and pack more in the containers, I think that's feasible, but as far as I can tell, this is a fairly new concept. People have only been talking about it recently, and I don't believe that the criticality problems have been looked at. I know the calculations have been made showing that this can be done.

If you have a rock with low thermal conductivity, basically we're already in the lowest thermal conductivity rock of any of the major media that are considered around the world, so I don't think it's feasible to go to rock with lower thermal conductivity.

As far as drift emplacement goes, I think there are possible technical and cost advantages to that. We really haven't looked into that well enough yet. I don't think there's enough solid data, but there are possible advantages.

I just wanted to point out before going on, the normal curve that you see of kilowatt heat versus years out of core is on a log/log plot. This is not on a log plot, and what you see here is a very, very rapid decline in the first few decades. What this is, the calculation is the actual waste package heat load for the current conceptual design, and it's 3kW at ten years out of core. Basically, you can see within a few decades you drop to about half of the heat load that was put in there, and it continues to drop down to about a little beyond a hundred years, at which time it levels out. So storing the fuel for a very short time, or aging it, really gets way down on that curve.

Now, what this means--and you're going to see from

Tom Buscheck this view graph and others later on, but you're going to see a substantial increase in boiling and dryout benefits for 60-year-old fuel. What you'll see here--and Tom will talk about this later--you've got a curve for 30-year-old fuel, 60-year-old fuel, and I'm not sure whether you can see this yellow line across here, which is the boiling temperature.

You can see with the assumptions that are on this view graph that the drift wall temperature--this is a drift emplacement for an APD of 114 kW/acre, and you can see that the drift wall peaks out a little above 275, but actually, it stays above the boiling point of water, nearly above 100 all the way out for 10,000 years. So this is the basis for some of the concepts that I'm going to be talking about, and this can be explained more later.

The advantages and disadvantages of hot temperatures. The main advantage that I see is that you have this presumption that aqueous corrosion and dissolution do not operate, and of course, we'd have to have a lot of arguments and discussions about that, but I think it's a fairly sound presumption, and that is something that's very easy to explain, either to a licensing board or to the public.

You also have the ability to validate models of fluid flow, because you're in a matrix-dominated flow regime.

You're going to get more homogeneous response, and it's more amenable to verification by field testing. You put in a very strong signal, you actually have something to monitor. In the G-Tunnel test we found that we could monitor the dryout zone. We could monitor the migration of that. If you're just heating rock, other than the thermal field itself as far as moving the -- around on the rock, it's very, very difficult to monitor small degrees and changes in saturation.

The disadvantage of this concept of hot temperatures is that it's pretty much unique to the United States. Now, I think that we're going to hear some talks from the people in other countries. I'm not sure that it's totally unique to the United States, but generally speaking, this is one that I have commonly pointed out to me, and within the United States it may be unique to Yucca Mountain or to other unsaturated sites. I certainly am not saying that we should apply this to salt or granite sites if we get back into that.

There's going to be a possible change in the hydraulic properties of the rock, and I'm not talking about the rock that's right immediately adjacent to the repository, but actually, to some of the units that are at the top of the Calico and other surrounding areas. Again, Tom Buscheck will talk more about that. And there's also a possible effect on retrievability. Keeping the temperatures this high for this long, or getting them up that high, it is not going to be easy to get in there and retrieve the waste, necessarily; particularly if it's borehole emplacement.

Turning to a warm repository, for the idea of a warm repository--and I'll remind you that this is below some limit, say 90°C--we're still going to have to show that that repository is not going to flood in either 1,000 or 10,000 years, just as you had to in the hot scenario, so nothing has been gained there. We're still going to have to show it won't flood.

If the temperature remains below boiling, you now cannot assume the absence of liquid water on containers of waste. You have to prove, container-by-container, that you don't have fractures that are going to transmit the water down onto those containers. You can still have many of the containers where we estimate maybe 90 per cent or more will still be dry, but you'll have to demonstrate that at a much more specific level.

At temperatures between ambient and boiling, you're still going to have to model and understand nearly all the processes involved at temperatures above boiling, as well as the processes in the sub-boiling region, because many of these processes just go across the boiling isotherm. They project in an Arrhenius plot. They don't change whether you're at 105 or at 90°. And then the question is, how will these sub-boiling hydrothermal process models be validated by field testing? We think it's a little harder to validate the models if you don't have the boiling than if you do.

You have to decide two issues in this, and one, I've never seen a specific technical justification that says what upper temperature limit is technically justified, because there's a whole range. For example, you can get changes in some of these clays that we're talking about at temperatures as low as 50°C, or even in the zeolites, you can get long-term structural changes, and so you have to pick out a temperature and say we want to keep it below this temperature, and there's some technical justification for that, and I haven't seen that for the conditions in the U.S.

And then you have to say how this is going to be achieved. Now, that gets, again, to the implications of the methods of keeping the repository. The first method is you can store it on the surface for 50 to 100 years, and I want to say here that what I'm doing is assuming in that statement that we're still going to go with the standard SCP conceptual design borehole-type of emplacement. That's why you're storing it on the surface. There are other ways to cool it, but I'm assuming that.

Therefore, we will require a long-term central storage facility, which is going to lead to safeguards and security issues, and I think that many people point out that if you store it on the surface for any length of time, that it's just less safe than it would be if you got it underground and got it into the repository, and this is true, also, if you're storing it in a hot repository, ambient, or whatever. You still have those kinds of issues. I don't have them all on every view graph. This talk hasn't matured to that viewpoint yet that I've checked everything out.

You can do it by decreasing the areal loading, and if you do it by spacing, you're going to have an increase in cost. If you do it by less waste per canister, you're also going to have an increase in cost, and then there is another viewpoint which Tom Pigford has pointed out in a number of papers that he's written, and that is if you are depending upon a solubility-limited release, you're going to have an increase in release which is pretty linear with a number of containers. So if you have 50 per cent more containers, you probably will have 50 per cent more release, everything else being equal. The larger the number of containers, as long as the release from an individual package is solubility limited, so if you get more packages, you'd have more release.

Now, you could redesign using drift emplacement and an engineered cooling system, and I have "Cost(?)" there, and some people said, "Well, that would run the cost up." Others I talked to said, "Well, no, you'll have great cost savings," so I'm not sure what would happen and I don't think we've really looked at the cost well enough to know which way that's going to go. If you did that, I think you'd have to re-think your entire isolation strategy. If we go to drift emplacement, we're going to have to do that.

So my summary of the advantages and disadvantages of warm temperatures, the big advantage of that is this is the international standard conceptual design. I got some questions from some of the technical people, "Why do you even put that in there?," but over the last several years, what I have come to recognize is that anything which is agreed upon by the international community is very powerful, and it's something that does carry weight with Review Boards, with various groups.

Now, the disadvantages I see at Yucca Mountain is you've still got a possible change in the hydraulic properties of some of the rock. The temperature's at 90°. Once they're distributed down to the Calico Hills level, they're not going to produce temperatures significantly lower than starting with, say, 105° temperatures.

Most potentially deleterious processes that operate above boiling also operate in this range. I think I've already made that point.

This concept appears to also have some of the disadvantages of the ambient concept; the difficulty of model validation and fracture-dominated flow, and there's a possible effect, again, on retrievability. So I'll go to strategic implications of ambient temperatures.

Again, we're going to have to show that you're not going to flood the repository in 1,000 or 10,000 years, just as in the hot or warm scenario, so that's something we're still going to have to do. However, in this case, you're relieved of having to address processes at greater than ambient temperatures in the repository. You simply don't have to deal with all those, so if you really could get something very near ambient, there's a whole series of processes that would not have to be addressed, and it could be quite favorable.

Now, there is a thermal gradient in the site, and that cannot be neglected in the modeling, so you'd still have to go ahead and have the models that would show the temperatures and the effects. That would have to be in your hydrologic flow models, and you still will have vapor-phase transport would be very important, and you must model two-phase transport, because at Yucca Mountain site it's an open system, if the containers fracture or crack and somehow you can get the Carbon-14 out and it can get up to the surface.

And then you will have to be able to describe and model scenarios for water to contact and corrode containers and dissolve the waste.

The implications of the methods, there's only one method that I'm aware of, and that is partitioning and transmutation. However, a lot of people--that's what I've been working on for the past six months or so, and a lot of people are very seriously suggesting that, and one of the advantages is if you go ahead and take the cesium and strontium and store it for several hundred years in a surface storage facility, you can get nearly ambient temperatures in the remaining waste that goes in the repository. There's going to be a very large increase in cost. I'm talking about quadrupling or maybe ten times higher, or whatever.

You're going to need to locate, construct, and license and operate all of those facilities, the various reprocessing facilities, the transmutation facilities, storage facilities, transportation and others. That's going to require a lot of licensing and legislative changes. However, it is technically a feasible idea.

An advantage of the temperatures near ambient is that Yucca Mountain ambient, which is around 23°C and atmospheric pressure, is very close to standard temperature and pressure, and there are thousands, if not millions, of measurements of all types of physical and chemical phenomena. You have a large thermochemical database that's instantly available to you, and a large number of measurements in many fields. So basically, you don't have to make these elevated temperature measurements, which we were required to in the other concepts.

I think the validation of the near-field flow models is going to be harder. At Yucca Mountain, under current conditions, flow appears to be fracture-dominated, and if you have pluvial conditions there's even more chance of this fracture-dominated flow, and I think you'll hear some more of that in some of the talks. This fracture-dominated flow could lead to faster transport should the waste ever be dissolved.

I tried to put this together, the implications of heat on the selection of Yucca Mountain as a repository site, so I'll go through a number of points.

First, the site characterization plan, conceptual design would lead to a 1,000 year hot repository if it's implemented as it's now in the conceptual design. That's what we would get, is a 1,000 year hot repository, but the performance assessments, to the best of my knowledge, have always been conducted for warm conditions, because we haven't ever assumed any of the benefits of keeping it hot for a thousand years because we haven't done the testing yet to demonstrate that that fact is absolutely feasible. So we've basically done these assessments for warm conditions.

Assuming the expected Yucca Mountain conditions, most of the containers would remain dry, even if you had ambient temperature. So I think one could make the arguments that we don't need the heat at Yucca Mountain at all.

Now, aging of fuel will increase the length of time that a repository at Yucca Mountain can remain hot. Again, I think that's counter-intuitive, but I showed you those curves earlier, and it simply boils down to the fact that as you age it, you put it in the containers, pack more in the containers, pack it closer together, then put it in at very high areal power densities, like 100 kW/acre or something like that, and so this could increase the length of time.

However, aging also helps the warm repository concept at any site or media, because if you age it, then you can spread it out, put less in the containers, and you can keep it below the boiling point of water, or down to some temperature, 80°, whatever it is.

Now, the conclusion I get from that is that either remaining at Yucca Mountain or switching to another site is not impacted by technical issues regarding fuel age. So it neither, in my view on that, neither favors nor disfavors Yucca Mountain, and so it doesn't impact whether we find Yucca Mountain to be suitable or unsuitable, whether or not we let the fuel get older.

However, hot repository concept, that may be unique to Yucca Mountain or to some unsaturated site, so it's possible that for a hot repository, particularly for 10,000 years, we may only be able to execute that at Yucca Mountain.

You could put warm and ambient repositories anywhere.

The implications of heat on the need for long-term surface storage. First, I think that surface storage could be replaced by enhanced ventilation and engineered cooling in an underground facility during the 50-year retrievability period. So if you did the design, I think you could totally do away with surface storage, but I think that needs not to borehole emplacement, but to drift emplacement. But I think that you could do the engineering and do away with the surface storage facility.

At Yucca Mountain, there's a wide range of thermal environments that you could achieve by repository design without having long-term surface storage. You could have a hot repository without surface storage, but you'd have to redesign it, as I pointed out, to achieve that 10,000 years. It would have to be a drift emplacement.

And you could have a warm repository at Yucca Mountain without surface storage, but by spreading everything out, you may not be able to handle 70,000 metric tons.

Now, an ambient temperature repository simply requires long-term surface storage for the cesium and the strontium, period. I should make it absolutely that certain. Los Alamos has come up with an accelerator transmutation of waste concept where they say they will transmit everything beyond the 11-year half-life, but they haven't really demonstrated that yet. But they claim that they could do that and it wouldn't need surface storage, but for the standard partitioning and transmutation, you need surface storage.

Now, the general conclusions that I draw from this is that the ambient repository simply requires partitioning and transmutation. Now, that has vast strategic implications, well beyond high-level waste management, and I think I'd just simply say that the Department of Energy is simply not authorized, from a statutory viewpoint, to do something like this at the present time. So if this really were a serious consideration, there would have to be a lot of changes in the law, and it also has very broad implications to nuclear power and the whole nuclear enterprise. So I'm not sure how practical that is from the viewpoint, strictly, of OCRWM.

With the warm repository concept, the issue that I get out of it--and you may not agree, but I make the leap to say: Is there a simple licensing strategy that can keep the site characterization effort bounded? Because what I see with the warm repository concept, which is what we're executing in the SCP since we're not taking advantage of the 1,000-year hot, is we have a very, very large and extensive site characterization, an almost unbounded site characterization effort; unbounded and unfocused. Personal opinion; not DOE's opinion, I'd point out.

With the hot repository concept, site characterization at Yucca Mountain, in my view, becomes focused on one issue, and that is: Is that repository going to flood in 1,000 or 10,000 years? I think the other issue that's unstated there is one has to get underground, one has to carry out the tests, one has to demonstrate that these concepts that underlie the 10,000-year hot repository actually work. I'm assuming that those--and also, that's not considered a site characterization under the present definition of site characterization.

So you have to demonstrate that, but if you can, then the main focus of site characterization becomes: Is it going to flood in 1,000 or 10,000 years?

Well, I hope that this stimulates some thinking for the presentations that you're going to have later. Thank you very much.

DR. DEERE: Thank you very much, Larry. I'm sure that there will be questions from the audience. We're going to hold them all until we finish this session. You've done exactly what we hoped you would do, lay out some alternatives; a good combination of scientific thinking and philosophical musing, and I think that's valuable at any stage.

We now will move on to the three speakers representing the international points of view for their own particular countries, and the first speaker is from Sweden. It's Nils Rydell, who is Chief Engineer and Technical Director of the National Board for Spent Nuclear Fuel, also known as SKN.

SKN is a small oversight agency very similar in function to our Technical Review Board. They do have one additional important function, and they distribute all the monies to the various work that goes on.

They are just celebrating their tenth anniversary, and the week before last they held a two-day symposium in Stockholm, and Board member Pat Domenico and I were both on the program, together with Nils. Our Executive Director, Dr. Bill Barnard, and our Senior Professional Staff Member Russ McFarland also attended, so we had a fairly good representation, we think, from this Board and staff, and a highlight of that particular meeting was the field trip that followed own to Oskersham and to visit the CLAB facility and to see the nuclear station.

I guess it was about 1972 when Nils was the project manager for the first nuclear station. Around 1978, their efforts started to turn to the problem of waste disposal, so although he's been in nuclear engineering since 1952, his work in waste disposal starts in 1978. So it's with a great deal of interest that we welcome you and listen to your remarks about the Swedish program.

MR. RYDELL: Thank you, Don. And first I should, perhaps, thank you and your colleagues for inviting me to come here and present our work. The subject of the meeting, thermal loading, is interesting. I don't know very much about it myself, as you will see, but I'm sure I will learn some interesting things and useful things.

The title of my presentation, "The Swedish Geologic Repository," is somewhat broader in scope than the subject of the meeting, and that's a good thing because thermal loading has not really been the issue in most focus in the development of our repository. It is an important issue you have to observe, surely, but it had not been contentious in any way.

But I have also been asked questions about thermal loading, to tell about the rationale leading to our proposed repository configuration, including the reasons for a 100°C temperature limit on the waste package. Now I would be only too pleased if I could say that our design was the result of thorough, meticulous deliberations, but that would not be true. The truth is, rather, that the people deciding on the concept said, "Well, this looks simple enough. Let's try it. We haven't got the time to look for something better and more complicated."

And the background for that was that the nuclear controversy spilled over to Sweden in the mid-seventies. It was not so much concentrated upon the hazards of nuclear power. We had not many nuclear power stations, and those we had had a good record. People were more impressed by the fact that spent fuel had very long-lived radioactivity. No one had told anyone how this was going to be disposed of, how could man devise a system to take care of poisonous things for such a long time.

So it was really the waste issue that became a public concern and a political issue. We got a new government, possibly because of different opinions about nuclear, and that government wrote and issued the so-called Stipulation Act, and that Act said that the utilities had to take the full responsibility for developing and implementing waste disposal. Now, that was news, because before that the state had been supposed to do this. The state was supposed to be the only institution long-lived enough to take that responsibility.

Now, the utilities were requested to describe how they could safely dispose of the spent fuel waste before they could get permission to fuel new reactors, and that took the utilities completely off their guard. They hadn't really thought much about these things before, but they reacted instantaneously. They created a task force, the KBS group, and that group was given a tall order: Describe how we're going to dispose of the spent fuel in a safe way, and do it quickly, because there were a whole series of nuclear power plants in the pipeline ready to be fueled.

It took this group 12 months to conceive of a design, to substantiate it with laboratory research and field investigations on prospective disposal sites, and to write a comprehensive and comprehensible report on this. I think they did a good job when they did that in one year.

It took another year before the second report came. The first one, incidentally, was on disposal of vitrified high-level wastes, since we had reprocessing contracts at that time sufficient for the new reactors. The second one was on disposal of spent fuel without reprocessing. That came one year later. And the third report that came--yes, three--was again on disposal of spent nuclear fuel.

Now, the first and the third report were important to gain permission to load new reactors, fuel new reactors, and therefore, the government gave them a thorough national and international evaluation, and following that evaluation, the governments--two different governments--approved of the report for this purpose.

Now you can ask yourselves: If we have a design which has passed two evaluations and government approval, are we stuck with this design? And the answer is no. The same governments have said that SKB, that the nuclear utilities' arm for development work on waste disposal; that SKB shall search for the best solution. Now, the best solution is a dangerous thing to search for, but anyhow, something possibly better than what they have set out with.

Now when I'm going to say something about the rationale or our design and our thermal loading, I have to stress that the design and our work is, of course, determined by the geology we have available, and we don't have the choice of geology that you have here in the States. We are restricted to Precambrian, hard fractured rock, saturated up to our feet. We live in a cold, moist climate everywhere, and that is decisive for the considerations we make and the conclusions we draw. We hope they are valid, but they may not at all be valid for a different geologic medium like salt or dry rock, as here in Yucca Mountain. You have to keep that in mind for the rest of what I'm saying, because that's important.

I'll show some slides. I think many of you are familiar with the KBS-3 design, but following the order to search for possibly better solutions, SKB has recently developed their thoughts and I'm going to use two designs to exemplify our rationale, and this is the classical one, you know, where we have fuel encapsulated in cylindrical canisters, imbedded in bentonite clay, put in pits drilled from the floors of tunnels. And the new design--which isn't really our invention. The Swiss have used it for some time--is to have, in principle, larger canisters--again, of the same materials--surround by bentonite, but placed centrally in long drilled tunnels.

What difference does that make? Well, here is a drawing of a canister. We used the fuel assemblies as they were taken out from the reactor, no reconstitution, put them inside a cylindrical, shallow copper six centimeters thick, fill the space with lead, molten lead to attenuate the ventilation and back out, support the copper so it doesn't collapse under the external pressure.

Following the order to look into alternatives, we have recently changed it somewhat. Now, if you are quick, you can count the number of assemblies; one, two, three, four, five, six, seven, eight. I'll come back to that. Now we intend to make it somewhat differently, same basic idea; nine assemblies and a copper cylinder, but in between the copper and the assemblies a steel canister. These steel canisters shall be strong enough to take the external pressure without buckling. That leaves us a free choice of filling material. We can use something, for instance, which suppresses fuel leaching. That material will be at the right place at the right moment when the canister is penetrated by groundwater. It will do its service. So that's an advantage. We haven't decided on the filler material yet, but that's an improvement we can take advantage of.

Now if we look on the other alternative, with the long drilled tunnel, we come to some interesting consequences. Count assemblies. We have now 24 instead of eight or nine in one canister. Otherwise, the same idea. Steel, copper for corrosion protection, some filler material of useful property.

Talking about costs, I should say that when the work started and the utilities had to satisfy the Stipulation Act, two things were important: safety and credibility of the design. Costs were of really secondary importance. If you have to wait with nuclear power plants, it costs money. So at the outset, we said choose the safest, the system with the most emphasis on safety.

Now if you take, for instance, a hot, a high thermal loading to reduce repository volume and decrease costs, you can achieve the same end to reduce costs with little ingenuity. Actually, this alternative saves something like 70 per cent of that excavation volume, and 70 per cent excavation volume costs money. And the reason for that is that we only need to make the tunnels wide enough for the canister, plus the bentonite, and that will be 2.4 meter in diameter. Here we need five meter high tunnels simply to move over the canisters from horizontal transport, vertically to lower it down, and we need all the many pits. By the way, we have three times as many canisters with this system.

What about thermal loading and repository temperature? In the very first design with the vitrified waste, we had steel as outer material in the canister, and we had quartz sand as backfill material or buffer. The design was the same, but here we had quartz. Now, neither steel nor quartz sand put restrictions to temperature. Quartz sand was selected because it is a good heat conductor as minerals go.

Nevertheless, the temperature was limited to 80 to 90°, and the reason for that was that we didn't want to bring up difficult questions about the effects of thermal expansion of the rock mass at high temperatures. These rock mechanic subtleties could probably have been sorted out, given enough time, but before we came to that, we had switched over to bentonite as filler material, and as I think many of you know, bentonite is a clay of volcanic origin. It swells when it is soaked with water, but there is one thing, if the temperature is at least substantially higher than 100°, and if you have potassium ions in the groundwater, bentonite will transform into illite, which is also a clay, but then it loses its swelling properties, and we think the swelling is useful because the swelling will seal all excess water to the canister. Water cannot percolate through bentonite. Anything that has to pass into the canister or out from the canister must pass by diffusion through a very tight lattice, and we will lose these properties if swelling disappeared. So from then on, 80 to perhaps 100° has been the decisive factor leading our thermal loading.

We have, on one occasion, been looking into higher temperatures. We had an ingenious design called the WPK, which was based on an entirely different idea. We stored a very big--a rather large amount of fuel in a configuration some 1,000 to 1500 tons inside a common groundwater barrier of sand and bentonite, and these 1,000 to 1500 tons were arranged so that heat could be carried away from the fuel by natural convection of the air.

In this case, we could, with the fuel in its repository, have an extended dry interim storage and allow the fuel to cool for 100 years or something like that, without the hazards of having it on the surface, and still be able to have a more compact storage. But when we choose the storage, then, for economic reasons, the design called for high temperatures, 150°, and one of the factors that killed the project, or at least put it on the shelf for some time was that our main advisor on chemistry stated as his opinion that there will be needed about 1,000 man-years of meticulous, tedious, not very meritorious laboratory work to establish the database for all the chemical reactions that could take place at these elevated temperatures in the system, you know, where we have a plethora of minerals. We have water which may change with time in salinity. We have a whole bunch of radionuclides, and before we had a database which could be compared to what we have at room temperature and which can be extrapolated to 80°, this amount of effort was required.

So I would say that if bentonite is the number one factor limiting our repository temperatures to 80°, then this is the second one.

We also, of course, have studied lately the effects coupled, thermal-hydraulic-mechanical effects around our repositories, and high temperatures, of course, rock swells, fractures first, perhaps, close, then later on open up again. Rock stresses may perhaps cause new fractures. It's very difficult to assess the coupled effects, and if that is a bit difficult already, up to 90° it would be even much more difficult later, so that is a third factor why we won't go above, say, 80 to 90°. And, again, I repeat, this is for hard fractured saturated rock where water could be saline or fairly fresh. This is now varying with time.

I can, just by way of illustration, show you--well, I got stuck on this one. Just for the fun of it, let me go back to this design. We have made some drawings which would please, I think, some of the members of the Board. A big drilling machine drilling long tunnels with 2.4 meter diameter, and a new contraption not yet in existence to push the canisters onto a bed of compacted bentonite lying there in place, and then shoving bentonite on top of this. It's an imaginative drawer. I don't think the designer has yet had his--

DR. DEERE: Leave a copy with us.

MR. RYDELL: I'm not at all proud of our temperature calculations, but these were the very first one for KBS-3; actually, not the very first one, but for KBS-3. It illustrates, anyhow, a little of the time development. You see, this is temperature on canister surface. That peaks rather quickly. It's two-dimensional, three-dimensional calculations. Forget about that. After some 30-40 years, that has reached its maximum.

The maximum temperature in the rock comes much later, but again, after 200 years, but the mean temperature in the rock goes much slower, and you're on top to the mean temperature until some 400 years, and these delayed effects are of some importance.

We have also one interesting thing. That is the heat flux to the ground, and this also is for a conservative design, but it shows that we have a fraction of -- per square meter reaching the ground. I don't think that will be noticed. I mean, how the heat touches to the ground from the sun, and so on, is much stronger, but if it was considerably higher, you would probably see--at least in the air--the area where you had disposed of the fuel, because it would be somewhat darker in the early winter simply because the frost would melt. Now, that happens in 2,000 years' time, so people would be scratching their head and wonder why, because they would have forgotten that there was a repository.

You can understand that this is, then, after about a thousand years, the temperature distribution in the rock, and 5° near the surface is a great deal, or 10° if it had been a more compact configuration.

Now, on the other one, on the first one I had, 60° at the canister surface, the new calculations for the tunnel and big canister design looks like this. It's fresh from the print. Now you'll see, depending on the heat conductivity of the bentonite--and that depends on the saturation--we are up to 100 or 110°, so we have become a little bit more bowed with time and less conservative.

So as I say, we have, we feel, at least three good reasons in our type of rock to stay at the low levels we are at now. I showed Ed something, by the way, on design philosophy, which I have omitted in favor of the temperature, and which you can see from what I have said, we have copper canisters. Their lifetime against corrosion is expected to be upwards of a million years, pending favorable environmental conditions.

Under favorable environmental conditions, we reach, with the bentonite, which keeps the water out from contact with the canister except by diffusive transport--so we have gone to great lengths to protect the fuel with a long-lived canister, and to protect the canister with bentonite, a natural mineral operating at temperatures which has often been met in nature, so we have natural analogs to prove the life length, so to speak or the function of the bentonite over the extended times.

You know, originally, the accepted wisdom was that it was the geologic barrier that could be trusted, because the geologic barrier had been there for a billion years, and it would still be there--perhaps not for a billion years, but tens of millions of years, so that was dependable. But human design couldn't be relied upon for such long times. I mean, after all, we have only the experience of science and technology over some hundred years.

But we feel, as we work, that the natural barrier, reliance on that has lost ground. Not necessarily that the natural barrier isn't good or a very valuable barrier, but it's very difficult to make quantitative assessments of the function of the geosphere, we discover, because there are experts on colloids or microbes in the bedrock, or organic acids like uvic and falvic acids, and they can tell us, for one thing, how much they know and still, even more, how much needs to be known before you can say something with safety about the barrier.

It is, of course, also very difficult in fractured rock like this to chart or map the groundwater routes through the bedrock, and to assess the transport capacity of these fractures. You can have channel flow, or you can have sheet flow, or various terms, so for credibility and demonstrability, it seems that it is easier to go along with strong engineered barriers whose function can be validated from thermodynamic principles, or by natural analogs, as bentonite.

This is not to say the natural barrier is not an important component of the safety, but that it is difficult to quantify its contribution.

Well, that's what I intended to say about the rationale of our design and how we look upon thermal loadings. Now, our opinion about thermal loadings is perhaps not very sophisticated, so I look forward to what I will learn from other presentations these days.

So again, Don, thanks for the invitation, and thanks for your attention.

DR. DEERE: Thank you very much, Mr. Rydell.

We do have about six or eight or ten minutes, so I will ask the Board if they have questions either of Nils or of Larry at this time.

Yes, Don Langmuir?

DR. LANGMUIR: A question for Nils. It's apparent from the design of the tunnel system you're suggesting there that retrievability is not a possibility. What did your program think about this concept of retrievability and the importance of it? I gather you didn't consider it required or necessary.

MR. RYDELL: Retrievability is not a requirement in our case, although you are, of course, entirely right that if you put fuel like this, you can get to any fuel canister you like as long as you don't backfill the tunnel; whereas, here, it would be very difficult to reach the innermost one. No, right, we do not require retrievability. That's not a consideration.

DR. LANGMUIR: Wasn't this considered, perhaps, an argument for public acceptance, the benefit of the concept if the public you could retrieve it? Since you're concerned about credibility here, this would be valuable.

MR. RYDELL: Well, retrievability, if you retrieve it, you can retrieve it for a good purpose or you can retrieve it for a bad purpose, so I don't think it has an influence on the acceptance of the concept. We have been stressing ourselves repairability, and I think it's not so much to retrieve the material, as rather to be able to repair if we have made some oversight, which is important. And, of course, in that case, if we know that something is wrong, we can always get access to them. I mean, it's not impossible, but it's difficult.

DR. DEERE: Are there other questions from the Board?

DR. DOMENICO: I have some points for Larry. I can't let that go unchallenged, Larry.

Out of everything you've said, I've sort of cited six over here I'd like to bring up to your attention. First, according to what you're saying, the hotter it is, the easier it seemed to be to model. It seems to me that when you have energy involved, you have the transport of mass, momentum, and energy, and the coupling that goes along with that.

The advantage of drying out; how long? How much of the water that goes up will collect in the upper part of Yucca Mountain below the Paintbrush Tuff, which is a barrier, and only to percolate downward? The one good thing about Yucca Mountain is the low natural flux of the mountain. It seems to me that the Paintbrush Tuff is a barrier that prevents a downward percolation. It's going to be equally an equal barrier to prevent the upward movement of that water, and if you saturate the rocks below the Paintbrush, the fractures will take over and eventually you'll have all that water coming back through the repository.

Third. How much of the desire for a hot repository is being driven by the NRC 1,000-year sub-system requirement?

Are we saying that we can't provide a satisfactory engineering barrier in the presence of the natural water there?

The fourth point, how much of the desire for a hot repository is being driven by a potential space problem, when you recognize that you may have to keep clear of major fracture zones and you're not quite sure how much of this mountain is going to be usable?

And the Swedish problem, how much lab work will be required to understand the chemical changes and reactions that will be driven by the heat?

And fourth, I believe the burden of proof and associated uncertainties of demonstrating mass release compliance increases with increasing heat reduction.

I don't know if those are statements, questions, but those are my concerns; six.

DR. RAMSPOTT: Okay. I think that, basically, I would like to defer on that question of the hotter it is, the easier to model to Tom Buscheck, either now or later in his talk, if you wouldn't mind that.

DR. DOMENICO: Sure.

DR. RAMSPOTT: That's not something that's simple to lay out and discuss.

I think, also, the advantage of drying out and the re-fluxing issue, I have some view graphs here I could try to dig out, but basically, what we see is a condensation and, essentially, a shedding around the repository. Basically, the steam flow is going to go out radial, and then it will drain down vertically under gravity in the fractures, and I showed those view graphs, I think, last spring, and Tom has also given those in a talk. So we don't see a ponding above the repository underneath the Paintbrush. We see condensation and draining and shedding around the repository.

I think that gets a little bit more difficult as you coalesce all of the drifts, and so forth, and you get a rather large volume underground, but I think perhaps Tom can speak to that a little bit later.

It's very clear on the NRC 1,000-year sub-system requirement. I think a lot of what we think about and do in the program is driven by the sub-system performance objectives, which, as Commissioner Curtis has said, have no clear nexus at all to the EPA requirement, which itself has no nexus to dose demand, a direct one. And so we're having to deal with artificial requirements in the repository, and showing that we meet the 1,000-year and 10,000-year requirements.

As far as a space problem goes, I think that you can look at that in several ways. The first place, we have more spent fuel and more waste than is statutorily allowed at Yucca Mountain anyway. There's at least 84,000 metric tons of spent fuel is going to be generated from the current reactors. If you look at the defense waste, it's going to take the total up over 100,000 metric tons, and we're only allowed to put 70,000 in the mountain.

So I think, unless the law changes, we're going to be forced to a second repository anyway. Carl may have some comments about this, but frankly, in my view, and in talking with the technical people at Livermore, they're not driven so much by a space problem as cost effectiveness, in terms of looking at the designs, and there may be some people that have some comments on that.

How much lab work, I think, is--well, my reaction when I heard it was a thousand man-years is, what a bargain.

(Laughter.)

DR. RAMSPOTT: Basically, I think we've put several thousand man-years into the program and don't get very much out of it right now, and so if you could really get something that would demonstrate licensing, I wouldn't disagree with that. I think, though, that since what you're dealing with is fundamental thermodynamic properties in many cases, just because you have a bigger program, a bigger repository, and so forth, or even different things, I think, for example, there's a number of international commissions that are looking at these thermochemical properties and databases, and I think once you determine a certain thermochemical property of Americium, it doesn't matter whether it's a repository in the United States or Sweden or Germany or any other country. You can use the same database. So I think there's a great chance for international cooperation there.

But I absolutely would agree that there's going to be a great deal of lab work, but I also thought when you asked that question, I don't see the lab work, I don't see the modeling. I think we've got to get underground. Now, we may not have to get underground at Yucca Mountain. We may do it at Fran Ridge, or may do it in Arizona or California or someplace else. We've got to get underground and test out these ideas. We had one test in G-tunnel, a prototype test there, and I think most of the concepts, the things that Tom is going to talk about, the modeling, a lot of that has come out of that prototype testing, and that was only for a horizontal emplacement. We've never tested the vertical emplacement, and we've never tested the idea of a drift emplacement. We have to get underground and prove these theories, basically.

I agree, I think on the burden of proof, if you go against what I would say is "the accepted international concept," the ones that are widely accepted in other countries, I believe they're absolutely right. The burden of proof is on us. We have to show. If we're doing something different than the international technical community is doing as a whole, we're going to have to demonstrate it.

So those are just my quick answers to this, and I hope Tom and others can come along and answer that number one for you.

DR. DEERE: Carl Gertz.

MR. GERTZ: Pat, I just hope you'll re-ask those questions after three days, and we can provide you maybe some better answers to it. Certainly, Larry--as we pointed out--is speaking for Larry Ramspott and not for the program, and that's fair, because we like differing opinions.

DR. DOMENICO: That's a very honest response. It was very honest. Thank you.

MR. GERTZ: And so we hope to answer some of the questions a little bit later.

DR. DEERE: Thank you. We'll be back to this topic later.

I suggest that we take our coffee break and come back at about 10:17.

(Whereupon, a brief recess was taken.)

DR. DEERE: May we reconvene, please?

It's my pleasure to introduce the second speaker of our international group, Dr. Klaus Kuhn. His Company for Radiation and Environmental Research, the title has been changed. It is really the National Research Laboratory for Environment and Health. Dr. Kuhn is the Director of the

Institute for Underground Storage of that particular government agency.

He hosted our Board when we had a trip in June of last year to visit their various facilities in Germany. We found them very interesting, and we knew from the various experiences, that their program has, over the past few years, would be of interest to us, and I'm sure we're looking forward to his comments on our topic and on his system, in general, in Germany.

Klaus?

DR. KUHN: Thank you, Mr. Chairman, members of the Board, ladies and gentlemen. It's a great pleasure and honor for me to be invited to testify before the Board, for as you just stated, we know in Germany from your recent visit last year, that you are doing an important job over-viewing the DOE research and development facilities, and especially with regard to the location of the Yucca Mountain repository.

As a matter of fact, the German and the U.S. program have a long common history. Larry mentioned in his presentation the report which was published by the National Academy of Sciences in 1957, and not only originating from that report, but partly founded on this report, the German program focused from the beginning on the use of natural salt deposits for the disposal of radioactive wastes, and for quite some years we had a very intensive cooperation with different agencies like AEC, Office of Waste Isolation, Office of Nuclear Waste Isolation, and Office of Radioactive Waste Management within the different government systems.

Unfortunately, the American program focused, until recently, only on one specific geologic formation and on one specific site, so that our cooperation is a little bit looser than it was in more former years, but still, DOE is operating or is going to operate the WIPP facility near Carlsbad, New Mexico, and we still have close cooperation with Sandia Laboratory, the lead laboratory for the WIPP site.

Coming to our topic of today, mainly the thermal load in the German geological repository, it is one of our generic approaches to have, as much as possible, flexibility in the system. That means that we do not want to fix firm numbers very early in the program, but that we achieve a working base, and starting from that working base, develop the total concept further, and then go and recheck if the assumptions which have fit into the working base are still valid, or if they have to be upgraded. That is also true for the thermal loading of the repository.

Therefore, we have no fixed terms, no fixed numbers for the time being. We have certain assumptions with which we work, and we are trying to fulfill our task with ameliorating these figures with continuing results from research and development.

There is one main difference between Germany and all the other nations which are going to be presented and which have been presented. This is the nuclear fuel cycle. As you certainly know, we in Germany, our main line in the nuclear fuel cycle is still reprocessing and vitrification of high-level waste. Therefore, we intended to construct the known reprocessing plant within the Federal Republic of Germany, but within the international efforts heading to a united Europe. This plan was given up and the German fuels from German nuclear powerplants are now reprocessed in La Hague in France by Cogema, and in the United Kingdom by BNFL at Sellafield.

Part of these contracts for reprocessing the German fuel elements in the U.K. and in France is that all the radioactive wastes generated by reprocessing our fuels in France and the U.K. have to be taken back for disposal in Germany. So the main heat, the main waste source from reprocessing is indicated in this scheme here. This is the so-called Cogema canister for vitrified high active waste. With these dimensions--which are all given in millimeters here--that means this is a stainless steel canister 430 millimeters in diameter, with a wall thickness of 5 millimeters. It holds about 150 liters of glass, and is produced at La Hague or at Sellafield, and then intermediately stored there, and then shipped back to the customers, also to Germany, for final disposal in the Federal Republic.

Also, by the reprocessing, some other types of wastes are being generated, like solidified medium level wastes; as, for instance, cemented hulls, fuel element structure materials, and also, dissolver sludges. They are not placed into these types of canisters. They are placed into drums or different containers with different volumes, between 200 liters and up to one cubic meter.

I should mention that the nominal heat output of such a vitrified waste container is about 2.3 kilowatts at the time of vitrification, and that the heat output of intermediate level wastes, which I indicated, reaches up to about 100 watts per container.

I mentioned that the main option for closing the nuclear fuel cycle in Germany is reprocessing, but since about ten years, an optional R&D program is underway, also, for the direct disposal of spent fuel without reprocessing. The concept which is being investigated is somewhat different from this which was shown by our Swedish colleague, and we are going, or we are investigating the use of a different container. This is the so-called POLLUX container, which is the twin of the CASTOR container which is used for transportation and storage of spent fuel elements.

This design heads for a self-shielding container so that the dose rate on the surface of this container is not higher than 200 millirem per hour. Another difference between the German container and the Swedish concept is that the main part of this container is consisting of cast ductile iron, which is a proven material in Germany for the transportation and intermediate storage of spent fuel elements, and this material is also under consideration and under investigation for the direct disposal of spent fuel elements.

The cross-section shows that there are four cradles which will hold the single fuel pins. That means the fuel elements are not put, in total, in this container, but the fuel pins are single-ized, and then the structural material is compacted and fit into the center core area opening in the center of the container. This type of container can then hold about 8 fuel elements from a pressurized water reactor, and it has the dimensions of about 1.5 meters in diameter, and nearly 5 meters long, and the total weight is about 65 metric tons.

Another difference between the concept in your country and in Germany is that in Germany, it was decided from the beginning that all types of radioactive waste should be disposed of in an underground repository. That means there are no near-surface land burial sites in operation in Germany, and there are no plans for it.

Another consequence from this first decision is that there is no differentiation between true wastes and non-alpha wastes, because all types of radioactive waste have to go into the same repository. Therefore, it is not worthwhile to make any efforts to differentiate between these two waste types and, as a consequence, not only heat-generating radioactive wastes will be disposed of in the German repository, but like, for instance, vitrified high-level wastes, spent fuel, and specific medium active wastes, but also, non-heat generating low-level and intermediate level wastes from reprocessing, from the operation of nuclear power plants, from industry, medical application and research will also be disposed of in the same underground repository.

The concept for the high-level waste repository is summarized in this overhead here. The first one is that we are going to use a Permian salt formation in the form of a salt dome. The disposal shall go on at a depth of about 800 meters below the surface. From this level at 800 meters, deep boreholes should be drilled into the salt, deep unlined boreholes between 300 and possibly even more meters deep. The dose rate at the canisters are about 2.5×10^{3} Gray per hour. The specific heat power is about 17 watts per liter. This is equivalent to the already mentioned figure of 2.3 kilowatts per canister. The maximum salt temperature permitted is about 200 Centigrade, and I want to underline again, this is a working figure. This has not been definitely decided, but this is the working figure with which we calculate and do our experiments for the time being.

One very important difference between our concept and yours is that we do not foresee retrievability of the high-level radioactive wastes once they have been disposed of. That means this is true for high-level wastes or vitrified residues originating from reprocessing. It may change, may be for the disposal of spent fuel, but that has not been decided. Also, in this respect, we are heading for the time being to dispose of, also, spent fuels contained in POLLUX casks, without foreseeing retrievability. I will come back to the technical aspect of this later on.

What I always stress when we have visitors from other countries in our facility, that it is, in my personal opinion and according to all results which we have achieved during our research and development program, counterproductive to talk about retrievability and disposing of heat-generating waste in salt. For one main advantage of salt is its plastic behavior of creep at elevated temperature, and this creep at elevated temperature circumferences the base in a very, very short time of about a few weeks up to two or three months. That means one main advantage is the complete isolation of high-level, heat-generating wastes in salt at higher temperature, and therefore, we are not considering retrievability. Of course, it would be technically feasible. You can imagine some sophisticated machines to over-core and to regain the waste disposal in salt, but it's not our intention to do this.

As the Chairman indicated, the Board has visited last year the Federal Republic of Germany, and this, again, now is the total Germany after reunification, and we have four projects under operation in Germany. We have the salt dome of Gorleben, which is presently investigated to host a repository for heat-generating, high-level radioactive wastes. We have the former iron ore mine, Konrad, which is in the licensing procedure to become a repository for non-heat generating, low and intermediate level wastes. We have the former Asse Salt Mine, which is being used as a research and development facility, and we inherited from the East Germans the operating underground repository in salt, Morsleben, which is being used for the disposal of non-heat generating reactive wastes of the reactors which were formally under operation in Eastern Germany.

Just to give you a short overview, this is a plan view of the Gorleben Salt Dome. That's the Elbe River, and the Gorleben Salt Dome has the shape of a cucumber. The long axis is about 12 kilometers, and the short axis is about 4 kilometers. Indicated are the different drillings which were performed for the investigation programs from the surface.

We have four deep drillings to about 2,000 meters into the salt itself, and we are sinking two shafts for the time being in order to explore the internal structure of the Gorleben Salt Dome.

The view of the surface installation is given in this view graph. We have two shafts. This is Shaft No. 1 and this is Shaft No. 2, and this is the total area of the Gorleben repository, and up in the back of the figure you can see the starting of the salt pile which is presently under hot discussions between the local population and the applicants.

Both shafts are about 250 meters deep for the time being. Shaft No. 1 has already reached the surface of the salt, and Shaft No. 2 is reaching the salt surface within about two or three weeks from now, and after some very strong discussions, both shafts are under operation again and, hopefully, this will hold for awhile.

Mentioning salt is something difficult, for normally, everybody understands a salt halite or rock salt, NaCl, but as a matter of fact, if you look somewhat closer into the stratigraphy of Permian salt domes, especially in Germany, the picture is much more complicated than only consisting of rock salt.

This is a geological cross-section for the Asse Salt Mine, which is also a Permian salt dome, and you see the proper salt dome is surrounded by different overlaying and overburden rocks, and is consisting of two different types of salt, blue in dark, and light blue is the halite, and we have in between some potash seam, consisting of carnallite, which is a potassium magnesium chloride mineral with six crystal waters, and we also have in the Asse only located very limitedly what is called the main anhydrite, A_3, and some pelite. This has to be taken into account when we are talking about salt, the usage of salt for a radioactive waste repository.

In the former U.S. programs, there were different candidate sites under investigation, like the Permian basin in Texas, Deaf Smith County. In the Gulf interior region, there were the Richton Dome, the Cypress Creek Dome in Mississippi, the Vacherie Dome in Louisiana, and also, in the Paradox Basin in Utah, the Gibson Dome was investigated. The majority of these domes, especially in the Gulf interior area, have a very deep lava bed, up to about 10,000 meters deep. So by moving upward of the salt, your salt domes are very clean, mainly or nearly only consisting of rock salt, whereas our salt domes are much narrower, only coming up from 3,000 meters, and therefore, we have this complicated internal structure and this complicated internal stratigraphy.

The situation in Gorleben is somewhat similar.

What we know from the deep borehole drilling program is that we also have the deep blue and the light blue. This is the main halite, the rock salt which we are looking for, especially at a depth--unfortunately, there is no scale. Yes, there is a scale. The scale or the target horizon which we are looking for of the repository is here between 800 and 1,000 meters, and from our present knowledge, we know that it is consisting of pure halite, but nevertheless, within the Gorleben Salt Dome, we also know that we have the carnallite, which is indicated in red here, and that we have the anhydrite seam, which is indicated in green.

We also have potash beds like you can see here, the orange color, and therefore, an intensive underground investigation of the salt dome is necessary. If we investigate the halite itself, we also come to the result that halite is not 100 per cent NaCl, but about, in the Asse Salt Mine, about 95, and in Gorleben 96, so you always have some additional constituents, some other evaporite minerals within the salt, which is indicated here with anhydrite, polyhalite, kieserite, carnallite, and also some other trace minerals.

Therefore, if we are talking about the thermal loading of a rock salt repository, we also have to take into consideration these other salt minerals, and we know that polyhalite, which is potassium magnesium calcium sulfate, with one crystal water, starts to decompose at a temperature of about 230 Centigrade. This is one of the main rationales, that we want to limit the maximum temperature in the repository at about 200 Centigrade in order to have a large enough safety distance from the decomposition temperature of polyhalite.

Even more sensitive to temperature is the mineral carnallite, which I mentioned several times, which is a potassium magnesium chloride which has six crystal waters, and within a very extensive laboratory and in situ investigation program, it was figured out that the water release temperature at ambient rock pressure starts already at 85 Centigrade, but at the same time, it was figured out that this water release temperature is increased drastically if you increase the rock pressure. And as a consequence, we have a rock pressure of about 100 Bar or so in the planned repository. We have to take into account a temperature release limit of about 145 Centigrade that carnallite starts to release part of its crystal water and changes, also, its mechanical stability.

The consequences, therefore, are that we have tried to limit the maximum temperature in the Stassfurt rock salt, which is the main part shown in dark blue in the former overheads, which contain, also, some kieserite. The temperature limit of about 145° within carnallite, that can be achieved by proper safety distances; that means by proper geological and geometric emplacement areas between the rock salt itself and the carnallitic seam.

In order to know especially and exactly the underground situation with the occurrence of anhydrite and other salt minerals, an intensive underground exploration of the Gorleben Salt Dome is necessary, especially at the foreseen level and the emplacement area. In order to give you a slight indication how complicated it could be, this is showing a speculative geological situation at the repository horizon at about 850 meters. That's in the Gorleben Salt Dome, constructed by the geologists, originating from the results which are available up to now.

The violet part is the older rock salt, which is mainly looked for in order to emplace heat-generating radioactive wastes, and the blue is the younger halite, which can also be used, but I want to draw your attention to the red seam, which is the potash bed, and to the gray seam, which is the anhydrite. That means that we have to carefully explore the underground situation in order to be able to locate properly the areas where heat-generating wastes can be disposed of, taking into consideration the necessary safety distances to the respective members.

On the other side, if you see that this distance here is 1,000 meters, you can see that space enough is available in order to allocate properly the respective areas for the heat-generating wastes.

I have to say a few words about the technical layout of the planned Gorleben repository, and this is mainly based on the quantity of wastes which should be disposed of in this repository. The geometric situation, of course, has to be taken into account, as well as the geological and the stratigraphical situation. Therefore, we intend to make use of very deep boreholes, 300 meter, and we discuss, also, even 600 meters deep boreholes for the emplacement of heat-generating, high-level radioactive wastes.

The quantities of wastes which shall be disposed of in a lifetime of about 70 years for the repository is according to a nuclear power generation of about 2,500 megawatt per year, which results in about 1,000 vitrified high-level waste canisters a year, in 23 POLLUX casks per year, with the foreseen strategy, and in about 2,800 heat-generating medium active waste containers.

In addition, quite a large amount of about 30,000 cubic meters per year of non-heat generating low and intermediate level wastes have to be emplaced into the repository. This leads to the present schematic layout of the repository.

There are the two main shafts coming down from the surface. This is then the repository level at about 840 meters, and there will be different areas for the different types of radioactive wastes. We are mainly talking about this area here where the boreholes will be located for the emplacement of heat-generating, high-level radioactive wastes between 300 meters and 600 meters deep. This is very schematically shown here. The proper location of the disposal boreholes, of course, have to be adjusted to the local geological situation. In the other part of the repository, we will have the chambers for the disposal of non-heat generating wastes.

I showed you a graph of the POLLUX container. Because of the dimensions--I want to repeat, six meters long, 1.5 meters in diameter--and because of the weight of this POLLUX container--65 tons--it will, of course, not be possible to dispose of this POLLUX container in vertical boreholes, but they are going to be disposed of--as was shown in one of Nils' slides--in a drift emplacement, and the drifts are properly being designed so that these containers can then be emplaced correctly in the drifts.

We were mainly talking about thermal problems, but the thermal problems in a radioactive waste repository in salt are only of second priority. More important are the consequences. These are the thermal-mechanical consequences, the thermal-mechanical problems which arise from the disposal of heat-generating wastes into the Salt Dome Gorleben.

Stemming from the performance of safety assessments, it is recognized that the thermal-mechanical behavior during the operational, and also during the post-operational phase, is much more important than the thermal problems. Just to mention, a few consequences which have to be taken into account are the following ones:

It must be avoided that in the shown anhydrite seams which are interbedded within the salt, that undue thermal stresses occur in these anhydrite seams because undue thermal stresses could open or reopen some of the joints which are originally closed, or filled by secondary minerals like halite, or some other evaporite minerals; and thus, if those joints could be opened by thermal-mechanical stress, these joints could be opened again and, thus, give potential pathways for intruding groundwater. This is what we call the so-called anhydrite scenario, which we investigate in our performance assessment, and which could lead to the contact of intruding groundwater into the repository with the radioactive wastes.

Also, the occurrence of undue tensile stresses at the contact of the salt and the overburden must be avoided for the same reason, that the undue tensile stresses could open again fractures or joints where through, again, water could come into the repository.

Another aspect which has to be taken into account is the future uplift of the surface, and we have fixed a figure that over the long time, the uplift should not be greater than about one meter, but this is a very slow process, taking place over a very extended period of time.

In order to solve these thermal-mechanical problems, a thermal-mechanical consequence analysis must be performed in order to fix the number and, therefore, you again have a interdependent system, with a different set of variable parameters which you can adjust in order to meet the overall safety objectives of the repository.

In addition to the thermal-mechanical issues, there also a few thermal problems which have to be regarded. I mentioned the carnallite, which is a mineral having some crystal water. It must be achieved, it must be guaranteed that the thermal load of the carnallite is not higher than about 145 Centigrade, which can be achieved by proper distances.

Also, one figure which also was mentioned, I think, by Nils is that the groundwater flowing in the overlying rocks must be not heated higher than a very few degrees--between one and two degrees Celsius--for flowing groundwater is very sensitive against heat changes.

In total, this means that numbers for thermal loading of a high-level waste repository can only be fixed by a complete thermal and thermal-mechanical analysis, taking into account a site specific geological and hydrogeological situation. This, again, is a very important factor, that a definitive analysis can only be done when you have the site specific parameters available. You can prepare your tools, but the final assessment can only be achieved and only be done successfully with the site specific parameters.

On the other hand, there are quite a number of parameters available to adjust the thermal loading of a repository. They were already shown by Larry Ramspott in his presentations. Just to mention, the most important one, again, is interim storage of the waste at the surface, and this, again, is not a log-log scale. The cooling time which is available, I mentioned that the heat output is 2.3 kilowatts at the time of vitrification, and you see it is not worthwhile to wait more than about ten to fifteen years. Then the output, or the additional success which you would achieve by storing your high-level heat-generating waste longer is minimal. The most important decay time occurs in the first ten years.

You also can adjust the geometric layout in the repository according to your thermal load. Available for this is depth and distance of high-level waste disposal boreholes, the density in which you emplace your POLLUX containers in the drifts, the number of emplacement levels, especially for drift emplacement--one to three on top of each other. You can use it and you can adjust it to your thermal loadings. And you also have available the filling strategy of the repository.

You either can continuously fill one of these deep boreholes at once, stacking one of the waste containers on top of each other. You can simultaneously fill a number, a variety of single boreholes, or you can also emplace dummy canisters in between the stack of high-level waste canisters so that you can address the proper thermal loading of your high-level waste area.

Many of these aspects have been and are being looked at in extensive R&D programs, which are underway since many years, and they consist mainly of three parts with regard to thermal and thermal-mechanical behavior of the repository.

The first one is modeling. We are developing suitable thermal and thermal-mechanical computer models in accordance with the international society, international scientific community. The program which is presently being favored in our country is the program, ANSALT. The second part of the R&D program are extensive laboratory programs in order to identify proper materials data, and the development of appropriate constitutive laws, and finally--and we stress this especially--we have to do extensive in situ tests, for you cannot modify, you cannot test all the in situ parameters in the laboratory or by calculating. You need to have some in situ tests in order to combine all the different effects.

So we have done quite a very extensive program in rock mechanical measurements at ambient and at elevated temperatures underground in the Asse Salt Mine. We have performed quite a large number of heater tests in order to test and to evaluate the thermal and thermal-mechanical behavior. In the early eighties, we had a common U.S.-German program where, for the first time, radioactive irradiation and temperature was used within the so-called Cobalt-60 test. We used Cobalt-60 sources, together with electrical heaters to look into the combined effect of heat and radiation on the salt, and we have now under preparation a so-called test disposal of high-level radioactive wastes, and just to outline the most important items here, this is the technical scheme.

This is indicating the shaft, which is our bottleneck. Therefore, we can only bring underground one high-level waste canister at a time, so we have to reload the canisters on the surface, and then underground. A respective test area has been prepared.

We are also doing, at the present time, a so-called drift emplacement, where six mock-ups of the POLLUX containers have been emplaced in extra mined galleries. The backfill material was then installed, and we are now measuring all the thermal and thermal-mechanic behavior of the backfill material and the surrounding rock salt. There are no radioactive fuel elements in. This is only electrically heated, and we have also under operation a test for the investigation of heat-generating intermediate level wastes, which will be filled in these 200 liter drums.

I have also some slides with me. Maybe at a further possibility I can show them. Unfortunately, the slide machine is not here for the time being, but if there is some interest, I can show those slides later on.

In summary, I would like to state that we, in the Federal Republic of Germany, are, together with some part of the U.S., with the Netherlands, with France, and possibly with Spain, are those countries which are looking into the usage of salt formations for the disposal of heat-generating, high-level radioactive wastes, and our so far achieved results by the R&D programs indicates that it should be possible to dispose of the heat-generating, high-level radioactive wastes with regard to the 200 Centigrade maximum temperature, so that this--or let me put it the other way around. We didn't find any result up to now which caused us to give up the 200 Centigrade. All the results which we have achieved up to now are backing the 200 Centigrade temperature limit, so that this is the target which we are going forward with.

Thank you very much.

DR. DEERE: Thank you very much, Dr. Kuhn. We'll have a chance for questions a little later, but we will proceed now to the last of our international speakers.

The next topic will be the Canadian geologic repository, presented by Gary Simmons of the Atomic Energy of Canada, Ltd. He is Manager of the Branch Office of Technical Studies at the Underground Rock Laboratory, and also in the design studies for the repository. He was the host for our Board's visit to the Pinnawa site, and several of us had the good fortune to be led through the site by Gary himself.

We felt that the information that they are developing and the studies that they are doing will be of interest to all those present, so, Gary, we welcome you here.

MR. SIMMONS: Thank you very much, Don. Thanks for the opportunity of coming and presenting some information on the work we're doing in Canada.

First off, I thought I would tell you what Atomic Energy of Canada, Ltd., or as we're now called, AECL, is. It's a company incorporated under the laws of Canada, wholly-owned by the government of Canada; in particular, the Minister of Energy, Mines, and Resources. We have an independent Board of Directors, a company President, and two operating units. The one of particular interest today is AECL Research, who do research into areas of furthering the

CANDU reactor concept, and do fundamental research in all areas of science related to nuclear and radiation, and we have an environmental sciences and waste management group. We operate at two sites; one in Chalk River, Ontario, the other near Pinnawa, Manitoba, the Whiteshell Laboratories. As a company, we have a staff of approximately 4,000 to 4,500 people.

What I'm here to talk about today is our concept for nuclear fuel waste management, and I want to begin by pointing out that what we're doing is different from what many countries are doing. The mandate that we have in Canada right now is to operate a program to conduct research and to develop and demonstrate the technologies for safe, deep geological disposal of nuclear fuel wastes. The key here is research to develop and demonstrate. We're not siting. We're not trying to define a location, design and build a repository. So everything that I'm speaking of today is in the context of technology development and demonstration, not of siting, licensing, and operating.

Our program, which has dealt with all areas of high-level waste disposal, is at this time being reviewed under an office of the Canadian government called the Environmental Assessment Review Office, and the process was initiated by the Minister of the Environment at the request of our Minister.

There has been a panel of experts, actually, a panel of the public established who have their own panel of experts to guide them, and they are going to review the technologies that we have in several areas, so we're right now in a public review of our technologies for high-level waste disposal.

The schedule for this review is basically that the panel was formed in 1989. They established their scientific review group and held scoping meetings in several provinces in Canada, and are now in the process of establishing the guidelines that will guide what we have to submit to them on our technology, and the basis for the review that will take place.

We're currently planning to submit our technology to them in 1993. They will do a technical review, presumably ask for supplementary information, and then the whole technology will be submitted to public review. The panel will make recommendations to the federal Ministers, we think, in late 1994 or early 1995.

We have approached developing our technologies and this assessment process with the idea of identifying the criteria that govern the safety of nuclear fuel waste disposal facilities. Now, in this case, we don't establish them. We just identify those criteria that have been put forward by other agencies that are regulatory agencies within

Canada. We then develop and demonstrate the technology to site, design, construct, and operate a disposal facility; develop and demonstrate the methodology to evaluate that facility against the criteria; and to establish confidence that we can find an acceptable site within Canada.

There have been some general criteria issued in Canada. The Atomic Energy Control Board has said the objectives of disposal are to minimize the burden placed on future generations, to protect the environment, and to protect human health, taking into account social and economic factors--without saying how it's to be taken into account.

Specific criteria are that the individual risk of a fatal cancer or a serious genetic defect must not exceed one chance in a million per year, and we have to assume that a group of people is located where and when the discharge, therefore, the risks are likely to be the greatest, no matter when in time or where, geographically, that would be, and we have to demonstrate compliance with that criteria quantitatively for at least 10,000 years, and beyond 10,000 years, we have to show that there will be no sudden and dramatic increase.

The concept that we're putting forward is similar to that that you're looking at and everyone who's spoken this morning is considering. It's the emplacement of some kind of stable waste form in some kind of container deep in a geologic rock body. In our context, we're looking at the plutonic rock of the Canadian shield because there is a lot of it in Canada, making site selection a relatively widespread thing, and because it was recommended out of a series of studies that were done in the late seventies that basically led to the development of the Canadian Nuclear Fuel Waste Management Program.

More specifically, we're looking at a reference depth of 500 to 1,000 meters, but there's nothing particularly magic about that. I participated in the selection of those numbers, and at the time they were rational. Now it becomes a design and a site specific issue.

The multiple barrier system includes the stable waste form, which is either used CANDU fuel, spent CANDU fuel, or reprocessing waste from reprocessing CANDU fuel, a corrosion-resistant container. We use engineered excavations sealed with low conductivity sealing materials, and we deal with the geosphere as a barrier.

Now, as was pointed out by Nils in his presentation, we also have water lapping at our feet. The Canadian Shield is a saturated environment. The water table tends to be within a few meters of ground surface wherever ground surface is, so we're working in a fully-saturated environment, except as we alter that environment by what we do in it.

Our waste form, our primary reference waste form is spent CANDU fuel. For those of you who are not familiar with a CANDU reactor, it's a natural uranium-fueled reactor with on-power refueling. We have fuel bundles that are a half-meter long and 10 centimeters in diameter. They contain 18.93 kilograms of natural uranium in the form of UO_2, and burnups vary, but generally it's 7200 to 7500 megawatt days per ton.

So the used CANDU fuel bundle has--this is time after discharge from the reactor--a decreasing heat load, as with everybody else's, and a decreasing radioactivity with time, and that is sort of a log scale for Larry's benefit.

So that's our primary waste form. Because it's sintered uranium dioxide, it is basically a ceramic material and is quite a stable waste form. We also have done some work on the reprocessing waste from reprocessing that material. In that waste form, our primary reference was a borosilicate glass, but we looked at a variety of wastes.

In the area of containers, we've looked at a variety of container designs. This isn't a particularly good slide, but we haven't put a better one together yet. Looking at stressed-shell containers, where the container shell will handle all the loads imposed on the system by hydraulic head sealing material swelling in a variety of materials. We've also looked at supported shell containers, where we have structures within the container to carry the loads that are imposed on the system.

Further on that, our primary reference is titanium. We've looked at Grade 2 as being our primary option, because we have a better understanding of the chemical characteristics and thermodynamic corrosion stability of Grade 2 as compared to Grade 12, but Grade 12 is an alternative material. It's preferred over other materials, because in welding, it's not as altered as some other materials are in welding, so we don't seem to get sensitization around the welds in any tests that we've done, and because of a concern about the onset of crevice corrosion in Grade 2 material, we limit our repository heating to 100°C for that reason, or that is one of the things that limits repository heating.

We're also looking at oxygen-free copper as an alternative design, and there we are comfortable with our understanding of the corrosion, both uniform and pitting corrosion, up to 100°C. Beyond that, the people working in the corrosion area are uncertain, so we're also, if we work with a copper container at this time, we would limit it to 100°C.

Other materials that we have considered are iron-based materials where our primary concern in our particular repository design is the gas that would be generated from corrosion. We've looked at stainless steel materials, and our concerns have to do with corrosion in saline waters, because at our repository depths in the Canadian Shield, the waters are saline.

And we've looked at nickel-based materials, and again, there's insufficient material on their performance in a saline environment to be comfortable with their use. So we have, right now, the two primary choices and other materials that, in our program, where we're looking at demonstrating technical knowledge, understanding, and performance without going into a major engineering/construction project. We're picking materials we understand.

Our titanium container is a thin-walled container which, in this particular geometry, holds a stack of four fuel bundles in each of 18 pipes, giving us 72 fuel bundles within the container. The dimensions are 225 centimeters high, roughly 64 centimeters in diameter, and the shell thickness is just over 6 millimeters, or quarter-inch plate thickness. All of the voids within the container are filled with a particulate material; in this case, glass beads to make it a mechanically solid system.

We've just started looking at a copper container with 25 millimeter wall thickness; again, holding 72 fuel bundles, but this time in an array of two bundles deep and 36 per level, so it gives us a shorter, squat container, having about 111 centimeter height, and about a 90 centimeter diameter, and the difference between the two is this particular design lends itself more to in-room emplacement or drift emplacement concepts, whereas the long, skinny container lends itself to borehole emplacement concepts.

If you look at heat output from a typical container versus time out of the reactor, you'll notice that, as with all other wastes, we have a fairly steep gradient in heat output, but you'll also notice we're only starting at about 300 watts per container at ten years out of the reactor. So what we have is a relatively high volume, low-heating waste form; high volume due to the nature of our reactors, which take natural uranium, but do not achieve particularly high burnouts as compared to other reactor types.

When you look at our typical geological environment--and you have to expand the scale of this, because this is on the scale of our underground research laboratory, which is basically a kilometer by a kilometer, roughly, but generally, through the Canadian Shield wherever we have looked, we find that there are major sub-horizontal fracture zones in the rock bodies that are the pathways for groundwater movement. Because of the low hydraulic gradients in the Canadian Shield, water doesn't really flow in the sense that you imagine it. It moves at millimeters, centimeters, or maybe a meter or a little bit more per year.

We also have an upper zone which tends to be vertically fractured, generally extensional fracturing, and we have a lower zone with lesser fracturing, or, as is the case at the URL, with virtually no fracturing at all. But, typically, there are lower zones where the hydraulic conditions are tight; upper zones where the hydraulic conditions are less tight.

Now, when we look at designing a repository within that, we came up with a set of criteria that are not dissimilar to what other people have put forward. We came up with a maximum outer container shell temperature, based on corrosion considerations and our ability, without a major research program, to make statements about container life, of 100°C. This is total temperature, so you have to include the geothermal temperature in the calculation of this number.

The maximum buffer backfill temperature--although I haven't gotten to it yet--is 100°C, and we have taken the maximum depth of a perturbed fissure zone. That's the zone near surface, where if you do an elastic calculation, you would go into tension largely due to thermal expansion effects, to be 100 meters maximum, and if you relate that back to the typical image of our geosphere, where you have vertical fracturing near surface, the situation we're trying to avoid is opening those fractures and propagating them to great depth.

We have picked a variety of other criteria, and we're working in a geothermal environment where we have a 5°C surface temperature average, and 12°C per kilometer temperature increase with depth.

We've looked at a variety of emplacement methods, all of which are room and pillar designs, including the emplacement of containers within the boundaries of excavated rooms--our in-room emplacement concept--emplacement of single containers in boreholes into the floor of rooms, and emplacement of containers horizontally into the pillars between rooms.

We've looked at these two particular alternatives for single-level and multiple-level excavations underground. We took the minimum depth of 500 meters, and the maximum depth of 1,000 meters, and we did the thermal and thermal mechanical analyses to see how many levels we could stick in there and still look roughly economic when you compare it to a single-level emplacement.

Interestingly enough, if you're working with spent fuel, in our particular case, you work single level, just to keep the temperatures below 100°C; otherwise, your two levels, the waste gets so widely spaced out on them that the economics, even in a very cursory analysis, look very bad.

We also looked at the possibility of emplacing the waste volumetrically within the rock body. Again, the spacing ended up very wide to accommodate the K heat from the waste. Now, keep in mind that in our reference case, we're emplacing waste that's ten years out of the reactor. If we cooled for 20, 30, 40 years, all of these analyses would look somewhat different. However, in most of the emplacement configurations, you might not be able to put the waste any closer together because you run into some physical limitations on just close-packing it, particularly where you're drilling boreholes in the walls or the floor of the excavations.

So we looked at all of these. They were all technically feasible, but economics would come into an optimization if you were actually choosing one for any particular site. So you would have to have site conditions, real information on the age of waste, and you would have to look at which one of those alternatives was most economic at any particular site.

We have, in this concept, three sealing materials. Immediately around the containers, we have a buffer material which, in our reference concept, is 50 per cent sodium bentonite clay, 50 per cent silica sand, mixed with 18 per cent moisture and compacted to a density greater than 1.67 megagrams a meter cubed, which gives us sort of a maximum hydraulic conductivity of 10^{-12} meters per second, and it's probably much lower than that.

Because of the sodium bentonite component in that, and the issues that Nils raised about thermal alteration and some of the ones Larry raised, we worry if we get over 100°C, because then the effects get significant. The other factor with this is that if the temperatures within that mass, particularly during the initial resaturation of the geosphere, get up to the point where you get boiling, we have here an extremely dense, low permeability material, and we're not sure what volumes of steam might do to it. So we want to avoid that. I'll get back to all of these in a moment.

When we're backfilling the rooms, we have a two-component backfill. The lower part of the backfill is a mixture of general glacial lake bed clay and 25 per cent of that, and 75 per cent crushed excavation material from the repository, and that is emplaced at about 6 to 8 per cent moisture content. And we expect to get at least 10^{-10} meters a second or lower in hydraulic conductivities for the emplaced material.

The upper part of the backfill where we can no longer compact that material is a mechanically flung or pneumatically placed material which would be 20 per cent sodium bentonite clay and 80 per cent silica sand, and there we're trying to achieve a placed hydraulic conductivity, similar to what they achieved in STREPA, of maybe 10^{-10} meters a second. So we have a very low conductivity environment around our waste form, and that leads to some of the factors that control our temperature choice.

Basically, with the sodium bentonites, we're not likely to have significant thermal alteration if we work below 100°C, and therefore, based on all the testing we've done and that has been done in other programs, the swelling properties of the material will be maintained, which is the important aspect for sealing.

It also will not be subject to steaming, and there's two aspects of that. One is if the steaming takes place near the container, which is the logical place for it to occur, it may mechanically damage the buffer material, but there's also evidence in uncompacted materials that the swelling and hydraulic properties are affected if it's exposed to steam. We're not sure that happens with dense materials, but it's an issue that we have not totally dealt with, so we would like to keep our temperatures below 100°C for that reason.

Other materials that we've looked at are cement-based grouts and concrete bulkheads which make up parts of our sealing systems for the underground. We have not completed our research on the thermal mechanical stability of those materials. We believe they'll be stable at less than 100°C, and we will do much more work on that when we look at broader use of the multi-purpose casks that Peter Stevens-

Guille will be speaking of later in the program.

A reference repository from all of those alternatives that we have devised and put forward in our hearing process is a single-level repository to take the 265 x 64 cm container, and single containers and boreholes in the floor of rooms, and based on thermal mechanical analyses that we've done, the minimum spacing that we can have for thermal reasons is about 2.1 meters across the room, and 2.1 meters along the room, so we get three containers across, and 90-some sets of containers along the room. Each one of these emplacement rooms has 282 emplacement boreholes. Although all of them may not be used, they each have to be inspected as they're drilled to decide if they're appropriate for use. If they're not, they would be sealed in the same way, but without the container.

We have to take an estimate that, again, I was involved with and, in retrospect, might have been done differently if we were doing it now. We came up with 10.1 million used fuel bundles generated from nuclear electric generation in Canada from the start of generation in the sixties, through to 2035. Current estimates from Ontario Hydro for the Province of Ontario alone are in the four million to five million fuel bundle range, so we might have a slightly larger repository here than we would now consider designing.

It's two kilometers across and two kilometers along, assuming that we have none of those geological features that would affect its arrangement. However, we fully expect that we're going to end up with something that has the disposal areas separated and spread around in the blocks of rock that are defined by the actual geological conditions at the site, and we would design our tunnels and accesses so that we could maintain our operating logistics, and also, we would space the waste so that we could achieve the temperatures that we're trying to achieve if, in fact, temperature is the limiting criteria.

This has nothing to do with temperature, and also has no times in it for these little triangles, which have to do with licensing processes. We feel it would take us 20 years from permission to do site evaluation to completion of construction, which is basically the state shown on that previous figure, and that had quite a leisurely schedule for our 10.1 million fuel bundles. We would emplace at the rate of 15 containers per day, but we would only work five days a week, two shifts a day, so we could certainly accelerate the operation. This would take us 41 years, and then there would be a period of decontamination, decommissioning, and final closure.

For that referenced single-level disposal vault, that too has suffered the rigors of transportation, you can see the temperature distribution with time, where the squares are ambient temperature. You see a peak occurring initially, very localized around the repository, and as time goes on, that peak gets much larger and spreads and becomes more diffuse within the rock mass.

Now, we also think that looking at thermal and thermal mechanical effects in the rock mass, that if we keep the temperatures below 100°C on the container or in the buffer--whichever one ends up controlling--and we're in moderate stresses, we will not run into any thermal mechanical instabilities in our boreholes or in the operation of the repository during the period after waste is in it, and we don't believe there'll be any problems after we backfill, because we will put a slight restraining load on the rock backfill boundary.

And those are stress, not temperatures, so I'll pass on those and I think I'll close there and try and address any issues people want to raise during the question period.

DR. DEERE: Thank you very much, Gary.

Would you allow me to ask the first question? What's the quantity of, or metric tons of fuel that you'll be dealing with?

MR. SIMMONS: Metric tons, in terms of elemental uranium, it's 191,000 metric tons if we go for the 10.1

million fuel bundles. That relates to something like 225,000 metric tons of fuel, with all the cladding, oxygen, and other bits and pieces that are in it.

DR. DEERE: Thank you. I ask you to bring that out because I was quite surprised to find that you will surpass our quantity considerably.

MR. SIMMONS: Yeah. Even if I'm wrong by a factor of two in the waste arisings, we will still have a larger mass than most other countries.

DR. DEERE: What do you have at the moment?

MR. SIMMONS: At the moment, I believe it's about 15,000 metric tons.

DR. DEERE: So you'll pass us in a year or two, I think.

MR. STEVENS-GUILLE: No. I'll show you tomorrow.

DR. DEERE: Oh, thank you.

I'd like to open it up for questions from Board members and other speakers.

Yes? Don Langmuir.

DR. LANGMUIR: Gary, you showed a table and a plot of the cooling history which did not agree.

MR. SIMMONS: One is a bundle, one is a container.

DR. LANGMUIR: The bundle dropped incredibly in one year.

MR. SIMMONS: The container started at ten years. There's a time difference on there. The container, which is the graph, is for 72 bundles starting ten years out of the reactor. The table, which is for a single bundle, started one year out of the reactor. So you have to truncate the table before it would relate to the graph, and multiply times 72.

DR. DOMENICO: Don?

DR. DEERE: Yes, Pat?

DR. DOMENICO: I have a question for Dr. Kuhn.

Salt domes are remarkably dry normally. Do you know, by any chance, the weight per cent water in the halite regions, as well as the carnallite and the kieserite?

DR. KUHN: It depends, of course, on the geological history of the salt dome, and we have made a very careful laboratory investigation program so we know exactly the moisture content of the rock salt of the Asse Salt Mine, which is, on the average, .04 volume per cent of moisture in the rock salt, which is extremely dry if you compare it, for instance, with the figure for the WIPP site, which is in the neighborhood of about 1 per cent.

DR. DOMENICO: Is that liquid or crystalline or total?

DR. KUHN: That is total water, and we have three possibilities for moisture in rock salt. One is in micro inclusions. The second one is interboundary grains, on the interboundary grains, and the third is crystal water, which is not true for rock salt, and we have figured out that it is negligible, the quantity of moisture contained in liquid inclusions. The most of the moisture is on the intergrains, the intergrain boundary.

DR. DOMENICO: But even if you assume that that was all liquid, that would result in an incredibly small porosity.

DR. KUHN: Yeah. The permeability of Asse rock salt is about 10^{-23} square meters.

DR. DOMENICO: Minus 23?

DR. KUHN: Twenty-three square meters.

DR. DEERE: Additional questions from the Panel; from the other speakers?

DR. LANGMUIR: Don, another question for Dr. Kuhn.

You pointed out one of the benefits, a major benefit of the salt repository, the creep effect. I presume this also is a benefit when it comes to, in our case, with the crystalline rock or non-flowing type rocks, the more exploration you do, the more you compromise the ultimate integrity of the repository. In your case, presumably, you can just keep drilling holes and they'll flow back in again, particularly when you're dealing with a high-temperature waste. But this, presumably, might not be so beneficial when you're dealing with a low-level waste, which isn't as thermally effective in terms of causing the creep?

DR. KUHN: Yes and no. The benefit is extremely good for the heat-generating waste for, as I mentioned, we have measured creep rates of about one millimeter per day for the borehole closures, but the same procedure will occur in principal, also, for non-heat generating wastes. It will take some more time, but the creep, the phenomenon of creep is the same. It is only accelerated at elevated temperature.

We have investigated all old backfilled drifts in our Asse Salt Mine which have been there for about 50 years, and it's really hard to differentiate between the natural rock salt and the backfill material at ambient temperature, at rock temperature.

DR. CANTLON: I have a question for Gary Simmons.

DR. DEERE: John Cantlon.

DR. CANTLON: What is the forecast surface temperature increase from the projected repository, and when do you get the highest peak?

MR. SIMMONS: We haven't actually done that calculation. We've assumed no temperature rise at the surface, and used that as an infinite heat sink. But the surface upheave, which would be an indication, again, of maximum temperature, peaks in a period of a few thousand years.

DR. DEERE: Max Blanchard, DOE.

MR. BLANCHARD: Thank you, Don. I have a question for Dr. Kuhn.

As you were considering establishing an upper limit for the repository operating conditions, were there other things besides the breakdown of polyhalite and carnallite that contributed to you establishing that limit, or was it mostly focused on the degradation that occurs as a consequence of those minerals breaking down under temperature?

DR. KUHN: There was, in the early days in the U.S. program, some investigations about decrepitation of rock salt at elevated temperature, and they were also investigating mainly samples from the WIPP site, and they also figured out that at temperatures above 230°C, salt started to decrepitate. We couldn't quite confirm these results. Our temperatures were somewhat higher, but starting from a certain degree of temperature--in the range of about 250°C--it starts to decrepitate, but then again you have to take into consideration the stress situation.

If you heat up a sample in the laboratory under ambient pressure, then you have quite different circumstances than compared to those within the repository. No, the main reasons for the present working temperature of about 200°C is the degradation of kieserite.

DR. BARNARD: I've got a question for Larry Ramspott.

Larry, you showed a graph, increase in boiling and dryout benefits for 60-year-old spent fuel, and you also had a plot for 30-year-old fuel on the same graph. I'm assuming that you're going to be able to put more 60-year-old fuel into a canister than you would 30; is that right?

DR. RAMSPOTT: Yes, you could.

DR. BARNARD: Approximately how much more?

DR. RAMSPOTT: I don't think that the specific designs have been done for this. That's a calculation that Tom Buscheck did, and it's like some of the paper from PNL last year, where basically they're looking at consequences of certain loadings, and there hasn't been a design for a specific canister that would have that. That's why I said I thought there might be problems with criticality. People have just looked and said, "What if we put this much fuel in?", and Tom, do you know how much fuel was put in that container?

MR. BUSCHECK: There would be no more fuel than that recent Swedish design we saw this morning. We think less than 24 intact assemblies, and this is assuming drift emplacement where now we're actually loading a drift versus having variably emplaced canisters, and to attain that heat loading, I think it required like 16 intact assemblies per 15-foot-long canister. That's PWR.

At the EBS workshop in Denver, we were discussing drift emplacement options, and we were considering that recently in our calculations. I think, as Larry said, we have to consider criticality, but while there was other issues, we were also looking at different drift emplacement spacings. You can get more waste in if you go to the drift emplacement mode than you can with variable packages, and you have a lot more flexibility in terms of average areal power densities.

DR. DEERE: Thank you. I have a question for Gary.

In the presentations that we received during our Canadian trip, it seemed that there was no program for definite closure, you didn't mention that aspect; that this was going to be left for a decision to be made some 70 years from now as to whether it would or would not be closed.

Can you expand on that?

MR. SIMMONS: Gary Simmons, AECL.

Our concept and schedules do have decommissioning and decontamination as an activity; also, closure as an activity. We haven't gone too deeply into what that might be, except to conceptually talk about how we'd do it if we were doing it today, because if that schedule I showed you did not start until the mid-nineties, the decision would have to be made sometime in the late 2060's or seventies or later, and it seems a little presumptuous now for us to be doing anything but stating that technical capability does exist, and not set criteria for it or anything else.

So we are showing that we have the technical capability of sealing up anything that we do to the rock mass or the geosphere, such that the models of the geosphere that are developed and validated during the process of construction and operation would still be valid after closure.

DR. DEERE: Thank you.

I'd like to take a couple of questions from the audience now.

(No audible response.)

DR. DEERE: All right. We'll have a chance during the round-table discussion in a couple days to come back to any of these gentlemen.

Let us break now for lunch, and we will be back and begin at one o'clock.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. DEERE: Good afternoon.

Well, the Technical Review Board was responsible for getting the morning's program to get a background of information, and this afternoon, DOE will be making presentations, bringing us up to date on their plans and the background of how we got where we are at the present time, and therefore, for the rest of the day, I will turn the meeting over to Carl Gertz, and he will be the first speaker.

MR. GERTZ: Don, thanks a lot.

As you pointed out, let me give you a brief overview. This afternoon, I'm going to talk about the historical perspective; kind of, generically, how we got to where we were, where we are, as a matter of fact, and then Mike is going to talk about the rationale, the technical rationale that went behind some of the programmatic decisions that got us to here. We'll then take a break, and Tom Blejwas with Sandia will talk about the repository design considerations, including the aspects of thermal loading, how they affect repository design, and Eric will supplement that with the thermal design considerations and temperature changes over time. He even tells me that he's got a film he's going to show us a little later, so we'll get to go to the movies around four-thirty or something like that. That's the view of how we're starting today.

As I said, I'll give you a little historical perspective, and the first chart I'm going to put up is at the end of my presentation, and it precedes Mike, and it's kind of a bridge between what I'm going to talk about and what Mike's going to talk about, but I'll talk about how many of these things evolved in the United States Waste Program, until we got where we were, in effect, today, and then Mike will talk about the technical considerations of that.

In summary, we've been working on criteria for waste disposal over a long time. It's been a logical process, as we view it. Some top level criteria were established by the National Academy in 1978. That included thermal considerations at that time. Thermal criteria were proposed in our rulemaking for waste confidence. Thermal loading margins were also proposed in the final EIS in 1980, and general and specific thermal constraints have been established through some of the documents and stage-setting documents I'm going to talk about.

First of all, if you saw my time line, we started back in 1955 with involvement with the National Academy in developing some early criteria. The AEC, DOE's predecessor agency, asked the National Academy, help establish a scientific base for the waste management program. In '57, they stated mined geologic disposal was feasible. They said salt was promising, but this was based upon a low concentrate liquid-type waste, an assumption that the waste would be a low concentrate in liquid.

In '78, we established, the National Academy, different criteria for repositories for high-level waste. They looked at long-term stability criteria. We knew the scientific community was heading towards some type of long-term, 10,000-year type activities, and part of this, they talked about thermal loading, or heat should not reach levels high enough to compromise geologic containment for the long term. So many of these things, as I said, were discussed.

Simultaneously with the National Academy talking about the criteria, the Department's predecessors had a group called the National Waste Terminal Storage Program, and this program initiated some studies. We looked at 36 states, different sites. We focused on different rock types other than salt, and in 1978, we evolved in a decision to consider a repository in tuff. The National Academy, even at that time, was thinking about a repository in tuff; albeit, at that time, it was in the saturated zone.

In our early program rulemakings, we set the stage for some of the thermal criteria and some of the other criteria that we're going to be involved with. I talked about the waste confidence rulemaking, provided guidelines for thermal design. This involved a lot of scientific and public input throughout this process, whether it was the waste confidence rulemaking, whether it was 10 CFR 60, which had extensive comments from the scientific community; talked about technical criteria associated with thermal loads, and then our siting guidelines in 1985 once again talked about the thermal effects and the thermal aspects of site suitability and repository design.

Just going back one other step, in our final EIS on the management of commercially-generated waste, it discussed many generic factors relative to geologic disposal. At this time, one of the concepts proposed was a three square mile repository area with a 65 kW/acre thermal loading. It should be noted that probably the information generated in this document was generated in '78, '79 before it got into the document, and at that time, tuff was not considered extensively. In the unsaturated zone, tuff was not considered at all, but still, it happened to be someone picking out some numbers that are about equivalent to our three square miles which we have right now, and thermal loading that's not too much different than what we're looking at, and that, as I said, was before Yucca Mountain unsaturated zone repositories were in the thought process.

Concepts were controlled, waste emplacement concepts, whether it's from uplift, whether it's for emplacement or rock mechanics. Thermal criteria were established; salts, 50; shale, 80; granite and basalt, 130. As I said, at this time, tuff wasn't considered, but the point I guess I'm trying to make by these is that there's been a lot of thought about thermal loading through the years. It's nothing real new. It's been going on and on.

The siting documents also set the stage for specific guidance. In 1981, we came up with what we called repository performance constraints in the far-field. It developed constraints for design and performance evaluation, included thermal loading approaches, and repository performance and development criteria, another document which was about the functional requirements.

Lots of requirements were developed. We were moving on with the program, and then came the Waste Policy Act of '82, and I guess the point I was going to underline, but I'll underline it here, the Waste Policy Act and the Congressional Record behind it established the federal responsibility and a definite policy--and I'll underline this--for timely disposal of high-level waste and spent fuel.

A lot of the thoughts behind that was let's get on with it and solve it with this generation. As a result, they established an ambitious schedule for development of repositories, not only the opportunity for interim storage, but key behind Congressional policy and national policy at this time was get on with the final solution. Let's find a repository if at all possible.

It directed DOE to put together guidelines, and with those guidelines, we would then recommend specific sites. I guess I wanted to underline that because that comes into play in lots of discussions about thermal loading, is the timely disposal, and I think the Congressional Records' are very clear on that.

In order for us to maybe be even more timely, Congress amended that Waste Policy Act and said rather than studying the three sites that DOE had narrowed our characterization activities to, they directed us to study only Yucca Mountain, to cease all the studies at the other sites, and to cease the second repository activities in crystalline rock. So we end up studying a single site in the unsaturated zone.

Part of this Act, of course, that told us to study Yucca Mountain also created your Board, your entity for technical oversight. That's kind of leads us where we are today, because in your first report and second report and third report, you address the issues that we're going to be discussing for the next two days or so.

Your first report talked about thermal loading concerns and a reduction of uncertainty by reducing thermal loading. We hope to be addressing some of that in our section called, "Uncertainties," tomorrow; concerns about uncertainties in the second report, in factors that influenced the thermal loading of the host rock in Calico Hills. We'll be able to show you some of our studies as to what thermal loading the Calico Hills would see, and what effect that would have on the zeolites in the area.

And you talked about concerns about thermally-induced changes in conditions and effects on engineered barriers. Certainly, we have had engineered workshops. We'll be alluding a little bit to that in this presentation, but what we recognize is, that's another area for discussion.

The third report in May, '91, once again, I've talked about some concerns about our repository conceptual design alternatives, and did they address thermal loading. I think three weeks ago when we were here, Mike Voegele talked a little bit about the different alternative designs for repositories that we had looked at, and how some of them could accept different thermal loadings; be it a step repository or whatever. So, certainly, we have looked at that and we will be discussing that and looking at it in the future as we go into our design activities.

And then you had concerns about thermal loading and waste aging and their impact on design of the repository, and

Tom and Eric are going to talk about that a little bit later.

So I guess for my opening concluding remarks, I'd like to state that we have a repository conceptual design that appears to meet criteria that have been developed over maybe 15 years of the program. We believe it does meet that criteria at this point in time. However, very clearly, scientific data from site characterization is needed so we can reduce uncertainties in the design inputs, and determine if that is the right design after we get more data.

We believe we understand the concerns that you've expressed, and we hope to address at least the majority of them in this meeting. Certainly, we may not come to a meeting of the minds on the solution for all of them, but we sure hope to be able to articulate the concerns and differing points of view, and that we're trying to do in this meeting, as we look at this meeting as an opportunity to discuss the constraints on thermal criteria, so a range of thermal loadings can be examined in future design activities.

As you appropriately pointed out in the draft mission plan amendment, we have a conceptual design. It's a reference. We're not absolutely locked into that by any means. We will consider all things as we move forward, and that's why we value these kind of discussions with independent Boards such as yourself, with the NRC, with the utilities, in order that it can enhance our understanding of the environment around Yucca Mountain and where we think the most optimum design would lead us. And by optimum, I mean what's the best design that can safely isolate radioactive waste.

Don, with that, I will then turn it over to Mike, and he can go into the history and the technical background in many of the documents and concepts that I talked about.

DR. DEERE: Thank you.

DR. VOEGELE: Thank you.

Good afternoon. As Carl said, I'm going to speak a little bit about the history and evolution of a repository concept for a potential repository at the Yucca Mountain site.

DR. DEERE: Welcome back.

DR. VOEGELE: Thank you. All right, I'll take a little bit of my precious time to note that four people, four members of the Board and their staff, just buckled up their seat belts. Last time I stood before the Board was three weeks ago, and I think I gave an eight-hour presentation in three hours. My commitment today is to go a little bit slower, but we got it in.

DR. DEERE: It was very good; well appreciated.

DR. VOEGELE: Thank you.

I'm going to flip back and forth between the two projectors as well, and I'll take a pointer in each hand.

Carl used his presentation to really give an overview of what we've done in the program since the waste disposal program started in the country in the 1955 time frame. Carl's emphasis was on some of the higher-level documents, some of the documents that set the stage for the way the program evolved.

I'm going to spend a lot of time on the two programmatic documents that Carl talked about; NWTS-25, which was "Repository Performance Constraints in the Far-Field," and the "Repository Performance and Development Criteria." I'll spend a little bit less time on that. Most of the emphasis of the early part of my talk will be on the types of constraints that were developed in NWTS-25, and I'll use the bulk of the remainder of my talk to show how those programmatic level requirements in those documents evolved into site-specific requirements, and then how we used those in the various evaluations and design studies that were done in the program. So I will spend less emphasis on the overall portion of the program, and more emphasis on the latter part of the program, with a few stops along the way to emphasize some points.

I also would like to try to focus my presentation in such a manner as to hopefully illustrate what I believe the logical evolution of some design constraints related to repository-induced impacts, and the focus will be on thermal design constraints. I'd like to talk about some of the performance measures that have been established to ensure the repository performs as it's intended. I would like to talk about site specific technical considerations, and the evaluations that we have done to address those technical considerations in support of our repository design efforts to date, and I think at that point in time, as a transition between these two bullets, I must remind you that the repository conceptual design that exists today has really been developed to support the site characterization plan, and to give us some concepts to work with. That is the primary focus of that document. It was required by the Waste Policy Act, and its purpose was to support site characterization.

As Carl noted, we have a repository conceptual design that we believe meets the performance measures and constraints that have been developed throughout this process that I'm going to talk about, and I'd like to tie my talk directly to Carl's. Carl's last bullet said something to the effect that we view this meeting as an opportunity to discuss some of these thermal constraints so that we'll be able to take that input and go into further design activities, considering that input.

I'd like to emphasize that from just a little bit different perspective, and that is, the perspective is that the discussions that I would hope we would initiate in this meeting would focus on the constraints themselves, rather than directly on the design products. I view something like areal power density, which is what we normally talk about when we're talking about a hot repository or cold repository, as being a design output. Okay, what I hope we can focus some of the discussion on is some of the constraints that go into the design, that you then develop a design output to meet. So rather than talk about absolute hots and absolute colds, I hope we can set the stage to talk about some of the constraints that you have to meet to ensure repository performance.

I will take one little detour and talk about that 1978 letter to the National Academy of Science, where we recommended siting a repository in tuff. The letter that went to the National Academy addressed the favorable and unfavorable aspects of disposal in tuff, and with particular relevance to the theme of this discussion, the letter did, in fact, address some of the thermal impacts that a repository in tuff might have on the site where we would be considering.

The situation at that time--this is actually from the letter to the National Academy--looked at many possibilities for possible repository sites in the southern Great Basin, and, in fact, they are illustrated by the dots here, and you can see that people were considering Caldera locations within the granite, within welded tuffs above zeolitized tuff aquitards, non-welded tuffs, and so forth. In fact, if you turn this diagram over, it sort of looks like Yucca Mountain, depending on which perspective you're looking from.

So anyway, at that point in time, people were addressing some of the questions that we're still addressing today. That letter contained a statement that suggested that the dominant zeolites that were present in this environment, in fact, would be stable for short periods to very high temperatures, and still metastable around temperatures of 250°C. You're going to hear a presentation tomorrow afternoon by Dave Bish from Los Alamos that will give you a far more current update on our thinking regarding that matter.

With respect to the welded tuffs, we were proposing to the National Academy that these were comparable to other igneous rocks that were being studied in the program at that time; granite and basalt, comparable in strength, thermal conductivity, heat capacity, and mineability. One point that was noted that might be viewed as unfavorable was that the repository could be relatively shallow, and we certainly identified issues related to water content and zeolite stability arising from the thermal regime that we would impose on this site.

Carl mentioned the final environmental impact statement for management of commercially-generated radioactive wastes. This is the document that the DOE prepared, which really provided the substantive arguments for why we should proceed with disposal in a geologic repository.

As Carl noted, the repository concept that really came out of that FEIS was, in fact, a repository that does not look very much different in size and scale, I guess, from what we're dealing with today; less than three kilometers on a side, 600 meters, 800 meters depth were the numbers that were being thrown around at that time, which would give you an indication why, in the letter that went to the National Academy of Sciences, the shallower repository would have been viewed as being something different from what was being considered in the program at that time.

That FEIS discussed general factors relevant to disposal in all the different media considered, and they looked at depth, at different rock properties, tectonic stability, hydrologic regime, resource potential, multibarrier safety features. Now, this document is dated 1980, and that's the publication date for a document like this, and so you can guess it was just coming in on the end of when people were beginning to consider the unsaturated zone as a possible environment for disposal, but tuff, per se, was not considered in the FEIS.

Carl mentioned that the waste emplacement concepts were controlled by thermal criteria. They set specific thermal criteria in this document related to uplift of the surface, ground surface and aquifer temperature rises, questions about retrievability, and temperatures of the high-level waste itself, the fuel pins, the canisters, and the rock surrounding it. With the safety margins which were set at 50 per cent at that time, which were considered to be the allowable impacts, they came up with design areal power densities ranging from 50 kW/acre for the salts, up to 130 kW/acre in granites and basalts, and 80 for shale, which was in the middle. So the significant point there, once again, is that this is the document where people had a chance to have some input into the decision to move forward with high-level waste disposal.

There are several other interagency documents that exist in that time frame. President Carter's National Energy Strategy in 1977 really had these same types of concepts in it. I didn't discuss them particularly in this presentation.

The two documents I said I was going to focus a little bit on were NWTS-25--that's the National Waste Terminal Storage Program document, "Repository Performance Constraints in the Far-Field Domain," and I think that's the document that you will see had the most heavy influence on the thermal constraints that we have developed in our program. And likewise, there were some things that came from

NWTS-33(3). This document and its concepts evolved more in the direction of what you'll find in 10 CFR 960 today. So that's a precursor to 10 CFR 960, if you will.

Let me talk about NWTS-25 for just a moment. As the title would suggest, this is a document that developed performance constraints for design and performance evaluation for questions related to repository performance constraints in the far-field domain. The kinds of things that you'll find in that document--I'm going to go into a fair bit of detail in a minute--are things that address issues such as irreversible thermochemical perturbations in the far-field, and actually, specification of temperatures. This is one that you'll see in the next slide I'm going to put up.

They would recommend certain temperatures not be exceeded in certain regions, so why don't you just let me put that picture up and we'll go through these in a little bit of detail so you can see where some of the things that we talk about and, I guess, take for granted that you're more familiar with than some of our other presentations, where those things came from.

I'm going to go through several categories of performance constraints that are found in that document, and I'll refer to them in the context of this picture. This is just a simple picture of the repository horizon at some depth, H, below the ground surface, and the performance criteria were set relative to areas of 15 per cent of the depth above and below the repository, a total of 70 per cent of the depth between that boundary, and then what was called the 85 per cent boundary, slightly below the surface and on either side. You'll see different categories of performance constraints apply to those.

With respect to fracturing, now, I've only pulled out the ones that are specific to tuff. There are criteria in this document for all the media under consideration. For granite and tuff, thermomechanical stresses should not cause shear failure in the middle 70 per cent of the rock between the repository horizon and the ground surface.

Let me just comment on that, that many of the sealing concepts that exist in our program today date to concepts having to do with water moving up from the repository to the surface rather than water moving down to get to the repository. So there was a focus in our program to prevent fracturing within these regions where we would want to seal it from the ground surface.

For all the rock types, the vertical extent of the perturbed fissure zone should not extend downward from the surface more than 15 per cent of the repository depth. I think it was Gary Simmons who talked about that this morning. That's an uplift-type concern, where you would be opening fractures at the surface due to the rock mass moving upward.

With respect to thermally-perturbed groundwater for the igneous-type rocks, basalt, granite, and tuff, the time for groundwater to travel from the repository facility to the ground surface--again, that's more considering moving up--as a consequence of thermal convective forces should be greater than 1,000 years.

There were plenty of discussions going on in this time frame so that people knew what 10 CFR 60 was going to look like by the time it was finally promulgated, so you'll see numbers in these documents that are very consistent with the numbers that you'll see in some of the NRC documents which were coming out in the same time frame.

DR. DOMENICO: Excuse me. One point.

DR. VOEGELE: Sure.

DR. DOMENICO: That last bullet, is any mention made of steam as opposed to thermal convection?

DR. VOEGELE: No, no, not at that time. At that time, really, all of the considerations for all the repository sites were in the saturated zone at that time. We had not yet really formally moved into an unsaturated zone consideration. You're going to find that same question relevant in a couple of later view graphs, where you'll wonder why they did things a certain way, and it was because it was a saturated zone repository site.

With respect to shaft and borehole integrity for all the rock types in the pre-closure phase, the shaft and its components should undergo no fracturing due to the thermomechanical stresses that would be induced by the waste, and have no significant water leakage. And that's an operational constraint, obviously, but it is traceable to thermal impacts.

For all the rock types, again, deformations of the shafts and shaft liners during the pre-closure phase be sufficiently small to not impede routine operations and major remedial work would not be required. That's a criterion that would lead you to a sufficient shaft pillar to prevent any deformations which would give you operational problems with the shaft itself.

With respect to tuffs, there was a specific recommendation that the shafts should be located so that they do not intersect major faults, and for granite and tuff during the post-closure phase, the permeability of the sealed boreholes and shafts should be approximately the same as the host rock permeability for the middle 70 per cent of their vertical length. Again, that's this region that we talked about before, right in here.

There were sections on thermomechanical perturbations specifically, and the performance constraints that were developed were that the temperature should not exceed 125°C for granite, 100°C for other rock types in the region extending from the near surface outward to 15 per cent of the repository depth. That's this region right in here, of course, and it would go on the other side as well. Temperatures should not exceed 100°C for granite, 75°C for the other rock types anywhere outside of the region defined above, and that specific emphasis was on this .7H region again.

There were criteria for heating of the ground surface and near surface for all rock types. The maximum temperature increase within 3 meters of the ground surface should be less than 4°C, and that's a number that came from a professional judgment as to what would limit the impacts on the environment. The vertical surface displacement should be less than the variations of natural processes, such as glacial rebound and erosion, and they said that that number would approximately be 3 meters, and should occur in a smooth fashion over time.

With respect to NWTS-33(3), this is more of a requirements document, higher level requirements document. As I said, this document went more in the direction of evolving into things like what's in 10 CFR 60 and 10 CFR 960, which are the NRC, Nuclear Regulatory Commission, and the Department of Energy's licensing requirements and siting guidelines, respectively.

NWTS-33(3) addressed functional requirements that the repository system had to contribute to the containment and isolation capability of the system as a whole, and it also had to limit the adverse impacts of repository development and operation on the site performance. It tends to be more of a post-closure, perhaps some post-closure/pre-closure concerns right there.

Specifically, this document states that the DOE's intent is to restrict temperatures to limits within which thermal impacts can be shown to have no significant degradation on the system's containment and isolation capability, and said that the DOE was to prescribe thermal limits, including thermochemical interactions, that would accelerate the rate of transport of radionuclides.

Now, as I said, this is less of an implementation document and more of a higher level guidance-type document, but what's embodied in this document is a requirement for the DOE to set temperature limits to guide their design and performance evaluations, and I hope to show you that, in fact, that was done in the program.

DR. DEERE: Excuse me. Where was this document from, the NWTS?

DR. VOEGELE: These are publications out of the Department of Energy or its predecessor. The NWTS was AEC--actually, probably an ERDA program that then became the Department of Energy. In this time frame, between '78 and

'82 and '83, you will find all the wonderful acronyms you would like in the program. That was the era of ERDA, which when the Atomic Energy Commission was split, it was split into ERDA, the Energy Resource Development Agency and the Nuclear Regulatory Commission.

Several years later, ERDA became the Department of Energy, and at the same time, you will find ONI, ONWI, NWTS, and a whole host of other acronyms in the program. This is the time when there was that change in the program where we began to get focused in a little bit different direction. So '82 was probably DOE, okay, so ERDA had become DOE, but I believe the NWTS program actually originated in the ERDA era.

MR. GERTZ: It's not Nevada. It's National Waste Storage, National Waste Terminal Storage.

DR. VOEGELE: Well, we have a clouded history right here. Definitely, by 1982, this was a DOE publication to answer the question that was asked.

Now, those were higher level documents, especially NWTS-33, as I mentioned, was a document which basically set the stage for the Department of Energy to develop specific criteria, and in that same time frame, the 1980's proceeding onward, there was something in place called the Reference Repository Conditions - Interface Working Group that developed specific reference conditions, reference repository concepts for salt, basalt, tuff, granite, and shale.

This group addressed the temperature effects, pressure effects, fluid, chemical, and radiation effects of the system on the natural environment. The purpose of these reference conditions was to guide the testing programs, to guide the designs, and be a technically conservative basis for license application in the waste form development. Even at this early stage--it doesn't pre-date the NWPA by very much, Nuclear Waste Policy Act by very much, but again, the idea behind this design effort was not to develop a final repository design, but it was to guide the testing programs with a set of conservative repository concepts.

Now, they developed reference repository descriptions for each of these media, that included depths, room dimensions, canister thermal loads, local areal thermal loads, and average areal thermal loading. There's much more in it than that, but those are the ones I've chosen to abstract for you. They also evaluated some of the peak near-field temperatures for these repository descriptions.

Incidentally, I have tried, I think consistently, to include references where you could go back and find some of this information in its more complete form. I think if you see something down here, that's a reference to where you might find this information. Now, having said that, I have to say you will not find this information in that reference, because the consensus of a working group of two--consisting of Mike Cloninger and me--was that the columns were probably transposed in the original reference. So we have taken the liberty of what we believe is straightening out a ten-year-old reference for your enjoyment this afternoon.

The kinds of things that you could see, the depths that were being considered in that time ranged from 600 meters to 1,000 meters, and you'll notice that the tuff under consideration at that time was at a depth of 800 meters. That was well into the saturated zone at Yucca Mountain.

The room widths ranged from, I think, 4.3 up to 7½ meters, depending on whether you're talking about spent fuel or commercial high-level waste; likewise, spacings. Interesting points here are in the initial thermal loadings for the spent fuel, canister loadings, the difference here between the BWIP--excuse me, the basalt. If I lapse into acronym-ese, I think just poke me. I'll speak English for you. The Basalt Waste Isolation Program was using two reference concepts at that time, one with three assemblies, one with one assembly in the canister; likewise, comparable numbers, lower for granite and shale.

The interesting thing, and the reason I chose to include this diagram, are the thermal loadings that were under consideration at that point in time. Now, these are in watts per meter squared, and the correlation factor is four. You multiply these by four to get kW/acre. So we were looking at numbers in the 50 to 100 kW/acre range in that Reference Repository Conditions - Interface Working Group study.

I can point out some other things on there. There are obvious differences between the commercial high-level waste and the spent fuel approaches, lower temperatures. Tuff and granite did have relatively high areal power densities. I cannot explain why the basalt was as low as it was. I have no explanation for that; don't remember.

I also noted that they did do some evaluations of peak near-field temperatures under those reference repository conditions, and this is a tabulation of those. These are the in situ host rock temperatures at the repository horizon, and these, then, are the total temperatures that would be induced. Now, to be consistent, we also transposed these columns, and I believe that's correct, and I also can't explain to you some of the inconsistencies in this table, such as why the differences go up there, that they go down in both of those locations. I don't have that detail from the Basalt Program at my hand, but I will point out to you that you're seeing some numbers here that are virtually no different from the other numbers that we've been telling you all along.

This is program generic development of repository conditions, and so the origination of some of the numbers that you've seen--like the 275° waste temperature, the 235, 250° canister wall temperatures, 200°--they're traceable way back to this stage of the program where people were looking at the reference repository concepts, trying to be consistent with what was being developed in NWTS-25 and NWTS-33. There are documents--and I'll talk about some of them in a minute--that were doing the calculations to support the development of these reference conditions, and the output of that is this is an example of one output from that type of evaluation, which would be the peak near-field temperatures.

I'm going to do a couple of site specific design concepts, and there's a couple of references here that are very similar, but there are a couple of differences that are worth pointing out. This is a publication from 1980. In fact, there is a nice workshop on thermomechanical-hydrochemical modeling for a hard rock waste repository, and there's a paper in there on thermomechanical modeling for a tuff repository, and that's a saturated zone repository still at that time. It's a depth of 800 meters.

The primary focus of that study was on fracturing of intact rock, and changes in the rock properties due to boiling of the water; still in the saturated zone. The numbers that they were working with at that time, still trying to address the types of criteria that we found in NWTS-25, suggested a gross thermal load upper limit of 100 kW/acre was a reasonable number.

DR. DEERE: A question. Don Deere here.

DR. VOEGELE: Sure.

DR. DEERE: Was the tuff that was being talked about the hard fractured, welded tuff?

DR. VOEGELE: Yes.

DR. DEERE: Not the softer tuffs?

DR. VOEGELE: Unfortunately, there is no way that--if I had a circular view graph machine, I could probably have all the view graphs up at once and go like this and answer all the questions. I had to do this topically, and there are some presentations later in the talk that will really help you understand how all these pieces fit together.

There were four horizons under consideration at Yucca Mountain; coming up from the bottom, the Tram, the Bullfrog, the Calico Hills, and the Topopah Spring, and three of those are welded, and the Calico Hills is non-welded.

Here's a plot of temperatures for gross thermal loading of 75 kW/acre, and you can see a technology transfer in this diagram. They didn't have a number for the in situ ambient temperature for a repository depth at 800 meters, and so they picked 57, which is the number that the Basalt Program was using. It's technology transfer. That's how that worked.

Anyway, as I said, this is a depth of 800 meters, and here is the temperature profiles, and you can see, oh, within the--the .15 number is somewhat less than 150 meters, and you can look at the temperature changes in the 50-year time frame, the 500-year time frame, 5,000 and 50,000 years. People were quite comfortable with those types of temperatures. Rather simple models, but again, they had in mind the kinds of constraints that we were talking about in the NWTS program at that time.

There was a comparable study published in a different reference at that time. Very similar, but they do go in a little bit more to some of the far scale heat effects as a function of boiling temperature, gross thermal loading, extraction ratio, and changes in the rock thermal properties. The graphs and output look sufficiently similar to the one I just had up, but I just thought I'd identify another reference for you that may be easier to find.

Now, this is probably one of the more important early program documents in terms of setting some of the technical constraints that we're still working with in the program today, and what was done in this particular document was to try to develop quantitative limits for assessing the performance of the repository site, and basically, this report summarizes the technical constraints, including temperature limits in the very near-field, the near-field, and the far-field, and I happen to have selected one phrase, that impact mineral hydration or dehydration. They did have many other elements that were under consideration, and I'll spend a little bit of time showing you some of that information right now.

This is the summary of preliminary technical constraints for the very near-field, and you can see that they addressed repository system components and they addressed three different time periods; an operational period, which was less than 110 years; a containment period, which ranged from 110, which would be the closure of the repository, to 1,000 years; and then an isolation period for greater than 1,000 years. Remember, this is the very near-field component. I'll show you in a moment one for near-field, and then I'll show you one for far-field.

They set temperature constraints on the spent fuel and the commercial high-level waste, the cladding and the center line, much higher than we've seen before and from what we're using in the program today. But as I said, this was their attempt to put together the first comprehensive quantitative set of criteria; did not have constraints on the spent fuel; did have a concern that the temperature of the surface be less than 100°C if it was going to be exposed to water.

Now, you're going to see in this time frame a publication that the NNWSI--which is an acronym I was not going to use--the Nevada Nuclear Waste Storage Investigations Program, which is what we were called before we were the Yucca Mountain Project, so I have to use it. It's on this diagram anyway. I was hoping that the first time I used it I would use it deliberately. I accidentally used it deliberately. Okay, Nevada Nuclear Waste Storage Investigations Project.

Again, we're in the very near-field. We're looking at the canister, the overpack, some of the backfills, and the tuff rock itself. These are surprising to me, that they would allow, or consider allowing that kind of a temperature in a sodium montmorillonite backfill in the operational period with the requirement to be so much lower in the containment period, but again, this is their first attempt to get these out and get them documented. There's much more work done with them at later points in time.

Let's look at the near-field constraints, again, looking at the repository system components, same three time periods. Room stability during the operational period is an operational serviceability, and that's just basically meet normal mining-type requirements. They had a safety factor on pillar stabilities; floor heave, again, within operational serviceability. They have a mineral dehydration criterion set during that operational period to be less than 150°C. This is the near-field. I think what you can deduce from this, since there is no corresponding constraint in the far-field, is that they were not intending to rely on the near-field component of the rock for isolation and containment.

These criteria are actually the two that will stay with the program and result in the 57 kW/acre that we've talked to you about so much. There was a concern that especially if we were backfilling and had to go back in and retrieve that waste, they wanted that rock to be less than 100°C. They didn't think they could sufficiently cool the backfill material if it was hotter than that. They wanted to keep it at that limit. Again, I'm not sure I would want to handle 100°C backfill, but that was the rationale behind those numbers, was the retrievability and operational concerns, and as we've told you before and as I'll tell you again, those are the numbers that drove the 57 kW/acre that's in our design today.

And then what you'll see, very near-field, you'll see the one part in 10^5 release rate during the isolation period, and no release during the operational and containment period, which are very comparable to what's in 10 CFR 60 today, the 300 to 1,000 year total containment, and a gradual release rate after that, one part in 10^5.

Okay, here's the far-field ones, and let me put back this picture just so we have the 70 per cent depth up there, because it's relevant to this. They looked at the same kinds of criteria that were in NWTS-25. They stayed with the program and, in fact, we took them into our preliminary technical constraints and tried to make them site specific. You'll recognize intersecting no major faults. Changes in alignment have to be within what can be managed within the construction program. That's literally the only far-field operational constraint, and those are the shaft alignment.

With respect to the containment period and the isolation period, you'll see, again, the sealing question, try to seal those so that the effective permeability of the seals is the same as the tuff itself, and again, that's within this 70 per cent region. In fact, at this point in time, they've stated it above and below the repository.

Mineral dehydration out in the far-field, less than 75° change in temperature with respect to mineral dehydration and alteration. Surface uplift and subsidence, less than what you would get from natural analogs. Maximum surface temperature increase, less than 6°C, and again, they believed that was comparable with natural analogs. Thermally perturbed groundwater flow, they've captured that as the travel time greater than 1,000 years.

DR. CANTLON: Why did you choose 6° and why did you choose surface as opposed to subsoil temperature there?

DR. VOEGELE: Somebody asked me that exact question and

I've gone back to the original references to see if I could find more information about that, and I can't. There are some references. Dr. Ostler is going to be talking tomorrow afternoon. In fact, I tried to ask him that same question, tried to give him that NWTS-25 document which cites a couple of references, and ask him if they were useful to his talk. If he's sitting here and wouldn't mind standing up to a microphone, he might--I don't see him. I believe he told me that it wasn't a very useful reference for his purposes, but again, this is history. I can defend a lot of it. I can't defend it all. I can tell you what it was. That's basically the beauty of history.

DR. DEERE: Another question. On the surface uplift and subsidence, what do they mean by the natural analogs?

DR. VOEGELE: Erosion, uplift within the southern Great Basin. The rocks are moving in the southern Great Basin. They were basically talking about keeping the rate at the same kind of rates that you would experience in the southern Great Basin, either coming up through the uplift that's going on in portions of that basin, or the downward movement which you would get from erosion. So you'll see that kind of analog coming up again. In fact, I have it in more detail in subsequent slides.

Okay. Well, this is the document where I thought I was going to introduce NNWSI to you, but I guess I've already done that. It's a pity I've spoiled this, because most of the people in the program believed that that acronym stood for Next November We Start Investigating.

(Laughter.)

DR. VOEGELE: In 1983, we were really firming up our repository design approach, and we're dealing with thermal loadings in the range of 12 to 15 W/m^2, which is 48 to 60 kW/acre, and again, what's driving that is that operational constraint of the floor temperature and the back temperature of 100°C. Now, these thermal loadings were selected to satisfy the performance constraints to ensure isolation was not significantly degraded, but as I just said, what drove that lower number was, in fact, the operational constraint.

DR. DOMENICO: Mike, excuse me. We're still dealing with a saturated tuff here?

DR. VOEGELE: No. At this time we've pretty much moved into the unsaturated, and unfortunately, the way I had to do this, I can't give you the unit evaluation study just yet. There is a study which documents the selection of which of the four repository horizons we wanted to go to, and I have quite a few figures from that diagram that I'll show you momentarily, but in this time frame, we were really moving--

DR. DOMENICO: But this point on, we're talking unsaturated tuff?

DR. VOEGELE: Well, this particular reference carries saturated and unsaturated connotations with it, okay? But we were really, by 1983, pretty well into the unsaturated zone. In fact, that would turn out to be the worst view graph I have. I guess it's readable.

Okay. This is the assessment from that particular study of what the repository impacts on the host rock would be, and basically, they've tabulated near-field and far-field impacts. They believed that they had mining-induced impacts in the near-field, but not in the far-field. With respect to stress redistribution, they felt it would be felt in the near-field, probably not in the far-field. Rock temperature changes had impacts in both the near- and far-field, as did thermally induced stresses. The thermally induced uplift, not applicable to the near-field, but we definitely had it in the far-field.

With respect to alteration of the site hydrology, at this particular point, 1983, for the repository being above the repository, the position that these authors took was there will be slight impact on the site hydrology in an unsaturated zone repository; whereas, if it was below the water table, there would be impacts. I think that we've probably come quite a long way in the past eight years and none of us would make that same statement today, but that's what was said in 1983.

Radiation induced rock property changes were not believed to be of concern at that time. They also look at thermally induced mineral alteration. They believed there was a potential for it in the near-field. They did not see a potential for it in the far-field, and that's based upon the thermal modeling that they had been doing at that point in time. With respect to rock-groundwater waste interaction, they saw a potential impact in the near-field, but not in the far-field.

That brings us to about the same time frame as the 1983 National Academy of Science Board on Radioactive Waste Management publication, where they assessed the status of technology for waste disposal. That particular document evaluated geologic disposal performance and release control mechanisms for all the sites that were under consideration at that time. The favorable conditions that they were looking for in that study were the delay of water, slow dissolution, slow release, long travel times, sorption, dispersion, and dilution.

With respect to the tuff repository, benefits were seen in that report with respect to being in the unsaturated zone and the retardation capability of many of the rocks at the site; however, this particular report identified uncertainties in hydrology and the thermal effects on geochemistry, still well known in the program at that time.

Now, I think I should say two things about this.

The first one is that the National Academy had much help from the scientists on the, at that time, NNWSI project in preparing this particular chapter of the document. Basically, they interviewed quite a few people. I think the one significant thing that we've really lost in the program, in that particular report you will find a very strong emphasis on the ability of the Yucca Mountain site to meet the EPA standards based on the saturated zone groundwater travel time literally alone. So we've lost that confidence that we once had in the site suitability to perform in the saturated zone as well. We've lost that, and it's due to uncertainties in things like the effective porosity that would be used in the calculations. So I just wanted to point those two aspects of that document out.

Okay. Every once in awhile you remember that here's one I haven't told you about before. In 1984, there was a document prepared within the project called, "The Preliminary Repository Concepts Report," and that particular document addressed all these technical constraints, using ten-year-old out of reactor spent fuel as a basis. This document was really a primary reference for our environmental assessments that were being prepared.

The conclusions in that particular report are that 57 kW/acre was an acceptable areal power density for all the preliminary constraints, again, and the one it violated was that near-field--excuse me--that operational temperature, that backfill temperature constraint. There were other bounding calculations referenced in that document. In particular, they found that you had to get up over 90 kW/acre to violate the far-field constraints that we've been talking about, coming from the NWTS-25 document, through the reference repository coordinating group interpretation, and the site specific work that was done, and they were finding that with respect to the near-field concerns specifically, probably 76 kW/acre was an acceptable number for some of those more near-field concerns, and so this is an operational concern. They felt you could meet the far-field constraints with loadings in that range, and to meet the near-field concerns you probably would be in loadings in that range.

I want to show you a picture of that repository concept, because it's different from, I think, what you've seen before, and in particular, if you look over here on the section, the section goes right through this part of the repository. You can see that it's a stepped repository, so we've not shown you that picture before. This is a Sandia publication, 83-1877, has a design that has that stepped repository in it.

This particular one, at that time--as long as I've got it up there and an audience who cares--we had a single ramp access at that time. Ventilation exhaust shaft was over here. Waste exhaust shaft was over there. Intake ventilation shaft was right there. The mine shaft, muck and material, was right there, and we had a single exploratory shaft right there. So that's another one from the past.

DR. DEERE: Excuse me, Don Deere.

We were still, however, in the same horizon of the Topopah?

DR. VOEGELE: This is the unsaturated zone. This is the Topopah at that time.

The only way to say this is these guys knew the answer. This, in publication date, slightly precedes the unit evaluation study, but I think in fairness, the unit evaluation study was well on its way to being completed. People knew what the answers were going to be, and some programmatic decisions were made at that time to settle on the unsaturated zone as being the location for the repository horizon. That will explain some of the apparent inconsistencies in the dates. It was all the same group of people working together.

The program was very good in that time frame in terms of the different labs interacting with each other. Working groups would be set up and they would share information, so that even though this had a Sandia report number, I think you could be pretty confident that the GS people were involved in that document, as well as Los Alamos and Livermore people.

Okay. Well, I brought that particular document up as a lead-in to the preliminary evaluations of site suitability, specifically the thermal aspects that were done to support the environmental assessment. In a moment, I'm going to show you a couple of conclusions from the environmental assessment, some of which we probably wouldn't state in exactly the same way today. We probably would express more uncertainty on some of them. I'm not certain. We are doing that today in the early site suitability evaluation, and I have not yet read that document, so I'm guessing we would have stated some of these things slightly differently.

The two in particular that are of interest are the geochemistry guideline, which addressed thermal impacts on retardation, and the post-closure rock characteristics guideline, which addressed thermal impacts on isolation, which is the same thing; not quite, but close.

Okay. Now, there was another study that was done at that point in time to calculate specific temperature profiles to support those preliminary findings. Now, this particular study looked at the maximum temperature reached as a function of the distance from the repository. It used a single rectangular panel, comprising about 1260 acres, at 57 kW/acre, and a 390 meter depth of repository, which is the unsaturated zone horizon, and the thermal properties that were used were chosen to simulate the detailed thermal stratigraphy that was used in the unit evaluation study. I'll apologize again. I think it's the next thing I'm going to talk about after this, so rather than do the full, detailed thermal analyses that were done in the unit evaluation study, this particular study tried to just use a simpler model that would simulate that detailed stratigraphy.

The two plots that I wanted to show you are on the next page. The first one is a vertical temperature profile downward through the center of the panel that was used in the modeling, and you can see no discernible temperature effect at the surface. This is a 500-year contour. This is the maximum temperature that would be experienced by a point, so these points physically would have different times associated with them, but this is the maximum temperature that a point below the repository or above the repository would experience.

We can spend a bit of time on this. You can see temperature changes. The repository horizon is about the 400 meter level, and the ambient would be something about in there, so you can see temperature changes of 100 meters below the repository on the order of 30 to 35, maybe 40°C at that maximum time frame.

DR. DOMENICO: Excuse me; Domenico.

Is the 57 kW/acre still part of the program? That's what that profile was based on right there.

DR. VOEGELE: This is 1984-1985. This is 57 kW/acre, right.

DR. DOMENICO: Is that still--

DR. VOEGELE: That is still the baseline. We have not changed the repository baseline. You're going to hear several presentations, Tom's presentation, Eric Ryder's presentation--Tom Buscheck's presentation, where we're talking about the different APD's that are being looked at, both higher and lower, I believe, to see what the impacts are. But if there is a baseline in the program, it is the repository conceptual design that supports the SCP. In fact, that is a baseline, I should say.

DR. DOMENICO: Well, I'm confused because I see maximum temperature of 100°C here, whereas you project 220°C temperature, using the later models, and with the same thermal loading. What has changed?

DR. VOEGELE: I think you might be looking at a borehole wall temperature. I don't know which figure you're looking at.

DR. DOMENICO: Right at the canister.

DR. VOEGELE: Right at the canister, right. This is not looking at that level of detail, the near-field. This is basically a far-field model, so I think the thermal loading is probably more smoothed out over here than it would be for the point loading along a canister.

Okay, and then here's temperature contours around the repository horizon. It's pretty graphic of what those are, and, excuse me, this is a different profile, obviously. This was a vertical profile through the center of the repository. This is a profile from the center of the repository out to the edge, and then a slice through that. So you can see the little line right there. That's where the profile is. That's why this doesn't start at zero. It starts somewhere in the middle of that configuration. I can just emphasize, again, these are maximum temperature contours. That is not a point in time or space. That is a plot of the maximum temperature that a particular point would be subjected to. This was a calculation that was done specifically to support the environmental assessment, to update and to assist some of the other calculations that we had in the program at that time.

All right. Well, I said I would talk about the final environmental assessment. The comment I made earlier about the program might state something differently is, in fact, this particular comment, and the particular question was the post-closure geochemistry guideline. It was a favorable condition, and the concern was whether or not mineral assemblages, when subjected to repository conditions, would retain or have equal or increased retardation capability. The question right there is a simple reflection of whether or not the system that you impose over the natural barriers actually changes the ability of those natural barriers to perform, and the position that was taken at that point in time was that most of the sorptive zeolites in the system are more than 300 meters below the repository, where the maximum induced temperature is 60°C, which is a change in temperature of 23°C, and it was unlikely that significant zeolite decomposition would take place over 100,000 years.

I'm not sure the program would make the statement that most of the sorptive zeolites are more than 300 meters below the repository. That's the part that I was suggesting might be said differently. I don't believe the program will take a different position on the impact of the temperature, the thermal loading on the mineral dehydration, but that remains to be seen.

Okay. There were several questions in the rock characteristics guideline of 10 CFR 960 that were assessed in the environmental assessment. One of them had to do with high thermal conductivity, a low coefficient of thermal expansion, or sufficient ductility to seal fractures. We concluded that that particular characteristic was present at the Yucca Mountain site. We have a low thermal expansion coefficient, and everything that we could calculate about the behavior suggested we would not have an adverse response to putting a repository at Yucca Mountain.

There were questions related to rock conditions requiring engineering measures beyond reasonably available technology to ensure waste containment or isolation. We could not identify any conditions that would require anything other than just ordinary measures to operate a facility at that site.

The question of thermally induced fractures, hydration or dehydration of mineral components, brine migration, or other physical, chemical, or radiation-related phenomena that could be expected to affect waste containment or isolation, the calculations that we had done at that point in time suggested that the site would remain physically and chemically stable. We could not identify impacts of thermally induced fracturing that would be significant.

Finally, the last one of the post-closure rock characteristic guidelines was a potentially adverse condition dealing with a combination of geologic structure, geochemical and thermal properties, hydrologic conditions so that heat could significantly decrease the isolation, and our modeling would indicate that the properties and conditions at the site would not be expected to cause a decrease in the isolation capability of the site when the heat was imposed on that site.

Okay. The unit evaluation study. This is the document that was completed to support the horizon selection, and in that study, thermomechanical evaluations were undertaken to confirm that none of the technical constraints were violated, particularly the mineral hydration/dehydration limit that they were using in this study was set at 150°C, which may be higher than we would be using today. They did develop numerous plots of temperature history at the far-field boundary, and once again, the maximum gross thermal loading of 57 kW/acre was needed to meet the operational constraint of the drift floor temperature, as well as the backfill if it had been used, a temperature of less than 100°C.

There are two figures in the report. Again, these are the preliminary technical constraints that were used in the unit evaluation report. This is the near-field set of constraints that were used, and with respect to system components, they looked at the room itself, environment, floor and backfill, pillar safety factors, mineral dehydration/alteration, and the engineered system. This is the near-field component, and these are very comparable to what you've seen in the charts I showed you earlier. In fact, I don't think that any of them have changed. There's those temperatures, 100°C temperatures that we're concerned about in the floor and the backfill; same safety factor in the pillar; mineral dehydration/alteration limit of 150°C during that operational period, and once again, I am unable to tell you why it was only a constraint in the operational period. And the engineered system, no releases.

And then there's a comparable chart that they were using for the far-field technical constraints, and we've been through those. I don't believe any of them have changed at all. Far-field, mineral dehydration/alteration limit of 150°C, same natural analog, 6°C, travel time number that we were looking at. These numbers here, the permeability, the creation of no new fractures, and that mineral dehydration/alteration number were intended to apply to this 70 per cent region, which is consistent with what the program had evolved through the NWTS-25 series.

Okay. I can give you a little bit of information of the model that was used to create the figures we're going to look at in a moment. I think it's mostly self-explanatory, with the exception of this issue of atmospheric boiling and hydrostatic boiling. With the atmospheric boiling, the energy involved in that boiling taking place was removed from the system; whereas, that was not done under the hydrostatic boiling situation. The energy was left in the system. It had a convective boundary at the surface, reasonable heat flux, reasonable boundaries, and so forth, and we were in the unsaturated zone.

So let's look at some of the pictures, some of the results of that study. All four of the units are on some of these plots. Topopah Springs is a solid line. Calico Hills is the dashed line, and that's the non-welded unit. The Bullfrog and the Tram, successively deeper, and this is the 15 per cent boundary below the potential repository horizon, so that's this point right here, and that's where they were looking at the thermal alteration concerns, and this is a period of 100,000 years, and here are the temperatures that were induced, or the total temperatures in the system at that boundary over that period of time.

The solid line is the one of most interest. That's the Topopah Spring. The Tram follows it very closely in that time frame, although it tapers off. The temperatures remain higher for a longer period of time. Significantly, the number that was calculated in that study is about 80°; a 1,000-year time frame. Okay. Those are using the average properties of the system.

Using limit properties of the system, trying to force the system to give it its worst response, if you will, is shown on the other figure. Temperatures are higher, but five degrees, five or six degrees higher; a little more separation. Again, that's this boundary right here, below the potential repository horizon, so we're at 390 meters, .15 would be about 60 meters, I think, 60 meters below the repository horizon. We're looking at that plot of temperatures.

Okay. The next plot is just the Topopah Spring information at the 15 per cent boundary below the repository horizon, and it has both the average properties and limit properties. That's on there for comparison to this figure, which is the 85 per cent boundary below the potential repository horizon, this number down here, and you can see the numbers go--very little change. This is the Topopah, the solid line. It starts at a lower temperature. You're out in the 1,000-year time frame before you start getting temperature effects at that boundary. It goes up, and the number, I believe, here is about 78 or something like that. For the Topopah Spring, it's less than 70.

DR. DOMENICO: I have one question. Maybe you can't answer it, but I'd like to get it on record so that perhaps someone later can answer it. It has to do with your conceptual model.

You're treating this whole thing as isotropic. Do you consider conduction alone, conduction with convection, or maybe even perhaps a vapor phase, or do you consider it in layers, and did you consider the fractures at all? In other words, what went into the model?

DR. VOEGELE: My recollection, it's a relatively simply conductive model. I don't know if Tom Blejwas can add any more to it than that. We can certainly get you a better answer to that question.

DR. DOMENICO: Well, I just brought it up so when someone addresses it...

MR. BRANDSHAUG: Terji Brandshaug. I was working for Wiesbak (phonetic) at the time and I did these calculations here. It's a simple conduction model. We did differentiate between the different layers, obviously, for the thermal properties, and we did model the convective boundary, but we did not take account of fractures or any convective heat transfer in the matrix itself.

DR. REITER: Just a quick question, Mike. In all these four models, where's the saturated zone with respect to the 85 per cent boundary below the repository?

DR. VOEGELE: The depth, when you're in the Topopah, you are maybe 900 to 1200 feet above the water table. When you're in the Calico, you are on the south end of the block. You still could be five or six hundred feet, easily, above the water table, but it would be much closer at the near surface. The Bullfrog is about as far below the water table as the Calico is above it, and the Tram is below that. I have a figure. In fact, why don't I just do that?

This is the second or third to the last figure in your package, if you want to see it closely. It's very difficult to read, but this is the Calico Hills here, and there's the Bullfrog, and there's the Tram down there.

DR. REITER: So these four sites, these four horizons are both unsaturated and saturated?

DR. VOEGELE: Yes. This was looking at saturated and unsaturated horizons, yes.

DR. DEERE: I don't quite understand that last one you had over here where you, at the same time, had the Topopah Springs on the right-hand diagram there, and you also had it at 85 per cent boundary below the potential repository horizons.

DR. VOEGELE: Okay, here's what I was doing. I took all of the information at the 15 per cent boundary below the repository horizon for all four. Now, these are not absolute depths. These are temperatures, so that basically, the 15 per cent boundary of the Tram is different from the 15 per cent boundary of the Bullfrog, and so forth. These are, well, different in places within the section. Okay, this is for the given repository horizon itself, okay, and this is also a factor, because as you get deeper, the 15 per cent boundary moves away from the repository horizon. So if you're at 400 meters here, this is a 60 meter boundary. If you're at 800 meters, it's 120 meters away from it, so that's also an impact.

Now, this is all four horizons, 15 per cent boundary, with the average properties. This is all four horizons, the 15 per cent boundary with the limit properties. Okay, that's just to show you a comparison of what the temperature profiles are, and I guess I probably should have pointed this out, but the reason that, for example, these numbers can separate--the Bullfrog specifically, down here--is because that's a thicker distance away from the repository horizon as well, although you can see for the Tram, it does not separate farther away from it as you get deeper, and that's, in fact, the deepest one of the bunch, so you would have expected that if all the thermal properties were the same, to have been the farthest one away from this one. So there are differences in the thermal properties as well.

DR. DEERE: So what we're interested in here, essentially, is just the solid line of Topopah Springs. These are three other repository, potential repository horizons?

DR. VOEGELE: That's what this study was for. This study was to look at the four repository horizons under consideration and select one for continued consideration, okay, and so that's why I showed you all four of them in this one.

DR. DEERE: Yeah; okay.

DR. VOEGELE: And then when I went to this page, I only brought forward the two figures from the previous page related to the boundary for the Topopah Spring horizon, just so you'd have that as a reference for the Topopah, because that was the eventual answer, that we should go in the Topopah Springs. This, however, is the 85 per cent boundary for all four of the repository horizons. Again, it's for consideration.

Okay. Now, going on to the next one, bringing forward only the 85 per cent boundary, which is this number down here below, and I only brought that forward as a reference because the other one we're going to look at is actually above this 85 per cent boundary, just so you can see, you know, what they look like. This is the one three meters below the ground surface, okay, and this is only for the Topopah Spring horizon, and the maximum temperature change at a depth three meters below the ground horizon, with this model, was six-tenths of a degree Centigrade at 3,000 years, comparable to the numbers we heard this morning.

Okay, and the final one I wanted to show you was the uplift. Again, this is all four evaluations. The one of particular interest is the Topopah Spring, and this is at a time of about 3,000 years. We have an uplift of about 35 centimeters for a repository depth of about 400 meters.

You can see, not surprisingly, I guess, that when you put a comparable amount of energy into the system, you get comparable amounts of uplift at the ground surface, even though the repository horizon would be deeper in the other units.

Okay. Well, that was the study that was done to select the Topopah Spring, that resulted in the selection of the Topopah Spring, and as I mentioned earlier, the actual selection of the program's concentration on the Topopah Spring horizon was made before this document was published, but it was with knowledge of the results of this study that that selection was made.

All right. There are a couple more things I need to talk about. This is one that I've talked about before with the Board, or actually, with the sub-panel, and that's a document that addresses the area available for a potential repository at Yucca Mountain, and that study is based upon an area requirement of 1520 acres, based upon 57 kW/acre APD. Now, that number--if you remember the slide I put up for the EA calculations, it said we needed 1260 acres for 57 kW/acre loading to emplace the waste. This particular study addressed uncertainties in the fracture orientations. If you think of the bounding-type fractures on the site, we're not very certain about what they're going to do at depth, and so there was more conservatism put into this particular study to account for the potential to have to stand off from fractures within the block mass, rock mass block, or the fracture orientations might be different from what we think they might be.

There was a very heavy caveat, knowing how sensitive that was to other conceptual design changes, and how sensitive it actually is with respect to the actual site information as well. That primary area that came out of that study is 2200 acres. That's this Area No. 1. Now, the various expansion areas, potential expansion areas are categorized with respect to how similar they are to the material within this central area, which we felt we understood the best, and the primary expansion areas are this area up in here, and this area down in here, and the conclusion was there's probably expansion area comparable in size and properties to that primary area of 2200 acres out there. We needed to go out and do some site characterization information to substantiate that conclusion.

So the question of how limited the mountain is, I think that I've given you three numbers. We probably need something on the order of 1260 acres of what we would call the best rock to put this 57 kW/acre in. If you account for the kinds of uncertainties that we think we have, it's probably about 1520 acres that you need for 70,000 tons. This block has 2200 acres, and we believe there's probably something like 2,000 acres of material that's comparable in these properties off in this direction and down in this direction. That's a 1984 conclusion, and we've done no deep drilling since then to substantiate or refute that conclusion.

MR. GERTZ: Not next November, either.

(Laughter.)

DR. VOEGELE: Okay. The final thing I wanted to talk about is the NRC generic technical position on the extent of the disturbed zone, which came out in 1986, and addressed four factors of concern having to do with stress redistribution, construction and excavation effects, thermomechanical effects on the rock, and thermochemical effects on the rock. They look at the effects of putting a repository system into a rock mass, and remember, this is a generic technical position, and their conclusion was five diameters away from the opening is a reasonably conservative estimate, but that's very heavily caveated. They're not giving you an out on that. You have to do a site specific demonstration of what that number is.

The disturbed zone is important not so much for the total system performance, but as a starting calculation point for the pre-waste emplacement groundwater travel time, which is a particular barrier requirement that we have. The 1,000-year pre-waste emplacement groundwater travel time calculation starts from something, a point called the extent of the disturbed zone, and to determine that you have to look at stress redistribution, construction and excavation effects, thermomechanical effects, and thermochemical effects.

We looked at that question in 1987, and this is a very preliminary study, and in our study we looked at it with respect to the volume of rock where there would be significant changes in the flow of groundwater resulting from those four items that we just talked about.

DR. DOMENICO: Five diameters of what?

DR. VOEGELE: The excavation opening. So if you had a five meter diameter emplacement drift underground, five diameters away from that was judged by the NRC staff to be a reasonably conservative number.

Okay. So we approached the impact of these four factors in the context of looking for the volume of rock where there were significant changes in the flow of groundwater; and again, let me emphasize, this is a very preliminary study, but we looked at these hydrochemical, thermochemical, and so forth, effects. The units with the large amounts of clay and zeolites, we concluded were far enough away to ensure that the temperatures remained below these values at which changes in their hydrologic properties might occur. Again, that is a specific uncertainty in our program which, in fact, you'll hear somebody talk about tomorrow.

They looked at silica dissolution and deposition. They looked at temperature effects on permeability through the creation of fractures, and they concluded that the disturbed zone extent was probably less than ten meters, which would be roughly two diameters as opposed to five diameters. Again, it's a very preliminary estimate of the situation, and I only brought it up to let you know that we were looking at the same kinds of questions that you're talking about uncertainties tomorrow, in some of the early stages of our design activities.

Okay. Then this is going to wrap it up pretty quickly, actually. I don't feel like I was racing; pretty much on time.

Our basis for the site characterization plan conceptual repository design, which Tom's going to talk a little bit more about following me, was the equivalent energy density through 2,000 years of ten-year-old average burnup spent fuel emplaced at 57 kW/acre. The numbers we were looking at were borehole wall temperatures less than 275°C, and that was to ensure that the waste temperature was less than 350°C.

There were temperature limits set in certain of the barriers; 200°C one meter from the borehole wall, less than 115°C in the Calico Hills, less than 115°C in the TWs3. Those two limits were derived from zeolite, glass, and clay alteration below the repository horizon, and the potential disturbed zone boundary, and I think those are very important numbers with respect to the uncertainties that the Board has asked the Project to talk with them about, so let me just wrap--

DR. DEERE: Before you move that, isn't this the first time you've talked about 275 and 350°?

DR. VOEGELE: With respect to the other criteria constraints that I was developing, yes. We had a center line temperature for waste in the site specific documents. I believe it was--we had only looked at it in the operational period for the cladding temperatures and the commercial high-level waste fuel at that point in time. So we had not set constraints at that time on the containment period and the isolation period.

I think Eric and Tom will probably be addressing comparable material during their talks, and you'll want to discuss that question with them at the same time. It doesn't look like Tom wants to talk about it. Where's Eric? Eric looks like he wants to talk about it.

Okay. So, as I said, those were the numbers that we've shown you before as the basis for our site characterization plan design.

DR. DOMENICO: Again, you probably can't answer this now, but these are model predictions, what you have at the temperatures, limited and selected barriers determined by some model, and when someone talks about this a little bit later, I'd like to know a little bit about the guts of that model in terms of, again, what was considered properties and what processes, and whether or not a double defractured media as opposed to a porous media.

DR. VOEGELE: All right.

DR. DOMENICO: But that's not necessary right now, Mike.

DR. VOEGELE: Okay. Let me identify some people for you. Certainly, Eric Ryder and Tom Buscheck will be putting up the results of model calculations, where you'll be able to see some of these thermal profiles. You have somebody on tomorrow afternoon to talk about the near-field geochemistry, as well as the far-field mineralogy, so there'll be people who will entertain, I think, the kinds of questions that are in all of our minds with respect to how reasonable are these numbers with what we know today.

Okay. Well, there's the famous picture of the repository. The Structural Geology and Geoengineering group had three hours of me showing you these kinds of pictures, so I decided we wouldn't take our time on that.

Let me show you just a much simpler version of the picture that's in your view graph in your package. Yours is labeled with all the units, and so forth. I just wanted to highlight a couple of things; the Topopah Spring horizon, the potential repository horizon, the Calico Hills, and the water table. I think those are some of the relevant aspects of the site that will come up over the next couple of days.

Let me just have a couple of concluding remarks, and then if you want, I can try to answer questions or we can move right on. I think what we've tried to show you this afternoon is that thermal impacts on the site and induced changes have been addressed in both the requirements that we set out before our studies, and the design studies themselves. I think the conceptual repository design that we have, it appears to be consistent with these program concepts that were developed early in the program and that have matured through the program.

I think the point I would like to close with is, once again, reiterate that the focus, what I would like to see the focus of our discussions over the next couple of days be, would be on these constraints. How do you set better constraints than we have in the program today so that we can derive the design products to meet those constraints?

So, Dr. Deere, unless you want to entertain questions, I'll...

DR. DEERE: I think it might be appropriate to go ahead and entertain some questions because you've finished with your presentation, and we're ahead of schedule by twenty minutes.

DR. VOEGELE: Right. Carl gave me ten minutes, and I gave you twenty minutes.

DR. DEERE: Board?

(No audible response.)

DR. VOEGELE: Maybe I did go too fast.

MR. GERTZ: Mike, excuse me, I'd just like you to verify when you were going through the areas that might be potential repository zones, if you add them all together, you have something like 4,200 acres of potential repository disposal area.

DR. VOEGELE: That is consistent with the report that I was referencing, the 1984 report. Let me try to--I've lost the figure.

The problems that you run into around here are you can't go very much farther in this direction before you run up against a 200 meter disqualifier that's in the DOE guidelines; likewise, up in this area. There's some of the same stuff over in here. There's a significant offset between the Topopah Springs as you go in this direction.

Let me try to remind you about some of the reasons these boundaries are drawn here. This was believed to be a structural feature, Drill Hole Wash Fault. Any geologists in the room can feel free to correct me, but I don't think they've found it yet and they've drilled a lot of holes in that area.

The geology that constrains this area over here and down in here are the imbricate fault zones that we've talked with you about before, and I guess I've probably been on record before at least raising the question of whether or not all those faults are really there. I think there was a combination of indications of potential faulting in this region. I think there was some license on the part of the people drawing the cross-sections early in the program, that pieces fit together the way they thought they should fit together better if there were some faults in this region. There is evidence of faulting, but I don't there is evidence of as widespread faulting as we've often portrayed in this part of the block.

This is a major structural feature, and of course, the Ghost Dance Fault has the potential to be a significant structural feature. Now, those are the kinds of things that really set these boundaries that we're talking about in the first place, where we decided to focus our effort on this central block. I think the program has moved away from the original concept of this being such a structurally bounded block, but we still have this drawing in the program, and when the study was done by Sandia, the conclusions were basically that it's reasonable to expect that some of this material up in this region and some of this material down in this region are comparable to the material that's inside this block, and that was based upon information that was seven or eight years newer than the original information that was put together when these boundaries were drawn on paper.

Now, adding up the information--I believe I'm correct, and I can check this for you--I believe that the statement in that Sandia report is, when you consider this area down here and this area up in here, there's an area that's comparable in size to the primary area, and this is 2200 acres and this is something approaching 2,000 acres, so I think what that report concludes--and I will check it for the Board--is that there's about 4,000 acres out there that's about the same quality of rock.

DR. DOMENICO: Mike, when was that diagram constructed?

DR. VOEGELE: 1984.

DR. DOMENICO: All the way back to '84 you were cognizant of the fact that you had more space than you thought you had in the main block?

MR. GERTZ: I'll answer that question. Yes. Mike, you might try the current conceptual design on there, too.

DR. VOEGELE: Yes. Did anybody bring a pen? Let me answer Dr. Domenico's question. There was a real effort on the part of the Department of Energy to be conservative in the environmental assessment, and basically, there was enough--the program had made, in the 1977-1978 time frame, had made an issue of these--they called this the central block, okay, and basically, this was a block-bounding fault, and these were block-bounding faults. So this was a block-bounding fault, and that's where that idea of this limited block came from.

And as I said, as the program moved forward, I think that there was much less emphasis on this being a bounding fault, and these being bounding faults, and I think there's still uncertainty about how significant the structure out here is and the structure out here is with respect to the performance of that site, but I can tell you with 100 per cent certainty that there was at least one person working on that environmental assessment who was just adamant that we should not conclude that this was the limit of our area. That was moi.

Okay. Carl asked me to draw, roughly, the shape of the current repository itself. It fits roughly in that area, maybe comes down just a little bit more. I think I probably cut it off.

DR. DOMENICO: Which way do the rocks dip; to the west?

DR. VOEGELE: To the east.

DR. DOMENICO: To the east? You sure you've got an unsaturated Topopah over that way, or you don't know yet?

DR. VOEGELE: I think the program is sure we have unsaturated Topopah over that way, but the water comes up higher in the Calico on that. The water table is also dipping slightly to the east, not nearly as much as the rock strata, but remember, I think we're 1300 feet above the water table back here; 680 over here.

DR. PRICE: Did I understand on the unit evaluation report that it's really confounded because you use 85 per cent boundary or 15 per cent boundary, which actual distance varies by depth, according to these comparisons?

DR. VOEGELE: I guess I wouldn't have used the word "confounded."

DR. PRICE: Is the assumption that the difference in distance is not a significant contributor toward the differences found?

DR. VOEGELE: Yeah. I don't believe the difference in the distances of that 15 per cent boundary was at all significant in the selection of the Topopah Spring. There were a number of criteria identified in that report, and I didn't go into anything but the thermal ones with respect to this presentation, but basically, a number of criteria were identified, and they went through and evaluated each of these repository horizons against those criteria, and the Topopah was clearly superior. Well, it was superior.

DR. DEERE: Staff? Bill?

DR. BARNARD: Carl, in your presentation, you mentioned that the National Academy of Sciences had made several statements regarding the thermal loading of geological repositories. As I recall, the USGS came out with some pretty significant papers in the late seventies and early eighties that mentioned something about thermal loading. Can you review those conclusions or opinions?

MR. GERTZ: I can't, but I hope Mike can. Maybe Gene Roseboom can.

DR. VOEGELE: Yeah. In fact, you have one of the authors of some of those documents sitting in the audience. Rather than--did he leave?

(Laughter.)

DR. VOEGELE: This is not fair. Gene Roseboom was in the audience this morning. There he is.

Gene, it would probably be more appropriate for you to answer that question. While he's walking up here I'll basically state that there were interagency groups set up in the 1977 time frame.

MR. ROSEBOOM: I don't recall any specific effort on the part of the USGS to look at the thermal aspects of the problem, but of course, we were strongly interested in loading the repository in the unsaturated zone, starting with Ike Winograd's first paper on Item 74, and we made a major effort to push that in early 1982.

DR. VOEGELE: Probably worth mentioning, GS was a major proponent in tuff as a medium for the repository as well.

MR. McFARLAND: I'd ask a clarification on that. If I recall Ike Winograd's most recent paper, wasn't the recommendation for a below boiling, a 100°C limit repository in the unsaturated tuff?

MR. ROSEBOOM: I believe that in our thinking with regard to the unsaturated zone, we generally favored the idea of a low temperature for a repository. For instance, in my 1983 paper, I was discussing the potential for a very extended period of retrieval, and of course, keeping low temperatures and following along with that idea, you can make use of the unsaturated zone in that regard.

MR. McFARLAND: One other question to Mike. I wonder if you could clarify the operational constraint of 100°C with "handling backfill," I think you said.

DR. VOEGELE: Yeah. Let me see if I can find that diagram or that chart that we had up there. There were two aspects on it. One of them was the drift floor temperature, and the other one was the backfill temperature, and the reason the backfill temperature would be of concern was specifically in case we had to go back in and initiate retrieval.

MR. McFARLAND: Initiate retrieval after backfill?

DR. VOEGELE: See, many of the program concepts in that time frame were to backfill the rooms as you emplaced.

MR. GERTZ: The concept, of course, now is not to backfill the rooms and leave them open for 50 years for ease of retrieval.

MR. McFARLAND: Thank you, Mike.

DR. CORDING: Mike, is there currently now 98° for the Calico Hills boundary? I've seen those numbers below boiling. Is that for the non-welded tuffs?

DR. VOEGELE: I am not aware of the baseline documents having been changed. I think there are certainly questions where people are asking, should that number be changed, but I'm not aware that we have changed that number. Certainly, Dave Bish is going to talk about that tomorrow, those kinds of concerns. That'd be a very appropriate question for him.

DR. REITER: I'd like to ask a question to you, Mike, and you, Carl, and I'd like to tell you why I'm asking it. I guess the question is first to you: In all your going back over the program history, did you ever come across any concern about the effects of thermal properties on Carbon-14 generation? And Carl, the question to you is, are you going to talk about this during the next few days?

And the reason I'm saying that essentially grew out of a meeting several weeks ago, the EPRI meeting, which we heard several things. One, as we all know, there are calculations that show that there is some chance of exceeding the EPA limits with the Carbon-14 generation. The second aspect we learned last week was that there certainly is no overwhelming response to change the Carbon-14 value in the table, since contrary to what some people had thought, it was not determined by a liquid pathway; and third, some calculations that we saw done by Ben Ross, which shows that the effect of temperature upon Carbon-14 is to quicken the travel time to the surface.

DR. VOEGELE: I can answer the first one. No.

Leon, I have not. I really put my focus more on the older programmatic documents to bring us up to the repository design. If there is a programmatic document that addresses the effect of temperature on Carbon-14, I think maybe the Livermore people would know; maybe Les or... It does not look like we have such a document. At least nobody can remember having seen one.

MR. GERTZ: And Leon, in putting this presentation together, over the next two and a half days, we have not at all focused on that issue, on Carbon-14 and effects on heat.

DR. DEERE: Are there questions from the audience?

MR. BLANCHARD: If I might, this is Max Blanchard from the Department of Energy.

I'd like to make a couple of observations relative to the history here. One is something that didn't come out in Mike's briefing this morning when he discussed the history and the importance of the unit evaluation report was a letter that was written by the U.S. Geological Survey, their management, to Don Vieth, the project manager of the NNWSI program at the time, which provided the geologic and hydrologic basis for the Department of Energy shifting from the unsaturated zone at Yucca Mountain to the saturated zone at Yucca Mountain. That was an official letter with a relatively thick technical analysis that supported that, and that, also, was factored in at the same the Sandia National Lab's report came out on the unit evaluation, and fortunately, we have, if you care to discuss with Gene, one of the authors of the information that went into that report here with us, so if you want to discuss that sometime in the future, you can.

The second thing that I'd like to point out is there is some discussion or confusion about some of the acronyms that were used in the early 1980's, and I'd just like to clear that up in that the NWTS stands for the Nuclear Waste Terminal Storage Program, which was not an ERDA program. It was a DOE program. It preceded the passage of the Nuclear Waste Policy Act.

And ONWI, whenever you see references for ONWI, that was Office of Nuclear Waste Isolation, which was the integrating contractor for the NWTS program for the salt work, and that was at Battelle Memorial Institute in Columbus. And there was a period during those days when ONWI became the integrator for the entire program, all the sites. So some of those ONWI sites, they were all contractor reports. The DOE officially sanctioned reports were the NWTS reports.

DR. DEERE: Yes. Thank you very much, Max.

Gene, would you like to offer any comments at the present time?

MR. ROSEBOOM: I would just like, while we're looking at the history of the--we had two major concerns. Ike Winograd, of course, was the person who proposed the unsaturated zone, and at the time we were putting it forward, we had two major concerns. One was, of course, the difficulty of doing hydrologic modeling in the unsaturated zone, because aside from soil studies, it was a practically unknown territory; and the other was that we felt at the time, also, that the NRC and EPA regulations having been developed for model repositories in the saturated zone, that there would have to be a complete, new look at the regulations because working in the unsaturated zone, you're dealing with such a very different environment, and many of the rules are changed completely.

However, the NRC looked at the problem in 1983, and was able to arrive at a solution continuing the same regulations.

DR. DEERE: Thank you.

Are there other questions or comments?

MR. McLEOD: My name is Barrie McLeod. I'm with the M&O from E.R. Johnson Associates.

You had two temperatures in your conceptual repository design site characterization plan, one of which was your borehole wall temperature of 275° that you said was to ensure waste temperatures less than 350, and the second one was your one meter from the borehole wall. Unless I missed it, you did not show any history for those temperatures, or any particular basis for those.

Is the 275 relative to the waste temperature 350, is that purely related to waste temperature, or does it have some other rock?

DR. VOEGELE: No. My understanding is the 275 is purely set to keep the waste center line temperatures below 350°C.

MR. McLEOD: In other words, you could have a pure copper package there and get it up to very close to 350, and that was with no delta T across the--

DR. VOEGELE: I hadn't thought about it in that terms.

MR. McLEOD: In other words, it's dependent on waste packages. Okay, that was really the question. It's not a rock temperature.

DR. VOEGELE: No, it's not a rock temperature, but it's a waste package.

MR. McLEOD: The 200° limit one meter from the borehole wall, what is that determined by?

DR. VOEGELE: Do you remember, Tom, specifically?

DR. BLEJWAS: Yeah. The one meter from the borehole wall deals with the potential for significant spalling of the rock around the borehole. It was to limit that, since we had done testing in that temperature range to ensure that we wouldn't have problems.

DR. DEERE: Good questions and good answers.

Are there more?

MR. JOHNSON: I'm Cady Johnson with Woodward-Clyde M&O, and in the hopes that this group could make some progress towards settling on a usable disturbed zone definition, I'd just like to point out that in the NRC's general technical position that you referenced, they explain that the intent of the concept was to try and avoid the near-field region of difficult to model processes, and if, on the back of an envelope, you can look up, it takes an average liquid water contents of Topopah Spring tuff, and take 100-year isotherm and 100 years, and look at the amount of water that should be boiled from that rock, you end up with something on the order of an acre foot of water per acre of cross-sectional area of the heated rock.

And so, at that, you know, the point I'd like to make is that if that region of difficult to model processes is taken literally and you moved that amount of water into the known very small fractured volume, you'd have a fairly large volume of difficult to model processes. And so we need--my point, then, is we need a workable definition of that disturbed zone for the travel time calculations, but it's very elusive. What you might get from a look at the thermomechanical properties, and excavation effects and things like that is quite restricted with uncertainty of-- special uncertainty on the scale of the repository it's nothing that much to worry about, but if you look at it in terms of our uncertainty of the extent of, say, fugitive vapors, you know, the migration of water vapor that's been boiled from that block of rock, it could become quite large.

And so, you know, unless the criteria for calculating that travel time were changed, we'll need to settle on some kind of a definition for that, and I'm not sure myself why the draft generic position hasn't been refined and fixed by some sort of consensus that it would be an extremely useful contribution if we could settle on a, you know, workable definition of disturbed zone to suit the people that need it for a groundwater travel time calculation.

DR. VOEGELE: You can't point at me.

MR. JOHNSON: That's been my difficulty. I wouldn't know where to begin the travel time calculation on a defensible, you know, from a really defensible starting point.

DR. VOEGELE: If that was a question, I can basically say that from my involvement in the program, everything I would see would suggest that the DOE and the NRC have basically initiated communication to try to resolve some of these differences, but I think you have to be careful about your basic premise about the difficulty of modeling. I don't think that's something that's part of the definition of the disturbed zone. It may make it difficult to evaluate the extent of the disturbed zone, but I don't think you can call difficulty in modeling a parameter that defines a disturbed zone.

MR. JOHNSON: I'll agree. I just wanted to point out that that was the intent. That was the stated intent of the NRC taking a position on the definition of disturbed zone.

DR. VOEGELE: Okay.

MR. NATARAJA: This is Mysore Nataraja. I'm from the NRC.

He's right. That was one of the intentions of the GDP, but actually, the people who worked on the GDP were mostly rock mechanics people, so they did what they would understand best, and now it has been taken over by the hydrologists, and they say they're going to revise it, so hopefully you should wait for the waste version of the GDP.

DR. DEERE: Thank you.

I'll just answer that. I think Larry Ramspott said this morning you've got fractured rock, and a few more fractures here or there doesn't make very much difference as far as he's concerned in the disturbed zone.

MR. SMITH: Jay Smith with the Edison Electric Institute.

You mentioned some amount of defense waste as part of the repository thermal load. Can you say anything more about the magnitude and temporal aspects of the thermal contribution of that waste?

DR. VOEGELE: I think that's an Eric Ryder question. He's talking yet this afternoon. Are you going over to tomorrow morning as well, or is it all this afternoon?

MR. RYDER: It depends on how many questions we get.

DR. VOEGELE: All right. We're still on time. We're doing fine.

DR. DEERE: We are, and I think I'm going to cut it off now. Oh, you're supposed to cut it off.

MR. GERTZ: That's fine with me. Whenever you're ready, Don.

DR. DEERE: Be back in fifteen minutes.

(Whereupon, a brief recess was taken.)

DR. DEERE: May we reconvene? There will be a second coffee break because our second speaker of the afternoon needs a few minutes to organize a video. So, Carl, I'll turn it back to you to introduce your next speaker.

MR. GERTZ: Okay. Thanks, Don.

Tom Blejwas is going to talk a little bit about repository design, and then, of course, Eric's going to talk later, and we'll have a break while he gets his films ready midway through his talk, I guess, Eric; right? Okay.

Tom, it's all yours.

DR. BLEJWAS: Thank you, Don; Carl.

Mike observed some hesitation when he would turn to me and say, "Well, I think Tom's going to talk about such and such," and part of the reason there was hesitation on my part is that a year ago March, I talked on this same subject and gave a lot of what I thought were details dealing with some of the design activities that had gone on prior to that time, and today, I was not intending to go back and talk much about those details, but instead, in this talk titled, "Repository Design Considerations," I'm planning on talking more about the philosophy that went into the design process leading up to what we have in the site characterization plan, and a little bit about how I see the philosophy changing in the near term, and perhaps more into the distance. So that's quite a bit different, and I will not get into too much detail with respect to the design process.

So the three areas I'm going to discuss, first of all, I'll talk a little bit about the SCP approach that we took in terms of the design relative to thermal loadings; then talk a little bit about some external forces that are leading to changes in that process, and how we might react to those, and then give you my perspective on what our plans are for the next few years.

I'm going to spend quite a bit of time with some bubble charts that look like this first one, various modifications of this, and what I'm trying to do is give you an idea of what's gone into the design process with respect to thermal loading. There were some people that wanted me to invert this figure, but I like the idea that the main topic that we're discussing, thermal loading, is on top rather than on the bottom.

If I take you back to the time period that Mike was talking about, the site characterization plan, one of the processes that we went through was performance allocation, and you may recall that the purpose of performance allocation is to start with regulations, look at the regulations, and come up with a strategy of how we're going to show compliance with those regulations, but then look at what performance things we need to do in order to assure compliance with the regulations. That leads us, then, to mechanisms that the performance models are based on, and finally, when we look at the mechanisms for performance, we developed in the SCP process constraints or temperature goals.

In the SCP, they were actually called goals. They're not constraints, but we think of them as constraints, and so we went through this process in various areas; in the geochemistry area, the hydrologic area, and during the writing of the site characterization plan, we came up with a set of temperature goals for the repository.

By that time, though, we'd already done some design and we had some idea what temperatures we expected in the repository based on the kind of information Mike was talking about, whether 57 kW/acre or whatever the loading was, and that process looked something like this. What we really needed in order to decide whether or not we were reaching our temperature goals or meeting our temperature goals were some kind of temperature profiles, and in order to get those temperature profiles, we had to know what our thermal loading was. We needed to know what site data we had. We needed to know properties in order to convert the thermal loading into some temperature profiles, and we needed to know something about the design of the repository. We looked a little bit at alternatives.

In order to know that thermal loading, there are a variety of things that feed into that. You need to know something about the waste characteristics. You need to know something about the available area. There were time constraints in that the regulations and the law required that we do things on a certain time scale, so there were time constraints, and we also made some assumptions, some design assumptions, and the ones we used in this time frame were based on the conceptual design that's in the SCP/CDR.

So we took this information, came up with a thermal loading, then from this information we could calculate temperature profiles and we could compare them with these temperature goals, a relatively straightforward design process. Now, I put the dashed line for design alternatives there, and it's dashed instead of a solid line because in this time frame we didn't look a lot at alternatives specifically aimed at thermal loading. We weren't concerned with trying to optimize the thermal loading of the repository, but as I'll show you later, we did do a little bit in terms of alternatives with respect to the thermal loading.

And just to refresh your memory, this is a view graph that I presented a year ago March to you. This is a list of the temperature goals that appear in the site characterization plan, or my interpretation of those, and Mike's gone over several of these. You can see where they came from historically, but let me just go through them quickly. We have a goal that there be 200°C or less one meter from the borehole wall; less than 275°C at the borehole wall, and that's designed to limit the center line temperature of the container to less than 350°C. We might mention, less than 6°C delta on the surface, and surface uplift of less than .5 centimeters per year. We specified there would be no intact rock failure or continuous joint slip; that the local saturation would be less than 90 per cent, and why does that relate to this temperature question? That's because it limits the area that we can use which might affect the local temperatures, because you're limiting the area that you can use in the repository.

The borehole walls, it was desirable in the SCP for the boreholes walls to be above boiling for greater than 300 years. That goes in the opposite direction of most of these, and then one that's caused a lot of controversy is the temperature being less than 115°C in what we call the TSw3, the Calico Hills zeolitized, and vitric, and that was just intended to be a refresher. I'm not going to discuss the origin of those anymore than has already been discussed today. As I mentioned, we spent quite a bit of time on those a year ago.

Now I just want to get into the kind of external impacts we're getting in the program directions. Many people have suggested that our temperature goals, in some cases, aren't conservative enough, or are questioning whether they're the right type of goals. Others are questioned, why are we continuing to use these time constraints? Shouldn't we consider allowing the waste to age much more in surface facilities before we put it underground? Others have suggested, well, there's a lot of design alternatives for a repository that you haven't considered.

So in this figure, I've cut off some of these things and I've put this line in solid to design alternatives, because that's the kind of direction that we're getting pushed in, to try to answer these questions without these assumptions, and also without necessarily having temperature goals in the process. Why can't you just look at the temperature profiles and then calculate what your mechanistic impacts would be, your performance impacts, and then show regulatory compliance?

And this process is complicated by the fact that--if I can use the other view graph here--that if you look at these design alternatives, they're coming from all different directions. A lot of alternatives need to be considered over time. For example, there is suggestions of alternative ventilation concepts, alternative layouts, emplacement modes, waste package concepts, backfill concepts. We have a whole bunch of operational approaches that needed to be considered; emplacement schedules, whether or not the waste is going to be treated in some way, co-mingling strategies, consolidation strategies, et cetera. That was not intended to be an exhaustive list, but just the kinds of things that logically need to be considered over the next several years.

Well, getting back to this figure, then, if we looked at this process now without having these types of assumptions, and looking at a lot of different design alternatives, it looks to me like a problem that just doesn't have enough constraints in it. My analogy would be in linear algebra, you've got too many variables. You don't have enough equations or enough constraints in order to actually solve your problem.

And so, in looking at this, we don't think we can approach this problem this openly. We have to come up with a more systematic approach to all these questions, and so what I see our planned approach looking like in the future is something that's more like this. You notice that this part of the figure looks the same as my first one, but I've added a few things over on the side here. Over time, we're going to have a test program that's going to give us better site data and properties, which will refine the temperature profiles we would calculate, but perhaps as important or more importantly, it will provide information on what these mechanistic impacts really are likely to be from temperature. They're also likely to provide us with validated models, which will give us higher confidence in the performance impacts.

So, with time, we should have a better idea on how to update our performance allocation, and we should be able to develop better temperature goals, and I think we need to have these temperature goals so that this process, this design process up here can be a relatively simple one in comparing temperature profiles with temperature goals.

So in the top here, what I've done is I've said, yes, we do need to look at design alternatives in the future. We're going to have to look at those a lot. We are, though, going to have to make some system assumptions, and that is we're going to have to assume that the rest of the waste storage system--dealing with transportation systems and MRS--takes on a certain nature, because if we leave this open, then we have, again, too many variables. So we will make assumptions about this in time.

And so, what I see for our plans through ACD looks like this. We're going to have to conduct mechanistic studies where they're appropriate. Now, here I'm talking about the effects of temperature on various properties on various mechanisms that deal with the performance of the repository. We understand we have to do that, and if you look at our site characterization plan, we include plans to do those, and we may have to expand those plans.

We also are going to have to update our temperature goals, and that's something we can discuss again near the end of this three-day meeting, because I think last year it was presented to you as: Well, these are the temperature goals and we're going to live with these forever. That's not our view. These were the goals that we had selected during the site characterization process. We always recognized that they would have to be updated through time, and now we see, indeed, some problems with some of those temperature goals that we had previously selected.

And in particular, I think that the kinds of changes we might look at is we might need to better consider the uncertainties in impacts and benefits, emphasizing there the word "uncertainties." Perhaps, in some cases, we weren't prudent enough in terms of early conservatism. We're relatively early in the process. We don't have a lot of site data. We perhaps should have been more conservative in some of our selections, but we have gained an improved understanding of some of the mechanisms, and you will hear about some of that tomorrow, and we have improved performance models, and you've seen some of those performance models, and others may be discussed more in the next couple of days, also, and I'm particularly thinking of models that are not just conduction models, but where we deal with two-phased flow, et cetera.

So I see us updating our temperature goals, recognizing these factors, but another thing we're going to have to do sometime soon is we're going to have to develop boundaries on our design alternatives, because as we get to the point where we're constructing an exploratory studies facility where we intend to incorporate the exploratory studies facility into our repository, we are, indeed, eliminating some of the possible design alternatives for a repository in the future; not necessarily eliminating, but at least impacting those designs, and so it's important that we establish some boundaries on what kinds of design alternatives we want to consider in the future.

And finally, I see us performing these design studies, and what would these design studies look like?

DR. CANTLON: Tom, what does the ACD acronym there stand for?

DR. BLEJWAS: Advanced conceptual design. We have the conceptual design and advanced conceptual design, and then there's a license application design.

And this is a relatively simplistic look at how I see the design studies being conducted in the future. I think it's very important that we always have some type of a baseline for these studies, and the baseline would include something like the area that we're going to use in the study for the repository; emplacement design; temperature goals; waste characteristics, et cetera. We will establish that baseline. It may not be what we end up with, but we have to start with something, and then change things systematically.

We'll combine these studies where it's appropriate. We're talking about a lot of different studies, and we're probably going to have to conserve our resources the best we can so we'll combine where that makes sense; prioritize those studies, at least looking, in part, at how the selection of sequence might affect the final outcome; perform those studies using analyses. I'm talking about now the kinds of analyses that Eric Ryder and others will be talking about over the next couple of days in terms of what the thermal impacts would be; and then revise the baseline and continue, as we revise that, to perform the next study, and so on, and iterate on a baseline design that would be acceptable for the advanced conceptual design. And throughout this, we're going to have to rely on expert judgment and decision-aiding methodology. And so, simplistically, this is the way I see us conducting our design studies in the future.

And then with respect to those design studies, the general plans are that we will update our temperature constraints or goals as input to the advanced conceptual design. We will perform the studies during advanced conceptual design, and that will lead to a detailed design during the license application design phase.

And I realize that that was a very short presentation of where we've been and where we're going, but I intentionally kept it short because I don't think we have a lot new to tell you in this area than what we told you last year.

DR. DEERE: Okay. Thank you very much, Tom.

Are there questions from the Board? Yes, Warner?

DR. NORTH: I wonder if you could go back to the view graph titled, "Planned Approach."

DR. BLEJWAS: Yes.

DR. NORTH: I'd like to ask you to describe in more detail the relation to total system performance assessment that's going to be done and, in particular, the plans for validated models and updating the calculations of performance impacts. What I'm looking for is reassurance that what you're describing here and what the risk and performance analysis panel heard from Russ Dyer and a supporting cast last spring all fits together in a nice, neat way.

DR. BLEJWAS: Yes, and, indeed, it better fit together in a nice, neat way or we have a lot of problems.

DR. NORTH: Yes. Reassure me.

DR. BLEJWAS: Actually, one of the most difficult processes we have is trying to come up with a consistent and defensible way of including performance calculations and performance assessment in the design process. It's not a trivial problem, and it perhaps should be something that the Board might want to look at more in the future, because you can learn a lot from doing total system performance assessment. You can learn a lot from that, but you cannot necessarily get into the kind of detail where you'd be looking at some of the mechanistic impacts, so when we have performance assessment supporting the design process, we quite often are not doing total system performance assessment, but we're doing performance calculations that look at a part of the system, and look at, in particular, possibilities of mechanistic impacts here. So I see that team of people doing total system performance assessment also contributing to these calculations that would lead to answers relative to the mechanistic impacts.

DR. NORTH: Well, that's my concern over the issue of what is a validated model, how do you tell. Your colleagues talk about a pyramid of models.

DR. BLEJWAS: Yes.

DR. NORTH: Having things match from the top of the pyramid down to the base is a very non-trivial exercise.

DR. BLEJWAS: Absolutely.

DR. NORTH: Some of the questions that Pat Domenico has been asking earlier today include the issue of two-phased flow, the effect of inhomogeneities in the rock, and my understanding is the work that's been done to date is a rather simple model compared to dealing with those mechanistic details, and so what I'm looking for is more of a sense of what is your plan? How are you going to bring in the detail that you need to check the calculations and make sure you're within, shall we say, reasonable bounds, and what's the time scale on which this is going to be accomplished?

DR. BLEJWAS: Well, first of all, let me say that what we--in our total system performance assessment, our plans are to perform total system performance assessments, and then based on what we learn from that process, continue to update that process in the next cycle and do another total system performance assessment again, looking at what kinds of uncertainties we have and what areas need to be addressed in more detail, and where we need improvements in our models, and to continue that cycle through time until we get through the licensing process. So that's part of the plan.

Unfortunately, given funding constraints, I can't tell you how much improvements we'll make on our total system performance model in FY92, but we will be working on improving those models and conducting the next cycle, and that's where I think it all comes together, is by constantly going through the cycle and looking in terms of where do the uncertainties exist, and how can you improve on those.

DR. NORTH: Is it a reasonable expectation on our part that within a year we should see a detailed plan for how you were going to do the next iteration, both with respect to the SCP test program, the validation of models, and your two arrows going over there to mechanistic impacts and performance impacts.

DR. BLEJWAS: Um-hum.

DR. NORTH: In other words, you know, give me the plan within that period of time that shows just what will be done in these areas. Obviously, it has to be conditioned on the amount of funding available to do this effort, but what I'm looking for is a sense that at some point in the near future we are going to have a much more detailed plan than the sketch you have up on this slide.

DR. BLEJWAS: Yes, and I agree that that's necessary. I won't speak for the Department of Energy, because I'm not an employee of the Department of Energy. I work as a contractor for them, but I will give you my insight into where we are and where we think we're going.

We expect to be able to show you a total system performance assessment that's a significant improvement over what was in the EA by the end of the year. Carl mentioned that this morning.

DR. NORTH: Yes.

DR. BLEJWAS: And, indeed, that is something that we're working vigorously on.

We also are working in a preliminary fashion on something that I'll call a road map for performance assessment, and it's supposed to be the map of where we're going in the future and how we're going to ensure ourselves that we will be able to show compliance with the regulations, or non-compliance with the regulations, when we get to a license application.

And we have developed tentative plans for initial activities in that area at Sandia, but also, we've had discussions with the M&O contractor so that their people can be guiding some of that activity for the DOE, and indeed, my understanding that that is a part of the plan for how we will integrate with the M&O contractor over this next year is to make that one of the areas that we will address in performance assessment, and if Russ Dyer's here, I guess I'd like to see if he thinks I said anything that was in error. I don't see him right now.

MR. GERTZ: Russ was here a little earlier, and I'm sure he'll be back and forth through the couple days, and you may want to get him, on the record or off the record, to address that.

DR. NORTH: Would you like to put anything on the record in respect to these points, Carl?

MR. GERTZ: No. I think what you outlined is certainly a sound approach. It's the approach, I think, that we're certainly trying to implement; whether the timing will be one year or two years, whether the transition, bringing the M&O into the lead role will be accomplished in one year or two years, I don't know that I can say or that we even have detailed plans out, but certainly, the concept is one we agree with, Warner, but at the timing, I can't commit to it right now without talking to Russ or some other people.

DR. BLEJWAS: I can tell you that a part of our plans, within the limits of what we presently see for the funding for FY92, is, indeed, our own version of that plan. Now, how much that'll be coordinated with the rest of the DOE community, I can't commit to. But we do have that as a part of our plan for the fiscal year '92.

DR. PRICE: Tom, could you put that slide back up?

DR. BLEJWAS: Sure, if I can find it again.

DR. PRICE: In the upper right-hand corner, you've got "System Assumptions."

DR. BLEJWAS: Yes.

DR. PRICE: Everything we've been talking about, and perhaps will be talking about during this period of time, has some effect on the utilities, has some effect on the MRS, has some effect on transportation, has some effect on the receiving facilities at Yucca Mountain, has some effect on maybe some of the objectives for safety, for minimization of handling, for life cycle costs.

To what extent will the assumptions that you make here drive the system and become more than assumptions, and what assurance can we have that the impact of repository design considerations you're making right now will be properly bedded within the total system to arrive at the best possible system?

DR. BLEJWAS: Well, from my perspective, what I intended there to be system assumptions was the present plans by the Department of Energy for the entire system. I was going to get into this a little bit tomorrow in another talk, but part of what I see us doing is taking the assumptions that we presently understand within the system, you know; what is the baseline? What are those plans? And trying to clarify those before we start this process.

Going through and updating our performance allocation, looking at these factors right here and coming up with temperature goals, then based on these assumptions, calculating temperature profiles, if everything looks all right, then we will have no impact on changing those system assumptions. If, however, after we've gone through this process and we find that based on some temperature goals that we didn't anticipate previously, but now we decided are important, we find that we can't meet these temperature goals, then one of the things we have to consider is going back and trying to change the system and changing those assumptions.

DR. PRICE: But the system, as yet, you're making assumptions about the system and changing the system, but the system, as yet, is not designed.

DR. BLEJWAS: It's not designed, but we--

DR. PRICE: The concepts are not really there as yet, the basic concept work is not there, and to what extent will studies like yours with regard to repository design simply become part of the total system engineering? Or will it end up that you do a repository design that then drives the entire system?

DR. BLEJWAS: Okay. It was my presumption in this that by the time we did advanced conceptual design, we would have a fairly well-defined system. Now, there would still have to be assumptions within that system, but the intent here was that we would perform advanced conceptual design based on a system that had been put down on paper and that we could draw on the basis for that--we could look at the system analysis and use that in the advanced conceptual design. So there has been a lot of work done on looking at the system. I mean, we did an MRS study. We've looked at those things, and we've looked at ranges of things that are likely to come out of the system.

To be conservative in this process, if you don't have a well-defined system, you could take ranges of those things, and that may be something we have to do in advanced conceptual design.

MR. GERTZ: Dennis, I'd like to just clarify for the Department your question. We would hope that through the functional analysis, the systems engineering formerly headed by Dwight, but that has been started and will be carried out, will allow the repository program to then take whatever system constraints we have and drive it down, and as Tom pointed out, if those systems parameters that were given do not meet our needs, we then may say, "Gee, maybe you need to reconsider the system's requirements or system's parameters." But it's not our intent to drive the system. It's we want to take whatever the system gives us and move down with it.

Right now, it's the reference of 3,000 tons per year receipt, et cetera, and that's the references we're using. That's not been tied in as the final system, though.

DR. PRICE: We see these things going ahead; for example, this repository study and the considerations, as well they need to be going ahead with respect to thermal loading. We see considerations about the MRS because of the 1998 date that is looming, and assumptions being made, then, about what we must do to meet that 1998 date, and this drives the MRS, which may drive the repository, and these things going on somewhat right now, and so my question was really one, what assurance, as we watch these things going on--I mean, they're in progress--is this, indeed, going to happen in which we get them folded in in a timely manner--and the emphasis on timely, because I think you underlined timely a little bit ago.

MR. GERTZ: I sure did; right.

DR. PRICE: --such that the proper design selections can really be made.

MR. GERTZ: And your points are well taken. I think that's part of the reason the Department had chosen to go with a broad-based M&O contractor who has the broad-based systems-type analysis background, and the TRW team will be coming up and developing those functional analyses and systems requirements that we as a project--either myself on repository or Ron Milner on MRS--will then drive down into our solutions. But, in the meantime, we're making some assumptions and moving forward. We hope they'll be consistent, or at least within the range of assumptions that will be passed down to us.

DR. VERINK: Calling attention to that same chart in the matter of available area, I wonder, in light of some of the earlier comments today, whether or not the available area is considerably larger than has been considered in the past, and if that comes into play?

DR. BLEJWAS: I think whether or not there is a lot more available area is not only open to debate right now, but is also something that we need to look at after we've done some site characterization. Some of the factors that didn't enter too much into what Mike was talking about earlier in terms of available area were things like lithophysae content in the rock. That was one of the factors that limited the extent of the area that we chose as our primary area.

Well, we've looked at that more recently over the last couple of years from the time when we came up with that primary area, and decided that we had made some perhaps poor selections as to where the lithophysae got too dominant to limit the choice of rock, so we think already that there's probably more rock, at least in terms of vertically, over the primary area. I haven't looked myself at how much that might affect how much larger the primary area could get, but there are those kinds of things that we will need to look at in the future.

In my mind, we've still only made a preliminary judgment of how good the rock is for the repository. We need more data before we can make a firmer decision. So I think we need more data from the site.

DR. DEERE: I certainly agree with that, and as you know, the ramps certainly will get a lot of information on the potential size that you could have of a repository when it crosses those numerous faults on the east side, supposedly; alleged faults.

DR. DOMENICO: Getting back to that famous diagram--we don't have to flash it, though--just going back to temperature goals, you have temperature goals already. We've heard them.

DR. BLEJWAS: Right.

DR. DOMENICO: The temperature goals, whether or not you meet those goals will have to be established by a model of some sort, and they're going to tell you whether you made it or not.

DR. BLEJWAS: Yes.

DR. DOMENICO: And this is what concerns me the most, because we all know what's going to happen when you heat rock that has water in the matrix and a lot of fractures. You're going to drive it out of the matrix and it's going to follow a fracture. It's going to go up.

DR. BLEJWAS: Right.

DR. DOMENICO: It may go out of the mountain. It may accumulate underneath the Paintbrush Tuff. We don't know. But the whole point is that vapor transport is going to be a considerable heat transport mechanism, as well as just moving the water. It's going to transport some energy.

DR. BLEJWAS: Yes.

DR. DOMENICO: Now, your goals are established on a model calculation that has to simulate this complex transport process.

DR. BLEJWAS: Right.

DR. DOMENICO: Do you think, really, that you will, number one, develop such a model that now has to consider two phases; the single phase in the fractures, and et cetera, and would you really think that you'd be able to develop such a model; and number two, do you think you'd have sufficient information on the geometry, the fracture geometry to implement the model?

DR. BLEJWAS: Well, you're going to hear a little bit more about this over the next few days. Tom Buscheck from Livermore will present some information, and based on comparisons of conduction-only models, and models that include not only conduction, but two-phased flow, find that conduction does, indeed, dominate the heat transfer, and that's consistent with not only the experiments that were done by Livermore in G-tunnel, but also, by the experiments that were done by Sandia in G-tunnel.

DR. DOMENICO: Okay, but when he does address it, the key is did his model that included a two-phased flow include fracture flow as well.

DR. BLEJWAS: Yes.

DR. DOMENICO: I mean, that's the whole point, is to include fractures.

DR. BLEJWAS: Well, it includes two-phased--yes. After you've seen those models, then I think you may want to talk about that again, but to answer your question, though, can we predict the temperatures with a fair degree of confidence, I think that's what you're really concerned about. I will show you some comparisons tomorrow of temperatures around a heater test in G-tunnel that were based on just a conduction-only model, with actual measured temperatures, and lo and behold, the comparison, I think, is very good.

Now, that was a fractured rock. It's not conclusive. I wouldn't go forward to licensing with that, but I think it, indeed, based with this other evidence, gives us confidence that we'll be able to, indeed, come up with pretty good temperature profiles in the future.

DR. DOMENICO: My last question, then, has to do with the 57 kW/acre. Is it not true that you knew how many kilowatts you had, how many acres you had, and therefore, you come up with this design number?

(Laughter.)

DR. DOMENICO: Is that a fair statement?

MR. GERTZ: Absolutely not.

DR. DOMENICO: Definitely not?

MR. GERTZ: Absolutely not true.

DR. DOMENICO: Absolutely not true, okay.

DR. BLEJWAS: I wasn't involved at that time, so I'll say absolutely not.

DR. DEERE: Thank you.

Let's go to the next speaker, Eric Ryder of Sandia. He will speak about the thermal design considerations, after which we will take a ten-minute break while you're doing a little fiddling with the equipment.

MR. RYDER: Good afternoon. My name is Eric Ryder, and for the past three years I've been involved in repository thermal design, which I think means I'm at the right meeting, hopefully.

What I've been asked to speak about first is kind of give you an idea of what goes into coming up with a thermal design. If you were to strip down the equations down to conduction only, the time dependent diffusion equation, it's very simple mathematically. There is not a whole lot to do. The equations are very well understood.

Unfortunately, we have a very complex system leading into this, so it's not just the mathematics that come into play, but also, all the information; for instance, the degrees of freedom that Tom Blejwas was talking about, that makes it a very complex process.

The objectives of this first part of the talk before we take the break and get set up for looking at some actual temperature profiles, first of all, the overall objective is to, as I said, give you a feel for the complexities involved; following that and during the course of it, demonstrate why there is no unique set of temperature histories that correspond to a given areal power density. A misconception that's been going around for quite some time is if you come up to me and say, "For 57 kW/acre, what's the borehole wall temperature?", I can give you a specific number. That's not the case. As Mike Voegele pointed out, it's more of a design output number, and, as such, varying some of the parameters that go into it can vary the temperature profiles that are associated with it.

Also, I'd like to emphasize the dependence of calculated temperature responses on model and system assumptions, which follow the points that were made earlier regarding a coupled models and uncoupled models and how accurate are they. Also, and during the course of this, I'd like to point out some of the design and system changes that have changed since the SCP, that have come about and that we've had to reevaluate.

Just so we're on the same track and we're talking about the same things--because it's kind of an acronym nightmare when you start talking about areal power density--the first and the simplest definition for areal power density is what's known as local areal power density, and this is defined as the initial power output of a single canister, divided by the unit cell area, which is the product of the drift-to-drift and the canister-to-canister distances.

This is not exactly design independent. It's dependent upon primarily drift-to-drift spacing, but as opposed to the number that you see usually, 57 kW/acre, what's known as design basis areal power density, the limiting of the area of which you divide by to the unit cell makes it a much more convenient number to talk about.

This is a number you'll always see in the SCP and in most documents, and the difference here is that you take into account areas that you don't actively heat. You've got standoffs, access drifts, barrier pillars. All those areas are taken into account when calculating this number. For the SCP/CDR design, a good rule of thumb number to go from local areal power density to design basis is multiply local areal power density by about .82. So a 57 kW/acre design basis APD is about 69.1 local areal power density.

All right. Now that we have got the acronyms out of the way, what's the first thing that you try to do when you do a repository thermal design? Well, you have to establish waste stream characteristics, and that's a process in itself that has a lot of these, and these are some of them. I'm sure there's many others that I've forgotten. You have utility allocation considerations, waste package configuration and geometry, yearly tonnage requirements that are defined in the mission plan amendment; also, inventory projections and characteristics, and this is really where a thermal designer comes in, in picking the waste stream characteristics he'd like to look at, what are the effects of modifying which assemblies you take, and how you emplace them into the ground.

I'd like to quickly go through--I'm not going to get into utility allocations, but I'd like to go into these three and this one, also. In the SCP, the reference waste package configuration was a consolidated package. That's no longer considered the reference. That's one change that I said I'd point out. This had an initial power output on the order of 3 kW for ten-year-old fuel.

In the SCP, there is also an alternate waste package configuration that's documented, which is the current reference case, this one here, the hybrid case in which you have four intact BWR assemblies and three intact PWR assemblies. The loading is substantially less. I believe for a 20-ton start date, you're looking at initial power outputs of, depending on your waste stream characteristics, one and a half to two kilowatts. I also say that we have to take into consideration acceptance schedules, the early tonnage requirements.

This is from the mission plan amendment of 1988. The new one's just come out. I don't think there is any significant changes in it, in terms of acceptance, but you can see that we ramp from about 400 tons of spent fuel in the early times, 900, 1800, and then a steady state of 3,000, while we're also accepting about 400 tons of defense waste, and then a slight tailoff at the end, for a total of 70,000 metric tons. If this were to change, we, of course, would have an impact in the fuel assemblies we could choose to determine our waste stream. We may have to take younger fuel.

I also indicated that the inventory projections and characteristics have an impact. Oak Ridge maintains a database called the characteristics database, in which they have waste stream characteristics, as well as inventory projections. This has just changed, from what I understand. They're going to update this. This was the old reference case of no new orders, extended burnup, where the reactors had average lives of 40 years, and there would be no new reactors built, so it would be the discharge from approximately 115 reactors.

From what I understand, they're going to a 20-year extended life now, so this is going to change in the near future, if it hasn't already. This represents approximately 84,000 metric tons by discharge year 2037.

I also indicated that they have thermal decay characteristics. They calculate these from the ORIGIN II Code, in which they have isotope generation and depletion within the core, and also after removal, and they calculate these power output curves. 60,000 MWd/MTU or initial heavy metal produces, as you would expect, more heat than the lower values, say, 10,000. I believe the average, again, if you were just to take all the receipt schedules that are readily available and average them up, you're probably in the range of about 30 to 35,000, so you're in the middle of this clump of curves, typically.

The old SCP design, we'd start with ten-year-old fuel, so we'd be looking at, actually, still a relatively wide spread in the curves in ten years. We're now at about 30-year-old fuel with the 2010 start date. We've compressed them more. There's not quite as much variation that we had in the past, and that's had an impact on the predicted temperature profiles. And just for completeness, the radiological decay characteristics are of the same shape and a little more widespread at early times, and more clumped at late times, as you would expect.

Well, those are system things. Those are things I really don't have much control over. That's the input that I take, as a thermal designer, and then manipulate it in my models. The only thing I have control over is what kind of waste stream do I want. The reference SCP design is the oldest fuel first scenario, or first-in/first-out, where we take the absolute oldest fuel from the reactor pools and emplace it in ground.

There was an additional study done by Lynn Ballou at Lawrence-Livermore, the levelized receipt schedule. This is where we would select fuel assemblies on the basis where we could control the initial power output and age, and we could bound it in a nice range. If you go with the oldest fuel first scenario, you tend to have a monotonically decreasing age curve, and a correspondingly increasing power output curve, and we'll show those real quickly in just a moment.

But I want to add this third one, area minimization, just so that we don't get constrained to think that we only have two choices, FIFO or levelized. There are many out there. Area minimization is one that we've been working on a little bit at Sandia, and it's where you use the transportation algorithm to assign costs based on acres required per ton of material in place, and it's a way of minimizing area requirements.

There is others besides these, but, you know, let's not get constrained to FIFO or levelized, and just saying that, I'm only going do FIFO or levelized. We haven't rerun the area minimization one for a 2010 start date yet.

As I said, this is the FIFO curve. This is the waste age characteristics starting in 2010, ending emplacement in 2034, and this is the age characteristics for a levelized receipt schedule. As you can see, it's a little more bounded age-wise, in a bound of five to seven years; whereas--five to seven year bound. It's on the order of 28 years old, average. This has the same average, but it goes from almost 39 years down to approximately 22-years-old out of reactor.

DR. PRICE: And could I interrupt just to ask you, on the FIFO scenario, is it not true that that simply sets the assignment for utility pickup, and the utility can deliver you something other than their oldest fuel? It's just a way of establishing a priority for pickup.

MR. RYDER: That's a utility allocation question, I think. I will kind of warp your question a little bit. It is possible to take fuel on that basis, a FIFO basis or a modified basis, and emplace it in what is equivalent to a levelized receipt schedule. It's just a little easier to model it this way. For example, I can modify spacings based on a FIFO receipt schedule, and come up with an equivalent areal power density or equivalent design that produces temperature profiles similar to a levelized receipt schedule, and that was in the cooling paper, I believe, that you read. Has that warped your question enough?

DR. PRICE: No. I wonder if that is not, though, in fact, correct, that you can't--that DOE does not control the oldest fuel first in that scenario, that all they do is assign the priority for pickup by utilities.

MR. GERTZ: Dennis, I frankly don't know.

DR. PRICE: I think that's correct.

MR. GERTZ: It very well could be true. It's not an area I'm familiar with. I don't know if someone out there is familiar with it. Barrie has raised his hand. He might be familiar with it.

MR. McLEOD: Barrie McLeod with the M&O, E.R. Johnson Associates.

I'm one of the co-authors of a program called, "Waste Stream Analysis," which deals with exactly the question you're asking. The utility allocation is oldest first, as you correctly surmised. The actual selection within that allocation can be literally anything older than five years. The current standard contract gives utilities the right to propose what they want; gives DOE, however, the right of approval, which, of course, gives them a right to negotiate with the utilities.

The so-called ACR process, the Annual Capacity Report, where working with the utilities to modify the standard contract may give DOE additional rights in selection. I think the bottom line is that it's very likely that DOE will have enough control of the kinds of things we're talking about to be able to manage, at least at the repository, what goes in there.

DR. PRICE: Because you would think that if the utilities had put their oldest fuel into dry storage, that they would have a tendency to leave it there and take the fuel out of the pool.

MR. McLEOD: That is, of course, correct; yes, if they do it that way, which they are doing right now. However, there's enough other inventory that there's reasonable promise that DOE can control more or less to an average.

MR. GERTZ: I guess, Dennis, what we're saying is it's kind of a negotiated agreement between us and the utilities. We hope we'll be able to get what we want out of that, and Barrie, you tell me we're looking at some possible changes in that contract.

MR. McLEOD: Yes.

MR. RYDER: The age is all very well and good, but actually, what's more important are the initial power output comparisons between the two.

Here we have the FIFO case, going from about half a kilowatt per canister up over two. To me, that screams a nightmare in terms of design and trying to get that predictable and consistent temperature profile across. We can do scaling techniques, but when we get in this range here, we may run into geometric constraints. It may be very difficult, or the 7½ foot borehole center line spacing limit, we may not be able to get it that close, so we may have this cold spot somewhere, which would be very difficult to deal with.

That's why this is so much nicer. It's very consistent, easier to design to. Many other receipt schedules also show characteristics that are nice and well-behaved. So that brings us to--we've done the first step, the process unto itself, in determining down here, we've got a waste stream with a waste age and burnup associated with it, and from this, we can calculate initial power output. So we're ready to really start into determining temperature profiles.

And so we've gone down to here, through the dotted box, and the first thing that we need to do is like I was talking about scaling so you get a consistent set of temperature profiles or consistent regardless of waste age and burnup, or regardless of your receipt schedule--well, not regardless of it, but associated with it, and there's a dotted box. These correspond in color to what I'm going to be talking to.

This is actually a process, a design process unto itself that we've already gone through, so it's not an independent, nice flow chart. It's actually horrendously coupled, and we've simplified it a bit. We need to define a baseline thermal response. If we want consistent temperature profiles that are equatable to a baseline, we have to have some baseline. So we need a general repository layout, and we've been using the SCP/CDR design. We need to have baseline where waste characteristic is defined, and local areal power density that we're looking at, and that we've gone through this thermal design process to get this, some thermal response that we want to equate our new waste stream to, or our waste stream.

Let me switch this over to here and talk to a couple points real quick. As I said, this is what we typically use. We take a lot of liberties with it in terms of spacings. It's not the 126 foot spacing you see in the SCP/CDR, and it's not, you know, the 15 foot boreholes, because we have a different power output. We don't have 3 kW canisters anymore. We have on the order of 1½ per levelized, and between .5 and 2 for FIFO. So we play with those spacings.

Baseline waste characteristics, it's a convention, really. There's no real strong justifications, just what was used first. It's considered to be those used by Johnstone in the unit evaluation study, so it's what came up with the 57 kW/acre. Baseline waste is considered to have an age of ten years out of reactor, and if you want to equate it to something today, when it was used originally, it didn't have a burnup associated with it, just a thermal decay curve. It's approximately 35,000 MWd/MTU if you go beyond ten years.

Now, this section down here, when you have your baseline thermal response, and this part here all goes with scaling, scaling for waste age and burnup, so I'm going to kind of skip around in my chart there. The current method of scaling for waste age and burnup is called the equivalent energy density concept, and what it does is it bases its equivalence criterion on the assumption that an arbitrary waste or, for example, the waste on our receipt schedule that we receive in 2015 or something, would produce worst-case thermomechanical effects equal to those predicted for a baseline waste, which was the Johnstone waste, provided that the thermal energy deposited in the host rock over a specific time--which is deposition period, and I'll be using that term several times--is the same for both waste descriptions.

So we have this relatively simple equation here. These would be the exponential descriptions of the waste to K functions for the baseline waste. This would be the baseline local areal power density we're considering here. This would be the arbitrary waste, the waste from whatever year we were looking at, and this is what we calculate, Pa, which would be the scaled local areal power density, what we'd have to emplace it at to get the worst-case thermomechanical effects to be equivalent.

One note here, it's applicable on a local areal power density basis only. I've seen it applied on other scales, and it doesn't work. Now, a lot of you are wondering, well, why do we have to scale? I mean, it's producing 2 kW. Why don't we just emplace it at constant spacings and make everything nice and neat?

The effect of burnup is pretty obvious. The higher the burnup, the more heat output. For equal age waste, if we emplace it at the same local areal power density, this corresponds to a design basis of 57. The higher burnup waste produces higher peaks. It's intuitive.

What's not quite so obvious is this tail off at the end. You get a dip at the end, and that's because it produces more heat output initially, it has to be spaced further apart. Therefore, it's long-term interactions are lower. This is only 20,000 MWd apart, so you don't get a big change, but if you were to look at 10,000 and 60,000, it'd be much larger.

The effect of waste age is actually kind of more fun to look at. Now, before I get into this one, let's just say if you had waste that was emplaced of different ages--I believe this is 10 and 50 years--if you were going to emplace it at constant spacings, then the older waste would have produced lower peak temperatures, I mean, but that's not a constant local areal power density, constant spacings with older waste. If you keep the LAPD the same, what you get is a behavior like this, where the older waste produces higher peak temperatures, slightly delayed, and has a longer or a higher tail off, higher temperatures out in the long time frames.

The reason for this is, again, long-term interactions. The fact that it has lower heat output means to get the same local areal power density, we have to crush it a little closer together. We have to put it much closer in spacings, and because of that, you get higher short-term interactions for it because it's older, and also, long-term, so you get this peak.

So, obviously, if we were looking for something, consistent calculations, or consistent and predictable temperature profiles across the repository, regardless of the waste station burnup, we have to scale.

And when we get done with that scaling process--which is, as I said, relatively simple--we end up with scaled LAPD's that will produce worst-case--well, I didn't discuss choice of critical scale. You can only scale to one point or one area, one location away from the repository. If I choose the borehole wall temperature to be the dominating effect for the baseline LAPD I'm looking at, then I would scale to that peak temperature; for example, it occurred at 20 years. My deposition period would be 20 years. If, on the other hand, I were looking at a higher loading and I thought the TSw2-TSw3 interface temperature were the controlling factor, the peak temperature doesn't occur out there until on the order of two to four hundred years, so I'd choose a deposition period of much longer, say, three hundred years.

But by choosing those deposition periods and getting the scaled LAPD's, I only match those temperatures if I alter the others, which is when we get down to the pink box here, evaluation of temperature profiles, that's one of the things we have to evaluate, our choice of deposition period.

So not to jump ahead and confuse you, which I've already done, we're down here, scaled LAPD's, with a lot of things attached to them. Now we have to translate, based on those scaled LAPD's, and our waste stream, we have to translate those into specific model geometries, where our cans go, et cetera. So let me throw this up.

We got this from the selection process, waste stream initial power output. We just got our scaled LAPD's. Here's something I choose. Chosen source type for heat transfer model. Do I want a plate source? Am I going to look at a 2D simulation? Am I going to use discrete heat-generating cylinders with an analytical solution? It's something I have to decide and eventually evaluate my profiles based on.

Once I make that decision--actually, it's kind of interactive with this. I need to know the general repository layout. As I said, we have kind of a sub-process when we've got a baseline characteristic, so this has to be consistent with that. And, again, the same diagram; a little more detail. Let's look at the vertical emplacement option from this case.

Some of the things that would go into determining the actual specific geometry, suppose I chose to use the analytical code--which you'll be seeing the results from later, where it uses actual heat-generating points and white circular cylinders to represent the canisters. Then I would have to choose a drift spacing, which is consistent with what I scaled against, hopefully. Borehole spacing, my standoffs, my non-heated regions I also have to put into the model, because that has a significant effect, as you'll see in the second talk, but these are things that I'm capable of altering, so we're not stuck with 126 feet and 15-foot centers.

This has to correspond to my scaled local areal power densities, and I said at the beginning that I would show that I can't give you a single number for temperature at a given location based on an APD. For example, the current extraction ratio limit that I'm aware of is 30 per cent; extraction ratio being the ratio of the room width divided by the center line to center line distance, times 100 per cent. Actually, this is Mr. Brandshaug's diagram.

If you take 30 per cent and actually go to that maximum, you can have drift spacing on the order of 53 feet. If that were to alter, saying only 16 per cent is our extraction ratio, the drifts can be no closer than 100 feet. Now, we have scaled LAPD's. That's one of the inputs. We then take borehole center line spacings to match those scaled LAPD's, and this is for 30 per cent, which means the drifts are closer, so the cans can be further apart. Our peak temperature at the borehole wall is lower than when we go to 16 per cent, which means our drifts were further apart, but our cans have to be closer together, so this is one of the reasons why I can't give you a single number, and, you know, people say, "Well, it's just a near-field response." You can actually see the same effects in the far-field; similar case.

Thirty per cent, lower peak temperature. We have the canisters closer together. It peaks higher, on the order of 10°. Whether that's significant or not is, you know, again, part of the evaluations, but hopefully you get the idea that that's why I can't continue. I can't just give a specific number.

So what we've done, then, is we've translated here. We've got specific locations of the heated and non-heated areas. We've got discrete canister locations. We're ready to really work on the model, or pump it through the model and calculate the temperature profiles, which is this. We've got this from the selection process. We just finished this. Now let's take the site property values. Hopefully, they'll be updated as we do site characterization. Right now, I use what's in the RIB as my values. I can alter those, too, if I'm doing a sensitivity study.

We apply the heat transfer model that's appropriate to the source type we use, and also, the phenomenon that's under investigation. For example, I'd look at a, if I were looking strictly at heat, I might look at a conduction-only code; whereas, Tom Buscheck might look at a hydrothermal code, because he wanted to see dryout. Once we've processed all that stuff, we get temperature histories and, of course, we've documented all our model assumptions because your temperature histories are only as good as your model, and divorcing the two is kind of dangerous at times.

But we're not done there. We just don't stop there. We have to evaluate those temperature profiles, which brings us almost to the bottom of this chart here.

And there's actually two paths we have to evaluate: Design considerations leading into design goals are how we get like the SCP thermal goals that Tom Blejwas showed and Mike Voegele alluded to. We also have to evaluate some of the tradeoffs that we've done, the critical scale of interest. If we scale to the borehole wall, we need to check the far-field to see if the temperatures have been altered significantly based on that assumption. Was our baseline LAPD too high to begin with, et cetera, and our model type and assumptions.

Just very quickly, some of the design considerations that you're well aware of and that you've heard many times before: Near-field rock mass integrity, cladding integrity, surface uplift and environmental impacts, rock stability, and some of the generic ways of addressing those. There are others, but these and other considerations lead into these goals that we have that are hopefully iterative and are evolving, and as Tom Blejwas said, we need to re-evaluate periodically, and hopefully, soon.

I won't go into these, but, for example, if our temperature profiles violated one of these, I would consider it not something I would pass on. I would document it, indicate where the problems were, and try and trace them back to whatever model assumption, et cetera, but it wouldn't be something that you would consider passing on for additional design consideration until those goals were altered or changed or, you know, if they even were.

But more interestingly is when we get into this area. So this is just basically matching temperatures. If the temperatures are too high or too low, you know, we document that. This is where we get into my assumptions as a thermal designer, and as a quick four view graph example, for a design basis of 57, local 69.1, kW/acre, as I said, suppose we chose--these are deposition periods here, and these are the resulting curves, the baseline being this curve here, the ten-year-old Johnstone fuel.

Suppose I chose 20 years as my deposition period, we're very close. This is my curve here. My peak temperature's very close. That looks great. Well, let me look at the far-field, as what I said you had to do as part of your evaluation. All right. Here is my baseline response here, peaking out at about 80°. Here is my 20-year deposition period curve. It's higher, but it's below 95 and it's below the current goal of 115. It's acceptable in the current environment.

If, on the other hand, I were looking at a higher loading, 97 kW/acre local is 80 design basis, suppose I thought the same thing. Here is my baseline response, peaking out at 185; 20-year, right on top of it down there. Well, that looks great. Let's look at the far-field. Here's where we find some problems. Baseline response peaking out at about 100°. Here's my 20-year curve. It goes over the current SCP goal. That would be something that I would find a problem and go back and iterate the design or the solution and document that.

MR. GERTZ: Eric, excuse me, is 50 meters just what you think the distance might be to the area of interest or the interface, or is that a conservative estimate of--

MR. RYDER: This is an extremely conservative estimate of the closest approach with TSw2-TSw3.

MR. GERTZ: Okay.

MR. RYDER: The majority of it--in fact, this is one of the regions we'll show in the videotapes, the 50 meter.

MR. GERTZ: Because as I recall it, it was much further than 50 meters.

MR. RYDER: It is. It's on the order of 90 meters, the majority of it, but there is a little tail up at the end that comes up around 50 meters, and from my standpoint, if I can match it there--

MR. GERTZ: You're okay, obviously.

MR. RYDER: --even the greater factor of safety to go along with it.

You come to some decision points, and I'll just briefly, quickly, really fast go through them. Suppose that I found that the profiles indicate that the temperature goals are violated, like I said, and I traced it back to a critical scale that I chose as my deposition period. That means I could come from here back up to this blue box here, re-do my scaling, re-do the model, then re-evaluate it.

Also, I might find the waste stream characteristics, I wanted to choose the youngest fuel at all times, so I've got these, you know, three or four kW canisters--I don't know if that's possible, but it might be--that again violated the temperature profiles. I'd back up to the waste stream selection process and start over.

Design spacings, canister, drift, as well as standoffs, I might have shortened my standoffs somewhat to try and save area, for example, and I find that I've violated the temperature in the panel access drift. I'd go back and change my conceptual design, or my warping of the conceptual design.

Suppose I was wanting to look at dryout. Well, I should be using a code like Tom Buscheck's, but I use a conduction-only code. So maybe I didn't capture the phenomenon properly. I'd go back and change my model, et cetera, and it just goes on. But I might come and say the tradeoffs were acceptable, and I document them, and then I'll recommend that design for further evaluation from the system standpoint and from the, you know, for ACD, recommend portions of it for ACD. So that would be iterative arrows on this diagram here, going up and around.

So that brings me to the conclusion of my first talk. What it says is the degrees of freedom associated with coming up with a thermal design are sometime mind-boggling, and changes in any of these upstream characteristics or upstream system decisions can have a strong impact on what I'm doing and what I calculate as temperature profiles. And also, when you're comparing temperature profiles, be sure that you look at your model assumptions and that you associate them.

There's a problem with the literature that's been produced in the past. A lot of people just grab the diagrams and forget what goes into those models, so we're often waving our hands saying, "Well, that's a 2D finite only code that takes into account poor water boiling, and you're trying to compare it to a conduction-only analytical solution." So these are things that we're, in the second talk, hopefully, trying to avoid. We're going to show you a consistent set of calculations across a range of areal power densities for a single model type and model layout, so we can get some general trending information without having to wave our hands, and with that, I guess we need to set up the video projector, or if there are any questions?

DR. CANTLON: You've been talking about all the variables to go into the design of what I presume is a homogeneous repository.

MR. RYDER: What do you mean by homogeneous?

DR. CANTLON: Well, the density of placement of fuels, and so on, with some kind of a homogeneous mix of fuel inputs, but to what extent do you visualize a space-to-space difference as the fuel stream changes, as reactors change, military wastes change?

MR. RYDER: Just to give you an idea of what's coming up in the next talk, I can give you a range of spacings that we looked at here. It can range up to several meters. It appears to. I'm looking at the wrong graph, of course. Here we go. Between eight and ten meters for 30 kW/acre. They can range up quite significantly. You couldn't--I mean, if you did a levelized receipt schedule and kept the age and the burnup and the power output pretty constant, that alleviates the problem of widely varying spacings. That's one of the benefits of that sort of receipt schedule.

There are receipt schedules--for example, the FIFO case--if we did, in fact, emplace it in that method, you could have spacings ranging from 7½ feet up to, you know, over 20 feet, I believe, is what I looked at one time. So they can be very variable, or not very variable, depending on your waste stream, and that's a decision that has to be made in the future.

DR. DEERE: I think it does show that your 115°, which is controlling whether something passes or it doesn't, or the 250°, et cetera, those really sort of control things, don't they?

MR. RYDER: Yes. They're the first thing you evaluate against.

DR. DEERE: And then there should be evaluation of those--

MR. RYDER: Exactly; yes.

DR. DEERE: --if those are the right ones, or how much tolerance do we have?

MR. RYDER: Right. How much tolerance do you have. You might take a secondary goal that's not published, like 95°, and see if it meets below boiling and document that. You know, it's below 115; in fact, it's below 95, even better, or I think we'll find out later if it's better or not. Yes, I think those goals need to be evaluated and continuously evaluated, based on new information from site characterization.

MR. McFARLAND: Eric, you made the comment that at a point in time the decision was made to go away from a canister containing consolidated pins versus a fixed tonnage canister, which eliminated one variable in your ability to mix/match whatever. What was the rationale that led to that decision?

MR. GERTZ: I can address it, just very briefly. It's a kind of a systems rationale that we didn't altogether need to consolidate fuel at the repository, to disassemble fuel assemblies. We saw no reason to do that at the repository. If it comes to us consolidated for some other reasons, we can handle it, but we didn't think it wise to build facilities, mechanisms, machines to consolidate fuel when we didn't need to. So, therefore, we just took the simplest way, direct emplacement.

MR. McFARLAND: At the time the decision was made, there was no rationale for the consolidation, for taking old fuel. As was implied in the meeting this morning, if you were to try to maximize your above-boiling time period, maximize your hot repository duration, fuel consolidation would be almost essential.

MR. GERTZ: Is that what Larry said? I don't know if he said that or not. See, you can put four fuel assemblies, unconsolidated, young, into a canister and get it hot. I don't know that you could put eight consolidated assemblies and meet all our other constraints.

MR. RYDER: Actually, there might be a detriment in terms of keeping it hot. If it's hotter when you initially put it in, your long-term interactions are lower, even though it was more power output so it may not stay with that tail up like we saw.

MR. McFARLAND: If I had, for example, 60-year-old fuel, which was mentioned this morning, how would you handle that to maximize temperature for the longest duration?

MR. RYDER: Increase the areal power density, space it closer together so your interactions are higher long term, and as you saw in the effective aging plot that I showed, the older waste produces a higher and, actually, you know, it is delayed, but it's a higher peak and a higher tail, and that was for an unconsolidated, you know, hybrid case. So it's also a function of what scale you want to keep that temperature high at.

DR. LANGMUIR: Eric, have you integrated with your modeling ideas the concept of a heat pipe as another permutation on the whole process?

MR. RYDER: That would be what phenomena you're looking for. That would be choice of appropriate model. If you're looking at heat pipe, and whether it's important in the fractured rock mass, you would--you'd do some scoping studies with that sort of model, and then do a full-blown one and compare it to the conduction-only. I have not personally looked at it. I understand that Karsten Preuss is looking at that for us. So the answer is no.

DR. DEERE: What do you think, another five minutes?

MR. GERTZ: Do you want to take a five-minute break, Don? Let's do it.

DR. DEERE: Let's stand up for five minutes.

(Whereupon, a brief recess was taken.)

MR. GERTZ: I think we're ready to start. Eric's working wonders with this machine.

MR. RYDER: But the colors still aren't right.

(Casual conversation.)

MR. GERTZ: Eric, Talk Number 2; go ahead.

MR. RYDER: Talk Number 2, Talk Number 2, here we go. Everybody get popcorn? Okay.

All right. After that long, involved discussion about how had it is, we've gone and given you a consistent set of calculations here to look at, and the objectives of this section of the talk are to show near- and far-field temperature profiles generated using a consistent set of assumptions, so we don't have this hand-waving anymore; discuss trending at critical scales, which would be where the SCP thermal goals are defined, or some of them, for APD's ranging--and these are design basis areal power densities--

from 20 to 80 kW/acre; and then, hopefully, discuss and demonstrate some of the effects of aging, increasing the repository heated area beyond what's the current perimeter drift, and also, some modifications to the ventilation system.

There's a lot of data to show, and fortunately, we have it animated, so it helps a little bit. What we're going to do, or start with is a discussion of the model assumptions, and then we'll go into the presentation of the results, going from what you call hot to cold. We'll look at 80, 57, 48, 30, and 22, and I'll discuss the significance of these various areal power densities. 80, 57, and 48 will fit, with my modifications, into the current SCP/CDR perimeter drift. To go to 30 and 22, you have to age to get it into that area. 30 kW/acre, you age an additional 30 years, so it's 60-year-old fuel, then; and 22, an additional 30 on top of that, so it's 90-year-old fuel.

For aging fuel we have, across these areal power densities, both for near- and far-field, so we have a complete set there. Increased heated area, we just have near-field results for something on the order of 19 kW/acre, and for modifying the ventilation system, we look at two options; a five-year and ten-year ventilation for 80 kW/acre, near-field only.

DR. DOMENICO: Eric, excuse me. When you say to get it in that area, do you mean the area--

MR. RYDER: The perimeter drift defined in the SCP/CDR, approximately 1200 acres.

DR. DOMENICO: 1200 acres.

MR. RYDER: 1220, 1240; what's a few acres?

Okay. So after the model, a discussion of the model, we'll look at near-field results on the first tape for the range, and again, this is for the aging case where, when we get below 48 for the 30 and 22 case, we're looking at aging the waste, and then we'll look at the far-field results for the same areal power densities. In this case, we'll look at a vertical cross-section through the mountain, and then, also, at 50 meters below, which, as in the earlier discussion, is a very conservative estimate of the closest approach of TSw2-TSw3, and then the alternatives, we'll look at the near-field response to increased heated area for 19 kW/acre, and the effects of ventilation on 80.

Model assumptions, and fortunately, we don't have to go through all these because we brought most of them up earlier. We're looking at a modified version of the SCP/CDR design to represent the potential repository. Fully stepped emplacement of spent fuel is considered. Defense high-level waste--and I'll address the question that was earlier on defense when we get into the simulations--considered to be segregated in the first few drifts off the mains. This is a change from the CDR design, where it was co-mingled. The benefits from this--and it's just a choice I made when I did the model--is that you keep the main drift temperatures down during the construction or during the operational lifetime and the retrievability period very low, and because of the increase in the number of canisters, or spent fuel canisters with the change from consolidated to unconsolidated spent fuel containers, you can't really put them in between them anymore. First of all, there's not enough of them; and second of all, the spent fuel canisters have to be closer together than 15 feet a lot of times, so there's not room for them.

We're doing a levelized receipt schedule, assuming a 2010 start date, and the intact hybrid configuration that I discussed earlier as the current reference. The surface environment was modeled as a constant temperature surface, which is consistent with when Mike pointed out the temperature rise at the surface for 57 on the order of .6°C, calculated in the past, and I believe Mr. Brandshaug did some calculations at 80 kW/acre that showed it only at 1.1°C temperature rise. So that's a pretty good assumption, I think.

We scaled the emplacement densities for the levelized receipt schedule, using the EED concept and deposition periods of 20 to 300 years, and I note on a lot of summary graphs what deposition period we used. It's very simple, actually. For 80 kW/acre, we used 300, because it was felt the far-field or the 50 meter response was the controlling response, and that's approximately when that temperature peaked, so we're matching that peak and sacrificing some of the heating up in the near-field. For all the others, we had room in all the ranges, so we used a shorter deposition period of 20 years.

It's an analytical 3-D linear superposition code that use heat-generating points and cylinders in its solution. We modeled the site as a semi-infinite mass of TSw2, with constant material properties. We show the current RIB values, and I believe there's a cube missing right there which I forgot to put in.

Just going through the ones we haven't gone through, again, that's the basic layout, and as modeled, it'll take a little explanation on this. Panels 1 and 2 haven't been considered for emplacement in a long time, since I've been on the project, so I consider it a long time. What we typically do is take the 49 acres encompassed by these, and just attach them here, so it's approximately the same heated area for this layout, with just two panels missing.

For 80 kW/acre case--and also, as I said, we have the defense waste right in here--we're using the extraction ratio of 30 per cent, so we have 53--we've taken the drift spacing to its absolute minimum so we can have further spaced canisters. So in all these cases I show, the drift spacing is 53½ feet, and the canister spacings varied according to the local areal power density.

Now, with that in mind, for 80 kW/acre, emplacing that, we only need to develop 3, 4, 5, 6, 12, 13, 14, 15, and 16. We don't have to develop this section, which is in the perimeter drift, nor panel 17 to get the waste in. In this case, since we have fewer panels, we have more defense drifts between here. We have five, and similarly, for 57 kW/acre, you simply have to extend it two more drifts, or two more panels. But again, you don't have to develop 17, which is a funny little panel.

I said we did fully stepped emplacement of the spent fuel. This just gives you an idea. For 80 kW/acre scaled on a 300-year deposition period, in emplacement year 2020, we're going between Panels 5 and 6. The year before this, we emplaced a few canisters right here, so the model that I used picked up right here with the appropriate spacing, filled out the rest of this panel, came up here, and filled out a portion of this drift here. So the following year, we'll continue from that. So it is fully stepped emplacement that we're using across the 25 years of emplacement.

I said we segregated the defense high-level waste into the first few drift tuffs and mains. We made a slight modification. Because we're doing this and the temperatures are kept so much lower, we can shorten the current distance of 200 feet standoff from the mains, to 150, and that saves you about 70 acres if you use the whole thing. We're using triangular arrays for the defense, and in response to part of the question on defense, we assume it has 200 watts initial power output, which is in the range that was used in the SCP. They used 200 to 400. If you look at the Oak Ridge characteristics database, most of the tables show it on the order of 200. They're all worst-case projections that put it up, you know, if it were taken off the youngest tank with all this horrible slurry, and it could be up to 700 watts, but in that case, you could just take that canister and place it as a spent fuel canister in the same way, with the same scaling techniques.

So we're assuming they're 200 watts, placed in a 7½ foot array, triangular spacing, which is considered the minimum borehole center line to center line distance currently.

I've included in your package, just for future reference, a modification summary. This is some of the things that are in the SCP/CDR design as published, and these are the things I've modified on it, so that's something you can refer to in the future, and I've gone over basically most of these, so we're getting real close to the tape now, and the first thing we're going to look at is near-field response, the near-field environment. We're going to look between drifts, and the region we modeled was--here is the spent fuel canister--approximately just under 10 meters up and 10 meters down.

This is the line of symmetry with 53 feet canister spacing halfway between it, so you have a reflective boundary condition. We're going to look at the response in a central drift in a panel, so we're not taking into account edge effects in this look at near-field responses. They'll be easy to see in the far-field, the responses, and I'll discuss the edge effects.

Based on this section that we're taking, this is the sampling grid. As I said, we come out to--it's in meters, so it's 8.15 meters from the canister. The canister center point is the zero point, so this is just half the canister. We don't sample right above the canister. We start at the edge and move over. The borehole wall, we have a one meter sampling location. Then it moves out from there. I think it's 1, 3, 5, 7, and 8.15, and then we have the top of the canister, the bottom of the canister. Actually, it was reflective boundary conditions, so we just flipped it over so it's easier to see, and it moves up to approximately 10 meters above.

Hopefully, the colors will match something like this when you're looking at the simulations. Temperatures to look for or colors to look for in the simulations--because a lot of times it's kind of difficult to read--when it goes red, it's gone above 95, or it's gone to 95. If it goes to this blue--which shows up great on the sun work station, but on the movies it tends to look like this blue--it's 115. You won't see that in a couple of the models we looked at. 130 is a gray color, and then our maximum temperatures that we came up in all these simulations were on the order of 170°, so we've got this purple to be that. This is about 150 to 175. Yellow is 60°. So look for the red, and depending on the scale, I'll kind of give you colors to look for, and I think we're ready to do the first tape, the near-field.

(Whereupon, a videotape was shown, with commentary by Mr. Ryder.)

MR. RYDER: This does say, "Thermal Response, Near-Field," in red. We're going to look first at 80 kW/acre, a vertical cross-section like I showed you.

Time, one year. We've disturbed it. Okay. Two years we started boiling front. We've actually got localized boiling around the waste package. This green is confusing. I think it's on the order of 50°. We've now set up--this is that blue that looks like every other blue--115 around the waste package. The boiling front--we're at ten years now--

has not coalesced between drifts yet. It's going to happen now, at 12 years, and we've started a core of about 150° around the waste package. The temperatures continue to grow. We're at time, 14 years. We still haven't dominated the whole block at boiling yet, but we're coming close.

Sixteen years, 17, basically, we've dominated the whole block. Again, we're at 150, but our 115 isotherm is beginning to coalesce, 31 years it does. This gray area, or greenish-gray area, is the 130. So it's starting to move across. So there's a lot of action in this higher loading. Thirty-six years.

One of the things to look for in later simulations is the growth of this 140 or 150° isotherm, and this 130. In the next one you'll find this actually collapses, whereas, in this simulation, it will dominate at the end of the 200 years that we show. We're in 34 years. At 50 years, we'll jump to ten-year increments, and then we'll jump to 100-year increments, and stop at 200 years.

We're probably--in fact, I think we are--approaching the maximum borehole wall temperature of 170°, and I think that occurs at about 45 years. It's delayed from what you've seen in the past because of the age of the fuel. This is 30-year-old fuel, as opposed to a lot of the simulations you've seen in the past, which is 10-year-old. At 45 years, 46 years, this area is 130. We've got a huge region about 6 meters out at 50 years where it's 140-150°, and now we're jumping in ten-year increments. It's coalesced at 60, 70, it's growing quickly, so we actually got continuing heatup. This is dropping off slightly, but there was enough energy released where it's dominated the block at 200 years. That was about 140-150°.

Two things to look for is when the boiling region coalesces for 57 kW/acre, and what happens to the dark blue region towards the end of the simulation. We've got boiling setting up again around two to three years after emplacement. We're at six years now, seven years. It's growing as before, but not as quickly. Nine years, ten years. Now, this is when it coalesced. Twelve years is when it had coalesced for 80 kW/acre. It's substantially retarded based on the loading, and it will coalesce at 19 years, I believe, which is three steps from now. We've got 115 starting here, whereas before, we actually had a dark blue region here at this time, which was 150°. Now, here's 19 years. We've now connected between central drifts the boiling front, so it's coalesced.

DR. DOMENICO: Eric, how come you don't have any heat pulse coming out from the other side from the other canisters?

MR. RYDER: This is a cross-section through a canister.

DR. DOMENICO: Oh, through one canister.

MR. RYDER: This would be flipped this way. This is the other drift over here. We're using lines of symmetry here.

Thirty-eight years, the whole block is now boiling, the 10 meter block or the 20 meter block we're looking at, and here is our little region of dark blue, and I said watch for this towards the end of the simulation. Recall from the 80 kW/acre, the whole screen was blue at 200 years. You'll actually see this collapse, which means, you know, the areal power density is lower. There's less energy being put into this rock from its nearest neighbors, et cetera.

This is 115. Again, it's delayed from when it coalesces between the drifts. We're at 40 years now, 41 years. 130°, this gray-green area, which in the last simulation was out here by now. It's much retarded. Forty-nine, 50, we're going to jump at ten-year increments now. It's moving out a little bit. The whole block is now 115.

Ninety years. Now, do you see this starting to collapse? It's started to collapse at 90 years. At 200 years, it's gone, so that's a significant difference from the last simulation.

48 kW/acre corresponds to, for a 2010 start date, the areal power density that's required, to use the whole area. You would have to keep it 48 or above. Again, we have boiling setting up very early, and this will just be additional--the same trending that you saw between 80 and 57,

much less disturbance time seven years, eight years. You'll find that this one, it will coalesce between drifts, but it's at 39 years, I believe. So we had 12 years, 19 years. Now that we've gone between 80 and 57, now we go to 48, which is only a drop of 9 kW/acre. We actually have a delay on the order of 20 more years before we have coalescence between the drifts of the boiling front.

Sixteen years, 17. We're at 115 near the borehole wall. I'm sure this is supposed to be like a pink color, like 100. Again, it's moving out slowly, much slower than before, as you would expect.

We're at 24 years. So it's creeping slowly towards 39, when it will coalesce. Oh, maybe I lied, it was 31 years. I lied; 31 years. Keep me on my toes. But still, we've got no gray region. We've got no dark blue region. So our peak temperatures are substantially lower. I believe the peak temperature for 80 kW/acre is 170--and we have a summary view graph at the end of this to show you these things--and it's like 147 borehole wall temperature for 57.

We're at 43 years, and basically the whole block is boiling now. 45. But again, we had no 130, no dark blue region. This was the maximum, and it was also collapsing at 200 years, as we saw in the previous case.

30 kW/acre. This is where we've aged the fuel 30 years. It's 60-year-old fuel now. It's emplaced in the

perimeter drift as before, and these get kind of boring because they don't move very fast. The lower loadings are not a lot of fun. Sixty degrees, basically, is here. It's what's set up already. That sort of looks like that gray color, but it's not. Trust me. Six years, seven years.

You will get some boiling in here, but it's going to be probably very difficult to see. I could be wrong, but I think it just stays in this region here. On the simulations I believe I looked at, they're just pinpoints, but it does boil. We did check the calculations, and it does, in fact, go above 95, but you will not get any coalescence because it's certainly not a very fast-moving front and it does not get that hot at the borehole wall.

Twenty years. So you're looking at greens and blues, being mesmerized, no popcorn. I know. This is approximately 70° now around the waste package. This is a yellowish color, which corresponds to what should be about right there. These run approximately two minutes in simulation.

Now, 30 kW/acre for 60-year-old fuel, there might be some questions regarding I said older fuel produced higher peaks. Well, remember, this is at a lower loading. This is at 30 kW/acre. Now, here's the red I was talking about, just a little bit. It's at 40 years. So it does go above boiling.

One meter from the borehole wall, I don't think it even reaches that far, so it stays down below boiling, one meter away. So it's a very localized effect, and when you get to the edges of the panels where you have edges effects dominating, also, you wouldn't get that, probably. You would have edge effects. I looked at the 300-year time step with that and it actually collapses.

The last simulation for the near-field is 22 kW/acre. The waste is now 90-years-old, average. You definitely don't get boiling in this case. It's a very lightly perturbed system. This will be very similar when we look at, later, the additional options for increased heated area, where we extend beyond the perimeter drift. At 19 kW/acre, which is an unscaled value--this is scaled--these are very similar simulations, so this should compare very--almost directly to the 19 kW/acre simulation we're looking at later.

And in this case, let me look at the peaks real quick. The peaks are--borehole wall--actually, it does. At 22, you get one time step of boiling right there. It doesn't extend. See, the canister actually extends from here to approximately here, and you saw a lot of them were setting up right in the center, so some of the canister won't see boiling in some of these simulations, the upper sections or the lower sections. The peak temperature at the borehole wall for 22 kW/acre is 95°, right at. I think it was 94.7, so I rounded it. One meter radially, you only have 91°, and you get no coalescence, of course.

So this is a pretty easy one to trend. There's not a whole lot of surprises in these things.

DR. DOMENICO: How long does it boil?

MR. RYDER: How long does it boil?

DR. DOMENICO: At this loading.

MR. RYDER: At this loading, I think it was on the order of less than ten years, in a very localized fashion.

DR. DOMENICO: It comes in when?

MR. RYDER: I can look that up. I've got the actual model output here with me. I don't recall when. I think it's on the order of 60-70 years, something like that, and it was a very short-term effect. A lot of the others grew--well, we'll probably be able to see it. Seventy years. I think that's that little--no, that's yellow. I lied. 100 years, 200 years. It didn't show up, but it does show up on the actual numerical results. That's supposed to be blue.

Anyway, that's the near-field, so we can stop that and set up the next set, which are much more fun. Let's first of all do the near-field peak temperature summary, though, so that we're all familiar with what we just saw.

80 kW/acre, 170° was our peak borehole wall temperature. It was actually a very long-term peak for

several years, and if you remember, we had the block dominated by 150, minimum, at about 100 and 200 years, and I think it drops off after 300, so it's several hundred years, well above that.

One meter, 158. These are all well below the current goals. 57 kW/acre, we reduce that to 147 and 135 respectively, less long term. We had collapse of the 140-150 isotherm at 200 years.

48 kW/acre, these are, again, 30-year-old fuel down here. The average initial power outputs of the canisters and deposition periods used to emplace them are also on this chart. Time to coalescence of boiling, 12 years, 19, 31, so you get, you know, the increase that you would expect in time to coalescence. You didn't get coalescence at 30 or 22. This is, again, 60-year-old fuel and 90-year-old, but you did have very short-term, very localized boiling at 22 and 90 years.

If you look at the thermal decay curves, and you look between 30 and 90 years, the slope is very shallow. That's why you don't get a great reduction in initial canister power output, and that's why we still have these temperatures that are relatively high, or certainly related to one another; no big drop off. So that's what we just looked at, and what we're going to look at next is the far-field response.

We'll have two sections to that for each. The first one we look at is a vertical cross-section sliced from the surface down to 300 meters below my repository horizon, through Panels 4 and 14, which, from this graph, you see was emplaced in every areal power density we considered, so it was a good choice.

One of the things to look for, especially in the first simulation, is the fact that the boiling isotherm in Panel 4 will collapse at about 1400 years. It will collapse before Panel 14, and it was a little surprising when I first saw that, but it's a function of the edge effects. You see Panel 14 has two neighbors in every simulation, minimum, so it's got a stronger thermal core. It's also longer than Panel 4. It's got about 16 or 17 more drifts in it. Panel 4 only has one neighbor on this side, so it gets edge effects from here, and it's, as I said, about 15 or 16 drifts shorter. So it's a smaller thermal core.

And what that says is that we have to consider heated drift link. We have to consider the actual dimensions--other than the width, et cetera--of the drift when we're looking at the thermal response in the cores, so I was actually fascinated to see that, and you'll see it in all of these simulations, that it does collapse earlier.

Now, the grid that we're looking at, at this, what we did is we composed our model of horizontal grids of 300 points spaced 700 feet apart horizontally, and we took these grids at 10, 30, 50, 70, 90, 200, 300 and 350, I think, and then what we're doing here is just taking a slice through all those grids and showing you that.

What we'd also like to attach to each one of these, is let's look at the 50 meter response--which, as we said before, is a very conservative estimate of the closest approach of where we have goal, the TSw2-TSw3 interface, and this is one of the grids I was talking about, and in this case you'll see the first one will be a 2D representation, very similar to what we saw. In this one, it'll be a 2½D, where the Z axis is temperature. Again, it has a color scale to it.

There is a package appended to all of the notebooks, I believe, that has hard copies of contour plots and surface plots that go along with these videotapes, so if you want to refer to those now or later, they're available to you.

So we're going to look first at the 2D cross-section surface to 300 meters below, and we considered the surface to be 350 meters above the repository horizon in this model, and then we'll look at this horizontal 2½D simulation, and we're ready for Tape Number 2. Again, the color bar is kind of pointless to put up. They don't really match.

DR. ALLEN: Are we assuming real stratigraphy here?

MR. RYDER: No. We assume the whole mass to be TSw2, and we've done some simulations in the past where that's actually shown to be not a bad assumption. The properties are very close to one another.

(Whereupon, a videotape was shown, with commentary by Mr. Ryder.)

MR. RYDER: Again, look for the red. We don't represent the zero point, or the horizon, because it's so difficult to get an average temperature. You see the variations when it coalesces, et cetera, between non-heated regions.

Okay. Time 4, you can see Panel 4 starting to smudge in. I can't believe the colors. Anybody who wants to look at this on a monitor later, we can certainly set up another time to see these. Time 10, 11, 12, if you recall, we had coalescence between drifts, but it hasn't penetrated to 10 meters above or below yet. Panel 14 hasn't even been emplaced yet for 80 kW/acre, so you see no response to that as of yet. I believe it's at about 60° now in this 10 meter section.

Now you can see at 20 years, Panel 14's starting to come in, and one of the things you'll notice, it comes in from the perimeter drift inward. We've got boiling starting at 25 years, and that's how we emplaced our stepped emplacement perimeter drift into mains. This is now setting up 115. We've still got no boiling at 34 years in Panel 14.

Our disturbance is, of course, higher because it's been emplaced earlier. We should get boiling any time now in 14.

One of the things you need to--oh, 115 now--you'll notice these never really connect. We've got boiling started at 42 years in 14 and it's now complete. These two never really connect. You'll see in the simulation after this where we look at the horizontal slice, there's a hump where the mains are, and Tom Buscheck will discuss shedding zones and its potential impacts, and that would be one shedding zone.

We're now in 100-year increments. Two hundred years, we've got a substantial envelope of boiling established in both of them, but you can see this area is starting to collapse, and now the whole region of boiling is starting to collapse in Panel 4, and we explained that earlier. You're also seeing this one collapse now. This one will be gone at 1400 years, and I think this one may have a little bit left at 2,000. Our simulations go out to 2,000 years for this animation. 1500 years, 1600--well, my editor, effectively--or, affectionately, I don't know how affectionately, but he called it the frog-eye plot, and the next one's called the Jello mold plot, so no respect.

Here we are, again, a depth of 50 meters, and it considers the geothermal gradient. We overlaid that onto our results, so that is taken into account. Fifty meters away, you don't expect to see any results for quite awhile. In fact, it's 12 years before you'll see a smudge come in here corresponding with Panel 3, and that is coming up very shortly. The colors are better, because that's supposed to be blue.

Okay. You can't see it yet. There is a little smudge right there, and that's Panel 3 coming in as a darker blue. Panel 4, you can see now just a shadow of 5, 6, and we're at 20-some odd years. And now, see, we didn't emplace these panels, 7, 8, or 9 and 10, so it went 6, 12, so we've got all the panels almost in now, 3, 4, 5, 6, 7, 8--no, 6, 12, 13, 14, 15, and here's 16 coming in right now. You'll notice that they're not coalescing, as you expect, from the 2D cross-section we did, and at 400 years, which is the approximate peaks, or just after the approximate peaks, we'll rotate this and let you look down the mains and see the dramatic hump that we have.

It was emplaced approximately 20 years earlier, so these temperatures are, of course, higher. These will catch up. 100 years, 200 years, we've just got some tips of boiling, and now they're growing slightly. These are shorter panels back here, so the boiling tips are a little more sharp.

Now we're at 400 years and we're rotating. These are supposed to be a pinkish color, which are on the order of

100°, and here's the mains. You look straight down the mains, and you saw this bimodal distribution between them, on each side of them. It's an effect that we suspected, but this is the first time we've really been able to see it, and it's not only a function of the non-heated area that we have --now, they're collapsing, or should be. See, the outer panels, the edge effects have dominated those. They've lost their boiling tips. It's coming in now on all of these, and see, it's going boiling, and this is what we saw earlier. Panel 14, which is about there, still had a little tip of boiling left on it, but that main drift characteristic where you have the two humps on each side is a function of not only the heated area or the lack of heated area in the mains, but it's also a function of the fact where we put the defense high-level waste. When I did the calculations for defense, I got zero after zero. There was not a whole lot of influence.

Again, you see Panel 4 coming in earlier. We're at 57 kW/acre, so the things to look for here are less extensive boiling front. Again, you'll see the collapse of the boiling front in Panel 4 earlier, and less overall disturbance at the far-field.

MR. BRANDSHAUG: Are you simulating the boiling?

MR. RYDER: No, I'm not. It's a conduction-only. Boiling is not simulated. This would be a conservative estimate of how far it goes out. If you simulate the boiling, it will be much--get latent heat effects in there, much compressed. I think we have some studies underway that are looking at that. We had some work done.

Again, we've got boiling set up at least 10, a little over 10 meters away in Panel 4. Panel 14 does not have it yet at 40-50 years. Is that 60 years? Now we do. It's about 100-115° in here. Again, it's setting up in here, also, and the eyes are opening, and they get faster and faster now, because it's 100 years. There you go. But see, this one actually collapses much earlier now. We had 1400 years before collapse at 80 kW/acre. This will be gone, I think, by 900 years, and this one, also, is collapsing earlier and will also be gone by the end of the--well, maybe not. Yeah, I think it will be gone by the end of the simulation.

We have less perturbation out in these regions, if you can compare the two, and now we're below boiling. We might have localized boiling at the waste packages. In this case, we don't, because I looked at the near-field results. If you take this in conjunction with the near-field simulations I showed, you can get a good feel when boiling absolutely disappears.

Now, let's look at the Jello mold for 57. The same delay in when you get to see anything at 50 meters away, but as before, you will see the shades come in for each panel perimeter drift inward. You'll see two extra panels in this case, 7 and 11, but again, we didn't do any half-panels, and we didn't develop the whole length of it.

Fourteen, you can see the smudge coming in. Panel 3, Panel 4, 5, 6, 7. Yeah, you can see them now. It's at 24 years. So some of the trending we're seeing right now is that, certainly, the boiling front is reduced for lower loadings, but one of the interesting things is the dominance of edge effects on a lot of areas that should have, or in the past were considered to have a very strong thermal core.

It was my opinion when I set this up that Panel 4 should be relatively insulated from edge effects because it's in a panel, you know, and it's got several on this side, just like 14, but in fact, it showed that it wasn't the case, that we had collapse of that boiling front earlier than I would normally have predicted. I figured they would collapse at the same time.

Seventy, eighty years, we still don't have any boiling at 50 meters. 100 years. I think we just get little tips of boiling, too. We're at about 75° now at the tips. Again, now here is the edge effects in work again. Panel 3 and 16 and 7 back here didn't get tips of boiling at the times we did the rotation. Again, you see the V effect along the mains, and these will collapse almost immediately. In fact, they do. A hundred years later, they're gone, and the

Jello just melts from there.

Again, edge effects, much lower temperatures, or not much lower, but certainly visibly lower temperatures, and a slight tailoff out here. 1500, 1600. So if you were to change the, or if it's decided that 115 is too high and we go to 95, there are a lot of plots like this that we can look at and see if we--we definitely don't go above 115. Some of these show that we do 95 at certain locations, and we might want to adjust some loadings in those locations and do a hybrid loading scenario. We have multiple APD's to bring those interior temperatures down, and let the edge effects take care of the outer ones. So that's another option.

48 kW/acre, still, we're at 30-year-old fuel in 2010, and I swear that's purple. Now, what you'll see here is, again, a reduced boiling front, the same sort of behavior when it comes in, when you get to see some impacts. They're just much lower on the color scale. No coalescence, as before; much less perturbed system, and actually, if you play horror music to this, it's quite neat when it goes through the big time steps, which we actually were considering doing.

No boiling. We had boiling at the other two loadings by this time in Panel 4, but--there is some localized boiling that we saw from the near-field calculations, but nothing affecting 10 meters above or below. Also, if you go to a model that considers actual drifts, the absence of rock, you would depress the temperatures above the canisters and probably have another rippling effect in your simulations.

Now we've gone 60 years. We've gone to boiling, and also 80 years, it's completed it between the two. 400 years, collapsing, gone; 500 years, it's gone. 600, 700. So we have a very limited boiling front out 10 meters from the package in these cases for 48 kW/acre, which is a low loading compared to what I've looked at in the past, or 80 kW/acre, et cetera.

Now, we've lost actually 75. We're on the order of 50° around the model panels at the end of the simulation, so there's not a whole--it's only, what, 25° plus a little bit above ambient, or the original ambient. Jello mold. Again, the same behavior, and I'll look at my chart here, which is in my hand, and tell you if you get boiling. You do not. 50 meters. You don't get any boiling. 86° is your peak temperature at Panel 14, which is one of the hotter ones through the slice. Same behavior, though. You'll get the panels coming in, progressing around.

This time, actually, there's an interesting thing in this case which again demonstrates the importance of edge effects. See, we've got 3, 4, 5, 6, 7, 8--9 is a half-panel, but you can't see it. Edge effects have dominated that out at 50 meters, and actually smeared it. All you'll get from these effects are a slight decrease in the slope along the sides of these bumps at the edges. This one--which you won't be able to see because we don't rotate that way--will be more sloped because of that half-panel, and 16, also, which is right here.

That was unexpected. I expected to be able to see at least some sort of spike at that point. It turns out you can't. It might be a function of the grid, but from the calculations and the actual full-scale results I looked at, it does, in fact, just modify slightly Panel 16 and Panel--was that 8? Yeah. See, it's slightly more sloped than the one behind it, which doesn't have a half-panel next to it.

Peak temperatures, as I said, were 80, 85, 86°. Rotating around, once again, same behavior. In fact, it's a very low temperature between the two. We're looking at probably no more than 40°, so it's quite low in that reading. Collapsing quickly, 700, 800, and this one's actually already gone back to about 50°, back behind there. Again, here is the slope from the edge effect on Panel 17, a half-panel, so the dimensions of the panel actually have a very strong influence on the thermal core you set up, and the thermal pulse that reaches various fields of interest.

We saw that with the collapse of the boiling front in Panel 4, and we've also seen it now with the half-panels, much more smooth.

Thirty. Almost done with this. In this case, temperatures are very low. They're 10 meters away. We did have boiling, I believe. Yeah, we had boiling at--for 30 kW/acre--again, this is 60-year-old fuel--emplacement start date would be, of course, 2040 then. We had borehole wall temperatures on the order of 100°. One meter radially outward was 97, peak, and we go to 50 meters, all we get is 77°, quite low.

I'm not real sure--I can't recall--if you'll get boiling setting up 10 meters away. You might, but it'll be very late times and will collapse very quickly. I think this is supposed to be a yellow that's on the order of 60°; very light. Very sleepy eyes in this case. And this is kind of interesting. The function of the emplacement, the actual stepped emplacement gets you this region perturbed earlier, so, you know, that might be a concern; also, down below. But in the end, I think you'll, because of the length of the panel, this actually has a larger area of perturbation, because they catch up quickly. See, this one was well ahead. Now they've caught up to one another at 100 years. And at 300 we still don't have any boiling. We're on the order of 75°. That's collapse. We didn't get boiling out 10 meters away, which we recall from the near-field results.

A thousand, 1100, this is on the order of 60°, as is this, but as I said, because of the length of the panel and the scale effects, its effect actually reached further than Panel 4. That's a trend we noticed through all these, and you can verify that.

Okay. Jello mold. Nothing exciting in this one. The temperature, max, very low; 77, and if you were to go to, say, more realistic horizontal cross-sections to represent the TSw2-TSw3 interface, these temperatures would be correspondingly lower. I included those as part of the package; peak temperatures at 50, 70, and 90 meters below, so if you have a question regarding, well, what would happen if I raise the repository horizon to the interface temperatures, that might help you at least start with your answer.

Again, you will not see Panel 17, nor Panel 9 come in due to edge effects, but--and this would--if we had a finer grid, this would be actually a more smooth transition, but there would still be bumps in it. Our grid is spaced 700 feet apart, so it hits the center access drift, or the mid-panel drift and the access drifts, and that's why we get this sawtooth effect. It would be more smooth if we had a better grid.

At 400 years we rotate again; same behavior, same trending regarding down the mains. Again, you see the much sharper little points here on the back saying that we have a smaller thermal core, which just--I found so much stuff when I looked at these, I'm still analyzing the data and trying to come up with all the trending information. Panel 17, you can see, is again sloping it out some.

At 1,000 years, this green, I believe, is on the order of 50°. Panel 8 is brought below that, and they're dropping back this way. 2,000 years.

And the last one, honest, 22 kW/acre, we have 90-year-old fuel now producing approximately .66 kW at emplacement, very low loading. One of the things that was actually interesting about the Jello mold plots are that the defense has a very small influence on it.

At that location, it's on the order of one to two degrees in that region between the panels. I found that actually quite low. We could go considerably higher with those defense numbers and say, take worst-case and see what would happen then, as a second iteration on this.

Twenty-one years. Again, you should see Panel 14 start to smudge through, and there it is. It's smudging as a more diffuse source, rather than showing you the intense--or not intense, but the development this way that we saw earlier, like the burn through the film. Very low temperatures. Deep borehole wall temperature for a very short period of time was 95° in the simulation, 91° one meter radially outward. Our 50 meter peak temperature is--which we'll see later--74°, and that's only three degrees lower than the 60-year-old waste. That's a lot of years to wait just to get three degrees.

And in terms of borehole wall temperatures, you know, again, it was only on the order of seven or eight degrees difference, so aging may not be the solution if you want to keep temperatures down. We're going to look at additional options after these two simulations just very quickly, where we increase the area, and it will look very much like the near-field one for this case. We didn't even get above 60 or so degrees 10 meters outward, and again, we've got complete collapse of temperatures.

Scale effects, again, more reach on this effect. More reach from a larger panel. 1800, 2,000, okay. One more Jello mold plot, and then we're on to the last section of this. As I said, the peaks in this case for the additional 30 years aging only dropped three degrees from the 30 kW/acre case, which was 60-year-old fuel; 74 is your peak temperature. Again, it's all the panels. You can't see 17 or 9, and you'll see it come in about the same time, usually. Thirteen to fifteen years, you can usually see a smudge come in at Panel 3, and I lied. It's 17 years already, and there it is coming in.

Now, again, this was done on a 20-year deposition period so it would match a baseline with that drift spacing. Deep borehole wall temperatures would match, and your far-field would be slightly higher because the baseline would have younger fuel and lower 50 meter temperature response. Slow-moving, nothing to it. And here you can see the length of the panels again, much shorter on this side of the mains. These are on the order of 66 or 70--66 to 70 drifts, I believe, 14 through 16, and these are on the order of 55 drifts in this case, and these are substantially truncated out here, and again, are dominated by edge effects.

Fifty degrees. About sixty degrees now. Rotate at 400 years as before. Same effect. A little higher because we're at lower temperatures, you know, where it's this section of it, a large portion of the actual disturbance, or a percentage, correspondingly higher percentage is shown.

A thousand years. Again, as before at 30 kW/acre, we saw these collapse. Again, it's a scale effect, the edge effects coming into play on these shorter, truncated panels, and the slope coming up from 17. 2,000 years. So I believe that's it for the far-field.

There's one short tape now that's about eight minutes long.

MR. GERTZ: Eric, I wonder, since it is getting late, if--well, Don, would you want to ask some questions and then let Eric show the other tape later on for those who are interested?

DR. DEERE: The other one's one at 8 kW with cooling?

MR. RYDER: There's increased heated area. We look at the 19 kW/acre, where we--the current heated area in the SCP/CDR design is on the order of 1,028 years. We'd have to go to a little over 2100 heated--actually actively heated acreage to do this, which means we'd go into expansion zone--probably six, like Mike pointed out, into this region.

There is the area for it, so--but it shows a near-field simulation out to 100 years--or 200 years for that, and then the modified ventilation system looks at 80 kW/acre and shows you the reference for a quarter, and then five-year and ten-year ventilation.

DR. DEERE: Do you have that on the tape?

MR. RYDER: That is on the tape, yes, and it's about eight to ten minutes long.

DR. DOMENICO: Could I ask one question, one quick question? If the objective was to prevent boiling and you went with the 22 kW/acre, you said that's an average of 90-year-old fuel. How many years would--when would you start loading?

MR. RYDER: You would start in 2070 would be your first year of emplacement. So it really, and what you'll see from this additional option, when we increase the area that's available outside the perimeter drift now--it's just a similar near-field situation--what you get in this case is you do start in 2010, but you also limit the boiling.

So I think there's an optimal solution. If you wanted to go to lower loadings to limit boiling, there's an optimal solution between increasing area and aging somewhere. Starting in 2070 is kind of a "fur" piece.

(Whereupon, a videotape was shown, with commentary by Mr. Ryder.)

MR. RYDER: As I said, this is very similar, and if you want to, I have color stills later if anybody is interested. We can look at 22 kW/acre that you age the out to 90-year-old fuel to 2070 as a start date, and this is 2010 start date, 30-year-old fuel. You don't get boiling, as far as I know.

DR. ALLEN: Why isn't there identical symmetry above and below zero? Is that grid spacing or something?

MR. RYDER: Well, there actually is. It's just fuzzing out on you. It goes up 9.38 meters or--yeah. There actually is in the grid. I think you're getting fuzzing. See this section right here? That's really not there. You don't see that.

MR. GERTZ: But it looks like the yellow is--

MR. RYDER: Oh, you're talking about this not being nice and smooth. That's a function of the grid. The grid's relatively rough. If we had a nice, fine grid spacing, it would be very smooth.

DR. ALLEN: And it would be identical above and below?

MR. RYDER: Yes; absolutely. This would normally be very, very smooth. There would be a slight dimple here because that's where the center of the source is, so it would be that, but very smooth.

Again, as I said, it's very similar to the other, and if you want to just fast forward through this to get to the other one, that would be fine. Nothing much happens.

Here we go to the modified ventilation system. We're just going to look at a corridor now. We're not going to look at the full 10 meter up and 10 meter down. We're just going to look at 10 meters above. The ventilation sink that we put in is 30 kW, and we turn it on for five--well, this is unvented, for reference, and it's about--and 8.4 meters is the center point of that 3 meter radius ventilation sink. This is unvented. This is just like--this is just for reference--you saw earlier. It's the same simulation. You'll get coalescence at 12 years. We only go out to 100 years on these.

Sixteen. Again, you've got this setting up, the 140°, 130 setting up. Time, 20 years, 21, 115 is beginning to dominate at that section of rock, and again, this would be much smoother if we had a finer grid. In fact, it's probably just a fraction off between grid spacing, so the post-processor draws that little jog in it.

Okay. So this is the reference case if you'll compare the ventilation to it. A couple of things to notice were, one, when the coalescence of the boiling front occurred--which was at 12 years. Another one is coming right now at 41 years; 130 dominating the system, and about a 4 meter radius of the darker blue, 140-150° range. What you'll find in the five-year and ten-year, that what to look for are retardation in the coalescence, depending on the amount of time you spend ventilating; and also, retardation of when this 130 dominates, and a slight distortion of this blue region, depending on the time.

So at 100 years, it's all blue. As we saw before, 100 and 200 years were all blue; 140-150°. We vent this for five years. We turn this sink on for five years at 30 kW, which is consistent with some work done previously by some ventilation people regarding sensible heat loss due to ventilation systems. If you add latent, it would certainly be much higher. I've seen calculations that raised 30 kW up to 90, but it will be short-term 90 because you remove the water.

Fourteen years, fifteen years. So we delayed the coalescence of the boiling front by three years with a five-year ventilation system. Not terribly significant, I don't think.

MR. GERTZ: This is drifted panel ventilation?

MR. RYDER: Yes. This is just the central canister exposed to emplacement drift ventilation after emplacement.

The current design actually calls for a very short period of ventilation; two years, I think, during construction, and then a fraction of a year when you emplace the drift, because then you close it off. Then it's leakage airflow, which is a negligible effect. In fact, the more prominent effect I think you'd get during construction is water removal during the construction period by the ventilating air.

If you remember, this was actually much farther out for the unvented case, but for five years, you don't get a very significant effect. You get a slight retardation in it, but it will eventually dominate this block that we've modeled. Five years is almost doubling the current SCP design in terms of total ventilation time, and it's certainly much longer than when you actually ventilate when you're emplacing.

These are preliminary. We're going to continue the studies. At 49 years, we've now gone about 130 across, whereas, earlier, it was about eight or nine years earlier than that, I believe. But we still have this front coming through, and at 100 years, it's just that section up above that was not at blue, or at 140.

Ten years, you get a little more significant effect, but that's quite an increase in the ventilation system. The sink is still on. You'd see more of a distortion, but we placed our grid points so that it didn't really give you a false sense of what's going on, so one more time step, the ventilation--no, two more time steps. Ten is the last year of ventilation. We've retarded this somewhat. Remember, one year from now in the unvented case, it had already coalesced. The ventilation is now off. Fourteen years. Fifteen years is when we coalesce for five years ventilation. We double the ventilation--I forget when it coalesces, so we'll learn together.

Seventeen. We still haven't set up a dark blue region of 140. We've got a little bit of 130 starting down here. Twenty-one years, I guess, is when it coalesces. It's a little different shape, and it was also delayed by--I'll do some math quick--nine years. Now we've got some dark blue setting up. It's much delayed, though, so it has had an impact. Ten years of ventilation has had an impact. In terms of final impact, though, I believe it's--

DR. LANGMUIR: But another implication of the ventilation for ten years is you take all the moisture out of the system while you're ventilating it, so even when you get to boiling, there's not much there.

MR. RYDER: Yes. Right. And that's actually why I went with the conservative 30 kW sink, because it will go up, maybe briefly, up to 90 kW or so in terms of sink strength, but once the water's gone, it drops back down to a sensible heat loss, along the order of 30 kW for these kind of temperatures.

Forty years, we had already seen this 130° isotherm move quite far out, but instead, it's about 15° lower, and it's only 115 in this region. But we're beginning to see this hotter region grow, whereas, before, it was suppressed during the time of the ventilation and for a period afterwards, but it will eventually still go out quite a ways. It won't be all blue, as it was in the unvented case, or 99.9 per cent blue with the five years, but it's--so it's 30 years delay, approximately, before we go 130; at 90 years, 100 years, which is the end of our simulation, it was only about halfway out, but it was on its way, and it doesn't collapse.

Actually, if you look at the data, it doesn't collapse. It continues to grow out.

By extending the heated area, you can virtually eliminate the boiling front. It takes a very large increase in that area, though; over doubling it, the heated area. I would say the optimal solution is probably some combination of increased heated area and aging, if you were to go to a lower loading.

DR. DOMENICO: Is that a simulation result? You say double.

MR. RYDER: Well, if you go with--what you saw for the increased heated area is a squarer rate, where you got equal drift and equal canister spacings. Based on what you saw, that would require 2100 heated acres of area as opposed to--

DR. DOMENICO: In other words, we could start loading in 2010.

MR. RYDER: 2010.

DR. DOMENICO: And maintain conditions with low boiling if you double the area, is that what you said?

MR. RYDER: With the understanding that you have to choose the proper assemblies. You have to go with something like a levelized receipt schedule. If you went with other receipt schedules, this may not be possible. You would still have probably localized boiling around a few packages for a couple of years simply because, as you remember from the curve, you have a slight tail up on the levelized schedule. And I think if you have loadings on the order of 1.6 to 1.7 kW/canister, it, by itself, isolated, will produce boiling, but we were on the order of 1.5 in 30-year-old fuel. So we actually just got under that for our average. So there's a lot of worst-case scenarios that have to be done.

Ventilation can be used to mitigate the near-field thermal response, but as you saw, the magnitude of the effects appear to be relatively small--especially like for five year. For ten-year, you do have a delay, but I think you still have the heating up at 80 kW/acre of that block region, too, on the order of 130-140°. So it's more of an operational kind of thing that would be kind of nice if there were a problem with that, but we still show that we meet all goals for retrievability, et cetera.

DR. DEERE: Okay, thank you.

I have a question. Could you go with the ventilation to allow you to go to the 80 kW/acre and still meet your criteria?

MR. RYDER: 80 kW/acre meets the criteria by itself now, without ventilation.

DR. DEERE: Without the ventilation.

MR. RYDER: Yeah. Our peak temperature borehole wall was 170. We still meet the 50°C for 50 years in the panel access drifts. We meet all the criteria by a good margin.

DR. DEERE: Okay, thank you.

Questions from the Board?

DR. BARNARD: Eric, you've got a slide here labeled, "Available Area," that you haven't shown us. Could you put that up there and explain that?

MR. RYDER: I skipped through it, yeah.

Yes. This is actually a repeat of what Mike Voegele showed you. It's from the same report. We have a little disconnect in how much area is in the primary block. We need to look that up. This just lists the acreage from that same report that Mike Voegele discussed in each one of these. There's enough acreage in 6, which is considered the

--I guess this is the primary expansion zone, is that true? 6 and 2. There's enough area in these two to do what I was talking about in terms of increasing the heated area if you wanted to. Is that your question?

DR. BARNARD: So those areas and acres are approximately right?

DR. VOEGELE: I will check those numbers this evening and tell you.

DR. BARNARD: I added them up and I end up with around 9,000 acres.

MR. GERTZ: Yeah, but when I asked Mike about what was available, he didn't--my answer wasn't intended to be all that. It was the 2,000 in the Area 1 or so, approximately, and the 2200 between 6 and 2. So he considered those, I think, as prime areas for expansion. The others are still in the more questionable, but potential; so you're right, 6,000 potential acres. Four thousand relatively may be pretty good, but we're only looking at about 1500 right now.

MR. RYDER: It's just a section of 6 that is considered; right? It's like this section and then this section up here, so it's not all these numbers. It's a portion of those numbers.

DR. BARNARD: Okay. Thank you.

MR. McFARLAND: Eric, would you explain your comment on ventilation? Is this what we perceive, you're merely ventilating the drifts and it's the heat that is radiated into the airstream from the rock that's heated by the canister. You're not in any way trying to get heat into the canister.

MR. RYDER: No. I looked at some past reports done by Parsons in their design phases, and they did some ventilation studies, and tacked on to the end of those were sensible and latent heat losses due to ventilation from their simulations. I took one of those numbers that was conservative for sensible. As I said, latent would be a short-term effect until you boil the water away, and I just used that as my sink strength, 30 kW.

MR. McFARLAND: One other question. In your summary, your modification summary chart, you indicate that the SCP/CDR design had 12,000 spent fuel containers.

MR. RYDER: Approximately, I think that's correct.

MR. McFARLAND: And in the modified design, you jumped to 31,000. What was the factor that shifted from 12,000 to 31,000?

MR. RYDER: It's from the first talk. If you look at how many consolidated assemblies go into the old SCP design. Anyway, Livermore can probably help me out with how many consolidated assemblies went into a consolidated package. It's a function of that. We get only seven assemblies into the unconsolidated one now for the hybrid case, and I believe it was--somebody throw a number at me. Six PWR? Okay. There were more BWR's, I would think.

DR. DEERE: We have time for one more question.

MR. DANKO: George Danko, Mackay School of Mines, University of Nevada.

I would like to have a question about this cooling by ventilation, if it was the research report made by Parsons & Brinkerhoff, using the simulation of Professor --?

MR. RYDER: I don't believe so. It was Mine Ventilation Services did it as a subcontractor. Okay. It was the CLIMSIM Code, I believe.

MR. DANKO: Right. You have got to be very careful when you use CLIMSIM, because that was developed for a very ordinary mine-type simulation.

MR. RYDER: That's very true.

MR. DANKO: And that is very critical to apply for a heat source distribution with the canisters.

MR. RYDER: That's correct. They have made modifications to the CLIMSIM Code to make it more applicable to this sort of work, but 30kW, it may be too low. It was an estimate that I was able to grab quickly. I think we need some refinement in terms of our discussions of ventilation. As you see, we've pretty well done the conduction over a suite of APD's. Now we need to look more at extensions of area and what we can get in tradeoffs, and also, ventilation.

MR. DANKO: Yes. I think it's reasonable to remove that much heat, and I have a comment on using a thermal simulation model, assuming only heat conduction in the rock.

These kind of models are used in a thermal simulation for deep rock underground mines, and those are very good models because they give a quick solution and are relatively easy to model a large area. However, they have credibility only for relatively low temperature, below boiling point, and they use a modified thermophysical property for both their conductivity and diffusivity, and those called effective properties, which include a certain modification which makes adjustment on the simple conductivity towards the enhancement by underground connection.

Now, this difference between a laboratory reading and an in situ thermophysical property can be 10 per cent, 5 per cent, 20 per cent, 30 per cent. This is the range in deep underground mines thermal simulation; 50 per cent or maybe even 100 per cent. I have never seen this question addressed in the site characterization plan. If there is a future plan for a simple conduction model application in the program, then I very much suggest to look into the possibility of measuring in situ thermophysical properties in the exploratory facility, and have a good number of readings on rock formation, which includes the natural flows and convection and whatever within the site, and use these site characteristics with these simple models.

But I can see it yet, when you use a simple model with laboratory data, or when you use a very complicated model with hydrology and convective effects plugged into the model, between these two, there could be a middle way; a simple model, and then adjust the thermophysical property, and that is the present day practice in deep underground mines, for the underground mines which can be an analogous program solution so we can learn from this area.

MR. RYDER: I believe that's why we do sensitivity tests, so we bracket what we'll learn at the site, and we can just go across and see our results, and pick the appropriate ones when we find better site data.

DR. DEERE: Thank you.

MR. BUSCHECK: Tom Buscheck, Lawrence-Livermore.

I wasn't intending to answer that, but tomorrow, I'm going to vindicate Eric in terms of his conduction model. In fact, you know, it is--well, I'll go into that tomorrow.

One point that I think needs to be clarified is this criteria he was using, which I think it's fair to say it was used for the sake of illustration. That TSw2-3 contact temperature, there's nothing really magical about it, and I was talking with Schon Levy from Los Alamos, and she agrees with me that a 115 criteria for that contact doesn't necessarily relate to any performance issues at this point.

So that was--I think it's fair to say it was used as an illustration of how you would use this procedure, but, you know, I don't want the Board to attribute that number at that contact as being very significant at this point.

DR. DEERE: Thank you. I wondered about that.

MR. RYDER: It'll be addressed in uncertainties and how important it is, and that's actually what--some of the points were made that we need to look at the goals rather than the modeling. I mean, we've got full suites of models that we can look at and change goals, and see what the effects are.

DR. DEERE: Eric, thank you very much, and I also wish to thank you, Carl, and for your whole suite of speakers this afternoon, and also for our speakers this morning, and the audience for their endurance. We think it's been a very productive day. We certainly do appreciate it.

MR. GERTZ: Thanks, Don. Unfortunately, I have to catch a plane tonight to Washington to make sure we have appropriate funds in out years, so I hope you and Max can keep the team together tomorrow.

DR. DEERE: Get going.

MR. GERTZ: Keep everybody in line. Thanks.

DR. DEERE: We'll see everybody at eight-thirty in the morning.

(Whereupon, the meeting was adjourned, to reconvene at 8:30 a.m. on October 9, 1991.)

APPENDIX A

ERIC RYDER

VIDEO PRESENTATION