UNITED STATES OF AMERICA

NUCLEAR WASTE TECHNICAL REVIEW BOARD

***

COMBINED HG&G/SG&G PANELS:

MEETING ON THERMAL MANAGEMENT

FOR A HIGH-LEVEL REPOSITORY

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

Thursday, November 17, 1994

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

PARTICIPANTS:

John E. Cantlon, Chairman, NWTRB
Clarence R. Allen, Member, NWTRB
Edward J. Cording, Member, NWTRB
Donald Langmuir, Member, NWTRB
John J. McKetta, Member, NWTRB
Patrick A. Domenico, Consultant, NWTRB
Dennis Price, Consultant, NWTRB
Ellis D. Verink, Consultant, NWTRB
William D. Barnard, Executive Director, NWTRB
Russell McFarland, Senior Profesional Staff, NWTRB
Victor Palciauskas, Senior Professional Staff, NWTRB
Leon Reiter, Senior Professional Staff, NWTRB
Daniel Fehringer, Senior Professional Staff, NWTRB
Carl Di Bella, Senior Professional Staff, NWTRB
Daniel Metlay, Senior Professional Staff, NWTRB
Sherwood Chu, Senor Professional Staff, NWTRB
Nancy Derr, Director, Publications, NWTRB
Paula Alford, Director, External Affairs, NWTRB
Frank Randall, Assistant, External Affairs, NWTRB
Dennis G. Condie, Deputy Executive Director, NWTRB
Ron Milner, OCRWM
Dan Dreyfus, OCRWM
Stephen Brocoum, YMSCO

PARTICIPANTS [continued]:

Samuel Rousso, OCRWM
Alan Brownstein, OCRWM
Jeffrey Williams, OCRWM
Donald (Buz) Gibson, M&O, TRW
Steven Saterlie, M&O, TRW
Michael Voegele, M&O, SAIC
Ardyth Simmons, YMSCO
Dale Wilder, LLNL
Todd Rasmussen, University of Georgia
Karsten Preuss, LBL
Scott Sinnock, M&O, Las Vegas
Ed Taylor, M&O, Vienna
Larry Ramspott, M&O
Thomas Buscheck, LLNL
Randy Bassett, University of Arizona
Jean Younker, M&O
Dan Bullin, Iowa State University
Richard Codell, NRC
Dan Kane, Department of Energy
Joe Stringer, M&O
J.J. Farrell
David Stahl

C O N T E N T S

C O N T E N T S [continued]

P R O C E E D I N G S

WELCOME/INTRODUCTION/OVERVIEW

DR. CORDING: Good morning. We're ready to start.

My name is Edward Cording. I'll be chairing the meeting today.

My background is civil engineering. I'm at the University of Illinois.

I'd also like to introduce to you my co-chairman for the meeting who is Don Langmuir, over here, professor emeritus of geochemistry at Colorado School of Mines.

I'd also like to introduce today the colleagues on board and the staff at Nuclear Waste Technical Review Board, and they include on the left over here, John Cantlon, who is board chairman, and John's field is environmental biology, former vice president for research and graduate studies at the Michigan State University, presently professor emeritus there.

We have also next to him Clarence Allen, professor emeritus of geology and geophysics. That's at California Institute of Technology.

John McKetta on my right, professor emeritus of chemical engineering at the University of Texas.

We also have Patrick Domenico, professor of hydrogeology at Texas A&M University, that's here on my left.

And then Dennis Price, professor of industrial and systems engineering at Virginia Polytechnic Institute, on the right.

Ellis Verink is with us. He's distinguished service professor of metallurgical engineering emeritus, University of Florida.

I'd also like to introduce two outside consultants we have with us for the meeting, Randy Bassett, who is professor of department of hydrology and water resources, University of Arizona, I believe, behind here.

Thank you.

And Todd Rasmussen, who is assistant professor, School of Forest Resources, University of Georgia.

Thank you.

Randy and Todd were involved with some of the thermal hydrogeological programs sponsored by NRC at the University of Arizona several years ago, and part of the team that wrote a report of validation studies for assessing unsaturated flow and transport through fractured rock.

I'd also like to introduce some of our staff. We have a number of staff present with us today, but two of those who are with us at the table who helped coordinate this meeting are Russ McFarland on my right and Vic Palciauskas on the left.

Thanks.

As we speak today, I'm learning how to use this microphone, but I understand we're going to need the mikes fairly close to make the system work, and so that the reporter can hear what is being said, as well.

Just a few comments I'd like to address at this point.

You may recall our first meeting where we talked about -- at least, we were focusing the meeting on thermal loading and repository design issues. This was held in Las Vegas in October of 1991. The purpose of that meeting was to review the rationale and effects of various thermal loading strategies related to the design and performance of a high level waste repository.

Subsequently, some of the conclusions of the board were published from that, on that topic, in its fifth report to Congress and the Secretary of Energy.

The report contained two chapters on the thermal loading. Some of that material, I believe, some of those chapters will be available later today. They made several recommendations in that report. I just wanted to read two of those to you.

One was that the board recommends that the DOE thoroughly investigate alternative thermal loading strategies that are not overly constrained by a desire to rapidly dispose of spent fuel. This investigation should involve a systematic analysis of the technical advantages and disadvantages associated with the different thermal loading strategies.

An assessment of each strategy's implications for other elements of a waste management system also should be undertaken.

And the second point, in assessing the different thermal loading strategies, it is critical that special attention be paid to evaluating the uncertainties and, in particular, the critical hypotheses associated with each strategy.

Certainly, there's been a lot of movement and a lot of work on thermal loading within the DOE and its contractors since that time, and today's meeting entitled Thermal Management for a High Level Repository will be focusing on those issues. Some of the -- we've seen a broadening of some of the potential thermal loading strategies, or looking at a broader range of potential thermal loadings for the repository, there's been a lot of work that's being carried out and we're going to be very interested learning in this next two days, today and tomorrow, the results of some of the studies and the progress that's been made in understanding the thermal management approaches.

We would like to understand the direction that

OCRWM is -- will take, in the next five years in this area in order to meet its objective of seeking a license to construct a repository by 2001.

Today we're going to concentrate principally on management strategy, thermal management strategy, management options, site response to thermal loading.

Most of the emphasis tomorrow, then, will be on looking at the analyses, the in-situ testing requirements and information from people that are doing the work, as contractors to DOE on evaluating thermal -- the technical aspects of thermal loading.

You'll notice that in our agenda today we've scheduled a round table discussion at the end of the presentations, and we will also have a round table discussion tomorrow at the end, and that's a presentation -- or, a period that we really want to have all of you involved, particularly, the speakers available at that time, as well as the board, its staff and the audience, in general.

So, we're going to include as much as possible the interests and the comments that will be -- we're looking forward to receiving from all of you.

And, then, I think we will also, if we have time, and we hope to do that by keeping on schedule, to have the speakers themselves be able to answer questions during the presentation. So, we'll try to stay on schedule and have time, even after the speakers make their comments, make their presentations, to have questions and things -- questions and time to discuss things from the floor, as well as from the board and its staff.

So, I'd like with that to go to our agenda and look at the first presentations. Again, we're talking about this morning the program approach, thermal management strategy on several presentations, and we're happy to have with us today Ron Milner, who will be making the initial presentation from the OCRWM office.

DEPARTMENT OF ENERGY PROGRAM APPROACH

THERMAL MANAGEMENT STRATEGY

OFFICE OF CIVILIAN RADIOACTIVE WASTE MANAGEMENT

PROGRAM APPROACH

[Slide.]

MR. MILNER: Dr. Cording and Dr. Cantlon, members of the board, I'm happy to be here today and talk about a timely important topic.

I'm the acting director of the program management office of the Civilian Radioactive Waste Management Program. It's a new office that was created as a result of our reorganization this past summer.

One of its primary missions is to help insure integration of the different elements of the program, so thermal management is certainly a topic to start with.

Before I really get started, I'd like to, if I may, introduce a new member of the OCRW staff, Dr. Stephen Hanauer, who has recently joined us. Dr. Hanauer is coming on board as a senior technical advisor to Dr. Dreyfus and, while not trying to go through Steve's long, impressive credentials, and -- they're too many for me to remember and I'd probably use up all my time.

[Slide.]

MR. MILNER: I'd like to start off and emphasize two points. We certainly recognize the points that the board has made in the past, one, the importance of thermal management and, secondly, the fact that it very definitely is a system position. Our thermal management strategy is still in the early stages of development. I think we have made considerable progress thus far in that area, but certainly there is still a ways to go, so, essentially, what you're going to be going to be hearing today and tomorrow is basically work in progress.

[Slide.]

MR. MILNER: I know the board has been briefed in detail on the program approach that we've been using. I would just say thermal management is considered in the development and that is very much an integral part of the revised program.

[Slide.]

MR. MILNER: Basically, two points have been a guide or an approach to thermal management. One is that we want to emphasize prudent development of our designs consistent with the development and acquisition of information on the repository. Secondly, we recognize that changes could have a cost and schedule impact, therefore, we need to maintain a flexible design and a flexible approach to thermal management

[Slide.]

MR. MILNER: Thermal management certainly is a systems issue, but it is a two-sided coin, I guess you might say. The repository design certainly has to consider the impacts on multi-purpose canister in the rest of the system.

On the other hand, the activities in that area, be they waste acceptance or storage or transport, how that function works can certainly impact and, perhaps, assist in the repository design.

[Slide.]

MR. MILNER: The graphic illustrates basically three elements of the system that are very much a part of thermal management. Essentially, you can look at it and there are three valves in the system, if you will, that can at least moderate the thermal flow, if you will, of the system.

On the utility site, waste selection currently, according to the contract, fuel is accepted on an oldest fuel first basis. However, that's an allocation accruing to the utility and not necessarily indicating the specific fuel. There may be things there that can be done to select the fuel that's taken into the system. Do you take older, cooler fuel or the younger, hotter fuel? So that can have some impact on the thermal input of the system.

In the storage area -- and whether that storage is at the reactor or at the repository or at an interim site -- you may be able to do some things in that area which can assist in the thermal management of the system, age of the fuel and, again, waste selection or blending at the repository and there are a couple of things that can be done, how you go about in placing the waste, the areal density and so forth, and then what you do in preclosure operations in terms of ventilation, perhaps, or other things.

[Slide.]

MR. MILNER: Looking at thermal management overall, there have been a number of top-level objectives that have been determined for the system. These particular objectives are for the repository site, to develop a cost effective thermal design, a design waste package compatible with the MPC, and to design an underground facility to achieve the necessary thermal conditions.

[Slide.]

MR. MILNER: The waste acceptance storage and transportation site. These are the objectives in that area.

A couple of things. One, there certainly are technical constraints, if you will, on the system and then you have opportunities in the same regard. But there also is an institutional environments. I wouldn't call them constraints, but certainly situations that we must work under, and that also impacts the objectives and the strategy.

[Slide.]

MR. MILNER: This is a list of some of the key milestones for the program relating to thermal management. We want to begin deploying multi-purpose canisters by 1998. We would like to be in a position to make the determination as to the technical site suitability in 1998, as well, submit a license application in 2001, and then an update of license application in 2008.

[Slide.]

MR. MILNER: I will just summarize the thermal management strategy at this point, and Steve is going to go to that into that in a lot more detail, but, essentially, we want to develop a flexible thermal design, conduct the evaluations early on for a low thermal loading scenario in the repository, and then, as data is gathered, additional date is gathered, conduct evaluations that would look into higher thermal loading and see if that will improve cost and performance. Subsequently, of course, we would do performance testing of the strategy that was selected.

[Slide.]

MR. MILNER: Essentially, there's a brief rationale for the strategy. Strategy doesn't necessarily go into all the details of the physics or any academic arguments, in terms of whether it's better to have a hot or better to have a cold repository. It looks at more of the pragmatic things that we have to face. One is that we have to recognize that we are going to have to deal with cold waste, as well as hot waste, so we need to have information both in terms of colder repository, as well as hot. We need to proceed with slow areal loading scenarios early on, and look at the higher densities later on. And, as I mentioned before, we want to maintain flexibility as we go through that.

These are some of the top level milestones. Again, I won't go through all of these, but simply the top level milestones that affect thermal management and I think all the rest of the speakers today and tomorrow will be going over in detail the activities that support these milestones.

[Slide.]

MR. MILNER: Lastly, again we recognize that thermal management is a systems issue, a cross-cutting issue. We have a plan to arrive at a thermal design that is consistent with the program approach and maintains flexibility and, again, I'd like to stress that we really are in that process and what you're going to be hearing is work in progress and very much welcome the board's advice in that regard.

If you have any questions, I will be happy to answer them.

DR. CORDING: Any questions?

[No response.]

DR. CORDING: Bill?

DR. BARNARD: Bill Barnard, Board Staff. Ron, you laid out some thorough management objectives in one of your slides. I don't think anybody disagrees with any of the objectives but some may be incompatible with one another. Are there some that are more important than others?

MR. MILNER: No, I don't think so and I guess I would not see that there's an incompatibility there.

DR. BARNARD: Well, for example, some of your thermal loading schemes may be more cost effective than others. The question is what is more important, the thermal loading or the cost effectiveness?

MR. MILNER: I think in the overall obviously the thermal loading and the performance of the repository certainly cost is a factor, although not necessarily the most important one.

DR. BARNARD: Thank you.

DR. CORDING: Ron, the schedule for the thermal management describes the first heater test starting in about '97, is that correct, and the second one about somewhere in '99? Is that -- is that on schedule at the present time?

MR. MILNER: Yes, that is the current time.

DR. CORDING: So the results of the first heater test would, some of those results, is it intended that those would be available by 2001? Is that essentially what the schedule would be on that?

MR. MILNER: Yes, I believe that's correct. Steve, if you could answer that?

DR. BROCOUM: Yes, we would hope to have some of the earlier, some of the results available with the early applications.

DR. CORDING: Probably not; the results really wouldn't be available by the '98 date. Is that correct?

MR. MILNER: That's correct.

DR. CORDING: For those tests. I look forward, I think as we have discussions today and tomorrow, all those will be integrated into the plan and the testing. That will be interesting to hear.

Any other questions or comments?

[No response.]

DR. CORDING: Thank you very much, Ron.

The next presentation is Steve Brocoum, who is Assistant Manager for Licensing for the Yucca Mountain Site Characterization Office in Las Vegas. Steve?

[Discussion off the record.]

OVERALL COMPLIANCE STRATEGY

FOR MINED GEOLOGIC DISPOSAL SYSTEM

[Slide.]

DR. BROCOUM: We are going to talk about overall compliance strategy for the MGDS. My name is Steve Brocoum

There are basically four things we are going to talk about.

[Slide.]

DR. BROCOUM: We are going to summarize the program approach and I have one viewgraph on this and talk a little bit about this and talk a little bit about regulatory strategy and how it fits the program approach.

[Slide.]

DR. BROCOUM: We are going to talk about thermal management strategy and try and find the findings consistent, and finally we will mention where we think we will be at each key decision point regarding thermal loading.

We said all this before. The "program approach" and that focuses initially on past analyses that are most critical to evaluating site suitability and supporting the NEPA process. If the site is found suitable, we shift our data collection and analyses where we need to provide a complete license application that allows us to get construction authorization from the NRC.

We want to, in our license application, provide a high degree of confidence for the safety of repository operations and waste package containment. We will be relying, where we don't have enough information or final information, on radionuclide transport that will accommodate our range of possible site conditions including a range of possible thermal loadings.

Again for site suitability we are focusing on the site. Later on we focus more on the whole system and, through time, on increased confidence in the long-term performance, confirmation program that will start some time before site characterization is finished.

[Slide.]

DR. BROCOUM: Our "regulatory strategy" -- as we go through the whole regulatory process we want to be able to demonstrate compliance with any information that we have available at each given milestone. We want to, and the NRC requires, a defense in depth by multiple barriers. The natural barriers provide defense in depth by shifting the focus to the timeframe of geological processes. In the long, post-closure period, engineered barriers contain the waste, inhibit release and the transfer of radionuclides to the geosphere and help where we have residual uncertainties in natural barriers and help give us confidence in that strategy.

[Slide.]

DR. BROCOUM: Our whole goal in the year 2001 is to submit a license application that will allow the NRC to reach a finding of construction authorization and a finding of reasonable assurance. We're going to depend on flexible design, conservative analysis and comprehensive plans.

[Slide.]

DR. BROCOUM: The whole strategy has three elements -- demonstrating safe repository operations and the ability to retrieve, demonstrating the ability of the engineered barrier system to contain wastes and inhibit radionuclide mobilization to offset the uncertainties, and to rely on realistically conservative performance assessments to provide reasonable assurance that post-closure performance objectives can be met. And I'll talk a little more about each of these three.

[Slide.]

DR. BROCOUM: Demonstrate safe repository operations and ensure retrieval option exists. Obviously we want to define the design basis events and identify the system, structures and components that are important to radiological safety, waste isolation, and retrievability. For each of these we want to provide the appropriate level to meet the design. For example, for the waste package we are going to have a final design.

Some things are way off in the future like seals. You don't need a final design in our license application but we need to have that design flexibility so we can complete the design of the seals later.

We want to be able to provide analyses and assure that we have control mechanisms to preclude any criticality type events during operation and any nuclear facility needs to have quality assurance, training, emergency plans and proposed operating procedures.

[Slide.]

DR. BROCOUM: The second element, demonstratability of the engineered barrier system to contain waste and inhibit radionuclide mobilization, again to compensate for the uncertainties. We want to develop a flexible repository design that allows for a range of placement strategy. That is very important.

In getting ready for this meeting we had a lot of debate with engineers on all of this.

We want to evaluate alternatives to major design features important to waste isolation, that is important from a regulator's point of view. It is important in 60.21 of 10 CFR.60 and we don't necessarily have to pick the best design feature in every case but we have to evaluate different alternatives.

We must, we feel, provide a robust waste package design to maintain substantially complete containment for at least 1000 years. And we want to remain flexible, not eliminate any potential options that may be shown later to enhance our long-term performance, allowing the NRC to reach a reasonable finding, so we want to if possible evaluate that.

[Slide.]

DR. BROCOUM: Rely on realistically conservative performance assessments to provide reasonable assurance that post-closure performance objectives can be met. We want to allocate performance to a robust EBS. You have compensate for uncertainties to the natural system.

We want to provide realistically conservative analyses of natural barriers that are consistent with the available data.

We want to be able to reduce this conservativism as we collect data through time, thinking ahead what might happen with that whole 801 process of the National Academy of Sciences report. What might then happen in any rule-making at the EPA and subsequently NRC, we have to plan but we may end up with a dose standard, some sort of a dose or some change in our standard or modified current standards and we need to plan ahead and evaluate the dilution in the saturated zone.

[Slide.]

DR. BROCOUM: Part of our new approach is that we may keep the facility open as long as 100 years so we need to develop a comprehensive performance confirmation program that will last us 100 years. We don't know if it will last 100 years but it may go on that long and with any unresolved safety questions we need to obviously have comprehensive plans for resolving them.

[Slide.]

DR. BROCOUM: With regard now to the thermal management, we want to have a flexible design for the key elements of the system, the repository, waste package, MPC, that are related to thermal loading.

For technical site suitability and initial applications we will conduct these evaluations in terms of the relatively low side of the range of thermal loading that flexible design will be able to accommodate.

We will through time evaluate high thermal loadings to improve the long-term performance of a repository. For example some people say the high thermal loadings are better from the waste package point of view, also perhaps reduce overall TSLCC, total life cycle system costs, and we may update our thermal loadings when we update our license application, approximately in the year 2008.

Of course we will conduct confirmatory testing, performance confirmation.

[Slide.]

DR. BROCOUM: Flexible design -- we want a design that is flexible enough to support the 1998 suitability evaluation with 2001 license application and a 2008 license application update.

We should be able to encompass that range for thermal loads. We want to of course have a robust waste package and we want to focus on our primary area but knowing if we go for both thermal loadings, not having any guarantee that we'll be able to raise it later, we have to also consider the potential use of the expansion areas that were identified in the SEP.

This would allow us to meet the metric ton requirement. There may be some combination of the two. It's kind of hard to say and these will be discussed in detail later -- waste and storage options that allow us to manage the input of the thermal loading into the repository.

[Slide.]

DR. BROCOUM: So the second point, evaluate the thermal loading for site suitability, initial license application, we want to select on the low side from that range that is encompassed by the flexible design. We again want to look at the waste acceptance as to how thermal input is managed.

We want the early thermal tests in our design to help us support this case, the low areal mass loadings for the licence applications. This will be used in the 1998 technical site suitability and this analysis will be expanded as more information becomes available for license application in 2001.

[Slide.]

DR. BROCOUM: After that, we will be evaluating higher thermal loadings to approve the long-term performance, and perhaps, reduce cost, so we will continue testing and analysis to see if we can have higher thermal loadings. We will of course tailor our thermal loadings. There are a lot of issues involved in this and I am not sure, but I think that will discussed to some degree later on in this meeting.

We will determine whether or not we can in fact impose a higher thermal loading and we will select the license application update by 2008.

[Slide.]

DR. BROCOUM: Finally, we will need to conform to whatever strategy we have selected or whatever load we have selected. We need to conduct confirmatory testing for thermal effects, for emplaced waste packages. This is real live waste.

We may have more than one panel. Some panels may be hot or some panels may be cooler. For long-term testing we will evaluate the rock response during operations to assure that waste isolation and containment will be achieved, that we can conduct repository operations. And we will select a final thermal loading. There may be several different changes since it is such a long time period, from 2008 to the time we plan to close the repository.

[Slide.]

DR. BROCOUM: Now what are our respective positions on each of the key decision points -- on technical site suitability, design recommendation for an environmental impact statement in 2000, license application approximately 2001, update to receive and possess waste, approximately 2008, the amendment for permanent closure.

[Slide.]

DR. BROCOUM: Technical site suitability, 1998. Obviously the suitability will be based on our referenced thermal load somewhere in the low range.

This will be characterized at the site as it exists today with pre-existing conditions. We will evaluate the sensitivity of the range of thermal loadings to make sure some future decision is not precluding baseline information we know today.

For the environmental impact statement in the year 2000, the exact words obviously you could find in scoping hearings. We expect that it is likely that we will need to extrapolate thermal loading in any range the design is capable of handling for its impact on the environment, for example, surface temperatures above the mountain.

For the license application 2001 we will have a maximum design basis thermal load. It will be in the low range. This phrase comes out of 60.21 in the regulations, 10 CFR.60. That is the phrase used by the NRC in their regulations. We will have a maximum design basis thermal load. We expect it to be in a low range of the range of thermal loadings our design will be able to accommodate.

We expect to be able to support a reasonable assurance finding using laboratory tests and short or early ESF test data. We will be able to provide comprehensive plans for performance confirmation during construction, operation -- again, performance confirmation will start during site characterization.

It could include continuation of these types of tests. We will evaluate at that time the impact of higher thermal loads but we are considering to see how they impact the engineered barrier system, the repository performance and compare it with current, at the time, license application design basis.

[Slide.]

DR. BROCOUM: We are planning an update in approximately the year 2008 where we will move towards a high thermal loading, depending on the results of the long-term in situ heater tests so we will only move up if they are supported by the results of these tests.

In preparing this application we may have to consider the expansion areas depending again on the results of each test.

Again there is a long time period from the year 2008 to whenever we close repository. There may be more than one step but we may move towards higher thermal loading, again depending on results of additional long-term testing from actual waste packages and MPCs emplaced in the repository during the operations phase.

That is kind of how our strategy is laying out at this point in time.

I think Ron gave a lot of qualifiers. This is all kind of evolving. We had a lot of discussions with the engineers because we are trying to keep as much flexibility as we can early in the system not to preclude any options, for example, backfill or perhaps ventilation.

The engineers, of course, looking ahead down the road, they have to start making designs and they are saying gee, so there is a huge debate going on at the project trying to find the balance between flexibility and progress in the design.

Those are my comments.

DR. CORDING: Thank you, Steve. Any questions from board members? Yes, John Cantlon.

DR. CANTLON: Cantlon, board. Steve, if you're planning to make a site suitability determination in 1998, as you modified the ESF design in what you've given us earlier, then you're not going to be able to assert that that site will actually accommodate a cold repository because you will know virtually nothing about the major portion of the block. Your tunnel has been -- the extension of the north ramp which was to really get a look at some of those faults out there. It's been deleted.

DR. BROCOUM: The formal site suitability decision now is scheduled for the year 2000, but 1998 is a decision whether we should keep moving forward. It's very important to keep that in mind. The 1998 is where are we today.

We're hoping to have gone through all the guidelines on the geotechnical side, but that is just should we now continue to prepare the license application, do the designs, should we keep moving forward. So, I would assert that the major decision for suitability on the current schedule is the year 2000 when we issue the site recommendation report.

DR. CANTLON: But if the site will not accommodate a cold repository because there are problems with the faults out there, then your decision will not be very useful in '98.

DR. BROCOUM: Well, it's a decision on whether we should move forward or not or whether we should stop in large measure. I mean, I'm not sure we'll have the information, say, if the site's unsuitable also. So, I sometimes use the term that it's an investment decision. It's whether we should keep moving forward.

DR. LANGMUIR: It's Don Langmuir, board member. This brings me to my problem with the definition of the word suitability that you're using. I think all I can see is that what we're really saying it's not yet found unsuitable. That's what suitability means to me when I see all that's going on here.

DR. BROCOUM: Well, I don't want to predict the future, but we will try to make as many high level findings as we can. A high level finding by definition is a finding that you don't think additional information will provide any information that will cause you to change your mind. So, you have a degree of confidence for that particular finding.

DR. LANGMUIR: Which means there are no fatal flaws that you could have missed as of '98, which is "Well, that's a significant presumption."

DR. BROCOUM: That's part of it, but there are also system guidelines that tell you that the system as you understand it at the time will perform.

DR. LANGMUIR: But I continue with a question, Steve. It seems to me that one of things you're saying here, which is I think interesting and I think a switch around for the program is that you're going to allocate more performance to the EBS. In the past, the EBS has almost been an afterthought in terms of its contribution to isolation, but right now it's looking as if you're bringing it in as potentially, if I could say this, an equal player, to the natural barriers.

DR. BROCOUM: For the operations period and for the early post-closure period, we are allocating a lot of performance to EBS. That's one of the commitments we've made to the NRC in terms of waste package containment. So, we're obviously for beyond a thousand years, we're depending heavily on the natural barriers.

DR. LANGMUIR: Does that mean you're going to provide more funding, more research support for characterization of the EBS and determining its performance?

DR. BROCOUM: That is one of the major issues we're having internally in balancing all of the aspects of the program, is the right funding level for each part of the program. That is truly a difficult issue, okay, and there's a lot of struggle and debate within the program. So, it's not easy to give you a quick answer on this one, okay, so we're trying to make the best decisions for a balanced program. So, it's probably the best answer I can give you right now.

DR. CORDING: Other questions? Yes, Bill Barnard.

DR. BARNARD: Bill Barnard, board staff. Steve, when we discussed thermal loading in previous meetings, there was always reference to the SCP and thermal loadings on the order of 57 kilowatts per acre. Now you've gone to what you call a low range of thermal loading. Can you define thermal loading and the ranges?

DR. BROCOUM: Who's going to define what the ranges that were designed? I think we're talking about that later today, aren't we? But basically when we say the low range, it would be less than 57 kilowatts per acre, and in a primary area, it probably would not accommodate all the 70,000 metric tons, at least without aging the fuel or some other fuel management options. We're trying high if it's over 57 kilowatts per acre, okay? And some of the debates we're having with the engineers.

DR. BARNARD: It just seems like a fairly significant change in DOE strategy. Is there a high level of consensus within the program about this?

DR. BROCOUM: I would say, and I try to say at the end of my talk, with the engineers who are trying to advance conceptual designs and try what they call focus design. They would like to start making some decisions now that affect us later. From a regulatory perspective, we are trying to keep our flexibility until we have time to analyze all of these things. The attention, if you like, is in that area right now, at least from my perspective, between engineers. You say tell me, give me the requirements and I'll design for them, and the regulatory. We don't have all the information yet. We like to keep our options flexible. So, there is, I would say, a healthy debate going on. That would be a fair way, and in getting ready for this and doing a lot of dry runs, we had a lot of those debates that came up to the surface. So, there are a lot of issues there.

DR. LANGMUIR: Don Langmuir. In the same vein, Steve, does low loading as DOE would define it mean a situation in which there's no boiling whatsoever occurring because of the waste? Is that the definition you're going to use for it?

DR. BROCOUM: I don't want to say that because I think the MPC's themselves will be very hot and so there might be boiling in the vicinity. I'll let the experts answer that.

DR. LANGMUIR: We need to know what you envision happening as best you can explain it for the next two days with these choices.

DR. BROCOUM: Yes, I think you're going to get quite a bit of discussion on all these things in the next few days. That's the intent. We just want to put it in context. We're trying to make it all fit together, and it's very difficult to make it all fit together. We're trying to tell you where we are today. That was the goal here.

DR. CORDING: Steve, will the consideration perhaps in the next couple of days here, during the meeting, will we be talking about alternatives such as aging, and are you thinking about that sort of approach to trying to control the local thermal effects?

DR. BROCOUM: There will be discussions. I think Buz Gibson will be talking about this and overall system strategic approaches and these kinds of things. He has a fairly comprehensive talk on that. So, you will hear that I think this afternoon or late this morning.

DR. CORDING: Okay, thank you. Yes?

DR. PRICE: Dennis Price. You've made a statement

I wish you'd kind of explain a little bit. You said with regard to alternatives that it was not required that you pick the best. It was just required that you evaluate.

DR. BROCOUM: The 60.21 -- I don't have the exact words in front of me -- but requires you for major systems features -- is that what the words are -- to look at alternatives. You have to document to the NRC that you've looked at alternatives before you take a path and go downward. You have to have a rationale for taking that path, but when you're looking at the whole system or you're trying to plan ahead, you know, what is best for that particular moment in time may not be best for the whole system. So, I think you're trying to optimize a system by choosing among various alternatives. We've been concerned because in making some choices and in doing, say, a full design. we'd have to preserve the records for the NRC where we evaluate. It's really a regulatory issue. I think it's a sensible one that they ask you at each key step what alternatives have you considered and tell us why you've taken this one. I don't think there are any NRC people here to amplify that.

DR. CORDING: Others? Okay, Leon Reiter.

DR. REITER: Steve, just a couple of questions. One of the things that you had mentioned in the beginning, you read from a report about doing analysis of the various thermal loading options. Are we going to hear during this meeting an analysis of why you picked the low thermal loading option?

DR. BROCOUM: You will hear some of the logic of going that direction, yes.

DR. REITER: Who's going to do that?

DR. BROCOUM: I think various speakers. I think Mike Vogel in particular will be talking about it, yes.

DR. REITER: Okay. My second point is about the technical site suitability. I think you mentioned you're going to pick the low thermal loading option but you're going to do evaluations for other -- showing that the other thermal loading options are possible. Is that going to be done in the context of each finding, like hydrology you're going to show that it's okay for low thermal loading that is also okay for the other option? How are you going to accomplish this?

DR. BROCOUM: I think we'll probably do it in the systems wide approach. I'm not sure we're going to look at that for each single guideline. It may not be relevant for each single guideline in the first place.

I need to make one point for some of the questions that are coming up. Whatever case we put in front of the NRC in the year 2001 is the case that we have the plan to go forward with. We can't assume that we'll be able to change the thermal loading after that. I mean, we may be planning to raise it, but if we can't supply the data, so one of the reasons we're looking at expansion areas is because we may not be able to put all of the waste in the primary area. So, the point is we want to make a complete case to the NRC in the year 2001 so they can reach a reasonable assurance finding. We think it will be easier to do it on the cool side because it requires less information.

Even though our long range strategy is to increase our thermal loading, we're not guaranteed that. So, the point I'm trying to make, the case has to be complete at whatever thermal strategy we're going with in 2001. So, in a sense, you can't bet when you're dealing with the NRC on this issue, okay? They will consider updates as we put them in, but they will evaluate each of those based on the information we have. Some of the questions are beating around that issue, so I just want to make that clear.

DR. REITER: Just one last point. I'm getting a different view of technical site suitability from what you're saying now and what I thought we heard in October. In October, I thought we heard that final site suitability was essentially technical site suitability plus socioeconomics, environment and transportation with perhaps some additional data. The implication that you're getting now in response to the questions from the chairman, John

Cantlon, well, if that's not real technical site suitability, the heavy stuff we're going to do later on. I'm just a little confused there, particularly since you're bringing in the National Academy of Science to review the technical site suitability and not to look at the one later on. Perhaps you can explain that.

DR. BROCOUM: The decision on technical site suitability is a decision that the director makes. He has input from the technical basis reports. He has input from what we call our regulatory assessments. He uses any other information he wishes to use, and he makes his decision, but that's the director's decision. It's a decision made at that point in time that we ought to move on, and we see no reason not to move on. Or it may be a decision hey, the site shouldn't go. It could be that kind of decision also. It could be a positive or negative decision.

The decision in the year 2000 is what you call File A action. It's a decision the secretary makes. It has the whole NEPA process. It has site characterization. It's a much higher level decision. It's really the DOE at that point saying hey, we think this site is suitable and we're preparing a license application and we're confident that that license application is going to succeed. So, it's got a much higher, in the whole scheme of things, a higher standard.

Technical site suitability is based on each of the individual guidelines we have gone through until that date. We hope to go through all of them, and I know we have a very tight schedule, so I don't want to get into that debate here today. It's really not what we're here for today I don't think.

MR. McFARLAND: Russ McFarland, staff. With your definition of '98 being an investment decision essentially, how does this differ than the decisions you would make in normal budgeting process in '96, '97 or say '99?

DR. BROCOUM: It is a decision that we have a lot of public participation in. It's a decision that has external peer reviews. It is more than internal DOE decisions. It's a lot more than internal DOE decision. I mean, normal budgeting things is, you know, a typically internal DOE decision. It is a statement to the world that we don't see anything wrong with the site or these are the things that we see. To me, it's much more significant than internal DOE decision. Obviously it's a very formal process we felt.

MR. McFARLAND: But basically it's an investment decision. It's not a racial decision based on activities that you've gone through?

DR. BROCOUM: Well, it's not the formal suitability decision, and we've never said it was. So, it's very clear that it be understood. Someone else?

DR. CORDING: Someone is wanting to ask a question. One other point I'm at, Steve, and if it is an investment decision, it seems to me it has to be more than that because if you are involving the community and the people have concerns, they will probably not look at it as -- they have to look at it I think as more the DOE's investment decision by the very fact you've brought them into it, and that includes the board or various stakeholders. Isn't that --

DR. BROCOUM: I'm not trying to get a trivial decision, but it is not the formal suitability decision. I'm trying to make a distinction between the formal suitability decision which is now scheduled for the year 2000, okay, which encompasses all of the factors that need to be considered as declaring a site suitable, and the technical site suitability which only encompasses the technical factors as defined in the 960 guidelines.

DR. CORDING: Leon?

DR. REITER: Steve, but isn't the key technical decision -- that's the question. We realize it's not the final suitability decision, but your response to the some of the questions indicate that well, this is not really the key technical thing. We'll be looking at the rest of the block later on.

DR. BROCOUM: I think the key decision that the DOE makes is the site recommendation.

DR. REITER: Right.

DR. BROCOUM: That's the key decision that we have in the whole process.

DR. REITER: When will the technical input be -- when will the decision on the key technical end would be made?

DR. BROCOUM: It will be made to support the site recommendation report. That is the key decision, okay? But you're trying to make -- I'm not trying to say it's not important. I'm the one that's, you know, I'm not trying to say technical site is not important, but it is not the final suitability decision on the site.

DR. CORDING: Milner?

MR. MILNER: If I might say something if I could, Ron Milner, DOE. I think it's important to recognize that there's a difference between whether or not the site is suitable and whether or not it's licensable as a repository, and the suitability decision, I think, is the point where we think we have enough data to indicate the site is suitable, but there is not yet enough data to determine whether it's licensable as a repository.

DR. CORDING: I think you've brought us back on schedule. I appreciate it very much, Steve, and I think this is a question that perhaps during some of the further round table things that might come up again.

DR. BROCOUM: There have been so many questions on technical site suitability that we are preparing a little white thing on technical site suitability itself because there have been a lot of people asking questions.

DR. CORDING: Good, thank you.

DR. PRICE: May I ask one? What's bothering me here a little bit is what is the defense that there is a genuineness to the flexibility of design, that you've not really set your course for a cold repository? It really looks like that flexibility is kind of a semantic outfit you've got with regard to the alternatives.

DR. BROCOUM: If you had been in all our dry runs, I don't think you would feel that way because I truly think that the engineers think the hot repository, from their perspective, is the way to go. They think that it, you know, performs the waste packages better if it's hot and you can drive the water, you know, they see a lot of advantages. It's more efficient and you do less tunneling. You know, there's a lot -- certainly from an engineering perspective itself, there are a lot of advantages, as well as potentially from a performance perspective.

DR. PRICE: But if your suitability decision is based upon a cold repository and you just -- I thought I heard you say that we're probably going to indicate to go the direction of what justified the suitability decision. That sounds like you're biased strongly in that.

DR. BROCOUM: We are going to support the maximum thermal load, and we can support it at that time, but we feel it will be on the low end of the range right now. That's what we're trying to say. I mean, we will go as high as we can in our license application, but we think, based on everything we know right now, he'll be in the low end of the range. That's the message we're trying to get across.

DR. CORDING: Thank you, Steve.

We will have our next presentation now. Sam Rousso will be making the presentation. He is acting director of the Office of Acceptance, Storage and Transportation. And that will be the topic of his presentation.

Glad you are with us, Sam.

[Slide.]

THERMAL MANAGEMENT OPTIONS

WASTE ACCEPTANCE, STORAGE AND TRANSPORTATION

PROGRAM OBJECTIVES

MR. ROUSSO: Thank you, Mr. Chairman, members of the Board, ladies and gentlemen. Good morning. I am Sam Rousso, the acting director of what we call OWAST. Hopefully this will come on in a bit of time.

The OWAST program, as we call it, is Waste Acceptance, Storage and Transportation. Essentially, you can view that as the front end. We are the receiving group for the fuel, spent fuel from the producers. We arrange for the transportation and interim storage as necessary and take it to the eventual repository.

I am going to give a brief overview of the next section on your program which is the thermal management options.

[Slide.]

MR. ROUSSO: The strategy for my part of the program is to establish the waste acceptance process; that is, what is the waste form, what can we work with the utilities and the other producers to accept something that we can handle to develop a transportation capability to be able to move that fuel.

We are planning in the program to develop an MPC, multipurpose canister system to raise that level of technology to one where we can make a decision as to what should be the logical hardware to use in such a system. We are on a course with an environmental impact scoping hearing that will begin actually this coming Monday in Las Vegas, several scheduled around the country, to evaluate what are the factors that we should be looking at to weigh what systems ought to be applicable.

We think the MPC has a lot of advantages and we will weigh the environmental implications before going forward with that. If we have a favorable decision, then we will be prepared to fabricate and deploy the MPCs by 1998.

As far as an interim storage facility, we have stopped design work on that activity. We do not have a site. If we get a site, we will be moving forward to try and make that happen.

[Slide.]

MR. ROUSSO: The program has several uncertainties that we have to work within. There is current litigation, as I am sure many of you know. The utilities have brought suits against us, the NARUC, I believe, has a suit. We are trying to work our way through that and see what is our obligation in 1998. There are various congressional initiatives. We have at least three that I know of that are on the Hill that try to put some different focus on the Waste Policy Act and help us get this job done. We don't know how that is going to come about and we have to be ready for eventualities.

If that does define an early site, then we have to be able to move fuel earlier.

There is a notice of inquiry that is out on the street that has just been extended recently. I believe the final responses are due in the first week in December, which would lead to a potential rulemaking. And that notice involves a public process of what is our liability in terms of 1998, where do we go from here, how do we make this process better, so we are seeking that input.

I mentioned the MPC environmental impact statement record of decision and that we expect to come about with the scoping period in the spring and we won't have a final on that until a couple of years later.

[Slide.]

MR. ROUSSO: This is just a broad schedule. I don't know if you can see that very clearly; I doubt it. But in the handouts you might have it. It just shows how this is a complicated integration problem we have on the MPC line. We don't really look forward to deployment until this period in here, in the '98 time frame. In the meantime we have to go through the EIS process that I showed you and we will not be making a final decision on that until late in '96. We will have to process with the NRC the safety reports and get our 71 compliance and our 72 compliance that would feed into the deployment.

Transportation in the parallel line, we have to do some major efforts in criticality control and the burn-up credit issue and try to get an NRC position well before we have to close with some canisters.

The waste acceptance line is the NOI process that

I also mentioned is currently going along.

We also have -- yes, this shows the GA-4/9 which is a transportation option with legal weight truck capability. The MPCs are, of course, large rail casks.

[Slide.]

MR. ROUSSO: There is naturally major coordination that has to go on between ourselves and the repository people. The MPC line is on top with the deployment, as I mentioned, in '98.

In parallel, we are doing -- we have awarded -- we have not awarded, we have issued an RFP for MPC design efforts. We expect to award an MPC design contract in early '95. While that design is going on, we are doing Title I waste package design with the repository people. This is a typographic error here. We are continuing to do repository ACD design. This is a completion of repository work here and then go into more detailed MGDS thermal design.

This gets to be a tricky time zone where a lot of coordination activity has to take place and we will be hearing a bit more about that. Obviously, we do not want to be too far ahead in the MPCs before we get a good feel on the waste package before we have a solid feel on criticality control with the NRC. And to understand how that relates to the repository, are we going to go cold, hot, some combination in between.

Important to note that the dates for license application and we should know what our waste package looks like and our repository strategy is well significantly before the 2001. These are years of early waste acceptance. I believe it is 400 tons in '98, 600 -- 900, so there isn't much. There is some pick-up, probably less than 2,000 tons in that time frame before we actually have license application submittal.

[Slide.]

MR. ROUSSO: We are looking at certainly several interface issues. The one we are exploring today, of course, is the thermal loading. But we have to look at settling criticality control to know what we can load the MPCs with and how that affects the repository. We need to have compatibilities with the waste package. We have safeguards questions. We must, before we can load, satisfy IAEA and other safeguards and verification requirements and you want to know what's in a particular can, obviously, before you seal it. You have to maintain the integrity of that can throughout the total process.

Repository design and operations are, of course, important to us and knowing what we can put in a can.

And I should mention our range of what we can do to influence what we can do to influence what goes into a can. You will hear more in a moment about what our current contract authorities permit us to do. Whereas, we have an oldest-fuel-first principle, that is only the principle for giving the utilities their amount of allocation. It does not mean that the utilities have to give us their oldest fuel first. So it is not clear what we will get, and therefore what we will put in the cans, and therefore where we will seal and put those. But we do have ways to mitigate that and we will get into that a bit more a little later.

The rest of this part of the thermal management options session this morning is for talks first on the waste acceptance process by Mr. Alan Brownstein. And that is -- it will take us through the contract obligations we currently have and how we intend to work with utilities to see that along.

MPC specifications by Jeff Williams is the design and engineering side. What are the specifications we have on the MPC packages for heating on the package, on the cladding and so forth.

Steve Saterile will discuss thermal options and goals and, lastly, Buz Gibson will give us an overall system thermal management briefing that I think you will find very informative.

That is all I have to say to introduce the part. I will take any questions if anybody has any at this time.

DR. CANTLON: Board.

Steve, I guess I am a little puzzled that DOE perceives that it has no control over what waste comes from the repository. Every garbage outfit that I know of in the country refuses to accept garbage that isn't within certain spec. Why can't you lay down a set of specifications and only accept that from the utility. They're dumping it on you.

MR. ROUSSO: Not quite so fast. I do have a contract specified that is five years old and it must have other parameters to it. What I am saying is right now the utilities may give us their hottest fuel, they give us the fuel. However, that does not limit the options. That does not limit the options. What we collect, and we can negotiate, let's face it -- what I am saying is it is not unilateral on our part. We can negotiate with the utilities what we will eventually take and what quantities. If we have different strategies at the repository, it doesn't mean what we collect first has to go into the repository first.

We are also 12 years from possible first collection in '98 to a 2010 repository. So the minimum fuel age is going to be 17 years old.

DR. CANTLON: But it seems to me that if you are looking at putting up a national system, why should you optimize an individual utility's wellbeing. That doesn't make any sense nationally. This is a national program.

MR. ROUSSO: Absolutely. Absolutely.

DR. CANTLON: And you should optimize the system as a total system. And the utilities really have the cooling pool. It is to your advantage to get the oldest fuel first, whether you go for a hot repository or a cold. It seems to me that fundamental principle ought to be argued very vociferously by DOE.

MR. ROUSSO: Well, we do have, as I said, alternatives to try and make that happen to a better degree than it is right now. For example, to go to the MPCs it is quite likely to go back and look at the contract because the MPCs are not part of the contracts with the utilities at the moment.

So while we engage in that discussion and engage in trying to make people whole as best as we can and look at various options for compensations, I think we have some leverage to decide what fuel we take and in what quantity.

So we will be certainly looking at that and the only point I want to leave you with is that we intend to maintain as much flexibility for the repository side of the house as we possibly can. We are not going to prejudge the thermal loading issue at the repository because of mechanical pick-up questions on the acceptance side.

DR. CANTLON: Thank you.

DR. CORDING: Okay, thank you.

Other questions?

[No response.]

MR. ROUSSO: If not, I would like to introduce Alan Brownstein who will pick up the first part of the detailed discussions.

[Slide.]

WASTE ACCEPTANCE REQUIREMENTS

MR. BROWNSTEIN: Good morning. I am Alan Brownstein. I am the acting director of the Waste Acceptance Division and Dr. Cantlon, your questions are well considered. And what I hope to do this morning is to go through the waste acceptance processes that currently exist and then we can talk about where that process needs to go. But I think it is important to understand where it is today.

[Slide.]

MR. BROWNSTEIN: This is really the front end of the front end. The primary objective of waste acceptance is once the federal facility begins operations that we need to be ready to achieve all those -- accomplish all those activities necessary to achieve the legal and physical transfer of waste from the utilities. When we begin waste acceptance, the spent nuclear fuel, all of the spent nuclear fuel or as you indicated the right nuclear fuel doesn't magically or automatically appear. There is a process and the process is not a simple one.

But as we consider and as you consider and certainly we within the program in cooperation with the utilities what our thermal management objectives are, we need to link the waste acceptance process to our strategies for implementing. And that is really the key point.

[Slide.]

MR. BROWNSTEIN: Now, the relationship -- it is important to understand the relationship that we have with utilities. It is a different, atypical and I would probably say unique relationship. The Section 302 of the Nuclear Waste Policy Act authorized the department to enter into contracts with the utilities. We did -- we proposed -- the law, as you know, was passed in January of '83. We proposed as part of rulemaking a contract in February of '83. A month later we held public hearings. A month later, we adopted the rule as final in April and the utilities had until the end of June, according to the law, to sign the contract, in order to participate in the program.

That was a very quick process and it was a process done when the system itself was at an immature level and certainly at a time when MPCs weren't on the horizon. So that is important to recognize as we go through.

It is especially important as we -- when we recognize that time frame with the fact that the contract defines the waste acceptance process and includes all legal and operational responsibilities that both DOE and the utilities need to do.

Another thought that I want to leave you with is that the nuclear waste policy acceptance is a full cost recovery program and whatever we do, all of the costs are paid for by the utilities. It is a different relationship, as I said. They are buying a service from us. They are paying us to provide them a service. From their view, it is very simple. They give us the money, we take their waste. It is neither easy nor noncontroversial. Sam mentioned the lawsuit.

[Slide.]

MR. BROWNSTEIN: What do we draw from that? What we draw from that, this contract part of the 10 CFR 961, is that we can not unilaterally change the contract. We cannot change the terms and conditions of the contract. We have flexibility to select waste in cooperation with the utilities, but we can't unilaterally make that decision unless we go back into rulemaking and publicly negotiate a change in those terms. And when we do that, we have done that twice already, we publish a notice of proposed rulemaking, we usually hold hearings, receive comments, we have to consider those comments and then prepare or promulgate a final rule.

In doing that, our experience has been that the process takes at least two years, so we need to recognize that as we overlap an MPC program.

[Slide.]

MR. BROWNSTEIN: We understand that what we do at the front end is going to affect the back end. We know that what we take, how much we take, when we take it, who we take it from is going to affect our -- the thermal performance at the repository; we understand that.

The key here is the timing gives us flexibility. And that we understand that flexibility decreases over time. And if we talk about an MPC program, the longer we wait to make that decision, we understand that the utilities have the primary responsibility for storage and that we know that utilities are making their storage decisions every day and more of that fuel over time without any comment by us is going to be already loaded in cans. So we have to recognize that.

[Slide.]

MR. BROWNSTEIN: What I would like to try to do is give a brief overview of what the contract says about the waste acceptance process. The law requires that we take all the fuel. There is no dispute about that. But we recognize back in '83 as we recognize today, whenever we begin waste acceptance that our capacity is going to be limited. And so the question becomes how do we, recognizing that limitation, how do we develop a process.

We have established, the contract establishes that we develop what's called an acceptance priority ranking. And this acceptance priority ranking is based on the age of permanently discharged fuel. With the owners of the oldest fuel receiving the highest priority. This is what Sam referred to commonly as the oldest fuel first.

Now, once we have that ordering, in order to turn that into an allocation, we have to know what the waste acceptance rate is, so the contract requires that for planning purposes we project what our acceptance capacity will be for 10 years. We then take that acceptance rate, apply that to the acceptance priority ranking to come out into the individual capacity allocations. Let me show you how that works.

If we take the last -- last year's annual capacity report, again for planning purposes, we said, okay, based on our current plans, based on the restrictions between the time -- that so exist between the MRS and the repository, there was a 10,000 metric ton limit before repository operations began. We projected that we would have this four-six-nine rate.

[Slide.]

MR. BROWNSTEIN: We apply that rate to this acceptance priority ranking and what we come up with is individual capacity allocations by year for the individual owners. What I have done is taken just a snap shot to show you how this goes.

For the first 10 years, we have a nominal four-six-nine rate at the bottom and then we allocate that across each of the individual owners. There is an error down there on Wisconsin Electric. I think that should be a 16.

But this then is the individual capacity allocations.

As Sam said, this is the queue. This is how we order who goes first, who gets in the door first, so everyone is not there all at once trying to squeeze in the door with all of their fuel.

When their place in the queue comes up, as Sam has indicated, they are not required to give us the fuel that generated the right for the queue. But there is this restriction for allocation purposes.

[Slide.]

MR. BROWNSTEIN: The utilities have the ability and the right under the contract today to select virtually any fuel over five years old or other conditions that we list for standard fuel, physical and others. But principally they have the right to give us any fuel over five years old.

Now, as we get into the MPC program, we can work with and will work with the utilities to select the fuel we want when we load an MPC should the decision be made to proceed with MPCs. Or how we then take the loaded MPCs and put them into a repository.

So we can do thermal tailoring under the current scheme, if you will, with the cooperation of the utilities. What I said before in terms of timing being the key to flexibility, the longer we go, the longer we wait, the more complicated this becomes. But we can adjust. We have an opportunity to influence thermal tailoring within the current contract.

If that becomes inappropriate at any time, whether it is in a year or well into the next century, we can go back and change the contract and go through the rulemaking process. These are the constraints.

Now, the other thing that I want to point out is the contract is based on a bare, spent-fuel system and should the decision be made to go with an MPC system, we will then have to adapt this contract and the waste acceptance provisions in it to accommodate an MPC. So we certainly are planning on doing a rulemaking to add to the contract the MPCs.

Anyway, with that, I will entertain your questions.

DR. CANTLON: Following up then on the earlier question, if it takes you two years to do a rulemaking and you want to target '98 for delivery of an MPC, why isn't DOE in the process of getting a rulemaking started?

MR. BROWNSTEIN: Firstly, the department has not made the decision to move forward with the MPC. The record of decision for moving forward is scheduled for '96. Our current plans, however, now we have issued this notice of inquiry in May of this year to talk about broad waste acceptance issues and our current planning calls to begin the process of rulemaking to change the contract to accommodate that at the end of '95.

That still leaves open the issue of -- with respect to the thermal options of how and we want to change that contract. You know, do we know exactly what we know and what we need at the back end now? And I think we want to preserve our options. But we are not at the situation where we can say that we want, you know, X, Y and Z now from every utility. So we have to start building in that flexibility.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: This may be premature, the question. Perhaps we will be hearing about it later.

I am interested to know where we are with regard to ages of fuel in MPCs providing information on the temperatures that you find at exterior skin of the MPC.

This clearly will impact what is going to happen with the thermal loading. Are we going to hear about that from Jeff?

MR. BROWNSTEIN: Yes, Jeff Williams is going to talk about that.

DR. LANGMUIR: Good. Thank you.

DR. CORDING: Yes. Dennis Price.

DR. PRICE: Just for clarification, in 1998 that 400 MTU that you "accept," where is it most likely to be put?

MR. BROWNSTEIN: Let me go and discuss with you what we said in the notice of inquiry in terms of our obligation to accept waste. What I showed up there with the date and the amount was for planning purposes only. We needed to do that to be consistent with the requirements of the contract.

The department's preliminary view on its obligation to accept waste is that it is a conditional obligation based on the commencement of operations of a facility constructed on the Nuclear Waste Policy Act. So, absent that, it is our belief we don't have an obligation to accept in '98.

Having said that, recognize that we said that in May and a month later a large number of utilities, attorneys general and commissions took issue with that in the court and that is now a part of the U.S. Court of Appeals and they are considering that issue. So the next time we meet, perhaps I can interpret the Court's decision for you.

DR. PRICE: Is it true it is most likely to be put, if it is put anywhere, at the utility site?

MR. BROWNSTEIN: In terms of -- under an MPC program?

DR. PRICE: Yes.

MR. BROWNSTEIN: There is no facility now other than the utility; that's correct.

Thank you.

DR. CORDING: Thank you very much.

The next presentation is Jeffrey Williams on the multipurpose canister, considerations and system thermal management. He is acting director of the engineering division of the Office of Waste Acceptance, Storage and Transportation.

[Slide.]

MULTI-PURPOSE CANISTER RATIONALE

MR. WILLIAMS: I am Jeff Williams, the acting director, as he said. Basically, the MPC is a principal component of the program approach and it is a component of the system that could meet the near-term storage needs as well as be compatible with transportation as well as disposal considerations.

The goal of the programs to maintain compatibility with the other components of the system, recognizing the uncertainty related with the repository, while at the same time not trying to drive the costs out of sight in comparison to transportation and storage technologies that we have. So one of the most important considerations is compatibility including with the other parts of the system including compatibility with the thermal loading requirements of the repository.

[Slide.]

MR. WILLIAMS: I think we have talked about this before but we have done a considerable amount of system evaluations related to the MPC prior to the decision to move to the next phase. We did three levels of study at increasing levels of detail and in 1993, the end of 1993, we completed a large set of studies and we received various levels of support from NRC, the Advisory Committee on Nuclear Waste, who have had some comments, and who provided a lot of encouragement to work toward this program.

The utilities, the Edison Electric Institute has continued to reiterate their support of this program, as well as trade associations like NARUC and other people.

Some of the important things, throughout these studies, were identifying the issues related to the interfaces between the various elements of the program, including the repository thermal loading considerations. In February of 1994, the Secretary announced the decision to proceed with the next step of development and the next step being the design and certification of the multipurpose canisters.

[Slide.]

MR. WILLIAMS: Basically when we decided to move forward with this program, we made the decision to incorporate the experience of vendors. Instead of designing this internally, we prepared a request for proposal and, since these vendors primarily have experience in storage and transportation; they don't have experience in the disposal and repository aspects so we had to work these together into our specifications with our repository expertise melded into that procurement exercise.

The request for proposal was released in June 1994. Evaluations or proposals have been received and we are hoping to award a contract for design in 1995. I think Sam mentioned the EIS process, which is commencing next week with scoping hearings in Las Vegas and before we would move into any further phases, the fabrication phase in particular, that would await a record of decision in the fall of 1998.

The RFP procurement is broken into three phases. First, the design phase, then with an option to go into the certification phase and lastly with a third option into the fabrication phase. That fabrication phase won't be implemented until after the EIS is completed.

[Slide.]

MR. WILLIAMS: I think Alan and Sam have covered this pretty well, but the contract as it exists today didn't envision MPCs and there could be other distributions as a result of interactions among utilities and the fact that the five-year-old fuel, the way the contract currently exists.

[Slide.]

MR. WILLIAMS: Several needs and uncertainties that need to be developed associated with this program. First of all, we want to minimize the routine handling of spent nuclear fuel assemblies throughout the system. If you count up the number of reactors and how much spent fuel they are going to generate, they will generate approximately 295,000 spent fuel assemblies throughout their lives. And we want to minimize the number of times they would be handled. They could be handled as much as eight times, using the previous system; going into storage at a reactor, and then back to transportation casks and potentially into an interim storage facility, and lag storage, and so forth. An MPC would minimize this routine handling.

We also want to introduce elements of standardization and compatibility among the MPC handling components. The MPCs need to be consistent with near-term storage and long-term uncertainties. Storage and transportation near-term needs are key.

I think Alan alluded to the fact that today reactors are making decision about storage. They are putting a certain type of fuel into storage which actually reduces our flexibility. If they decide to put in the oldest fuel first in order to minimize handling, it would be more likely for us to pick up fuel out of the pool. So the longer this program takes to be implemented, the less flexibility we actually have.

And then again, we need to consider the disposal long-term uncertainties and also minimize program risks while continuing to maintain flexibility. I think that has been a consistent statement throughout all the speakers that flexibility is key.

[Slide.]

MR. WILLIAMS: The repository thermal loading considerations, basically any thermal loading decision related to the MPC has a potential to affect the MPC design. It also more likely could affect the way an MPC is loaded at a reactor or it could impact the way it is loaded into the repository.

There is a range of thermal loading options being evaluated that I think others are going to talk about in detail from 25 kW per acre up to over 100. The current thermal requirements for the MPCs state that, and this comes from -- it is a repository consideration. It doesn't come from transportation or storage. But it says that the MPC shall be designed so the thermal output at time of emplacement does not exceed 14.2 kW. The peak spent fuel cladding temperature does not exceed 350 degrees C when subjected to an MPC external wall temperature -- that's the MPC itself -- is 225 degrees.

So you can see if there is a decision to have a low thermal loading strategy, 30 kW per acre, we may only be able to put two MPCs per acre in the repository. However, if that is further defined where we want to minimize the near-term temperatures to 100 degrees, say, it may result in a need to redesign an MPC.

Lastly, I want to mention, keep in mind that obviously the thermal output at the time of MPC loading will be different than that at emplacement. I think Alan mentioned that if we start to load an MPC in 1998, by the year 2010 the thermal considerations will be quite different.

[Slide.]

MR. WILLIAMS: There are several ways to respond to the thermal loading contingencies. I think Buz is going to talk about these in a lot more detail. However, obviously, we can store longer on the surface to allow reduction in heat output if required. We can alter the spacings of waste packages in the drifts. We can derate the MPCs, which means not load the full amount assemblies or possibly redesign the MPCs.

One of the most important things to mention is that throughout the first few years of MPCs and the way the procurement is set up, the actual procurement will result in fabrication of less than 2 percent of the total MPCs that will be used throughout the program. So if you bought those first 2 percent and you loaded them, there is still quite a bit of flexibility to take into account any thermal -- repository thermal loading considerations.

[Slide.]

MR. WILLIAMS: The very last thing I want to mention is our efforts with NRC to try and identify what NRC has termed "MPC Busters" early on. What we are trying to do here is to get a certification for storage and transportation as quickly as possible by the 1998 time frame. So to do that, we know the storage and transportation requirements fairly well and we can go through the certification process and reach that objective. However, we don't know all the uncertainties related to the repository and to minimize our risk, we have proposed to the NRC that we submit a technical report that identifies any possible what we call MPC Busters.

For example, if NRC said the repository has to have a thermal loading of three kilowatts per acre, that could be a serious consideration to any MPCs.

Basically, I just wanted to mention that we have put this on the table to NRC. We don't have a response back from them yet. We understand that they are going to respond, that they are preparing a review plan as to how they may review these repository considerations early on and we will be responding. We really haven't started work on this but we have placed the question to NRC.

And with that I would like to open up to any questions before we go on to Buz's talk which I think will be important.

DR. CORDING: Question? Don Langmuir.

DR. LANGMUIR: Jeff, I asked this earlier and was told you were the one to answer it.

Your specs for the MPC suggests that external wall T's should not exceed 225?

MR. WILLIAMS: That's right.

DR. LANGMUIR: Obviously, that's boiling. That's well above boiling.

MR. WILLIAMS: That's right.

DR. LANGMUIR: So it is conceivable that you could have what you call a low-loading repository with maybe two

MPCs in an acre and still have boiling around each waste pack.

MR. WILLIAMS: The way my understanding of this, not being the repository expert, is that today the requirements at the repository are at 200 degree rock wall temperature which I think is a phase change in cristobite to tribymite or something to that effect, right?

[Laughter.]

MR. WILLIAMS: That is the established requirement. And this MPC with that 225 degrees meets that requirement. There has not been a requirement established at the repository as far as I understand that requires below boiling.

If there is that requirement established then, yes, we may have to change that specification on the MPC.

DR. LANGMUIR: Given what your guess is to the MPC designs you will be seeing, has anybody made a calculation of how old the average fuel would have to be within an MPC in order to not exceed boiling at the walls?

MR. WILLIAMS: No, I asked that this morning and I was told it was, back-of-the-envelope, about 50 years or so. Buz, are you going to address that?

DR. GIBSON: It is not in the talk but we have recently done some back-of-the-envelope calculations that have not yet been vetted to get a feel for the relative age of the fuel versus package size that would get you to local below boiling conditions.

DR. LANGMUIR: I calculate on the back of another envelope that in 2010, your average fuel will be 46 years old, whatever that tells us the average -- the temperature you might get at the exterior wall at that point. Does that get us below boiling, do you think, on your envelope?

DR. GIBSON: In a 21 PWR assembly MPC, no.

DR. CORDING: Other board questions? Dennis Price?

DR. PRICE: What is the anticipated -- given that 225 degrees C on the canister with the transportation overpack, what is the surface temperature at this point?

MR. WILLIAMS: The surface temperature of the transportation overpack?

DR. PRICE: Yes.

MR. WILLIAMS: I can't recall. We did have transportation temperature limits which were driven by the cladding temperature. The surface temperature -- there is a surface temperature limit in the regulations and I am trying to look for a transportation expert here. I really can't recall exactly what it is.

Joe, do you know?

MR. STRINGER: I am Joe Stringer with the M&O. The regulations for 10 CFR Part 71, I don't have a field clad temperature similar to the store.

MR. WILLIAMS: He is talking surface of the cask surface.

MR. STRINGER: Again, that would come from the -- we would specify in the regulations there is a specific limit for the MPC's temperature. It would be a design solution to meet the regulations just for that.

MR. WILLIAMS: Again, we don't even have designs yet. From the vendors. Right now, we are at the stage where vendors have submitted proposals. The requirement is a cladding temperature requirement. The surface temperature of the transportation cask --

MR. KANE: My name is Dan Kane and I work for Jeff at Department of Energy.

The true consideration with regard to transportation is slumping of the lead of the transport cask and therefore when we hit the lead layer of the transport cask, we have tried to contain that to about 187 degrees for the lead slump. So while I don't know what the outside external surface temperature would be, there is not a requirement in Part 71 less than 187 degrees.

MR. STRINGER: Actually we probably have some predictions on that where we have looked at the transportation casks. I don't recall what it is either.

I know that information is available in the MPC conceptual design report. At that time, what we were looking at was 10-year-old fuel, which is actually what we specified in the MPC specification. So we could go back and look at it and make that information available.

MR. WILLIAMS: I did want to mention within the last month there has been a transportation cask approved by NRC that carries 26 PWR assemblies, higher enriched fuel, 6.2 years, I believe, that met NRC's limits, whatever those were. I don't have them off the top of my head.

DR. CORDING: Board staff.

MR. McFARLAND: Russ McFarland, board staff.

Earlier, Steve Brocoum indicated that in order to maintain flexibility from his perspective in the program, they would like to defer decisions with regard to areal thermal loading. Did I hear correct from you that the longer we wait, the less the flexibility from the standpoint of the MPC and loading the MPC?

Is there a conflict here?

MR. WILLIAMS: What I was trying to say was the longer we wait to deploy MPCs, the more things will be done at reactors, the more fuel they will put into storage which will make it more difficult for us to pick and choose. There will be less fuel to pick and choose out there. They tend to pick the older, colder fuel for storage. And then you have to get into if you want that fuel, then you have a handling issue. That's all.

MR. McFARLAND: Then there may be a situation where the decision may be made for you, whereas you don't want to make that decision because you are "trying to hold onto your flexibility"?

DR. BROCOUM: Flexibility is a very complex issue. There are a lot of other options in a repository. What we said was we will make our decision in time for TSS. And for license application, those will be in the low range.

I don't want to imply that we are currently making those decisions. Our overall strategy is to collect information and so we will support as high a thermal loading at each of these decisions that we think we have on hand at that time.

DR. LANGMUIR: I would go back to Jeff's comment about flexibility. I don't follow that you don't have flexibility. You are going to receive all the fuel from utilities anyway at some point in time. What does it matter when you take the stuff that's difficult to get, at the beginning or end of the process?

MR. WILLIAMS: Well, it --

DR. LANGMUIR: In terms of your choices?

MR. WILLIAMS: It is basically when you are at a reactor and they have spent fuel that is placed in a concrete cask off to the side and they have spent fuel that's in a pool. We believe that they would prefer for us to pick up fuel out of the pool early on rather than go get it out of the storage containers, bring it back to the pool and therefore not freeing up the space in the pool that they would like to free up.

DR. LANGMUIR: So you are going to have to go to rulemaking at some point here to get the rights to take what you want to take.

MR. WILLIAMS: I think probably so.

I think Alan is the best one to address that.

MR. BROWNSTEIN: We are not envisioning that we are going to have if we move forward with the MPCs, an MPC-only decision, an MPC-only program. The contract is for fuel and we would expect an overlay to bare fuel and MPC. So we would have both.

In terms of acceptance, it is one thing in terms of what we put into an MPC and another of how we load in and place that at the repository, they are two separate, distinct issues. We recognize that we will have to make those contract changes and the sooner the better.

We have all talked about flexibility. If all the uncertainties could be removed today, we would know what to do and how to do it. The challenge is to maintain that recognizing that those uncertainties are there today and are going to continue to be there for some time.

We have worked closely in recognizing what their strategy is now and where they are going to make that determination assessment.

DR. CORDING: Thank you very much.

We will take a 15-minute break now and then get back to Don Gibson's presentation. At 10:32 we will meet again.

[Recess.]

DR. CORDING: Let's reconvene.

Our speaker is Don "Buz" Gibson, Management and Operating -- with the M&O and his presentation is system thermal management strategy.

DR. GIBSON: What I am going to do is spend some time talking about it -- am I turned on?

DR. CORDING: I hope so.

[Laughter.]

[Slide.]

SYSTEM THERMAL MANAGEMENT STRATEGY

DR. GIBSON: Hopefully I won't have to rephrase too many things during my talk.

I am here to talk about some thermal management options. And what we are doing -- what I am presenting are different alternatives throughout the system as a whole where we have opportunities to tailor or adjust the thermal load that goes into the mountain and also talk a little bit about how those kinds of tailorings help provide us with a certain degree of flexibility in our ability to meet a range of thermal loads.

And you have heard earlier about the strategy for thermal loading that's looking at both a low range load on up to higher loads.

[Slide.]

DR. GIBSON: We need to have some degree of flexibility in order to actually achieve that. So, again, these -- the operational alternatives I am going to talk about are to help give us some additional flexibility from a thermal viewpoint, some of them give us some operational efficiencies that I will talk about and also all of this work is simply an input into the overall development of the complete thermal loading strategy, which, as was indicated earlier, is a work in progress.

[Slide.]

DR. GIBSON: Let me talk a little bit about the ways in which you can tailor that thermal load. We can do that a couple of ways. One is by trying to do things that will help reduce the overall total heat load that goes into the mountain. The other is to do things operationally that help provide a little bit of flexibility. And you will see several examples of each of those as I go through. And we can do that in a number of places.

You can do that through a waste selection process, and that was talked about a little bit earlier. You heard quite a little bit about constraints and considerations associated with waste selection earlier, in the earlier talk. You can adjust the thermal load by aging the fuel not by waste selection strategies but by simply sitting it on a storage pad wherever and letting the fuel age. You can tailor the load that way.

You can also do a number of things at the repository itself. There are some things you can do to adjust the thermal profile in the mountain and the thermal load through emplacement strategies. I will show you an example of that. And you can also do things through preclosure operations and I will give you an example of that.

[Slide.]

DR. GIBSON: Let's talk a little bit about the selection strategies and what that does to us. I am going to give you an example of two pretty much idealized selection approaches. And show you how they impact the total heat load that goes into the mountain. And what the effect is of those different selection strategies is it changes the average age of the fuel when it gets to emplacement. So it changes the amount of thermal load that goes into the mountain versus the thermal load that's expended prior to going into the mountain.

As I go through this, realize that the cases I am going to show you which are representative of an acceptance of actual oldest assembly first, if you will, oldest fuel first into the mountain, that is one extreme. And I am going to go to another extreme which is if I accept youngest fuel first and by YFF5 we have a constraint to only accept waste that is greater than five years old. So it is starting from the youngest fuel with the constraint of it being at least five years old and accepting that first. So you can see the two ranges.

You have to consider when we actually go through and figure out what a real impact of a real selection or a real waste stream is, there are a lot of considerations in there. A lot of fuel will have already come out of the pools and put in dry storage. There is the possibility of trading of allocation rights to most of the utilities. So this is just going to represent a bound on that and kind of a start at understanding all of the potential implications here.

[Slide.]

DR. GIBSON: What we have here is an example of the average heat, thermal heat per assembly, not per MPC but per assembly, at pickup if you begin pickup of assemblies in the year 2010 at the rates that we -- at the throughput rates that we talked about now and you pick it up oldest assembly first for the most part. It is not rigorously oldest assembly here but the first order it is oldest assembly first.

What we have here is just a dividing line to give you a peg point if you will that is common to both of them and it happens to be if you took an average at 21 PWR MPC and looked at 730 watts per assembly you come up to the total thermal output, which is 15 point something, I would have to multiply it out in my head, kilowatts per package which is a transportation limit at the moment.

Don't confuse that with the 14.2 kilowatt emplacement limit that's been imposed on the program. That is a transportation limit.

So, as you can see, for the most part at oldest fuel first, almost all of the assemblies or the vast bulk of the assemblies, roughly about 80 percent, are below that limit and there are just a few assemblies that are hotter than that.

You can accept those assemblies. You can pick them up even in the system by derating casks, not fully loading an MPC, letting them sit in store for -- there are lots of ways to deal with that.

Now, let's compare how that shifts if I go from the oldest fuel first allocation, which causes each of those assemblies to be aged the greatest amount of time prior to acceptance at the repository, to the exact opposite, which is over here, which is the youngest fuel first or the youngest fuel first greater than five years old acceptance.

[Slide.]

DR. GIBSON: Here is that same line as before. Now, instead of about 10,000 assemblies or 11,000 assemblies being greater than that limit, we are up to 38,000 assemblies greater than that 730 watt per assembly peg point that we had.

Another phenomenon as you look down here, somebody asked in the audience earlier about this peak that occurs, that happens because a lot of the older fuel that you had there gets even older before you pick it up because you are delaying that and it has the effect of taking some of the fuel that you see on that chart that is in this range and shoving it down a little bit because it gets a little bit older.

You can see a wide range. There is a big swing in here between the two extremes. Oldest fuel first and youngest fuel first. The indication there is that there is a distinct change that can occur to the thermal load in the mountain depending on what the characteristics are of those assemblies as you pick them up and what the nature of that waste selection and that negotiation with the utilities ends up being.

So our conclusion from that is that this is a fruitful area to continue to look at to fully understand what that impact is when we get to the requirements at the mountain consistent with the thermal strategy that is ultimately decided on.

[Slide.]

DR. GIBSON: There is another example. I am not going to talk much about this, that is in the package and it just shows you what happens instead of youngest fuel first greater than five years old, you pick up youngest fuel first greater than 10 years old.

[Slide.]

DR. GIBSON: You can also adjust a lot of this through storage as you are all aware. Let me give you a couple of examples.

What you have heard earlier is a limit for emplacement of 14 kilowatts. It's 14.2, I believe, is what is in the MPC. This is the number 14 but it is close enough.

If I were to select waste along that oldest fuel first strategy, I am going again to the extreme oldest fuel first example, and I want to look at what additionally I can do to unload thermal energy prior to emplacement in the mountain and look at storage, what are the sorts of impacts

I might have.

If I pick up oldest fuel first, ship it at that transportation limit that I had before, this gives you a feel for the total storage needs I have somewhere, be it at a storage site other than the repository or in lag storage at the repository that I would need in order to make sure every canister is below a given limit. I want every canister to be below 14 kilowatts, I need to store, at most -- I'll translate these numbers for you -- a total of 40 canisters at any one time will ever be sitting out on that south 40.

The average time that any one canister will be sitting out there is storage, read off of this axis, and it is a little less than one year. So I don't require a lot of storage.

If I take a look at all of the canisters over time that I had sitting out there and add all of those up, I get the total number of cask years, if you will, that are required for storage and that is what you read off of this axis.

So with all those numbers on there, you can calculate just about anything you want. If you need to know the total number, for example, of canisters that had to sit in storage for any length of time, I can take the average amount of time that they had to sit in storage, which is a little less than one year in that case, divide that into the total cask years, and I have got the total number of casks that had to pass through that storage and get aged.

Now, as I move down for these different ones, I am looking at different emplacement limits. Let's say I want to reduce the average temperature of each canister to 12 kilowatts prior to emplacement. Since I have lowered that, obviously the number of canisters I need to store goes up, the amount of time I need to let them sit there and cool goes up and, in this particular case, if I look at the maximum size I would need in that storage area, the worst case at any one time, it is 270 units sitting out there. In this case, as soon as they hit that 12, I pull them back off of that and stick them in the mountain, so I have space for something else.

And the average time any one of those is sitting out there is roughly four years. And again, I can march down through this to different limits. And obviously as I go to lower and lower emplacement temperatures, my storage requirements go up. Subsequent cost to the system would increase and all of the things that go along with it.

But that is a parameter that the program can play with in order to adjust the -- total thermal load that goes into the mountain.

[Slide.]

DR. GIBSON: Let's look at some strategies in the repository itself. It is possible to tailor the way waste goes into the mountain, tailor your emplacement strategy in order to optimize the amount of area you need for emplacement. And we have on here a fairly simple example to give you a feel for the sorts of things you can do.

If I take a mountain, for example, where all of the waste packages are going to go in, all the same age, equally spaced, what I will expect if I look at the temperature across that repository horizon? You would expect it to peak in the middle and trail off at the end so you basically have a heat sink on each side and that is just represented by that. Eventually, you end up at the edge of your repository. The temperature doesn't drop to zero at the edge of the repository, it continues to trail off.

But I can take that same set of waste packages, all the same age, and I can change the spacing where I increase the density of spacing out at the edges of the repository. The consequence of that is to raise the temperature at the edges of the repository. My total thermal load that I've got in that is the same. And it then decreases the peak temperature in the center of the repository.

The net result is that for an equivalent peak temperature, and I have drawn this incorrectly; I have this line crossing right through this piece here. But for an equivalent peak temperature, the amount of area I need for the same number of waste packages will drop.

Now, the actual quanitfication of how much that is, is to be determined as part of the studies, but it would drop. So one of the things I can do with that, conversely, if I have a fixed area that I want to use, is I can put the same amount of waste in there and reduce the temperature. I don't have to reduce the area or I can reduce the area and get more waste packages in for an equivalent temperature.

The point is that emplacement strategy impacts the total thermal load that goes in any given area in the mountain.

[Slide.]

DR. GIBSON: On the other hand, I can do some things operationally to help reduce the total heat load that goes in the mountain. And the most obvious one there is ventilation.

If I am ventilating the repository, along with that hot air that's going out, I am dropping thermal energy out of the mountain. The question is, how much thermal energy can I reasonably eliminate through ventilation.

There are a couple of considerations here. There is the thermal energy that is in the hot air that's coming past the waste packages and out. As the water is boiling away or vaporizing, it is coming out into the drifts as well as out into the rock. There is a fair amount of thermal energy contained in that moisture and I can eliminate that. so there is an operational way to reduce the total amount of thermal energy that is deposited in the mountain.

[Slide.]

DR. GIBSON: Now, we've done some scoping calculations to get a feel for the potential magnitude of each of these effects that I have talked about here. The reason being, we wanted to get a feel for whether or not they are worth pursuing to get a more accurate indication of what those values would be. And these are the results of those scoping calculations.

Going to oldest fuel first versus a younger fuel kind of approach picks you up about 25 percent in terms of total thermal load. And that is due to that aging of the fuel. As you know at the early part of the curve it is very steep. So you don't have to age a young fuel very long in order to gain quite a bit in terms of thermal output.

Looking at aging the fuel before you put it into the repository, you pick up things like 10 percent or so. That thermal tailoring that I showed you, a fairly simple back-of-the-envelope calculation indicated in an idealized case you could pick up as much as 10 percent.

The ventilation concept, depending on how much water comes out into the drifts versus how much thermal energy has to go out only in hot air, you can get anywhere from five to upwards of 20 or more percent reduction in the total heat load that's going into the mountain.

We add all those together and they multiply out and in this case, I picked the highest number here to make it more dramatic. You come out with a factor of two. That's not an insignificant amount of thermal load. That is total integrated thermal load, that is not thermal output per unit time in the mountain; that is total integrated thermal load.

[Slide.]

DR. GIBSON: When you do all of this to reduce the total thermal load, the nice thing about all of these kinds of options is they are applicable, they help you whether you have a low thermal load, in which case for a given area you can get more waste into that area, that's beneficial, or a higher thermal load in which case you can reduce the area in which you put in all of that waste. They are beneficial in either case. There is nothing unique about any of these options.

So when we indicate that we are adding in or we are looking at some things in terms of flexibility in terms of design and operations, that flexibility exists independent of the range of thermal load you end up with.

That's an important point.

So what we are finally doing is we believe these analyses suggest that there are some reasonable trades to be done amongst all of these sorts of things. We have given you an indication of the kind of range we could expect in terms of reduction of the thermal load and some of those options. All of those things at the moment are very preliminary and, as I said, the indications are that we want to continue to pursue and refine our understanding of those. But all of those things again also have to be traded off against a number of institutional design concerns and the licensing strategy that we have. It is not a purely engineering kind of a problem.

You heard earlier some of the constraints with waste acceptance and some of the trades there. We are working to figure out what things we can do in the system to make the constraints at one end be consistent with the constraints and the performance objectives at the repository and we're looking at the kind of flexibility that we have in between to help make those two things meet.

DR. CORDING: Thank you, Buz.

Comments or questions? Yes, Don Langmuir.

DR. LANGMUIR: Buz, I want to commend you and the DOE for what looks like real flexibility in terms of how you are looking at the system here and how you can optimize its performance.

You had one overhead, number 9, which showed a profile, a hypothetical profile, of what the temperatures might look like around an MPC. One of my big questions remains and will remain until I am comfortable with it, is that even though you have an average loading that seems to be something that you have defined as low perhaps, Berkeley talked about this in an open letter they wrote, that they are concerned that even if you might have a certain temperature, maybe it's high, maybe it's low, in between it is probably something else. What you really have to worry about is whether these things overlap to decide on the performance of the whole system. And the overlap may be below boiling or above boiling no matter what you have chosen for the average properties of the system.

I assume you are looking at that too, the overlap issue?

DR. GIBSON: I think you are going to hear that later.

In terms of the thermal performance of the system, there are two pieces of that. There is the localized local effects around the waste package and then there is the performance of kind of the bulk average. And depending upon the density of those sorts of things, that profile looks very different. It will either look very -- if you look at the repository horizon, the temperature will either look fairly smooth with kind of minor variations. As you spread farther apart, you are going to see greater variations of temperature as you go from waste package to waste package and drift to drift.

There are two pieces of flexibility to consider in this and they are all part of the overall thermal strategy. One is the sort of things that I am talking about here, and these were aimed more at looking at the bulk average of the repository and moving that up and down.

There are obvious controls on the local piece of it as well, and that has to do with the size of the waste package, a number of assemblies and the trades you can do with that. Now some of the things in here help you adjust that. You can age a package and for the equivalent same-size package, you can reduce the local temperature.

So for an older package, the size for older fuel, the size of the package which keeps you below boiling locally goes up.

The strategy that you heard said we are looking for the maximum thermal load for the license application in 2001. We believe we will be in a low range.

What hasn't been determined yet is the specific load, thermal load that goes with that for both the local effect and the global effect. And you heard Jeff Williams talk earlier about the number of MPCs which, at that point in time, has defined a size that will not have been that great an investment, at least 2 percent. I forget what the date is that goes with that, but it is roughly that time frame or a little bit later.

So the system still has a flexibility with respect to both local effects and the broad effects and that is all being developed now as part of the details of the strategy.

DR. LANGMUIR: Another question. You showed a number of figures showing kilowatts per MPC maximum heat outputs. Is there correlation, a simple correlation of how that ties into above 100 degrees or below 100 Celsius at the wall of the waste package?

DR. GIBSON: No, I obviously can't translate it here. Those calculations have been done. I indicated earlier, we did a quick back-of-the-envelope calculation recently just within the last few days to get a better feel for tradeoff between fuel age and, waste package size, to check when you cross that threshold of boiling locally.

That -- those numbers I don't think are rigorously correct and there is a lot of vetting that needs to go on. I indicated that earlier. But those kind of calculations are ongoing. I know that the MPC folks have taken a look at it, at what point you stay below boiling when you are not in the repository.

When you are in the repository, you put this insulation around it and, of course, the temperatures go up. I think there was an indication earlier that configuration was extremely important in calculation of skin temperatures, for example, of an MPC or an overpack around an MPC.

But those calculations are actively ongoing and are part of all of this and will feed into the overall strategy.

DR. CORDING: Yes, Dennis Price.

DR. PRICE: Buz, I just would like to encourage this kind of thing. It is preliminary at this point by the looks of it; there is more to be done. And also I would like to see folded into the larger system a picture the M&O presented some time ago; a modified N squared type of diagram with feed-forward, feedback, loops that showed programmatic risks. It would be interesting to see this with the program approach and how, for example, this kind of thing fits into the programmatic risk.

DR. GIBSON: That effort has been modified. There is a new one of those with the program approach in it.

The process we are going through now is taking that data, that N squared chart that you were referring to earlier, is effectively a source of information for the program and looking at how to translate all of that into specific actions that the program needs to take in terms of data needs, in terms of the uncertainties and risks associated with all of that. So that's an ongoing effort --

DR. PRICE: And I would hope that also drives we're to do some of this analysis.

DR. GIBSON: The only reason I hesitate, I said, yes, I believe that the first order the studies and analyses that are ongoing now and that have been discussed in terms of the development of this thermal strategy that you are hearing about today are reasonably compatible with where that N squared diagram is. So I don't think it's at this point necessary to drive all of those. It will help us look for gaps and holes. But it is actually in pretty good shape.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: There has been a controversy going on with regard to the consequences of putting backfill around waste packages. And the sense that I think many of us have gotten is that DOE has not been disposed to putting backfills around packages because of the insulation that would create and the higher temperatures that might occur as a result of that on waste package skins.

Have you pursued that and looked at the consequences of backfilling with crushed tuff, for example, to the thermal effects you might see in the repository?

DR. GIBSON: I know there has been some work in that area. I am not conversant. That's one I will have to pass off.

DR. LANGMUIR: Is anybody here going to talk about that or could they?

DR. CORDING: Steve Saterlie.

DR. SATERLIE: Yes, this is Steve Saterlie and I am actually not going to talk about the backfill work that has been done. There have been some measurements made by Sandia about the conductivity of this backfill or effects of backfill and we have done some analysis and calculation with backfill in there.

This is something that still needs to be evaluated, certainly does have impact on the waste package. However, you know, it would be likely that we would not use this backfill until the actual closure, which could occur in 50 to 100 years. So the age of those waste packages would be significantly less.

But that impact is being looked at. It is still being considered, as I think Steve Brocoum indicated in his presentation.

DR. CORDING: Thank you.

Board staff or others?

MR. McFARLAND: Buz, on your chart 8, you indicated with an OFF pick-up scenario, you wanted to bring the package, the waste package temperature down to, say, 8 kW, you would have to age 2,100 packages for a period of about 14 years.

What is the total inventory of packages? How many total packages are we talking about? 2,100 is some part of that.

DR. GIBSON: To interpret on these curves the total number of MPCs --

FROM THE FLOOR: 10- to 12,000, depending on --

DR. GIBSON: Total number of packages, 10- to 12,000. That is merely at any one time there will be a certain number of packages sitting out there cooling. And in oldest fuel first, there won't be many early on. They will be dragging out later.

That's the most that you will ever need. So if you were thinking in terms of how big a storage pad do I need out there in order to get stuff down, that's how big it would be for this particular pick-up or selection strategy.

DR. CORDING: Thank you, Buz.

Thank you very much. It is interesting to see these options being considered. And I know that some of the vapor and heat flow studies being carried on right now will be very important input into the decisions made on this issue.

Our next presentation is by Steve Saterlie. He is with the mine geologic disposal system, thermal loading study. He is the manager of that group with the M&O.

MINED GEOLOGIC DISPOSAL SYSTEM

THERMAL OPTIONS/GOALS

[Slide.]

DR. SATERLIE: Thank you. I will see what I can do about telling you a little bit more about some of these thermal options and about how we are going to try to evaluate some of these options.

[Slide.]

DR. SATERLIE: When I speak of thermal design here, what I really mean is not necessarily the design of the repository or the design of the waste package, what I mean here is the selection of thermal options, thermal loading that we would ultimately select.

I am going to talk a little bit about our objectives there in doing that selection. I am going to try to discuss thermal options, and although I don't have numbers on my charts, I am going to try to give you a little bit better feel for what some of those numbers are based on the calculations at this point in time, and talk about how it is we are going about try to select that, at least from the system study sense, and the role that thermal goals have in this process.

I am going to see if I can describe that a little bit, and potential changes that we might require in thermal goals based on the strategies that we are working on, and then future actions.

[Slide.]

DR. SATERLIE: These words have been used before this morning. I think they bear repeating though. That in doing the selection of a system that meets the requirements we need to develop a design that achieves waste isolation and containment standards. The engineered barrier has some release rates that it can't exceed.

The waste packages must provide substantially complete containment. The underground facility must meet certain preclosure operation and monitoring conditions. And then a thought here is that, we must be able to demonstrate this. We must be able to provide with some assurance the we understand that these things are indeed met.

[Slide.]

DR. SATERLIE: The way in which the thermal loading system study is approaching this process is, as I have talked to you before. In '93, we did some system studies that were scoping calculations to try to get a handle on could we narrow the range, what were the issues that were associated with thermal loading.

Those calculations were done. That study is out. We have talked about it before. That did come up with some conclusions that above 100 Mtu per acre is likely too hot.

There were some other conclusions in that.

After that point, we said, well, now particularly with the program going the way it is and the need for test data, where can we have the most effect in the systems analysis end?

What we felt there was to be able to do some analysis that would help us decide on what tests need to be done, what are the parameters of importance for waste isolation.

That work is ongoing, and I are going to talk a little bit about some of the results of that tomorrow.

Then we have talked about the timetables of making some of these decisions, and we have talked about how, likely in 2001, we are going to be making a decision based on low thermal loading. Where in that range of low thermal loading? How high can we go?

Those are the kinds of questions that we need to answer in the system studies based on our program objectives and data that starts to become available, as well as, additional analysis. I will talk a little bit more about it.

[Slide.]

DR. SATERLIE: Now, let's talk about the options a little bit. What I have drawn here is, in fact, the repository in the mountain itself, there are certain things that happen when you get rock above boiling temperatures. When you get a large amount of rock above boiling temperatures, you get fluid and gas movement.

Below those temperatures, you can get some fluid movement, some gas movement, but the processes can be different and that is why I have drawn some break here.

What the program is considering right now is that these two regimes -- there are options all along these regimes. This is the above boiling bulk average. This is kind of the SCP regime that all of you have heard about, the 57 kilowatts per acre range where you have a limited time, maybe about 1,000 years, that the center of average repository horizon is above boiling, but not all the area of the repository is above boiling in that case. The edges are below boiling within a few years.

There have been some hotter regimes talked about up to 80, 100 or above Mtu per acre. Buz Gibson talked a little bit about spreading the waste out, thermally managing where you can move more waste out to the edges to avoid the edge effects.

In these kind of conditions you can have extended times of boiling for the entire repository. There you are talking about thousands of years now.

On the other side of the coin, down in the low area, the below boiling area there is -- some of the questions that Don Langmuir has been asking is how much of the rock boils.

There is certainly with some of these packages -- you can emplace them at a low thermal loading, but then you can have some local rock above boiling. That is something that I am not going to answer today is how much of the rock should you have above boiling. That is something we need to get some test data on because if we are going to go forward with a license application that says we are going to try to ambient data as much as possible plus limited amounts of data over a period of time of, say, three or four years, whatever time we have available there, we can only get thermal pulse that goes into the rock several meters.

Thus, we need to make some measurements to say how much that perturbation is going to affect that local area. We may have to define how much perturbation we are going to allow, and I am going to talk a little bit more about that in just a few minutes to be able to make a case the we understand what the mountain is doing. That is an issue that we are struggling with.

By the way, this line right here occurs somewhere. It depends on the fuel age, as you have heard, and a few other things, but this occurs roughly around 40 Mtu per acre.

We then have even lower perturbation where we might need to keep local rock below boiling. Those options are something we need to select from, and we need to select from that using the system study and data and analysis as we go forward.

Then, as longer term test data become available, as Steve Brocoum and others have mentioned, we might have confidence that we can select up in these regions.

[Slide.]

DR. SATERLIE: To select it, we are using system analysis. We do have to meet a certain standard of performance with what has been selected.

Beyond that, we would try to optimize the system and look at operability, testability.

[Off the record.]

DR. SATERLIE: We have some analytic models that we have some confidence in, and to have some data, both laboratory, surface and subsurface.

[Off the record.]

DR. SATERLIE: The thermal loading decision itself with technical backup, we are selecting from a range of options, from the various thermal regimes considered.

The timeframes have been talked about, but we need a bounding analysis in '98 to help us in the site suitability determination.

As Steve Brocoum indicated, a maximum design basis thermal loading must be chosen for the 2001 license timeframe.

Then, as more data, longer term test data, become available in the 2008 and later timeframe, we may amend those license applications to emplace at a different thermal load if it seems suitable to do so.

[Slide.]

DR. SATERLIE: Now, let me tell you a little bit about how we might come to that decision. Some of the things we might use to do that.

First of all, the aspects of compliance that are impacted by thermal loading are, in fact, many of those things you have already heard: preclosure safety and retrievability; substantially complete containment; releases from the EBS; and release to the accessible environment.

These are all things the we have to determine and allocate performance to.

[Slide.]

DR. SATERLIE: Well, how do we do that? If we had complete understanding of the mountain and could predict when a radionuclide would be released, and where this radionuclide would go and how it would be contained in each of the barriers, then we could make accurate predictions of release to the accessible environment and compare all the options.

We probably will never get to that stage. That is just a fact of life.

We are moving toward better understandings, better models, but there are going to be some unknowns along the way.

The way we have approached this in the past is that we believe certain barriers are important, certain processes are important, certain things that we have to meet, and so we have identified what we have called thermal goals as a way of meeting those.

They are developed from performance objectives, and we try to -- these are traceable, at least in theory to regulatory basis as to why we place a certain goal on a certain barrier.

They are based on licensing strategy and program objectives, and those may and do and should change if the strategy changes or if the objectives change. They are based on allocation of performance.

At this stage in the evaluation process, they may not be inviolate. We have talked a little bit about that in the past to you. You may decide that a certain goal can get exceeded if you could improve performance. And they may be coupled.

The thermal goals should also be developed to help focuse the testing program, so it is an interactive process. The data should be used to validate these goals.

They also can be and will be used to provide some guidance for the design. That is being done.

[Slide.]

DR. SATERLIE: We would like to move away from the goals as soon as possible, but they are going to be with us in some form or another very likely throughout the program, so we need to make them as consistent as possible and make sure that they translate as much as possible to the requirements and the performance standards that we are trying to achieve.

We also are going to, besides using thermal goals to evaluate options, clearly, we need to use total system performance assessment. You will hear more about that later today.

As I said earlier, once we meet a certain level of performance, we need to look at post-operability risk, testability, evaluate options to optimize the system, and primary requirements, to use them.

Following a selection of a thermal option, these thermal goals may, in fact, turn into technical requirements. If there is some temperature limit on a waste package, for example, that you set, then that may become a technical requirement that would come down through the requirements documents.

If we decide that a particular goal is not, in fact, going to provide us some performance, then it may be deleted.

[Slide.]

DR. SATERLIE: Now, as I talked to you before, we did some work to revise the SCP thermal goals. That was done back in '93. I need to put you in the framework that these revised SCP thermal goals were based on the top-level strategy of multiple barriers, but they were oriented primarily toward a hot strategy, if you will recall.

I will show you a couple of examples of what those were. They only used limited data available at the time.

[Slide.]

DR. SATERLIE: Some examples of the goals on natural barriers, the Calico Hills and Topapah Springs barriers of 115 degrees, there were some natural barriers the thermal mechanical displacement of these barriers, there was surface temperature rise, there were some waste package goals, fuel cladding temperature, waste package stays above boiling.

There were some more natural barrier goals of thermal loading, degrading the PTn barrier. That was a placeholder because we felt the PTn barrier was important, but we didn't put a specific temperature on it.

Then drift wall temperature of thermal mechanical, you have already heard that today.

[Slide.]

DR. SATERLIE: As I said, these are primarily goals associated with a hot thermal strategy. As you have been hearing, we are looking at a lower thermal strategy, which is probably the most likely to be licensable by the 2001 timeframe.

We need to rethink some of the thermal goals. What I am going to show you next are some examples. These are my speculations about what goals might be placed.

What we need to do is we need to take some recent analysis, some recent data that we have, and relook at these thermal goals and reestablish what we need our goals to be based on the objectives.

These goals may be in addition to the goals that we just talked about, the SCP thermal goals, or they may provide a set of goals that may be a low set of thermal goals and a high set of thermal goals.

One of the things that is important to us, we realize, is the aqueous transport, the local aqueous transport. How much water flows and where does it go, and how much can you disturb the rock before you can no longer consider using the ambient data that you have out in that mountain?

So we may want to place a goal on how much aqueous flow, how far into the rock that you want to go.

This should be testable on time scales that we are talking about, a few years. This would define this disturbed zone that Don Langmuir has been trying to question us about, what is the extent of the disturb zone that you are willing to tolerate.

Those are the kinds of information that we have to start putting together.

We may decided that we want to limit the amount of -- I'm sure we will want to limit the amount of geochemistry changes that occur in the near-field host rock. The electro-chemical Ph of the water because that will have an impact on the substantially complete containment issues.

This kind of reiterates this, but may provide some temperature limits for the waste package or for the near-field host rock.

We may provide limits of how far boiling goes in, whether or not it even goes in.

So those are some of the examples, and those are the things that we have to look at within the program in the next several months to a couple of years to be able to come to these conclusions.

[Slide.]

DR. SATERLIE: So future actions. We need to review and revise these thermal goals, and they have to be consistent with our strategy based on the thermal loading ranges. They have to be integrated into the test program, because the things that we have to develop now from these strategies is what test do we need and how do we get the data that is going to provide us the support in this license process.

We need to then use the system studies to help us make a determination. For example, if we are in the low thermal loading range, what is going to be our maximum thermal loading that we will be able to support.

You heard Buz Gibson talk about some of the options, some of the things that we can do. We are looking at all of those types of thermal management issues right now. Those are the things that we need to evaluate.

Thank you very much. Subject to your questions.

DR. CORDING: Thank you, Steve. One question. Looking at a low thermal loading, is it the feeling at the present time that you are going to have to consider the expansion areas as part of the submittal for licensing?

DR. SATERLIE: Clearly, if we were to go to a low thermal loading, we all recognize to get all 70,000 metric tons, which is a limit, not necessarily what we have to emplace, but we would have to go to the emplacement areas.

I am not a licensing person, but I do believe we could go for our license application that says we would emplace in this amount of area a maximum load.

Let's say it was 30 Mtu per acre, for example, and that might mean that we would not get all of the 70,000 metric tons in that, let's say we were characterizing 15,000 acres.

That is a program decision to make. It may not be a politically palatable decision, but it is a possible decision.

We may decide to increase the areas too that we are looking at.

DR. CORDING: You will be considering both of those possibilities?

DR. SATERLIE: Yes. I think that is being considered in this flexible design that we have been speaking of. Those are some of the options.

DR. DOMENICO: Patrick Domenico, board. Steve, I have looked at what you have said here for the thermal heating in both low and high heating, some additional ones.

It seems to me there is an inherent assumption here that under high heating you are going to achieve substantial containment, and when you go to low thermal loading you have to worry about such things as ground water movement and small changes in geochemistry.

Is this the idea that the low thermal loading is the most dangerous in terms of containment?

DR. SATERLIE: I didn't want to imply that, and I am sorry if those charts implied that.

Water movement is important at any thermal loading, and we have to be able to demonstrate a knowledge of where that water goes.

There is that concern, and you have probably heard arguments about it, that even in the hot repository we are moving large amounts of water. You can have a saturated zone above the repository and the question is do you have fast pathways where you get refluxing back into the repository and what does that do to package lifetime and those kind of things.

But the point is, you've also, I think, heard some discussions about the fact that in the low thermal environment we are not going to be drying out a large area of rock, so we have to worry about the impact that that has on the waste packages and their lifetime.

What we need to understand in either of these cases is where that water goes, and the reason for the choice right now, I think, that the program has chosen for the low thermal loading is that we feel that we have some confidence that in the timeframes that we have we can measure where that water goes, if we have a lower thermal loading, we can measure the "few meters" kind of scale rock disturbances, as opposed to what is going to take a much longer time to measure, which is the tens of meters to hundreds of meters kind of disturbances.

DR. DOMENICO: Can I see that diagram again, the cartoon that shows high boiling and below boiling?

DR. SATERLIE: Certainly.

[Slide.]

DR. DOMENICO: What would you consider to be a perturbation at the below boiling as I see some perturbations, and I see H,C,M. I don't understand what those are.

DR. SATERLIE: I'm sorry. That is the coupling here. The hydrological, chemical, mechanical. Thermal is obviously understood.

DR. DOMENICO: But that is in the blow boiling regime. Don't you consider those things to be probably more important in the above boiling regime?

DR. SATERLIE: Yes. What I was trying to indicate here is that at some point here we have to start considering these perturbations as important. Yes, we have to understand those.

Clearly, the degree of coupling and the implications of that coupling increase.

DR. DOMENICO: If you had to characterize the testing in the G-tunnel, thermal testing in the G-tunnel on the basis of that diagram, where would you put it? Was that above boiling, was it bulk average below boiling? How do you think that test was conducted in the G-tunnels, the thermal testing? What regime there?

DR. SATERLIE: Let me take a quick shot at it, and then I think defer to Livermore who has looked at that in more detail, or Mike Voegele, who is going to talk about that.

Clearly, around the heaters it was above boiling, and there was some rock dry out. I think Mike Voegele will get into that a little bit more. I don't want to steal his thunder.

DR. VOEGELE: That test program was designed to raise the temperature of the block above boiling uniformly.

DR. DOMENICO: How big is the block?

DR. VOEGELE: The block is two meters square.

DR. CORDING: Other questions? Bill Barnard.

DR. BARNARD: Steve, I think you indicated that we are at the second line from the left, right now, is that right? Local rock above boiling?

DR. SATERLIE: With the 21 PWr package, yes, I have made some calculations. There are some calculations in the studies that show that, depending on fuel age and the size of the drift, the boiling into the rock can be anywhere from one to a few meters into the rock, maybe a much as nine meters in some cases for the very hot field.

DR. BARNARD: I am trying to get a better fix on how you are defining low thermal loading. As I understand it, it is at the line and we are somewhere in the neighborhood of 40 kilowatts per acre?

DR. SATERLIE: At about 40 Mtu per acre -- again, I talk about the metric tons, that is roughly -- that is close to 40 kilowatts per acre. It might be a little bit low, but at that point, the boiling isotherms coalesce between drifts, then you have the repository horizon, the bulk average, being above boiling everywhere.

DR. CORDING: Don Langmuir.

DR. LANGMUIR: We heard earlier this morning that more emphasis would be placed on the EBS in the program than has been in the past.

Is that related directly to the initiation of the effort in the low level because that to me -- the DOE, I think, within the last year has talked about using a three-layer canister as their choice for a low loading option or a two-layer for the higher running option.

Obviously, if you are going to go to more corrosion, which is what the low loading system is likely to encounter, you are going to have to put more emphasis on what the EBS can accomplish in isolation.

How does this fit into where you are right now?

DR. SATERLIE: I am not a waste package designer, so I am not evaluating the -- I am a little reluctant to answer that because I am not evaluating the performance of the waste package itself at this point in time. I am relying on the other people to do that.

Clearly, it is one of our barriers required to meet substantially complete containment. I think they are taking the actions -- or evaluating the actions they need to take to try to meet that.

I believe what they are trying to meet is 1,000 year containment. I am not sure that answered your question.

DR. LANGMUIR: I'm not sure it answered the question. Are we still talking about the package for the low loading option?

DR. SATERLIE: I think I'm going to defer that to Dave. Do you want to field that one? Yes or no? The answer is yes. They will be talking about that later in the presentations.

DR. CORDING: We are running a bit behind, so we have one question over here, and then briefly we will need to continue on.

DR. BASSETT: Randy Bassett, University of Arizona. I have a two-part question. The first part is do you think there is a technically defensible methodology for measuring dH and pH and flux two to three meters into fractured unsaturated rock? That is the first part.

Secondly, why would we not want to monitor this in the high thermal loading regime as well, flux, pH, which, of course, is going to change dramatically, and dH?

DR. SATERLIE: Let me answer the second part of that first -- it is because I don't know how to answer the first part.

The second part of it. The tests, we are trying to establish a set of tests that will give us information early on, that will give us information more commensurate with the low thermal loading. Those kind of measurements need to be made.

The longer term testing, we are going to have to make very similar measurements. We still need, as I said earlier, we still need to know where that water goes, how much of that water goes, and those kinds of measurements need to be made.

As far as the first part, there are a variety of measurements that I think can give us that. I hesitate to say that they are completely defensible. You know, there are neutron probes, there are various types.

I think Dale Wilder is going to talk a little bit more about that. Maybe he wants to take a shot at it right now.

DR. CORDING: Let's wait for Dale's comments on that because we are running short on time. Let's come back to that question.

Thank you very much. We need to proceed on.

I hope the Board is not on the critical path of this project.

We are looking forward to Mike Voegele's presentation on testing requirements. Mike is Deputy Manger for Technical Programs for the M&O in Las Vegas.

SITE RESPONSE TO THERMAL LOAD

TESTING REQUIREMENTS FOR THERMAL-LOADING

DECISIONS IN THE PROPOSED PROGRAM APPROACH

[Slide.]

DR. VOEGELE: Thank you. What I would like to do is begin to talk about the testing programs that we are going to use to support the information that we need in a through program approach.

[Slide.]

DR. VOEGELE: I would like to do that by very briefly touching upon what we were doing when we developed our site characterization plan and how we have tried to keep track of the baseline that we developed at that time.

I am going to talk momentarily about the SCP licensing strategies and how we used those for the basis of developing the technical programs which lead to the testing programs and supporting the program approach, and then I am going to try to move into the thermal strategy perspective, particularly as it applies to the exploratory studies facility testing program.

[Slide.]

DR. VOEGELE: You have seen these presentations from us before, many times, on how we looked at functional performance to meet the regulatory requirements and tried to address that in our site characterization plan.

Steve just gave you a very good highlight of the goals that we looked at for component performance in developing our site characterization plan. Those goals formed the basis for developing our testing program.

Most of the thermal aspects that you will find in the site characterization plan arose in relation to performance or design in the other portions of the repository system.

Primarily: you saw total system performance issues feeding our underground post-closure design, waste package characteristics, to a large degree, feeding what we are doing for an underground post-closure repository design, and that was interfaced with the pre-closure repository design.

This is actually the design activity which had some other constraints placed on thermal goals. You may remember that we told you previously that the real limiter for the 57 kilowatts per acre was not a post-closure concern for us; it was an operational concern.

I think today we probably would make that statement differently. We have done a lot more sophisticated modeling, we know more about how a repository system could perform, but at that time a major limiter for us was operational performance.

[Slide.]

DR. VOEGELE: You have seen Steve talk about this several times. I believe this was a 1991 meeting that we had with the Board. Tom Blejwas was with us at that time, also, Eric Ryder. I talked about some of the SCP thermal goals.

More recently, you have seen the results of some of the work that Steve Saterlie's group has done trying to revise the SCP thermal goals.

Most of this has been from an MGDS perspective. The program approach itself brought to us a couple of issues that we had to address, including the incorporation of an MPC within a repository program.

So the fact that we have developed thermal goals in the past based upon repository issues now has to be somehow expanded because the MPC is an integral part -- could potentially be an integral part of a repository.

So let me tell you a little bit about some of technical nuances that we are underlying the program approach, and take it from there to something about or testing program.

[Slide.]

DR. VOEGELE: If you go back to the site characterization plan, it is based on a concept that through time we will acquire additional information and answer a series of questions that we embodied in the SCP. Those questions were designed to answer regulatory requirements from Part 60.

I have chosen to call this access "expected information." In the past when we have talked with this kind of a diagram on the board, we have talked about it as being akin to reasonable assurance.

I think that is inappropriate. I think that there is an expectation on the part of a regulator as to the amount of information that he will receive to conclude that you have addressed his regulatory requirements.

There is also an expectation on the part of an applicant about how much information they will provide to answer these regulatory questions.

So I choose to call this an expected information access, recognizing it as a couple of dimensions.

The most important thing, when the DOE committed to trying to find a way to accelerate the program, we looked back at our site characterization strategies and asked ourselves could we break these strategies down? That we had to deliver a lot of information at the end of a program into some substrategies that would allow us to deliver pieces of information or logical products that can be delivered earlier than a final product, and try to build some of our program upon that.

[Slide.]

DR. VOEGELE: We did that. We tried very carefully to not lose sight of the fact that there are a couple of elements of this that we can take advantage of. This is generally that same figure. I am not going to play very much with what is down here. These are some of those decisions, the technical site suitability decisions, the license application, the construction authorization, and so forth.

There are three very important pieces on this figure. First of all, at the time of license application the Nuclear Regulatory Commission requires that the information we submit be as complete as possible in the light of information that is reasonably available at the time of docketing.

That is one bound on the information that you have to give to the NRC.

They also have imposed a requirement that, at the time they make the finding for construction authorization, the demonstration of compliance may be able to take uncertainties and gaps in knowledge into account. That is the reasonable assurance concept that is in 10 CFR Part 60.

From our perspective, in the proposed program approach, what we were focusing on here was the difference between these points in time and the formal licensing interactions following the pre-licensing interactions and the fact that it is a performance confirmation program, part of the testing program that is starting early.

What we are trying to do is design a program that can get enough information in front of the NRC that they can accept a document for a docketing to being the formal licensing interactions. And then we can use information coming from the confirmation program, which is some of the stuff that can be in the exploratory studies facility. We can use the formal licensing interactions to refine our findings and our conclusions.

The important thing is there has never been a suggestion in the program approach that we are trying to do away with reasonable assurance. That is for Raj, wherever you are in the audience.

Raj and I agree on this part of it, I think.

[Slide.]

DR. VOEGELE: What we tried to do within the program approach was to develop our safety arguments in steps, and I think this is consistent with Part 60. I have not had a serious debate with anybody on this issue, but we have gone back quite carefully and tried to look through the statement of considerations for Part 60 in the rulemaking, and tried to revisit some of the issues that the NRC had before them with respect to trying to enhance the confidence in the performance of the natural barrier systems earlier on from the performance of the engineered components.

So we took the -- we tried to build upon that, and we tried to look at building our initial confidence in the performance of the overall system by looking at the demonstrated safety of the engineered components.

So starting with the components that are important to safety and trying to do safety evaluations addressing things, including, where we can, appropriate demonstrations in the exploratory studies facility of retrievability, criticality control, robust canister lifetime, to try to provide high confidence in the ability of a repository to function safely during the operational time phase. That was our first step in the program approach.

We wanted to move through to reducing uncertainty in the operational phases through the performance confirmation monitoring, and that, we thought, would allow the licensing decisions on the configuration to be somewhat sensitive to the demonstrations of long-term performance that would incorporate years of additional data gathered under actual conditions.

There is so much that can be read into that statement, but the important point from our perspective when we laid out the program approach was to get it started, to get a finding of reasonable assurance based on the information that we could deliver at the time of docketing, use the interactions during the construction authorization hearing, use the initial operation phase to continue to enhance the safety arguments, to continue to develop increased certainty in the performance of the system.

[Slide.]

DR. VOEGELE: That leads us to that word that is -- in fact, I've got them both in the first sentence -- what we have tried to describe this as trying to bound the natural barrier performance by using arguments that have sufficient flexibility to accommodate a range of conditions.

Our goal is, as I have said, is to provide sufficient information to docket the license application in 2001, and use those additional three years to gather additional data, strengthen our safety arguments and address specific licensing related issues, and then increase our confidence in the long-term performance through the performance confirmation program.

[Slide.]

DR. VOEGELE: What that looks like in terms of what could come out of a licensing arena, could be that there could be some terms and conditions on the license that could be modified as we get additional data. We could change.

You have heard several people this morning talk about moving from perhaps a cooler thermal loading to a hotter thermal loading through the licensing process.

I have used the word flexibility, I have used the word bounding. What we are working toward, I believe, is a situation of being able to demonstrate the best safety cases that we can, and using information from the performance conformation program to see if they can't be enhanced.

Pictorially, what we are looking at is we are looking at defense in depth, we are looking at the natural barrier system and the engineered barrier system as being a significant part of our argument.

We are looking at ongoing monitoring to help us build from some of the more straightforward engineering arguments out through to some of more difficult arguments about the natural barrier system, and increasing confidence in the long-term performance of the site by doing it that way.

[Slide.]

DR. VOEGELE: What that looks like in terms of activities and so forth, I am just going to show you this diagram to show you how it corresponds, and I will bring this particular chart up.

What we did when we developed the program approach was try to look at key performance related issues from 10 CFR Part 60, which are the same key performance issues that we had in the site characterization plan as the basis for developing our testing program, and looked through and tried to understand what kind of arguments we could make relative to safety to help support these decisions.

That looked something like this.

[Slide.]

DR. VOEGELE: It looks more than something like this; this is it.

This is our synopsis of the program approach. You will notice what we have tried to do here. This should look very much like performance objectives. These are the natural barrier performance objectives and pieces of them. Here are some of the repository design issues related to backfills/seals, materials interaction, retrievability, areal power density, emplacement mode, our pre-closure performance assessment, the need for lag storage, rail spurs and so forth.

Within the waste package design we have substantially complete containment, criticality containment, controlled releases, the materials that we would use, the waste form, and the EBS thermal conditions.

What we have tried to do is look at what we felt we needed to make the initial or the technical site suitability decisions and support the draft EIS in 1998.

We looked forward to what we could deliver and what we would need for the license application in 2001. Then we looked at how we felt. For instance, some of those things would be modified or further refined during the actual period of negotiation with the NRC proceeding the construction authorization.

Then we looked at further refinement of those safety arguments as a function of data that we would receive -- that we would acquire through the process of construction the repository, updating the license application and receiving process, and then the actual granting of a license to receive and possess, and continuing onward, acknowledging that there would still be reduced uncertainty possible in the program through a performance confirmation program that was running in the repository operational phase.

From our perspectives here, from the perspective here of areal power density relative to this meeting, we looked at that as something that would be bounded through a flexible design concept.

At the time of that license application, there would be a decision on areal power density, loaded. Before there was areal power density, before there was fuel loaded in the repository, but we did not believe that the final APD would come about until there was a significant amount of performance confirmation monitoring on the performance of the repository.

[Slide.]

DR. VOEGELE: To get that to the testing program, what that looks like for us in the testing program, I wanted to talk a little bit about the thermal strategy perspectives.

I've got this view graph in there. It is one that I just wanted to make sure I acknowledge to this group, that we know we have to change the design basis that we used in our site characterization plan because there are significant interfaces now between the part of the program that was responsible for acceptance, transportation, and disposal of waste, the old waste program, and what we have to do to design a repository to accommodate that.

We just wanted to highlight for you that we recognize the thermal goals that we are dealing with today as the basis for developing our testing program are more sensitive in the past to the MPC, sensitive now than they were in the past because of the MPC.

[Slide.]

DR. VOEGELE: I am going to try to tell you a little bit about what some of these flexibility issues look like, what they mean, and how that is impacting and is impacted by the testing program.

The key reason that we are looking at flexibility in our design is to permit modifications in the future that could allow improved performance in the system. That means that we would like to identify the key parameters to which that is sensitive, and define an envelope for those key parameters.

It is tempting to say that we will be able to look at thermal loading and find out that performance is overwhelmingly better at the cool end, or that performance is overwhelmingly better at the hot end, but I am afraid that we may find out that performance may be the worst in the middle and better on both the cool end and the warm end, and we have an optimization problem.

We have to look at how you want to move that design in order to get better performance. So when you hear us talking about reference thermal loading concept in the low range based on current design concepts for the technical site suitability strategy, that is driven by performance assessment, not necessarily driven by the perspective that one might have on engineered barrier performance. It is driven more by the natural barrier part of the performance and the concern that we have with refluxing, water being driven away from the canisters, condensing away from the repository, and coming back in at some date and providing a mechanism to corrode those waste canisters.

The argument that we have tried to develop to date says that if we can look at limiting the impact of our repository on the natural system components, it is more likely that we will be able to develop a safety argument sooner. Okay? I think that is the cleanest way I can say why it is that we put the word low-range here for the technical site suitability argument.

The licensing strategy which follows the technical site suitability strategy is really going to be more focused on that range of conditions over which the designs will work. The example that I was referring to earlier, for instance, that it may turn out that the -- you can get better performance cooler and you can get better performance hotter than you can get at a strategy that might be considered medium, something like we had in the SCP.

I don't know that we know the answer to that today, but I think we have to be open-minded, to realize that we probably cannot optimize the eventual decisions very early on in this program. We may have to use performance confirmation before we can say we have taken this to a more optimum condition.

The reason we would be modifying strategies compared to what we had used in the technical site suitability study is that as a result of our site characterization programs we had acquired data and developed models that allowed us to demonstrate convincingly that we could enhance performance by changing the initial argument that we used. That would be our driver.

[Slide.]

DR. VOEGELE: I would like to point out a couple of things. The program approach really didn't fundamentally change the overall SCP approach to how we were going to hopefully develop compliance and make our safety demonstrations.

What it did change significantly DOE's plans for getting information in front of the NRC. We are trying to find a way to make arguments that we can convince the NRC of the safety of a repository as quickly as we can, and that may mean we may have to bound them, the design may look more expensive than some of these other ideas of how you might do it, but the idea to get the information in front of the NRC as quickly as we can by finding a safety case and then working to see if we can improve the performance of the repository relative to that.

One thing that has been giving us problems as well as, I suspect, giving the board problems is that different components of the repository seem to react differently to different thermal loadings.

We can identify pieces of the repository components that seem to like cooler loadings; we can easily identify pieces of the repository that seem to like hotter loadings for their -- to find the optimal thermal loading for safety.

So our concern is that both design and performance assessment understanding suggest that different thermal loadings enhance different components of the repository for the safety argument.

[Slide.]

DR. VOEGELE: To show you what that looks like, I have just taken three. I have looked at the performance assessment perspective, the waste package design perspective, and the responsibility design perspective. This is a true systems problem: this one likes it cooler, seems to like it cooler today; this one seems to like it hotter today; this one could like it hotter but it is being driven by cost but not performance. That is just a quick snapshot of what we are looking at right here.

That is a definition of a systems problem. If you want to find the correct answer, or the optimum answer if you will let me look from that perspective, you need to investigate where you are going to be getting your performance, how you can buy more of the performance in a more cost effective manner.

That is where we are in the program today, trying to determine which of these strategies is the ultimate strategy which will give you the best safety argument.

As I have mentioned, the performance assessment perspective looks at minimal disturbance the existing site conditions of the 1998 technical site suitability decisions, and by 2001, it may continue to reduce some of the uncertainties. That could provide a stronger technical basis, the performance assessment people believe, for the 1998 technical site suitability determination. That is primarily because of refluxing, as I mentioned.

From the waste package design perspective, the engineered barrier, higher thermal loads may drive off the water and reduce the corrosion, and that could lead to a very high confidence in a safety element in a robust, long-lived waste package.

What we can't tell you today is what the true trade-off is in terms of disturbing the existing site conditions, and whether or not that is a more significant impact to the site performance than you can tolerate. That is where we are today with respect to those two conditions.

Then bringing on top of that the repository design perspective, if we can meet the requirements, including 10 CFR Part 60.21 where we look at alternatives, it may be more cost effective to favor a higher thermal loading for the obvious reasons that you would not have to develop so much repository if you could live with the higher thermal loading.

They have some operational considerations that would favor some lower thermal loadings.

So what I have here on this viewgraph is, I believe, a classic systems problem that we need some site characterization data to address.

[Slide.]

DR. VOEGELE: What does that look like, in terms of a site characterization testing program?

[Slide.]

DR. VOEGELE: We want to acquire data from our testing program to allow us to reach a consensus on the appropriate thermal loading for the key decision points that I have laid out in the previous viewgraphs.

We are also trying to, as quickly as we can, be able to respond to information that we get from the testing program so that we can redirect it as we need to to acquire the additional testing information.

If we begin to get performance assessment information that suggests that refluxing may not be as big of a performance impact as it appears to us to be today, then we might want to move more quickly in the direction of higher thermal loadings an we may want to accelerate some tests that will provide us information for that.

In the interim, we must use to appropriate bounding calculations and ranges of data to develop those performance and safety evaluations.

[Slide.]

DR. VOEGELE: What I want to do is show you what the test program for ESF activities looked like as our planning basis for the past several years as a test planning package called 91-5 which has a lot of ESF tests in it.

From the perspective of thermal loading, there are some related -- thermal-mechanical response, there are some related to construction monitoring and thermal-mechanical properties which are component of this 91-5 test planning package.

I think what you will see for the remainder of our presentations that are focused on the testing programs that we are trying to implement today, I think you will see that the DOE is looking for ways to accelerate these particular test components relative to the schedule that we are carrying in our current test planning design packages.

I think you will see new ideas emerging that look somewhat different from what we have here because the labs who have responsibility for providing some of this information are saying I can get some of that information more quickly if I change the test program a little bit, if I change the testing sequence.

So what I want to do, I want to give you this as a basis for where we have been from the site characterization plan through what you have seen from us before to set the basis for what you are going to see from people like Dale Wilder and Larry Costin in terms of where they are going with their testing programs.

I would like to lay out a schedule for that in terms of what we had in the existing planning documents as we began to implement to proposed program approach, the program approach so that you can use it as a basis to see jointly with us where we might be looking to get other information more quickly.

What we are looking at primarily in the early phases, construction phase testing, within the TBM envelope, construction phase testing within alcoves, primarily construction monitoring experimentation.

It looks like from our current planning basis more like the 1996 timeframe, we will be able to feel some of these mechanical tests that we have planned historically or tests that we are currently in the basis of modifying to be able to get that kind of information.

What will really hit home, I think, is my last viewgraph on this topic, to show you how that particular test program maps into a couple of those decisions that we were talking about as part of the program approach.

[Slide.]

DR. VOEGELE: Here is that 1998 Technical Site Suitability decision where we are looking at some bounding calculations on the natural barrier performance, ACD for repository, some bounding information, bounded information, on retrievability and so forth, ACD at that time.

I don't know how well this is projecting to the back of the room, but I tried to use the exact same schedule that I had before to see that we don't get a lot of this information related to thermal-mechanical testing started in time to be a significant contributor of this earlier 1998 tech site suitability decision where we address the guidelines in Part 960, but we do have it started fairly early relative to the information that we planned to have to submit to the NRC for the license application for the construction authorization.

I think what you are going to see from both Larry and Dale are ideas for enhancing our ability to get some of this information into these design decisions.

Questions?

DR. CORDING: Thank you very much. You made an effort to get us back on schedule, but I think we only have time for a few questions.

I did want to bring up one item, not so much a question, but a comment in regard to the expected information versus time curve.

I think the curve, as I see it at this point, is relatively flat with respect to expected information with respect to time, and it is going to start going up quite steeply as access is obtained to the underground and the work starts.

I think that this means that there is going to be a tremendous amount of information being obtained, a lot of things that are going to be looked at and analyzed. We are going to see so much more in the next few years than we have been able to see in the past, almost literally see these things and test them.

So I am concerned. The concern I do have is that if we are talking about delays in getting to the things or doing the work to give access to do the tests that may keep us longer on that flat curve and we are converging toward dates that we are quite tight on, so I am very concerned about the ability to accomplish these things or to get to the point to get to instead of being in the middle of trying to understand something, we have been able to actually accomplish an get to a conclusion.

I see particularly in terms of construction and the testing, the test program itself, and the construction, access is going to be extremely critical in terms of this type of curve and being able to have as much quality information as possible.

Mike, you might want to respond a bit.

DR. VOEGELE: I will. I think I see this from a subtly different perspective. I see it from the perspective of the expectations for information are very high, and our problem is to get up this curve as quickly as we can, to be able to provide sufficient information to the NRC to be able to convince them that we have adequate safety case.

I could not agree more that the time crunches are very real, especially as we have shown on this diagram.

I didn't take the time to go into a lot of detail about the backup behind this particular chart. If you remember, I showed you a series of technical components of -- it is the diagram that shows the elements of the proposed program approach decisions.

This particular one, there is quite a bit of backup information that supports what kind of testing programs we had in mind when we argued that we could bound that with this type of testing program.

I think, for instance, one of the Livermore tests, which I don't know if Dale is going to talk about it -- I will leave that to him -- but some of the testing program that we had down here with respect to waste package design, when we looked at the timing of that test as we had planned it, preceding the program approach, compared to the timing of the decisions that we were looking at today, we recognized that perhaps that big test that we had planned could be broken into two smaller tests, one to give some initial information and one to give some of the information at a later point in time.

I honestly don't know if that is consistent with Dale's thinking today. He is shaking his head yes, that it is consistent with his thinking today.

But the idea is to get the information out so that we can support these decisions, recognizing that we will continue to gain information even though repository operations.

DR. CORDING: I think it is very true that there are many things -- I think there are many types of tests and things that could be delayed because they aren't as crucial to site suitability decision or even to the license application as others.

But it just seems to me that -- we have talked in the past about things such as getting into the central part of the block, looking at major faults in those areas, getting across the block, seeing the faults on an east-west crossing; being able to do the hydrologic work to evaluate the effect of moisture content, the isotope conditions in and adjacent to faults; and the fast pathways below the repository in the Calico Hills, and then the thermal testing particularly related to the hydrologic features.

That is a list of what comes to mind in my mind of key issues. There are certain other things that can be, perhaps, delayed more, but certainly in terms of site suitability, those are areas where it is going to be a real push to get there, and it is going to take a tremendously efficient, organized operation to do it.

DR. VOEGELE: I agree.

DR. LANGMUIR: A related question over here, Mike. It excited me because it is the first time I have seen in any form a statement of what -- what a program approach is going to look at, focus on, and prioritize.

When you listed there under natural barrier evaluation travel time, which we have had quite a few presentations over the last few months and is very much into all of us nd will be, and that is -- those words by themselves, I could not identify what they meant, scenarios and subsystem analysis. What do you mean by those?

DR. VOEGELE: There are a number of potentially adverse conditions and favorable conditions in 10 CFR Part 60 that need to be looked at in the context of demonstrating compliance with a performance objective.

Those are the kind of scenarios we are talking about, right.

DR. LANGMUIR: You focused in on the source term.

Is there a missing link in that assumption?

DR. VOEGELE: No. I would rather let one of the PA people answer that question. I work on this part of it. If Gene would like to stand up and answer that question, I would not have a problem with that at all.

[Laughter.]

DR. CORDING: Jean?

DR. YOUNKER: This is Jean Younker. I think the answer is that in order for us to be credible we would like to think we would get a little bit better handle on the source term since those of you who are familiar with our TSPA right now know that that is one of our very, very weakest areas.

So that is what, to a certain extent, is being communicated there, Don.

DR. LANGMUIR: We are looking forward to sharing more about each of these as time goes on.

DR. CORDING: Thank you.

Leon Reiter, Board staff?

DR. REITER: A couple of quick questions. Steve Brocoum said that he might give us some insight as to the rationale why he picked the low thermal loading strategy at this point.

I just want to make sure I heard you say that the reason was that you didn't think you could demonstrate or make a safety case unless you went the low thermal loading strategy because you could not deal with refluxes. Is that correct?

DR. VOEGELE: That is my understanding. Once again, I would be happy to let Jean add anything if she would like to, but my understanding is that the refluxing was the primary reason that is driving us in the direction of the lower loading.

DR. YOUNKER: I think the principle -- and it is a fairly simplistic view of the world, but you know that fluxes are our most sensitive parameter.

If you look at our TSPAs you know that the highest correlation with the releases is with flux, and so if you have to deal with some additional flux besides that, which is just the ambient flux, meaning that we have to redistribute some and we have to face that as well in our release modeling.

But I think in a naive sense, at least, that not having to deal with that give us a bit simpler role in our modeling.

DR. REITER: This is a really important decision. Is there some analysis or laid out documentation we can look at?

DR. YOUNKER: I think there will be. We are not at the point of having that yet, but you are seeing something is very much in process right now.

DR. REITER: Could you put on slide number 7?

DR. VOEGELE: Could you tell me what it is?

DR. REITER: It is this one here.

DR. VOEGELE: Sure.

DR. REITER: I have to use that because unfortunately we didn't get all the slides. Some of them were not printed on the pages that we got.

But one of the slides -- I think the slide before -- said that you had to end with the SCP.

Is it your intention to do all the work of the SCP eventually?

DR. VOEGELE: Is this the one before that?

DR. REITER: Yes.

DR. VOEGELE: Is that what you are referring to?

DR. REITER: Yes.

DR. VOEGELE: Okay. When we prepared the site characterization plan, I think we tried to make it as comprehensive as we possibly could to give the NRC the maximum exposure to our thinking about how we would deal with issues.

I think it is fair to say that in the site characterization plan we were unable to precisely allocate performance to the components. This is exactly what Steve Saterlie mentioned earlier.

Consequently, we have a very conservative testing program in the site characterization plan.

The SCP also contains statements to the effect that we recognize that, we it has always been our intention to try to use information that we gathered from the site characterization testing program to continue to refine the program and do away with those tests that we did not need to do, to do our safety case arguments.

I think the answer I am trying to give you is that it was never out intention or expectation -- I think we intended to do it if we had to -- it was never our expectation that we would have performed every test that was in the site characterization plan.

DR. REITER: One last quick question. Could you put that other slide back on, the one you just took off?

Could you give us a definition of the level of information, the type of information, that you need in 1998 similar to that which -- how would you describe it?

DR. VOEGELE: That is a rough one. It really is. I think there are no procedural guidelines in 10 CFR 960 about the level of information that constitutes a higher level finding.

There are statements that the wordings of the higher level findings themselves indicate to you DOE's position. If I remember correctly, the statement of the higher level finding, it is something to the effect that the indication currently is that the site should not be disqualified, and the expectation is that further information would not lead you to believe that the site should be disqualified. It really does have all those negatives in it.

DR. REITER: What about the qualifying condition?

DR. VOEGELE: That is the qualifying condition. It is the same words. Okay -- I'm sorry.

The site is qualified and there is not information to suggest that the site should not be qualified.

There are two appendices in 10 CFR 960 -- I believe they are three and four -- that state what constitute a higher level finding from the DOE perspective.

DR. REITER: Stepping back, could you give us an idea of -- if somebody says what kind of information are you going to have in 1998, what would you say?

DR. VOEGELE: My goodness.

DR. REITER: You've got it on that chart.

DR. VOEGELE: But I don't have the 1998 DOE decision on the chart. I was focusing on the NRC aspects of it.

These are not DOE words; these are NRC words. I can't find you comparable words like that for Part 60.

It is going to be a process where the DOE and the scientists working on the program make their best argument against the qualifying conditions of 10 CFR Part 60, and we are going the have that external peer review.

I would like to make a couple of comments. We are going to put together these reports. They will be submitted to the National Academy of Science. They will also be published for the world at large.

The National Academy will review these. They will get comments from the world at large. They will make comments for additional information and identify what uncertainties that additional information will reduce.

We are going through that very detailed process. We will also then do an assessment based on the report, the National Academy Review, input from all the interested parties, and then we will make our recommendation to the director.

There is no magic amount of information beyond which threshold you can make a decision, below which you can't. It all has to do with the amount of uncertainty in the decision you are making.

DR. CORDING: Thank you very much, Mike. We appreciate your presentation and look forward to discussing more with you in the other session this afternoon, the open session.

We are going to need to have a little delayed lunch. We will reconvene after lunch at 1:30 rather than 1:25.

[Whereupon, at 12:20 p.m., the meeting was recessed for lunch, to reconvene at 1:30 p.m., this same day.]

AFTERNOON SESSION

[1:30 p.m.]

DR. CORDING: If you will please take your seats, we are ready to start the next presentation.

This afternoon we are going to be talking about some of those site response to thermal loads, continuing from what Mike Vogel gave to us just before the break.

Then following an afternoon break, we are going to assemble for a roundtable discussion with comments from speakers as well as others that are in the audience here today.

Ardyth Simmons with the Yucca Mountain Site Characterization Office will be describing some of the coupled processes.

COUPLED PROCESSES OVERVIEW:

THERMAL, HYDROLOGICAL AND MECHANICAL

[Slide.]

DR. SIMMONS: Thank you.

I am going to be setting the stage for some of the more detailed presentations that are given later this afternoon and tomorrow. Because of the importance of thermal loading to coupled processes, each of the processes we will be talking about will be linked to the thermal conditions.

[Slide.]

DR. SIMMONS: My presentation will discuss what we think is an accessible approach for demonstrating compliance with regulatory requirements. The fact that it is an iterative approach, I will be presenting our overall status and plans at the present time, and then talk a little bit about how performance assessment will use the coupled process models that we are developing.

[Slide.]

DR. SIMMONS: The Department of Energy recognizes that it must demonstrate a logical and systematic understanding of the way that thermal, mechanical, hydrologic and chemical processes associated with a particular underground facility design will respond, and we will primarily base our understanding on the mechanistic understanding of those processes which are highly coupled. I will make a point on that in a minute.

In order to demonstrate compliance with 10 CFR 60, particularly Part 133, but other parts as well, we must consider coupling these processes in a manner that doesn't overestimate the favorable conditions or underestimate the unfavorable aspects of repository design and performance. The performance assessment models that we develop will be capable of incorporating the predicted coupled responses associated with an underground facility design.

[Slide.]

DR. SIMMONS: The process that we are going to use is shown on this flow diagram, and this is taken from NUREG-1466, the NUREG which is guidance on thermal loads for an underground repository. Essentially, we ask ourselves a series of questions beginning by asking, "Whether there is a sufficient understanding, already, and sufficient experience for making a finding that the 10 CFR 60 performance objective is insensitive to thermal loading?" Of course, the answer to that is, no. So we go on to Question Number 2, "Do reliable predictive models already exist to quantify the sensitivity of the performance objectives to thermal loading?" And that is also a no, at the present time.

So we take a look at those phenomena which are thermally induced, and we develop a set of design goals and criteria which, of course, you have heard about to some extent already this morning, and we also develop a set of predictive models.

Then we go on to the application of those models to the underground facility design, and we look at whether those design goals and criteria are met. If the answer to that is no, then we may modify the design and we also may modify our testing and our models to help understand in a better way how the performance will be affected.

Then we look at whether the performance objectives are met, and if that is no then we go on to ask Question

Number 3, which is, "Is noncompliance with the performance objectives a design-related problem?" If the answer to that is yes, then we need to go on and look at our design again. If the answer to that is no, then we need to examine our models again and our testing again in order to be able to demonstrate compliance. This whole thing is a very iterative process.

[Slide.]

DR. SIMMONS: So I mentioned again a moment ago that we want to make sure that we are not going to either underestimate the unfavorable aspects or overestimate the favorable ones for repository performance, and in order to do that we have to present plans for the testing which we will conduct, and the monitoring, and the additional model development and refinement as may be appropriate to confirm the adequacy of the methods that we are using to support our application for construction authorization.

And we want to develop models which will predict the thermal and the thermo-mechanical response of the host rock, the surrounding strata, and the groundwater system based on a mechanistic understanding of the coupled process behavior.

We recognize, however, that we will need to balance the mechanistic/deterministic approach with an empirical or probabilistic approach, and this is because there will be some uncertainty along the way which we will probably not be able to develop a fully mechanistic understanding of.

[Slide.]

DR. SIMMONS: We will develop models based on an understanding proportional to the impact of coupling on the overall performance of the repository. As you have already heard this morning, we will be looking at two different thermal regimes. The lower, which is the one we are going with at the present time, and then also we have to understand the implications of the higher thermal loading, and these may have different couplings or they may have the same importance to couplings but of a different magnitude.

So our models and tests have to take these two possible different regimes into consideration, and we have to test at various scales to ensure that we will have an appropriate level of detail incorporated into our analysis. The rigor of our confidence building and testing and experimentation will depend on the temporal and spacial scales that are appropriate.

[Slide.]

DR. SIMMONS: We need to balance an unworkable complexity in our models against an oversimplification of the processes. Neither one of those is desirable. As I said, however, when working with mechanistic understanding of processes, it is likely that some residual uncertainty will remain, and we need to assess the effects of those uncertainties against our model assumptions and the predicted results.

When we are not able to have a mechanistic understanding at any particular time along the way that you have seen depicted this morning in our licensing stages, we will use conservative data and assumptions to compensate for those uncertainties.

[Slide.]

DR. SIMMONS: Now, I mentioned a few moments ago that we are going to look at those couplings that are significant to performance, and this is something that will be picked up in a moment in Dale Wilder's presentation. At the present time the most significant type of coupling has to do with that of the thermal conditions to the other types of coupling, to hydrologic, to chemical, to mechanical. Those are the wide arrows here.

We are also going to be looking at those couplings which are primary but of a lesser magnitude, the thermal to chemical, the thermal to hydrologic, the mechanical to hydrologic, and so forth.

[Slide.]

DR. SIMMONS: That is picked up again in this sequence which runs you through our prioritization, essentially, of the type of couplings that we will be looking at.

Here the first three, of course, are the dual coupling of thermal with mechanical, hydrologic and geochemical. Then we will be looking at those coupled processes which involve the geomechanics and the hydrology, and then the hydrology to the geochemical conditions.

At that time, we will have a pretty good understanding of the sensitivities of the various parameters to certain processes, and we may then be able to go to a second level of coupling which would involve the thermal condition with hydrological and mechanical, hydrological and chemical and so forth.

[Slide.]

DR. SIMMONS: This will be phased into our needs for 1998 and so on through the performance confirmation period.

[Slide.]

DR. SIMMONS: The next two pages in your handouts got reversed, but essentially what I have done here is to put in words what you have seen on schedules in some of the previous presentations this morning. By the end of today, you will probably have a pretty good idea of that schedule.

At any rate, in 1997 -- well, let me start from the bottom. In 1996, we plan to have the first access to the host rock for an early test in the ESF. This will probably be in the Tiva Canyon unit and, at the same time, we will be able to have preliminary data from small blocks. These are small blocks that have been excavated in association with the large block tests. We will have quite a bit more laboratory test data, and in '96 also we will be writing the technical basis report on subsurface geology, and this was one of the reports that Steve Brocoum mentioned earlier this morning which will contain the technical information as part of the suitability decision.

Then in '97 we will begin the early thermal testing. We will be doing the post-closure performance and groundwater travel time assessment that will go into the technical site suitability analysis for '98. We will have some data from the large block test and, although we won't have any data from the ESF, we will be able to make observations. We will be able to see where water might be occurring and the nature of the exposure of faults and fractures and so forth.

[Slide.]

DR. SIMMONS: Now to the previous page, in 2001 then we will be completing the subsystem and total system performance assessment for license applications, and as part of that there will be the substantially complete containment demonstration in which we will have tested the most fundamental hydrothermal hypotheses, and we will have put some bounding analyses on the remaining hydrothermal analyses. We will also have measured the fundamental thermal mechanical response.

So you get the flavor here that we will have bounded some things and be able to have tested and demonstrated greater confidence in others.

[Slide.]

DR. SIMMONS: Now I want to give you an example so that this all isn't kind of motherhood statements about what we are going to do. We have been -- that is, the testing community, part of the project, has been talking extensively with total system performance people about what information they need on coupled processes in the near term to help improve their models, and they have identified some conceptual model and hypothesis testing needs for us which are thermally dependent which I will get to in just a second to give you some examples.

What I want to bring out here that the abstraction and the sensitivity analysis associated with what will go into performance assessment will be developed at the process model level, that is the more detailed part of the PA pyramid.

[Slide.]

DR. SIMMONS: Some information needs which total system performance assessment has identified in the way of hydrological properties include porosity, permeability, capillary pressure, versus the saturation curve of behavior, capillary pressure at sub-residual saturations, also some geochemical properties such as solubility and distribution coefficient. Remember this is all how these parameters would be coupled to heat.

[Slide.]

DR. SIMMONS: In the way of testing needs, PA has identified as important an understanding of conductive versus convective heat transfer, the significance of enhanced vapor diffusion, the vapor pressure lowering due to capillarity and increased salinity, the potential for buoyant gas convection, and also the potential for non-equilibrium fracture flow. So these have been incorporated into the tests that will be conducted, and also into our models.

[Slide.]

DR. SIMMONS: This next table is really to lead you into some of the talks that will follow. What I have attempted to do here is to identify in the left column those tests which will be done in the ESF that are associated with coupling heat to hydrologic or mechanical or geochemical properties, and the processes are abbreviated here, thermal, thermal-mechanical, and so on.

The duration of the tests is here, and here I want to make a point that although there are numbers in this column, it may be that we want to run some of these tests out for longer periods of time. That doesn't mean that we would have to have a cutoff, let's say, of these first ones at two years exactly.

Here are the temperature differences and some of the information needs that will be extracted from those tests that we are trying to get information about, and in this column is when we would be getting this information, and you notice that it is primarily 2001 and later. We will have information prior to 2001, but the performance objectives that we are trying to meet and make an understanding of will be for 2001 or later.

Then these are the various characteristics of those tests that kind of help you understand how they will contrast in either the way they are laid out or the kind of access that we need for them, and that kind of thing. This should assist you in some of the later talks when you hear a variety of these different tests discussed.

[Slide.]

DR. SIMMONS: Dale Wilder, who is going to follow me, will go into a variety of these, as will John Pott and Bill Halsey tomorrow.

Now, in conclusion to my talk, to sum up on our coupled process modelling and testing, because we have a sequential nature to our repository licensing, this will allow us the opportunity to do in-situ testing for long periods of time during the performance confirmation period, and during that time we will be able to have confidence building in our coupled models. We are using the term confidence building rather than validation, but we will have improved understanding and testing of our models, and this is the process that we expect to use to be able to demonstrate reasonable assurance.

The detailed information needs for thermal testing will support the performance assessment models and these, in turn, will support the compliance strategies.

[Slide.]

DR. SIMMONS: Then the last page, which is in your backup information, is just a list of those studies which we are conducting which will have coupling of the mechanical, hydrologic and chemical processes with heat.

DR. CORDING: Thank you, Ardyth.

Time for one or two questions with the Board.

DR. DOMENICO: Ed, I have a question. Domenico.

Ardyth, is there a study plan prepared for these items you have?

DR. SIMMONS: Yes. Each one of them has a study plan or, in two cases, where it says SIP, that stands for a scientific investigation plan. All these others are study plan numbers.

DR. DOMENICO: Those study plans are ten years old, more or less?

DR. SIMMONS: No.

DR. DOMENICO: Some are hot off the press, I see.

DR. SIMMONS: They are. Like this one, for example, this one on the conceptual model of mineral evolution, that is hot off the press. I will give you another example, the chemical and mineralogical changes of the post-emplacement environment, that is so hot off the press that it hasn't quite gotten out of the project office yet. We just completed our review of it. So these tests are not old tests that have been hanging around since the beginning of the SCP, although they --

DR. DOMENICO: They just have old SCP numbers.

DR. SIMMONS: Well, our SCP numbers don't change. We need to provide continuity there. But the tests are kept up-to-date. As we get new comments on them, either from the NRC or from other parties, we incorporate that information.

DR. DOMENICO: My next question is, and maybe you are not the one to answer this, but I would like to keep in mind for the people following, I really don't see anything in here that can justify or give you some justification for going from below boiling to above boiling. I think there is a large emphasis on thermal-mechanical effects, which are probably the easiest in coupling. The thermal-hydrological is the thing that worries you. Things like refluxing, things like that, I don't see what sort of information you are going to collect in these heater tests that can address that, and consequently I don't see what how you could possibly use this information to justify going into the hot-hot regime from what I see.

DR. SIMMONS: Well, I think that, first of all, people like Dale Wilder will be able to give you very good confidence of what information we will be able to use to go into a hotter regime if we so choose to do that.

If you take a look -- I can answer the programmatic part of that question. If you take a look at the tests which are listed up here, the first, second, the fourth, the SIP on the large block test, all of those are very critical for understanding the hydrologic behavior and the way it is coupled to the chemical behavior and the mechanical behavior.

We have some other ones such as the altered zone study, these ones on mineral evolution, those, of course, deal with the coupling of the chemical effects with the heat. The same is true of the integrated radionuclide tests. These and the 8.3.1.15 series do, indeed, have more to do with the thermal mechanical properties. That is true. But I think that overall you see a very good balancing of the coupling of all those different processes throughout our testing program.

DR. DOMENICO: I guess this will be made clear later, but I would be very anxious to know just what you are going to measure and how you are going to measure it in the thermal tests. I think that is sort of important.

DR. SIMMONS: Well, yes, and we have that planned as part of our program for you in this meeting.

DR. CORDING: Thank you.

Other questions?

Don Langmuir.

DR. LANGMUIR: Don Langmuir.

Under geochemical properties that you need to know about for the TSPA, you have listed solubility and distribution coefficient or K_d. That is Overhead 13 of yours -- it doesn't matter -- those two items, and something I have wondered about is, how much interplay backwards and forwards there is in the program between TSPA and the geochemistry program in areas like this because, for example, TSPA, some of the TSPA program inputs have been -- Los Alamos has been unwilling to provide temperature coefficients for solubility, for example, for some of the modelling work.

I wonder to what extent the uncertainties in not having temperature coefficients for solubility and not necessarily having temperature coefficients for K_d's, for everything anyway, is warranted to worry about? I mean how much have we taken the uncertainties in these parameters which are required or needed according to this and decided whether it was necessary to measure them at all, if those were sufficient uncertainties to warrant additional work?

DR. SIMMONS: Well, I think you really asked two questions there, Don.

DR. LANGMUIR: Only two?

DR. SIMMONS: One had to do with the amount of communication that goes on between the geochemistry community or perhaps another part of the testing community in performance assessment, and then the second one was how important are some of those parameters, how sensitive are they to temperature, and do we really need to have an understanding of their variation with temperature.

In answer to the first one, on the communication issue, I should say the testing community has been making a concerted effort, as has performance assessment, to have more extensive communication, and in the area of geochemistry, we have a solubility working group that meets regularly and our next scheduled meeting will take into consideration just exactly those parameters that we have been talking about, and we will discuss them with performance assessment. We have another exchange planned in January to take up some of the other parameters and discuss their sensitivities.

So I think that there has been in the past couple of years a really concerted effort towards getting closer linkage, and now with the program approach we will see even more of that.

Then to answer your second question about the sensitivity of those parameters, the geochemists in the community would say that with regard to -- let's just take solubility for an example, that we are not going to be able to provide much better demonstrations of the sensitivity of solubility to temperature than what we have already provided, and the experiments that were done incorporated different temperatures. They were done across a temperature range.

So basically the results that you have demonstrate that sensitivity already, and it does not seem that it will be possible to improve that.

DR. LANGMUIR: But so far, even though you are aware of some experimental results indicating temperature sensitivity, I don't see that information going to the performance assessment people, at least some of the models are not showing any inputs like that in TSPA.

DR. SIMMONS: I think that is a correct statement, but the information is there.

DR. CORDING: Okay. Thanks very much.

We can come back to some of these questions again as well. It seems that the coupling of the tests is going to be as important as the coupling of the coupled phenomena in order to be efficient with it and get the work done in the time available.

The next presentation is Dale Wilder with Lawrence Livermore National Labs, and he is going to be presenting some material on site response to thermal loading, and then he will be talking about information expected from the large block test.

So we are really getting a start here on going into the details from the overview that Ardyth gave us going into what is really going to be done, I assume. So we are looking forward to that, Dale.

SITE RESPONSE TO THERMOHYDROLOGIC,
THERMOCHEMICAL, AND THERMOMECHANICAL COUPLES

[Slide.]

MR. WILDER: As was stated, I will be talking about the site response to the various coupled processes, and I understood that the information was to be at somewhat of a higher level, and so there may be some details that I will not be covering. There are people in the room, and certainly others who, during breaks, could get into some of the real details. I will try to answer some specific details as it seems appropriate.

[Slide.]

MR. WILDER: I apologize in time to get my slides put together, I didn't have all the slides in your package. You already have enough in that package, but just to show you what I am going to be talking about, this is kind of an outline.

What I want to do is spend a little bit of time just talking about the basis for the understanding, and I think that is very important because we talk a lot about what we think the site is going to do and I think it is important to realize that there is some reason for that thought, but there is also a lot of assumptions and so forth that is rolled up in that. I will spend a little bit of time talking about the basis for our understanding. Then I will talk about how we believe the site will respond to essentially the emplacement of waste. Then what our plans are for improving our understanding. Then I am going to talk about the large block test. There is some material in your packet that talks somewhat about the ESF test, and then tomorrow Bill Halsey will pick up on that material and focus specifically on some of the longer-range tests.

[Slide.]

MR. WILDER: The first item I was going to talk about is what is our current understanding. Of course, it comes from a number of things, but one certainly is site characterization. There were two or three slides I was going to put in here, but I figured, well, one as a representation is probably adequate, but we have a basic understanding that we feel that the repository horizon is going to be somewhere around 10 to 15 percent porosity, somewhere around 65 percent saturation. That has been rolled up into our model analyses, and so the information I will be showing a little bit later will reflect these kinds of understanding.

Of course, there is a possibility that there are specific zones within the repository and so forth where these specific parameters may not be quite representative.

[Slide.]

MR. WILDER: We have done some sensitivity analysis, and I know Tom Buscheck has almost bookcases full of different sensitivity analyses that he has done looking at what if some of these parameters are a little different than what we assumed.

The other presumption that we have made as we have tried to look at how the site is going to respond is that we have recognized -- first off, we don't have the ability right now with maybe a few very minor exceptions to fully couple our models. So we did try to break the coupling into some units that we felt we can handle.

Ardyth has already referred to this, but essentially we start with the temperature of the environment which is, itself, a coupled process. It is coupled from whatever the design parameters are in terms of the way the waste is loaded, how big the packages are, their heat output, and the thermal conductivities within the environment.

From that, we can produce temperatures which then couple, as Ardyth mentioned, to both hydrology, the chemistry and the geomechanics. A lot of the coupling to chemistry I am showing going through the loop of hydrology because a lot of our chemistry is dependent not only on the temperature but also on the hydrologic regime.

Of course, we recognize that the chemistry can impact the hydrology. It can also -- this should be a double-headed arrow. It can also have an impact on the temperature regime and, likewise, you have some coupling between the chemistry and geomechanics. The ones that we are stressing are those that are shown in the colored arrows. Those are the ones we are going to look at first.

[Slide.]

MR. WILDER: Having gone through the coupling, I also need to indicate that part of what our understanding is based on is our conceptualization as to what is happening in the mountain. This is almost -- I call it a prebody diagram. I know Tom has had people suggest that this is his Oscar Meyer viewgraph, but I think rather than talking about all the specific processes on here, I think the point that I would make is that each one of these is a process which we have to address in some way or another. In many cases they are processes we are going to have to address in field testing as well as in modelling, include such items as vapor diffusion from a saturated zone, bringing moisture in, certainly infiltration from the surface, vapor diffusion out of the surface.

Another point I would draw your attention to, and hopefully this comes out in your handout, there is a little asterisk in a circle in a couple of these. Those are really the only processes where we feel they clearly are only in the above boiling regime. All of the other processes we have to address in some way or another regardless of what your regime is. Now, they may not have the same magnitude.

For instance, if you have a thermal gradient such that it overcomes or at least minimizes this vapor diffusion, the magnitude will not be the same, but we nevertheless have to address that in our models.

[Slide.]

MR. WILDER: So let me then get into what we think the site response will be. I am going to start with some old data because I think it is informative. This is data that you have seen years ago in which we did some calculations and said, okay, we recognize that there is going to be a dried out zone where saturations are essentially at zero percent, and I won't quibble with whether or not that really is going to take place with a very small pore structure, but at least in this model you could calculate a zero percent saturation increasing as you get away from -- I should have said this was the heat source here -- as you get radially away from the heat source, saturated increasing back up to not only the ambient, but then we expected that there would be a saturation zone build up away from the heat.

In general, that still would incorporate the processes as we understand them with some minor caveats. That is, number one, magnitude, but secondly all these processes may not be as depicted in this simple model.

[Slide.]

MR. WILDER: Our G-tunnel experiments -- by the way when we were talking about G-tunnel experiments, there were a number of G-tunnel experiments. This is the bore hole heater experiment that Livermore performed, and before the test calculations of the saturation profile were very much similar to what I just showed you in which we expected a dried zone, and then a saturation zone build up.

What we saw at G-tunnel was the dried out zone developing, but we didn't see the saturation halo, at least in this particular series of bore holes.

[Slide.]

MR. WILDER: What we did see when we monitored temperatures was an interesting process. These represent thermocouples that are at various locations, and so we tried to put them in the report, you will see they are in terms of radial distance from the heat because heat should have symmetrical responses in terms of the moisture, but what we saw was that the thermocouples above and below the heater were essentially as we would have predicted. There was a constant ramping up in heat until we turned the heater off, and then, of course, a decay.

A couple of thermocouples that were off to the side of the heater, we saw some strange behavior. As you see the temperature building up, and then suddenly there is a kick in temperature, and then it levels off.

What we feel had happened there, and the interpretation is that we were seeing condensate which had built up in the fractures above the thermocouple locations coming down, and of course the condensate is hotter than the rock at the location of the thermocouples and, therefore, increasing the temperature where the thermocouples are looking at, and then as the boiling continued to move out with water in that system, it is pegged right at the boiling point.

So, once again, these are some issues that I am going to be addressing that we are going to be looking at in both our large block test and our ESF heater tests to look at issues like can condensate shed and does it build up?

[Slide.]

MR. WILDER: While talking about that issue, I did want to address just briefly another kind of a conceptual model, one of the parameters I jumped over rather quickly was our understanding of the porosity in the rock. Irregardless of what porosity you assume for the rock, one fact is quite clear, and that is that the matrix pore volume is perhaps a couple of orders of magnitude greater than the fracture pour volume.

So if we do dry out a region of rock, then the question is, where does that water go? You heard the comment about the water sitting up above the waste packages which could then come in and impact the waste packages at some future date upon the water ready to drain. Well, these are some back of the envelope calculations. All I just did was took the geometry and the ratio of the porosity and also the saturation allowing no imbibition in this case. I am looking at what happens if the condensate condenses more rapidly than you are able to imbibe it into the matrix.

The point is that depending on what the design parameters are in terms of drift spacing, you can build up a significant head of water in the fractures, that is fill the fractures, and that water is going to have a lateral driving force which could drive it away from the repository. One of the issues that we are going to be looking at in the large block test is, "Can you build up this condensate?"

If you do, does it? As one extreme case would show here, it almost has to go out the top of the mountain, you are almost going to have to be pushing water out of fractures. We don't think that is going to happen. I mean 300 meters of head on a fracture is certainly going to be driving it horizontally rather than continuing to build up vertically. But that is one of the issues that we are going to look at in our large block test.

Likewise, with 300 meters of head, you may have enough head to drive that water down through the thermal zone, and that is another issue that we will be looking at in our thermal testing.

[Slide.]

MR. WILDER: I have put in your packet a somewhat detailed table just starting to walk through the environment. I didn't do this for all of the spacial zones, but the point is, we need to, when we ask the question how does the site respond, we need to look at it both temporally and spatially. So what I have done is, I have taken first the waste package and drift, and that isn't the focus of this meeting, but I would just point out that depending on the timeframe you can have quite different environments. Also depending upon whether you are at high or low AMLs.

For instance, at the low AML we would expect -- now you can quibble with me and I guess I would have to quibble with myself because I called it warm down here, maybe hot isn't quite the right descriptor there, but certainly an elevated temperature regime, somewhere between 50 and 80 degrees C. We expect that it will be humid, somewhere around 70 percent. I will show you some backup calculations on some of this to support that.

But, again, depending on how the waste is emplaced, you could have local hot conditions. That is, if you have a large MPC, even out to AML by merely spacing those MPCs a long ways apart, then close to the MPC you may have temperatures that are locally high with an average temperature over the repository somewhere around the 50 to 80 degrees C.

With the high AML, because they are close enough together, you are going to be approaching in the early timeframes this 100 degree C or the boiling point, which will result in lower relative humidity and minimal liquid water contact. I would refer you to a paper that Tom Buscheck has recently published for the ANS which goes into the relative humidity conditions for the waste package. I think it would be informative. As I say, that is not the focus today, so I am not going to dwell too much on that.

The only other point I would make is, depending on what your humidity conditions are and whether you have liquid water, you may or may not also have to consider coupling with manmade materials and biological activity. So those are factors to consider.

[Slide.]

MR. WILDER: Having given you a table like that, I almost have to show where some of the information came from. Once again, this is an analytical model based on a number of assumptions. We certainly would not want to say that we feel this is the absolute numbers, however, we have done enough sensitivity studies, I think, to feel pretty good that unless there is some mechanism that we haven't taken into account that we are probably at least in the ballpark.

What we see is, depending on what the loading is and, of course, the ages, so I have tried to use the same ages for comparison purpose here, we may be looking at temperatures -- these are waste package surface temperatures now -- which could get up in the 130 maybe 150 C range depending on the AML, and could be maintained for the older emplaced waste for a number of years. This, of course, is extending out to a thousand years.

[Slide.]

MR. WILDER: You have seen, I am sure, this figure before which gets out to the 10,000 year timeframe. Once again it is a function of in this case it is the ADP and also the accompanying liquid saturation. From this, as I say, Tom has done a number of calculations looking at what the humidity temperature conditions are which will have a dramatic impact on the waste package performance. It also has a big impact on the geochemistry because you don't just have to have liquid water present to have geochemical reactions.

If you have high enough humidity, you can have geochemical reactions. So even within the rock that relative humidity is a very important factor to us.

[Slide.]

MR. WILDER: This is another plot which I believe you have seen before. In fact, I think this is '92, so this has been around for at least a couple of years. In this particular case, it is a calculation for 60-year-old fuel. Once again, we are not trying to suggest what the emplacement configuration ought to be, nor what the fuel age, it is merely one of a number of calculations that can be done, but it certainly points out one of the responses of the site, and I think it is an important response, and that is, if this is essentially the boundary of your dried out rewetting zone versus time, you can see that in this part of the repository system, which is about 75 to 100 meters above the repository, you can anticipate dry conditions to extend out to hundreds of thousands of years. I don't want to quibble whether it is 100,000, or 200, but certainly for very long timeframes.

However, in that same zone, the temperature will have dropped below boiling in a thousand, or in thousands of years. Likewise at the repository, you will see that the repository horizon will be essentially at below ambient conditions -- when I say dried out, I am defining it as below ambient saturation conditions -- out to almost 100,000 years. The temperatures, however, within 10,000 years will have dropped below boiling.

So the environment and the response of the site is that you can anticipate in this portion of the repository system to be looking at a rock which is seeing relatively drier conditions than ambient, and temperatures dropping below boiling for a good portion of the time that that rock is dry.

I'm going to skip these on relative humidity because you do have these in your package. I will also skip the one that shows there is an impact of the buoyant convection.

[Slide.]

MR. WILDER: This table, and there's three of them in your package, is too busy for me to be able to go through, except for a couple of points I would like to make. First -- I'm having a hard time reading it from up here. I don't know if that's focused for the rest of you, but the first point is that depending on the age of the waste at the time of emplacement, you can have quite different responses. What we're looking at -- I've got that too high for you to read -- is the peak temperatures during the period indicated. So, for the zero to 30 year time frame, we can be looking at peak temperatures of 103 to 170 degrees C, depending on the age of the emplaced waste. Out at the 100 to 1,000 year time frame, you're looking at 85 to 123. At the relative humidity conditions, you'll see that there's also a big difference, especially in the early time frame between about 9 percent versus 43, and of course that is a function not only of the eight, but it's also going to be a function of the AML. So, you see different AMLs reported.

I will let you look at those at your leisure. What I'm going to do now is try to summarize what this all means. There are about four viewgraphs that I'm going to take out of order. They were actually out of order in your package, and so if you'll just kind of put your finger there, I'm going to skip over those and we'll jump to the viewgraph that's getting into the geochemical processes. One of the things which has a big impact on the geochemical processes is what are the water conditions. So, Bill Glassley, working with Tom Buscheck has first tried to understand what the hydrologic regime is, and so as he's doing the coupling, he's looking at where am I looking at single phase, above boiling, and when you're above boiling the most likely geochemical coupling that you're needing to worry about are things like dehydration of minerals, possibly drain boundary dislocation, things that have to do with the geomechanics and the temperature but not so much with the hydrology.

When it gets into the two phase boiling, then of course that's an active geochemical region and certainly in this condensate zone, particularly where there's elevated temperatures. The other point I should have made was depending on the time frame, the zone where you can have that active geochemical processes taking place is going to change relative to the location of the repository. Here you can see it's within about 50 meters of the repository, below the repository up here about 100, maybe 150 meters. With additional time, and of course once again, this is a high thermal loading case, 30 year old AML with 154 MTU, at 1,000 year time frame, this zone which had been active in geochemical processes, now has moved down considerably.

So, one of the issues that Bill Glassley had been looking at and reported to the board about a year or so ago was using Dom Kohler's number approach to look at "Where am I?" in kinetics dominated versus equilibrium dominated geochemical reactions. I'm going to look at another approach that Bill has been taking to answer that question.

[Slide.]

MR. WILDER: In terms of how the site responds, it also is very much temperature dependent. This is lab data on essentially silica precipitation and dissolution work. The dissolution is more the quartz and the precipitation is more the polymers, but nevertheless, this is silica precipitation and dissolution. The thing that I would point out, this is laboratory by the way, laboratory data. This is not EQ3/6 calculation or model. These are laboratory data points.

The thing that I would point out is that there's about a two to three orders of magnitude slower rate in terms of dissolution than precipitation. So, there's going to be a tendency until equilibrium is reached at least, for plugging to occur more rapidly than the opening of fractures.

The second point I'm going to look at is "What difference does it make?" You'll see there's two sets of data. There's one essentially from 70 degrees up to about the boiling point, and there's another set of data down here below 70 degrees.

[Slide.]

MR. WILDER: What Bill has found is you can get a change in the porosity which is quite different for the lower temperature regime. In the 40 to about 70 degree C regime, the mineral suites -- well, the muscovites and the clays, the smectites and so forth are essentially always present, but some of those mesolites -- in this particular case he's looking at the clinoptilotes, is dropping out at about 70 degrees C. Above 70 degrees C, the mineral assemblage is dominated by albite. What this then has resulted in is that the relative change in pore volume up to about 70 degrees is a decrease, a plugging of the pores, and about a 20 to 30 percent decrease, and almost 40 percent increase in porosity above 80 degrees.

The issue this now raises, and the project will be evaluating this I'm sure over the next couple of years as we're trying to make decisions in terms of thermal loading, if you come in with a package and you're going with the large MPC and you space them out in order to stay at the low thermal loading, you have the potential at least, if the water is still present, and I should have mentioned, these studies may not be totally representative because they are assuming that you've got water present. That test in the laboratory conditions did have sufficient water available. When we are starting to do this in the field, we may very well have a situation where the water is isolated in a pore and as it starts getting caught up in the mineral changes, then that water chemistry is going to change and also the volume of water to rock is going to change. So, you know, we need to be careful trying to make too much out of this data, and I guess that's a point that's been made a couple of times, that we feel that we're starting to understand the processes, but we are early on in our understanding.

Anyway, given that caveat, if you have an MPC and you space them a long ways apart in order to stay at a low AML, you have the potential where the MPC itself is located being in this high temperature regime, and therefore creating greater permeability, or at least greater porosity, and somewhere down the drift or maybe in the pillars, plugging up the porosity. So, one of the issues that we will be looking at is can you then go in with a low AML initial strategy and then tighten things up in the future without impacting the response? I'm not sure that this is a bad response. It's just an issue that we do need to take into account. It could have a tendency to focus your flow in the future on those first in place packages, and maybe you just have to move them out of the way, I don't know.

By the way, this is somewhat I think consistent with data that we saw at G tunnel. Work done in laboratories show that we may very well seal the fractures up as we drive the water through the system. Our pre and post-test permeability data from G tunnel indicated that we actually had greater permeabilities after the test was completed, and we may very well have been in this sort of a situation where we were going through a different mineral assemblage. We had seen some powder, and we didn't know if maybe we were drawing clays out, and we still don't know, but at least it's possible that we were in this kind of a regime.

[Slide.]

MR. WILDER: I mentioned that Bill is using the Dom Kohler number to try to get a handle on the kinetic. What he's doing now is looking at mineral assemblages and looking at specific minerals and their interaction, trying to look at what is the rate of chemical involvement, the chemical reaction. So, if for each series you were to write a formula, which he's done, based on the time that's available to go to reaction versus the time required to achieve equilibrium, he can then plot that up.

[Slide.]

MR. WILDER: So, what he has done is looked at the -- this is probably pretty dark in your handout -- one specific mineral reaction, that is silica. What he found is -- and I don't have a color code for you but these are like several thousands of years longer than required for the chemical reaction to go to equilibrium, and what he's found is that except right in the very near field, he should be able to use equilibrium approaches everywhere. So that will be the approach that we're using. We're going to be using EQ3/6 equilibrium codes basically to evaluate the mineral assemblages out in the repository site.

So, as I say, with the exception of those regions very near the waste packages, we feel that we can use the equilibrium. Where equilibrium is achieved, the changes in porosity may be on the order of several tens of percent. They are sensitive to temperature as well as initial mineralogy and water chemistry.

So let me go back then and look at again some figures that you've seen before and talk about the coupling. These figures are figures from Tom Buscheck, and he usually figures to talk about what happens to the hydrology. I'm sure you recall them, in which the red indicates zones where the saturation has been decreased below the ambient conditions. Actually, if you look up at the top, you've got a scale for saturation, and so you can follow the plot in terms of what the saturation condition would be. In the blue are regions that are essentially greater than the original saturation and in this case may go to what would appear to be fully saturated conditions.

Along with that is the temperature. If you will note, we're looking at temperatures somewhere in the 70 degree range through a good part of that increased saturation zone. So, this is where we would expect to have, if everything else was equal, some plugging occurring either of the pores or the fractures. Likewise down in here. As long as you're above that 70 degrees, and high saturation, you may be making some fundamental changes to the hydrologic properties.

Within this zone, because there is little to no water, you would expect the geochemical changes to be of less magnitude. Certainly if you start getting down into the very low saturation conditions, and that includes, in this calculation at least, a zero saturation, right around the drift. These are average conditions. We're not saying that we expect this to be everywhere present, but the tendency is going to be where you need to worry about the coupling for the geochemistry, and particularly for the plugging of the fractures, is going to be up in these zones where the temperatures are still high. Where you're worried about perhaps opening things up would be down at the lower temperatures -- excuse me -- I flip-flopped that -- plugging down in the lower fractures opening up at the higher temperatures. So, you're talking about plugging up in these regions, down in these regions, opening up here and probably -- it's going to be awash here but once again, the zones change with time. So, it's a very complex issue when you ask how is the site going to respond.

[Slide.]

MR. WILDER: Same type of curve except now we're looking at rewetting, and the only reason I raise this as an issue is depending on whether or not your chemistry is completely irreversible, this may indicate now that your temperatures have dropped below 70 degrees pretty much through the whole system. The only place where it's still elevated is right at the repository horizon, but you still have saturation build-up up here. So, there is that potential for then plugging those fractures. Whether that's good or bad, you know, I'm not going to try to respond to. I think TSPA's going to have to look at that, but certainly in terms of the coupling, it tells us what our concerns are. It could have implications, for instance, for whether or not you can get flux coming through the mountain after you've placed the waste and it's cooled down.

[Slide.]

MR. WILDER: So, in cartoon fashion, and these are the ones that I said are out of order, if you'll go back to the middle third of your package, you'll find these cartoons. In this cartoon fashion then, if we're looking at the coupling of concern within the drift, we expect under either low AML or very early high AML to see a drift which has hot, humid conditions, the rock starting to dry out around that drift so that you have lower saturation. In this case, the coupling may be more critical in terms of thermal, mechanical and chemical, than hydrological, but of course it's going to depend on where you are in that zone as to how important hydrology is. If you get out into this increased saturation, then of course hydrology is going to be very important, and the coupling between hydrology and chemistry and thermal out in this region will be of great concern.

Somewhere out in here, and these are not precise zones. These are just conceptualizations -- somewhere out in here, you will be somewhat at ambient conditions and probably not impacting too much. However, the temperatures will extend much beyond the zones of saturation. So, to say that these are really ambient conditions is probably not an accurate depiction.

[Slide.]

MR. WILDER: At the higher AMLs, what we would then expect is that you're going to have a zone surrounding these drifts which are, if not essentially dry, certainly lowered saturation, with the zone of increased saturation as well as increased temperatures surrounding that. So, this is the zone where we need to be very concerned about the hydrologic and chemical and thermal coupling.

[Slide.]

MR. WILDER: Then of course as you get into the longer time frames, what you're going to start seeing is the drifts are essentially completely surrounded by a dry rock unit. This dry rock unit now will not be actively undergoing geochemical changes, but it may have already seen the geochemical changes from the time when the high temperatures have been moving through. This zone is going to be stable for some period of time, and so we would certainly expect to see geochemical changes down in these regions where we have saturations built up. This one will depend on whether or not that saturation can build up there, and so as I say, one of the things we're looking at in our test is can we get that kind of saturation increase.

[Slide.]

MR. WILDER: And you've got finally one that's looking at the very long time frames where you have increased saturation but now the temperatures have started to decrease again. There the hydrology and chemistry are probably more critical than the temperature and the mechanics because the temperatures have decreased considerably.

I apologize for getting those so out of order on you. Hopefully now we can get back into where everything is going to be in the order that's in your packet.

[Slide.]

MR. WILDER: Two fundamental regimes that we need to consider in terms of the repository heat altered flow and transport processes. One is the regime where heat drives the flow. The other is where heat alters the properties, either geochemical or hydrologic or mechanical properties. The point is that the heat altered property regime would continue long after the heat driven flow regime had ceased.

[Slide.]

MR. WILDER: In our test, and the reason for showing this is I'm going to be trying to talk about some of these issues in our testing. Regardless of the AML, we're going to need to worry about the spacial extent of heat driven flow. You will have heat driven flow for just about any AML. Now, if you really have an extremely low AML, then perhaps you won't have heat driven flow, but I think you'd almost have to do some sort of reprocessing in order to get down to the conditions where you're not going to impact the flow by heat. Just about any AML you can have will impact the flow.

Certainly the depth of the repository below the ground surface, that's not one that we're studying. That's a given to us. Likewise, the location of the repository relative to the water table is not something which we in terms of the testing community get involved in. This is a design input, as is the total inventory of the spent nuclear fuel.

I think this is an important point that sometimes gets lost when we talk about AML loadings. When you're talking about site response, especially as you get away from the very near field, the site really doesn't care how close you're spacing the waste. What it's really concerned about are these other issues like how close is the ground surface, what is the total inventory, how near is the water table.

[Slide.]

MR. WILDER: I am going to show you another couple of examples of concerns there are where we have coupling of hydrology and geochemistry. This is perhaps comparing a little bit of apples and oranges in that we've got different ages of fuel. The MPUs you can look at and certainly this would be typical of a low AML with young waste. The point is, depending on what the waste emplacement scenario is, you can be looking at coupling that's taking place near to the repository, and what these plots are, is the region where you can expect refluxing. It's the region where you're very close to the boiling point of water, somewhere between 96 and 100 degrees C, and so this would be the very active geochemical zone.

[Slide.]

MR. WILDER: Perhaps a better comparison except for the very near field which I just showed you, is to try to keep things on the same basis, and so this is for 22-1/2 year old fuel which I think is a little more consistent with some of the studies that Steve Saterlie had reported on -- I'll move that up and hopefully you can read the heading in your own packet. At 55 MPU, and the point again is that that zone of active refluxing will be maintained for about 1500 years in regions approximately 50 meters above the repository. That that's in red here or pink is the zone where you'd have active refluxing, and of course these are the time that that refluxing is maintained.

You'll notice that as you go up in the MTU, the times are increased somewhat, but the location of the zone is significantly different. So, when we're talking about the geochemical changes and the refluxing that can take place, the refluxing in this case is considerably removed from around the repository. So, if we're worrying about things like plugging and its impact on the repository, in this case, we're talking about units which are well removed from the repository. That's not necessarily to say that that's either good or bad, but that's what we have to take into account. Certainly it can have an impact, for instance, on some of those geologic units.

I should have also addressed a comment that was made about wanting to make sure that we didn't change the geochemistry. I guess I personally am not convinced that we want to avoid geochemical changes. I think what we need to do is evaluate what their impacts are. Some of our studies have indicated that if you have water present and you elevate the temperatures up into the 40 degree, maybe 60 degree regime, you can form a number of very absorptive geolites and clay minerals. So, when we talk about geochemical changes, I don't think we should jump to the conclusion that they're necessarily bad. It does complicate our job in terms of understanding the mountain because now we have to understand those geochemical changes.

The other thing that the site may do in terms of responding to the emplacement of waste is that as you heat things up, this heat is going to get down into the water table. These are calculations which Tom has done and others may have now, but I'm not aware of others who have assumed that the water table is not a constant temperature boundary. If you don't make that assumption, then you can see that there is a possibility of having these -- the heat impact at some depth down in the water table. Once again, this is a simplistic model. I'm going to have to talk about how we have to simplify our models. It doesn't show the layering, and so you may not get all of the circulation patterns that we're talking about here, but if you do get the circulation pattern and if we do have to to go a dual space standard, then we certainly are going to want to understand this because you have the potential now of getting quite a factor of dilution in terms of spreading any radionucleides that could get down here through a greater percentage of the saturated zone.

[Slide.]

MR. WILDER: Ardyth already talked about the sequence of trying to look at the coupling. We tried to bite off the easier to solve problems, that is, the coupling where you only have dually coupled, and the mechanical was one of the first ones we struggled with, and of course we did some testing on this one as far back as the spent fuel test. We are trying to get down to where we're able to couple, perhaps not fully couple but certainly coupling for instance in the thermal hydrological geochemical -- those are some of the results I just talked about. We are trying to get mechanically incorporated into that as well now, although we're further behind in the mechanical.

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HOW WILL THE LARGE BLOCK TEST AID
THE IMPLEMENTATION OF UNDERGROUND TESTS?

MR. WILDER: Let me tell you about the strategy that we're applying and how that's going to eventually lead up to support of things like technical site suitability and the license application. We recognize that we need to break the studies up into a number of different scales. These are more scale in terms of the test size. We are doing a number of lab scale tests, basically core to perhaps up to a half meter. There is a few one-meter type blocks that we're getting out of the small blocks of the large block test now.

Typically what we're looking at are property measurements and trying to understand matrix processes as well as single fracture processes. There's some ability with these larger blocks to look at multiple fracture processes, but we're really not able to look at issues like connectivity of the fractures and how that impacts the processes, nor the more fully coupled processes.

The large block test falls in this scale of -- well, it's essentially three meters by five meters scale. In this case, we can look at multiple fracture processes, certainly the interconnectivity. Eventually we're going to have to go to in situ scale and the ESF test that Bill Halsey is going to talk about are broken down into a couple of different tests, and I'll give you the rationale for this.

Some short term, that is one to three year tests, in which we can do site characterization, that we can do in situ over driven couplings, and test our models, but eventually going to the larger scale in which we can really look at the scaling effects and start to look at some of the heterogeneity impacts. As I will point out in a minute, there is no way that I will ever be convinced that you can get by without doing some performance confirmation monitoring.

Now, we have a great opportunity because of that period of retrievability to do some very large scale in terms of typical tests -- 50 to 100 year type tests -- in which now we're looking at the actual coupling. This is the representative scale coupling, and we can look at mountain scale heterogeneities. Up here, we are by definition going to have to be in overdriven conditions. So, there's always going to be some uncertainty up here. So, I think that's a very important period of time for us.

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MR. WILDER: I kind of hinted at some of the objectives of those thermal tests. Some of our tests will have as their objectives to identify the physics that needs to be included in our mechanistic models. Some of the reasons for those tests is where we can sort out the physics to help us to develop the empirical methods, to address the coupling. We certainly are going to be trying to build confidence in our modeling ability, and there's two key issues that we're going to try to address in our field test. One is whether or not our model abstractions are representative of the real world, and the second is, we have to put into our models some sort of assumptions and boundary conditions, and we're going to be trying to check the appropriateness of those assumptions.

The field test also allows us to gather rock mass property data which cannot be gathered in many cases from any other source, and then of course to look at the heterogeneity of the system.

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MR. WILDER: It's very critical to consider what the intended use of the data or the test results are. For prototype or early testing, as I mentioned, it's to identify the overall processes. For the ESF test, it will be to consider the impact of fracture networks, to do the characterization and so forth. As I will point out in a minute, it is not to be a direct test of things like thermal loading scenarios. There's no way you can directly test those, and I'll try to point out how we think we can support those kinds of decisions. The others are self-evident.

[Slide.]

MR. WILDER: To summarize a kind of logic flow diagram, the role and the way you do field testing, at least the way we're trying to do it is you first have to conceptualize the real world. We've talked about hypotheses and hypothesis testing many times. These hypotheses will help us to design the experiments so that we can test or distinguish between these conceptualizations. An example would be a condensate shedding versus build-up. If that's what we're trying to focus on, then we will design the test where we can measure shedding versus build-up. And of course perform the test itself, and there's a couple of different outputs.

Now one, as you may very well be focusing on, identifying the phenomena, or you may, through interpretation and analysis, be looking at parameter values. In both cases, there is an iteration loop, of course. If you identify the phenomena, you realize it wasn't incorporated, you may modify your test as the test is going on, or you may even have to come up with another test, but eventually you're going to have to apply some sort of analyses. Mechanistic model applications is what I'm calling them. You'll have to use some judgment in terms of your input parameters coming from back here. This sensitivity analysis, this kind of information will directly feed into our reports, the Near Field Environment Report and the Altered Zone Report. These reports all show you how they tie into TSS and the licensing.

To get below this in terms of the performance assessments, you do have to do some abstraction of the data. That is, you couldn't just take a series of temperature profiles and immediately plug that into a subsystem model. You have to do something with that.

[Slide.]

MR. WILDER: I know I'm running out of time. I'm going to skip a couple more of these. In terms of the hypothesis testing, I'm going to report on high and low AML hypotheses. This is not an all inclusive list. When we did this one day, we had, as I recall, something like 23 or 32 issues that we were concerned about, and we tried to judge where can we best address these. But these are the primary hypotheses that Tom Buscheck has put down in terms of hydrology that are of concern. Broken down by the low AML, where he's concerned about things like the mountain-scale, buoyant, gas-phase convection, or you can get binary gas diffusion affecting the moisture movement, and whether heterogeneity in either heat loading or the natural system is going to be a focusing mechanism.

[Slide.]

MR. WILDER: Likewise, there's a set of hypotheses, some of which are similar, some of which are different, for the high AML loading. In this case, there's a real concern over what is the mechanism for heat flow where there's convection. Whether or not you can get a significant reduction in the moisture that corresponds to the above boiling temperatures. How long the rewetting lags behind the collapsing of the boiling. And whether or not there's potential to build up this condensate.

[Slide.]

MR. WILDER: So, what we've done is we've looked at the various scale tests, starting at lab scale tests, large block test. Now, lab scale does include part of the small blocks coming out of this test. The lab tests versus the large block tests, the early in situ tests -- essentially those are abbreviated tests -- the longer duration, the four to seven year duration test, and then the performance confirmation.

This is all subjective, and the only point I would make is that we've given it essentially four levels of confidence. I think when we first did this, we actually gave it a numerical scale, but the problem with the numerical scale is that you think that the difference between one and two is the same as the difference between two and three, but that does not necessarily follow. So, we just used a letter grading system. As you can see, we don't expect to get too much information on the first couple of hypotheses for the low AML out of lab scale. We may be able to bump that up when we get into the early in situ test. Certainly with the -- I'm talking about low AML and I've got high AML here for you. Got to get these consistent.

Certainly when we're looking at whether or not the gas phase convection significantly effects the moisture movement, the subrepository scale, we can start to get a handle on that in our early in situ tests. We can quite well start to address this issue of whether the binary gas diffusion is going to affect the moisture. Well, I not only blew one of them, I blew both of them.

DR. CORDING: You've been presenting a lot of -- you've got a marathon going here that we've forced you into. I think perhaps we could take a few questions right at this point and then continue on. You're almost through to the halfway point anyway, Dale. I'm sorry to continue for that length of time, although some of us I know have taught more than one hour classes.

MR. WILDER: Well, if it's a chore. I appreciate your concern. I'm just not available tomorrow, so they graciously have allowed me to do both of my presentations in one day.

DR. CORDING: Okay, I'll ask John Cantlon.

DR. CANTLON: Yes, earlier in one of your charts you had biological couplings. Could you explain what you meant by that?

MR. WILDER: We certainly recognize that they are probably going to need to address biological corrosion, degregation of the cements, any biological materials that may be present. Some of the work that has been done -- well, some of the analyses. I think David Stahl has reported on some of this. Certainly Dan McCrite and a couple of others have indicated that there's a couple of temperature regimes in which you're worried about biological activity, but once the moisture's gone, the biological activity is of less concern. So, it's more of an issue when you have not removed the water from the system.

DR. CANTLON: So that the biological coupling here you're looking at in the near field.

MR. WILDER: Yes.

DR. CANTLON: And the question I have really is there any consideration at all for a far field biological coupling? The transpiration sucking out of moisture at the top maintaining a vapor pressure gradient of the system.

MR. WILDER: That's an interesting question. I don't know how much we've looked at that. Tom certainly has looked at the vapor transport out the top of the mountain, but whether or not we've included transpiration.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. We've been recently looking in a little more detail at the thermal gradient. Actually, not recently, going back to that meeting last March that we had on it, we've been looking at the extent to which the shallow thermal gradients is a function of thermal load, and it turns out that even for the coldest repository we've looked at, it could be enhanced by a factor of five, and at 10,000 years, it's enhanced by a factor of three for the lowest thermal load, which is only one-third of the enhancement for the highest 150 MTU per acre, which is actually way out of -- beyond what the system study is currently looking at. I think insofar as moisture loss out of the top of the system, we have a substantial change in the thermal gradient, and depending on how important vapor diffusion or how enhanced it may be found to be, that could be a very large component in terms of the overall moisture balance in the unsaturated zone, even for a low thermal load. So, we could be talking about substantial changes.

I want to mention that at least all the calculations we have done have assumed 100 percent relative humidity in the atmosphere, so that coupling has not -- we have not seen that impact because we haven't allowed that diffusive loss to occur, and that's one of the things we'll be looking at this year.

DR. CORDING: I have one question with regard to this -- the saturated zone above the repository and below. That magnitude of that zone is going to be very dependent on your assumptions regarding the diffusion and the other vapor transport mechanisms; isn't that correct? I mean, could this change by factors of two, three, whatever, in terms of the volume and the saturation?

DR. BUSCHECK: I want to mention that and then also mention the fact that we need to look a lot more thoroughly at much higher infiltration fluxes so that is going to have an impact as well. But the increase in filtration flux will have an impact, but we have recently looked at considering the atmosphere to be 90 and then 75 percent relative humidity and instead of a moisture buildup out beyond 10,000 years we saw a substantial deficit in the moisture buildup. Those results are rather preliminary but I think that once we start to address the coupling with the atmosphere, some of these results might change substantially.

MR. WILDER: Yes, and if I could also add on that, the shedding of course is going to also have a potential impact on that, a fairly significant impact, and that is one of the reasons we are trying to address whether or not shedding may take place.

DR. CORDING: You had mentioned something about high heads because of the saturated zone, but that again is going to depend on the flow out of the system. So I mean, just because you have a saturated zone of some depth doesn't necessarily mean you are going to have a head of that height.

MR. WILDER: No. My calculations on the head of that height was strictly if you don't get inbibition into the matrix. Now, the charts that I showed, that Tom had, are allowing for inbibition into the matrix. There is some evidence, at least in the field, that you may not get that inbibition taking place that rapidly. And if the inbibition does not keep up with the condensate buildup, my point was you have got to drive that water through this fracture system laterally away from the repository where it can then drain down through the cooler regions.

DR. CORDING: Pat Domenico.

DR. DOMENICO: Yes. You have a lot of silica being moved around in that system as well that can do some plugging up both for the inbibition that Tom permits in his model and as well as for the fracture flow. But my point is you got two phase boiling extending all the way to the water table, you got geolite dissolution possibly, you've got -- some of this silica is very, very mobile and you've got evidence from G-2 that suggests you get a reflex and model calculations that indicate that they are inevitable.

What is the advantage of high thermal loads and how can you use -- first of all, do you anticipate anything different in the extended -- the new heater test? And if you get some similar results, I don't see how you are going to make an argument for the going below boiling threshold that you are starting out with. I mean, you may do it but I don't see where that argument is coming from.

MR. WILDER: There are two parts I think I need to address to your question. First off, the question of what happens if this occurs. And the implications and terms of high versus low from AMLs. Depending on which zone you are talking about, the loading conditions are less of a factor than just the total amount that you are putting in there. And so this may be an issue we are going to have to address regardless of what the loading conditions are. I shouldn't say "maybe." We are going to have to understand it regardless.

The second is in terms of our testing how we are going to be able to address this. Now, our testing can address some of the hypotheses, some of the parts of our models. But eventually we are probably going to have to rely on monitoring at the right time scale, because we are over-driving things and we are not allowing for water to drain off the way it would normally and so we may not be looking at the right silica precipitation versus dissolution regimes.

But we will try to address as much as we can the silica redistribution within our test, jumping ahead to the large block test. One of the things we were hoping to do was do some chemical sampling and also then take the block apart and look to see where we have that silica redistribution and so forth. Part of that may not be in the cards anymore at the level of resources but we certainly still plan to take the block apart and look to see what has happened to the silica redistribution.

DR. DOMENICO: Has the G-2 -- I have never seen a report on G-2. And I just wonder, there is an awful lot of data collected. Has it really all been analyzed and published someplace and really looked at in terms of what it could tell us about the potential thermal behavior in Yucca Mountain? I have never seen reports on -- I see a few things in the literature on occasion but I have never seen it fully analyzed to the extent possible. A lot of data collected, but is there an analysis of that overall result?

MR. WILDER: A total wrap-up analysis that includes implications for thermal.

DR. DOMENICO: I think it has implications. I think that part is pretty important, to understand that fully before we think of another test.

MR. WILDER: I personally am not aware. I don't know, Tom, are you aware of G-2 wrap-up, total wrap-up report?

DR. BUSCHECK: If you are referring to Abe's report, we talk about the second --

DR. CANTLON: Are you talking about a bore hole G-

2 or --

DR. DOMENICO: I am talking about the whole testing effort.

DR. CANTLON: G tunnel.

DR. DOMENICO: G tunnel.

MR. WILDER: There was a wrap-up report by Abe Ramirez that summarized the results of the G tunnel experiment. He identified some surprises, he identified some things that were corroborated from our models.

We feel that one of the things that we really need to do is step back, catch our breath, and recalculate based on our current model understanding to see if we can learn anything else, and that has not been done.

DR. BUSCHECK: Also, the near field environment, the near field environment report also rolled up some of the G tunnel observations and I think that that -- the Abe Ramirez report which is quite lengthy in conjunction with the near field environment report, a lot of the insight that we gained was embedded in the near field environment report which is about a year old.

DR. DOMENICO: Thank you.

DR. CORDING: Are we ready to proceed a bit further here?

Okay, Dale.

[Slide.]

MR. WILDER: I am going to try, to the extent I can, to get through the two viewgraph presentations as quickly as I can because I understand that's what probably blows the circuit breaker here.

What I was pointing out was that this hypothesis or this issue, L-3, is the one which we really can address more specifically in the large block test. We will also be able to address to some extent the L-4.

[Slide.]

MR. WILDER: When you get into the high AML, looking at conduction and convection and so forth, we will probably be able to gain a little bit more information both out of the lab tests and out of the large block test on these hypotheses.

Having said that, I will turn this off so I don't blow the circuit breaker.

How long the rewetting will lag, whether there can be a condensate buildup, we certainly feel at least in the latter, the H-4, that we should be able to address that in the large block test.

[Slide.]

MR. WILDER: I hinted at a chronology of testing. There have been a number of tests and, of course, these are the tests which Livermore has been specifically involved in. There is a number of other participants doing tests which also relate to these issues.

But I would point out that we have been marching through a series of tests helping us to look at the coupling. I am going to talk about the large block test, the ESF will certainly move forward at looking at all of -- trying to wrap up all of the above issues.

[Slide.]

MR. WILDER: The way we plan to use this information is laid out in a flow diagram and I am going to walk you through this in chunks so don't try to understand this all right now. But what I would point out is I do have some milestones up here or some DOE milestones or reports that are key feeds for us in terms of our data and I will show how we are intending to support those decision points.

One of the things which is incorporated in that is a series of very abbreviated tests and we recognize the problem with scale. And it goes at all levels. This is just probably one of the easiest to depict, the scale impact. And this is just looking at the power that would be typical of emplaced waste. Now, this maybe isn't all that typical anymore. This was an old calculation done at the time of G-tunnel experiment so it is looking at fairly young spent fuel and it is looking at the referenced case in those days, which was the vertical emplacement.

Nevertheless, you can see that we are talking about powers that don't completely decay until out in the 1,000 to 10,000 year time frame. Whereas any test that we do by definition is going to have to be done within one- to 10-year time frame. So our tests are, by definition, going to have to be greatly accelerate.

The issue is how much acceleration can you tolerate before you get to the point where you can't defend the work that you are doing.

[Slide.]

MR. WILDER: Let me then walk through our philosophy and then I will tie it back to that figure that I was just talking about.

In terms of lab testing, I am going to be talking about small blocks. I have shifted, by the way, into the large block test phase of the talk here but if you are looking at the small blocks coming out of the large block test, the lab test will give us two things. One is we are going to be identifying phenomena. To a large extent we are focusing on the small blocks for our geochemical analyses. That is so that we can have better boundary control and we can control the processes and so we will be looking at the small blocks in order to identify the mechanistic models.

We are gaining parameter values but these parameter values are not going to support the license application or directly TSS. These parameter values are the ones that have to be applied in these mechanistic models to help us to analyze the large block test. And so when I talked about parameters, out of the lab right now, coming out of those blocks, by and large they are not going to be feeding directly into analysis of Yucca Mountain.

Having said that, we recognize that there is at least some similarities, some justification for trying to take the information from Fran Ridge and apply it. To the extent we can justify that in our mind, then we will feed the altered zone report.

[Slide.]

MR. WILDER: I was going to try to use the two viewgraphs and show both of these logic charts side by side but I am afraid I will blow the circuit breaker so let me just walk through all of these first and then I will go back to this logic diagram.

The large block itself is largely intended to identify the phenomenology necessary to do an ESF test. We need to make sure that whatever our conceptual understanding is that it doesn't have to be tweaked during the testing at Yucca Mountain, because Yucca Mountain will be giving us -- or our ESF testing, I should say, will be giving us information about Yucca Mountain which will be used in a license application. And so we need to make sure that we are not tweaking off so fast that we can't use that information. So the large block test will be very critical to us to make sure that we've got the physics, all of the right physics incorporated.

An example of that was earlier information I showed on G-tunnel where we had not incorporated the capability for saturation buildup to drain. We knew gravity exists, we knew that a long time ago. But the significance in our models needed to be increased.

[Slide.]

MR. WILDER: Just to complete the sequence, and this is what Bill Halsey will be talking about tomorrow, the engineer barrier system field test will give us parameter values that do go directly into both near field and altered zone reports to support the license application and to support all of the subsystem and total system PA.

[Slide.]

MR. WILDER: I tried to put it into terms similar to what Mike Vogel had shown. We feel that each one of these different tests are going to give you a different set of information, and therefore we'll increase your level of understanding. Of course, as you first start making either lab tests or field tests, you're going to gain information rather rapidly, and then there's going to come some tailing off.

The way the sequence is designed is that we will hopefully be completing these tests at such a time that we will have increased the level of understanding significantly at the time it's needed for both TSS and the license application, license to emplace. Now, you heard comments that we're not going to get much information out of the ESF testing for license application. I'll show you why I think maybe that is not totally accurate. I think we can get some information, and I'll go back to this diagram to show that.

[Slide.]

MR. WILDER: The current schedule is that we will have the small block testing done in the lab in sufficient time, and this little arrow should be coming up here and connecting down here, to give input to the technical basis report on rock characteristics in '96. Certainly that information will be available and will have been analyzed and incorporated in its reports prior to trying to put together the TSS. So, we think that there is some information which will be coming from the large block test. Now, admittedly, it's coming from Fran Ridge, and so there will have to be some dealing with how representative is Fran Ridge. Certainly some of the information in terms of how fractures are formed and so forth, should be able to support TSS in terms of the imbibition response, the dry-out and so forth of this typical rock.

We also expect to have enough of the large block test in terms of the heating cycle completed that we can at least in '98 when that report goes in, give some indication as to how the rock is responding, but it's not going to be as solid information. That's why it's shown in a lighter blue.

We're going to have to rely very heavily on the large block test and its analysis. We will be getting some lab information from core and from block samples that are available out of the ESF itself. So, this lab data will also feed into the license application, the 2001 license application.

The current plan is that we should have been able to get through at least the heating cycle and possibly partially into the cooling cycle in terms of analyzing it prior to that license application. So, I would submit that the test strategy that we've laid out right now will indeed support that 2001 license application. Recognizing that there's a lot of scaling issues that we haven't addressed. We will have an ongoing test which stresses more of the cool-down. One of the problems we saw at the spent fuel test was that 70 percent of the cool-down occurred within six months. So, it was felt that 70 percent of all of the mechanical interactions were going to be recovered within that six-month period of time. When we looked at the data, we had not yet got down to full recovery, and the tail of the data looked like we were asymptotically approaching in some cases a value other than where we started. We couldn't resolve that because we hadn't monitored it long enough. We don't want to get into those kinds of issues here, and so we have designed a test that's going concurrently with this accelerated test. It does not feed directly data for the license application, but it does give us the ability to look at -- is there anything that we didn't take into account the cooldown that we should have, and we can then ask that question, should we go ahead and submit the license based on this data, or do we submit it but recognize that the data may be suspect. We can at least deal with that before the license application is actually submitted. We will not, and we're not pretending that we will, have much information from the longer duration test. So, I think that part is accurate, but there will be a lot of direct information from those long duration tests.

[Slide.]

MR. WILDER: Finally, we do have the long duration tests. I have put in the back of your packet the criteria for determining why we need a four - seven year duration test. I think we've already discussed that before, and I know Bill Halsey is planning to at least touch on that tomorrow. So, I'm not going to try to get into the criteria, but it is in your packet. But the seven year duration test is designed to where we will have that test analyzed in time for the 2008 license application. So, if everything goes according to schedule, I think that we should have some of the answers.

[Slide.]

MR. WILDER: Again, getting back to the hypothesis testing, we should be at a level after we finish that four to seven year duration test, where we feel fairly confident in many of these -- in our addressing of many of these hypotheses, we won't feel totally confident, and I'm not going to quibble with the distinctions here. I probably would have done this a little different than Tom had, but I don't think we're really going to have total confidence until we do the performance confirmation testing.

So with that then, let me try to bring you up to date -- no, there was one point I did need to make. I apologize. I just found this. I know I pulled it out of order on you. I told you I wasn't going to do that.

[Slide.]

MR. WILDER: I'm supposed to be addressing the impacts in terms of high and low AML. One of the impacts on us in terms of testing has to do with what can you measure. What we're looking at here are the temperature profiles that we would predict based on our analytical models at this point for different AMLs, starting at 24 and going up to the

110, and this is 50 and 100 years with a 110 MTU. The point is, within the 50 to 100 year time frame during the performance confirmation monitoring, we should be able to see signatures which are monitorable and will tell us whether or not we are seeing an impact in terms of the vapor transport and the temperature flattening at the boiling point for the high AML. For the low AML because we never see this kind of signature, we're not going to have much so we can measure to distinguish between processes. So, one of the key jobs that face us in the next couple of years is to be sure that we design our test such that we are able to measure the processes during that performance confirmation period. With that, I will get into the large block test.

[Slide.]

MR. WILDER: I know that you have received a couple of presentations on the large block test where we talked about the objective and so forth. I'm not going to try to specifically get into all the objectives. I have already stated that we don't plan to use the large block test as a direct measurement parameters that would be used in the license application, except where we can convince ourselves that those parameters would be applicable.

What we do intent to use large block tests for is to measure those properties and so forth, those processes, so that we make sure that our physics is right in our models. So, what I'm going to do for the remaining few minutes that I've got is talk rather about what have we accomplished to date and how do we see that now fitting into answering some of these questions about the models. I've got a lot more photographs than I need to show.

[Slide.]

MR. WILDER: Let me just point out that the block has been excavated and the mapping has been performed. This is the top section. We mapped in sections because we had to keep the blocks supported.

[Slide.]

MR. WILDER: From that mapping, and this is a better example of what I'm talking about. We did support the block with a series of straps, and we only exposed about three to four feet at a time that we were mapping. From that mapping, we now have a pretty good understanding of how the fracture system, what it looks like. Claudia was asking me about the little model that I put out here, and I would suggest maybe you take a peek at it sometime during the break. The fractures don't totally line up, and part of that has to do with the way we made the model. They told me that it was going to cost $34,000 to make a plastic model, and I said you've got to be kidding me. So, what we did was we just took their maps and just Xeroxed them. Well, of course Xerox stretches, and it doesn't stretch uniformly, and I don't know if they always put the sheet in with the top at the top of the machine. So anyway, they don't totally line up, but I think it's very informative if you look at those fractures that have been highlighted.

Before the block was completely characterized in terms of fractures, we had done some permeability tests, and Tom Buscheck has done some analyses looking at basically uniform hydrologic properties and looked at temperature profiles and what the saturation profiles might look like. One of our other investigators who did the permeability testing said well gee, I saw this really permeable zone down about two-thirds of the way down, and so I'm going to put this in as a layer cake and see what that does. He's got another set of profiles.

If you look at the block, you'll see that -- at least I can convince myself that you can probably identify some fracture systems that need to be taken into account, and they are at an angle. So, one of the things that we're hoping to do with this test is now to run a series of model analyses with different assumptions and see how much difference it makes, and then when we do the test, we can see how critical that's going to be to us when we do the ESF test. Frankly, I don't think we'll ever have that good a three dimensional understanding of the fracture system underground. So, that's one thing that we're hoping to get out of this.

The other thing I would point out is that we do have quite a number of different types of mineral infieldings and fracture coatings, and you see some of them are fairly significant. Some of them are just almost like little pencil lines. They almost look like graphite, and Bill Glassley's trying to analyze the impact of these fractures. What we will be able to do after the test, now, is to take these blocks apart and specifically look at those fractures, compare them with the small block fracture characterization which the small blocks were taken just outside of the excavation and now we should have some handle on how does the geochemistry coupled to the hydrologic features and the fractures in the original geochemistry.

[Slide.]

MR. WILDER: I think most of the other photographs you've got in your packet are just some more of the same mapping. We'll report that we recognize the problem with leaving the block exposed during the winter months and for any long period of time. The block now is in a protected buttoned up condition in which we have installed insulation around the block. We've left the supports. You don't see them, but the support straps are inside this insulation. I should have also pointed out that we do have a hose running up to the top of the block which runs up the hill so that we can maintain the water -- I can't say maintain the moisture conditions because it's probably going to impact the distribution, but at least we won't be drying the block out.

[Slide.]

MR. WILDER: We eventually covered it with plastic, black plastic, so hopefully it is well protected, not only against freeze thaw cycles but also against drain out.

I'm sure that I'm just about out of time. Rather than try to get into the ESF test, I think what I ought to do is leave that for Bill Halsey tomorrow unless there's an interest in that.

DR. CORDING: Okay, thank you very much, Dale. You brought our schedule back close to being on time. Any other questions for Dale? Don Langmuir?

DR. LANGMUIR: Dale, I've always been interested in the connections between the different disciplines that we're using at the site, characterize it and predict its behavior. An obvious one here is whether the flow is dominantly in fractures or coupled fracture-matrix, and I have to assume that those very elegant plots you got from Tom Buscheck which show what seem to be very exact predictions of where the reflux will be. That sort of thing has to depend strongly on what the assumptions are about fracture flow. I guess I wonder what kind of uncertainties you've got and how critical those uncertainties are in the prediction before '98 of what reflux might do without getting into five years, seven years of tests.

MR. WILDER: I probably ought to let Tom respond since he's the one that's doing all the calculations, but a general response would be that Tom has done a number of sensitivity analyses, but of course those sensitivity analyses have imbedded in them their own assumptions. In some cases, Tom is using an equivalent continuum approach in which case he can look at what will happen on the average over a wide variety of permeabilities and so forth. He may have some specific tests or analyses looking at fractures. I know he's done some looking at the specific very highly permeable fracture.

But your point is well taken. Those depictions that I showed can be modified significantly. I mentioned the large block test. What we saw by merely taking into account the layer cake permeability distribution rather than a uniform distribution was that we expected a much smaller zone, condensate buildup where we would see the geochemistry taking place if you assume the more highly permeable zone below the heaters than if you take a uniform look at it. So, your input parameters and assumptions are going to be very critical. That's one of the things that I say that we want to do at this large block test, is to look at what assumptions and what boundary conditions have more impact than others so that we can then refine the models that we're using for ESF.

DR. BUSCHECK: As you know, we were the first to point out the limitations of the equivalent continuum model with respect to fast episodic non-equilibrium flow, and we continue to be concerned about that.

We have done bounding calculations with lateral heterogeneity, albeit with the equivalent continuum model, but we were able to get extreme amounts of focussing on the locations.

Recently, for simple idealizations, we have compared the equivalent continuum model for one-dimensional flow through a representative repository system and compared it to a fracture matrix model. I don't want to say I was surprised, but we got very good agreement for that idealization, and that is looking at one comparison.

What we were comparing there is a disequilibrium, and what I am concerned with is a disequilibrium between fracture and matrix flow with respect to drying and also with respect to the reflexing zone, but we found the same degree of dry-out. In fact, below the dry-out zone, the fracture matrix model actually saw some more dry-out than the equivalent continuum model because the condensate was artificially being held up.

I don't want to say this comparison alleviates concern, but insofar as the simple one-dimensional model was concerned, we were looking at those effects.

We have also done analytical modeling to look at what are the critical fracture fluxes in order to dominate the matrix, assuming different matrix sweating of diffusivities, and we need to also look at that analytically to see under what conditions the equivalent continuum model assumptions are definitely invalidated.

DR. LANGMUIR: One more related to that, how does a one-dimensional model handle the horizontal flow of groundwater?

DR. BUSCHECK: Oh, it doesn't. It does not. No.

What we wanted to do was we were just comparing the equivalent continuum model formulation. The fact is you assume that there is a negligible lag when you boil water in the matrix. It is just instantly in the fracture. That is conceptually what is happening. When the water condenses in the fracture, it is instantaneously embodied in the matrix.

So we were only testing in that comparison just that assumption of instantaneous equilibrium between fracture and matrix conditions. To compare that limitation, the equivalent continuum model, and also compare the reality of having bedded units that are sloping and other types of heterogeneity, in fact, if we were able to do that, we probably wouldn't have been able to sort out what was causing the discrepancies. So I think part of looking at the limitations of the idealizations is to break down the key components of what the simplifications are, and we have a long way to go on that.

DR. LANGMUIR: Let me carry that a little further. What do you think the uncertainties are in not being able so far to consider horizontal flow within the formations, within the bedded tuffs, as part of your model? It is not in the hydrologic model you have got. So it is not in the thermal model you are conceiving. How important might it be?

DR. BUSCHECK: Do you mean horizontal flow in the matrix?

DR. LANGMUIR: The possibility of horizontal movement of fluids and, therefore, the movement of heat as part of those fluids.

DR. BUSCHECK: I don't want to minimize the potential importance of that. I think that it needs to be addressed.

I think that no matter what we do when we look at a specific problem and take that lateral flow, I don't believe we will ever be able to do a detailed analysis fully three-dimensionally necessarily. So I think we have to break down the problem.

In the cross-sectional models, as they run at LBL, there are models that may get at some of that lateral variability.

We have to develop, undoubtedly, a very creative use of complimentary models, and we have been pointing that out in a number of papers that we have to rely on combining various models that compliment each other to synthesize our understanding.

We also need to do 3-D modeling, but to think that we are going to model everything in three dimensions and get disequilibrium flow accurately would be a fairly long stretch.

MR. WILDER: I think that there are a couple of other comments I would want to make.

The first is we have recognized that no matter how good our models are, they are, nevertheless, abstractions. When you get into some of these large-scale heterogeneities, it is going to force us to have large-scale tests or a number of small-scale tests, and that is one of the reasons I tried to indicate we think that performance confirmation is going to be so critical.

The other comment I would make, if during the break you look at this model, you see that there are a couple of very persistent horizontal fractures in the large block, and that may give us an ability to look at some of these issues in terms of what if you do get some horizontal flow, recognizing, however, that we have controlled our boundary as such that we are not going to get flow out of the block, out of the surface of the block. All we could look at is a very near feature, but, of course, that is all we can monitor, anyway, is a very small-scale feature.

DR. CORDING: Okay. Vic?

DR. PALCIAUSKAS: Vic Palciauskas.

You brought up some interesting points, and I wanted to ask you one specific question. If I combine 2 pieces of information that you provided, one was basically that the heater tests will be, of course, on an accelerated power cycle, about 2 orders of magnitude, 100 times.

It would seem that at a very slow heating rate, it is very likely that you have enough time to buy back into the matrix, and you will basically have a build-up of condensate.

At a very high heating rate, you can imagine that you would have condensated shedding, which implies basically that the heater experiments may give you a wrong conclusion with respect to the repository.

Is that reasonable?

MR. WILDER: That is certainly a possibility that we have to take into account.

One of the things that we are trying to do is to back up our field tests with some laboratory tests. They are, of course, of very short duration, but by going to a smaller scale, we are hoping we can start to look at the scale effects of imbibition.

Of course, we are measuring the imbibition rates as well, but your point is well taken that if we do a test which is highly accelerated, we may be forcing different physical processes to take place than if it is a very slowly developing test. That is one reason we have got so many different scales of tests we are trying to look at.

DR. CORDING: Being able to change rates if you have the luxury of changing rates and changing scales, then you can start at least to get a little bit of a feeling for it.

MR. WILDER: One of the things which Tom is pushing on and he feels pretty strongly about is that we ought to go very slowly in the heat-up rate to begin with on the large block test. I wouldn't say that I am opposed to that. I just haven't seen enough calculations to make me comfortable that if we do that, we are not going to jeopardize the end of the test.

So Tom and others are analyzing that, and we may very well want to slow down the heating rate of this test which would then give us a chance to look at some of those scale issues.

DR. CORDING: Don?

DR. LANGMUIR: One more, Dale.

We have heard quite a bit from Ardyth and from you on the hydrologic chemical test needs that you, hopefully, will learn something from in the block studies.

My understanding from reading the literature on this over the years has been basically that as you go through a thermo gradient and the water is heated up as it moves out of a system or moves towards a heat source, you are going to get precipitation as you come towards the heat source. Most aluminum silicates, they tend to have -- sorry. The other way around. As they get hot, they dissolve. As they cool, they precipitate. Except for carbonates, this is the way things go.

So, if you are looking at a cooling system, you are going to get precipitation of phases, and if you are going to be in a heating cycle, you will get dissolution.

I didn't get the sense from what you told us that that necessarily is what you were predicting in your test work and your modeling effort there. It is more complex from what you were describing.

MR. WILDER: For instance, in Wunan's lab test where we broke the fracture apart after it had been healed, we certainly saw a lot of evidence. There had been silicate dissolution, a significant amount of it.

How much of that was from pressure asperity contact, how much of it was from temperature, I don't know that we have got that sorted out, but there was a significant amount of redistribution of silicate.

There was also a significant amount of redeposition. That is why the fracture had healed. So I am not sure from our lab and field data that we have been able to sort out the relative contribution in terms of when the heat is driving you to dissolution and when the cool-down is increasing the precipitation.

Bill Glassley may be able to address that more specifically, but at least I am not aware of being able to sort that one out.

DR. LANGMUIR: Wouldn't there be a general thing you could say that as your repository heats up the rocks around it, temperatures are increasing? So you would not then expect reductions in permeability during that time frame.

It would be mostly on the cool-down process after you hit the maximum temperatures. You would have most of the clogging take place, and therefore, that is when you might expect to have pressure effects build up --

MR. WILDER: Yes.

DR. LANGMUIR: -- and possibly heat pipe effects build up on the cooling part of the process.

MR. WILDER: In general, I would say that that is true.

Of course, I have made the comment before that given that the plugging is probably going to occur late in the cycle, I don't think that you are going to see so much pressure build up because the moisture has already been removed from the zone right around the waste.

You may very well be creating a situation in which you, if anything, keep the pressures low as you start to decrease the temperatures, but I am not enough of a geochemist to be able to really speculate much beyond that point.

DR. CORDING: We have one more question, a brief question from Leon Reiter, or a brief answer.

DR. REITER: Leon Reiter.

This represents another attempt on my part to try and get a better understanding on what is meant by technical site suitability. This is triggered by your plot of maturation of understanding of environment.

We heard earlier that the DOE plans to go through technical site suitability and the license application, putting emphasis on the lower range with something just at the other end.

In protecting site suitability, what kind of statement can you make about the lower loading and the high loading, and the same thing with license application?

MR. WILDER: As I understand it, there are probably a couple of areas where we need to feed to technical site suitability, and Jean may have to help me out on this, but one is we certainly have to address the issue of substantially complete containment.

So anything that we can do to analyze what the environment is that the waste container is going to see, is there is an aggressive environment versus a benign, and to a large extent, whether or not there is water present or no water present. That will be one of the things that we are doing.

So, from the laboratory studies that we are doing on the rock properties at Fran Ridge, we would then deduce whether or not our model applications that are currently predicting dry out and benign water chemistry would be supported by those lab tests.

The second area is that I think we have to be able to make a statement that, in general, the natural system is compatible. It doesn't necessarily say that we can predict it, but I think that is the wording, isn't it, that it has got to be compatible with it?

So, once again, we would be looking at those processes that take place in a very small scale in the laboratory on Fran Ridge rock which, once again, is not necessarily representative, but at least should be somewhat typical of, so we would be able to look at the compatibility issues. I think that is about all we could expect to get out of these lab tests.

DR. CORDING: Thank you very much, Dale.

We are going to take a 15-minute break. The speakers are going to be available here at the roundtable and also joined by others, including the Board members and Staff. So we will take 15 minutes until 4:00.

[Recess.]

ROUNDTABLE DISCUSSION

DR. CORDING: Let's get back to the table, and we will start the roundtable discussion portion. We will have a small group discussion if the rest of you don't get here soon.

In this discussion session or this so-called roundtable, we are going to be discussing the topic today which is basically the thermal strategy and how that is to be carried out, particularly in the next few years prior to a site suitability decision and license application.

Around the table here, we have people who have been involved in the presentations today. We have some of the Board, particularly those that are on the hydrogeology and the structural geology area in just those areas, and then we have invited others who are in the program or others from outside the program to provide some of their perspectives as well.

First of all, I would like to start with Dan Bullen. Dan is a professor at Iowa State, and he organized the session yesterday and directed the session yesterday at the American Nuclear Society presentation, just up the street, and there were some interesting discussions that occurred at that time, and I will ask him in a few minutes to describe some of the conclusions and some of the discussion that was carried out at that meeting. I think that will start us on the discussions.

I think, perhaps, the next thing would be to ask particularly those who have not had the chance to give presentations today to make some short comments on their perspective on several things in regard to the thermal strategy and their opinions regarding the preference for an approach to low, medium, high loadings; how that would be implemented; what are the primary unknowns; how can these be resolved; and how does that fit into the site suitability and licensing strategies.

First, I will ask Dan Bullin to spend a few minutes. He gets the privilege of using the overheads if he so wishes.

Dan?

DR. BULLEN: Let me preface my remarks by saying that I would like to express my appreciation to the six members that served on the panel yesterday.

The title of the session was Thermal Loading Issues for a High-Level Waste Repository and Unsaturated Geologic Media, and this was the offshoot of some discussions that were undertaken about a year ago at a technical operating committee meeting for the Fuel Cycles and Waste Management Division of the American Nuclear Society.

During the course of organizing the session, what I did was talked to a number of people who have an opinion on these issues, and I should sort of preface my talk by saying that I needed to find someone who would be an advocate for each of the positions. So I sort of forced the individuals to take a stand.

The initial talk was given by Larry Ramspott, who did a very elegant job of providing an overview of the situation.

Basically, we looked at extended dry, a revised reference case, and the minimal disturbed or a minimum disturbance environment.

Then I asked Dave Stahl to give us an overview of the impact of thermal loading on engineered barrier performance and waste package performance, and he did a very nice job of that.

The third speaker was Steve Saterlie, and he basically provided some of the same information that you saw this morning with regard to the systems engineering and analysis approach.

Then our fourth speaker was Tom Buscheck, and you saw some of Tom's results provided by Dale Wilder. He spoke about the thermal analysis and some of the hypothesis testing required, and we got to see the Oscar Meyer slide. So it was actually very, very worthwhile, and Tom provided some very significant input to the panel discussion, also.

Then I had Dr. Scott Sinnock. Scott very aptly, while he wasn't necessarily forced -- but he took the middle-of-the-road approach for us, and in fact, he sort of paraphrased his talk.

I am probably going to apologize for butchering it here, but he said why should we vary from the middle-of-the-road approach. So Scott gave us a very good overview of kind of the revised reference case.

Finally, maybe sort of through duress, but early on when I was talking to Larry Ramspott about organizing the session, I needed somebody to advocate cold. I had to have somebody to advocate cold, and so we got Ed Taylor to step forward to do so, and he did a very, very good job about taking the approach for the cold repository. I want to summarize that when we get through.

[Slide.]

DR. BULLEN: First, I would like to take a minute to just talk about the concepts.

You might ask where I got this viewgraph. This is Larry Ramspott's viewgraph, and being a college professor and always looking for a free lecture, you ask your friends at meetings if you can have a copy of their slides, and Larry very benevolently decided to donate one, and I have a complete set of his slides to take back with me and share with my students.

I would like to summarize here the concepts that we discussed yesterday, the first being the extended dry which was, essentially, to use the waste heat, to make the repository dry instead of humid. It cools before it rewets, and Tom Buscheck showed some very nice figures that identified that, and we were trying to, essentially, separate the heat from the water, and in doing so, maybe we could change the performance of the repository, preserve the integrity of the containers, and influence how the repository performs long term.

The second one was the revised reference case, and essentially, Larry did a very good job of summarizing by saying without setting limits on the waste stream, this uses a design to balance the space minimization and thermal limits, and it gives you the flexibility.

The flexibility, essentially, ranges from the SEP design which boils for about a thousand years, all the way down to a sub-boiling repository horizon.

Finally, we had some very, very interesting discussion and debate about what a minimally disturbed repository site might be. Does that mean minimally disturbed that we never boil the package? Minimally disturbed because we boil the package but the whole repository horizon doesn't boil? Or do we bring the whole repository up to almost boiling?

So those are the kinds of conditions we discussed, and the definition here is assuming an ambient temperature repository to be safe, heat is kept low enough to not significantly ambient hydrology. So we have a cool humid region, and we avoid the hot humid environment which may be deleterious to contain a performance.

I have kind of briefly summarized what everybody's position was, whether it was their actual position or forced, and I want to take the last three viewgraphs that Larry Ramspott had to kind of initiate the discussion here.

[Slide.]

DR. BULLEN: It was very helpful in initiating the discussion yesterday during the panel discussion yesterday, and we talked about a couple of items, the first one being the unresolved issues for selection of a thermal loading regime.

The first question that Larry posed was: Are the hydrologic heterogeneities more deleterious for any of the concepts? Is there something in the heterogeneities of the hydrology that make hot dry worse or cold wet worse? Do you have to have less test data for any particular concept, or can you have sort of a more rapid, faster, simpler tests be made for any concept or thermal loading regime? Does any concept mobilize less water from the water table to above the repository than others? Do we have less imbibition from the saturated zone? Is there a discrete thermal threshold above which there are significantly more deleterious effects?

Maybe we can go to 80 C, or do we have to go to 96 C, or do we go all the way up to above boiling to have significant effects?

So these are the kinds of unresolved issues that we discussed.

[Slide.]

DR. BULLEN: Larry then stepped a little further forward and talked about the risk of selection now, basically the unresolved issues, plus the projected effects based on computer modeling, and how we need field tests to confirm it. So that is a very good point.

Present models don't necessarily contain discrete fractures and small-scale local heterogeneities that might significantly affect the results.

The mountain scale effects could also be very different than the local scale effects observed even in the field tests, even in the large block tests that we just heard about, and the system issues affecting minimal disturbance concepts, we tried to grapple with what a minimally disturbed site was. Is it practical to have a 20-to-40-kilowatt-per-acre loading as minimal disturbance? Do you need to go smaller?

I mean, we set the lowest extreme, and the lowest extreme might be one assembly per one container and that way we would have 170,000 containers. Well, that is not a real reasonable approach, but that is the minimum disturbance you could possibly make, all the way up to a large MPC, whether it be 21 or 26 MPC design.

If not, what thermal loading gives the minimal disturbance? We still have to define what minimal disturbance might be, and there was some very interesting debate about that.

[Slide.]

DR. BULLEN: Finally, are the claimed benefits for the minimal disturbance concept compatible with the current design or the currently planned MPC loading?

As a materials person, I disagreed with some of the people, and maybe we could have some discussions about that.

The MPC concept poses some benefits, but it may also pose some constraints on the thermal loading issue.

Finally, and before I summarize, we kind of talked a little bit about the flexibility versus selection, and this issue basically asks the question does present understanding of the effect of heat on the repository allow selection of a thermal strategy. If the answer to that is no, do we really have to postpone selecting a thermal strategy, or can we evolve some sort of strategy?

Basically, we select to evolve toward an answer as we learn more, and Larry, very eloquently, identified the select evolve options as starting with the revised reference concept, which we could have evolved either toward an extended dry or a minimal disturbance site, as we learn about, or we could initially license for minimal disturbance and then relicense for higher loading as we learn more.

I have a personal comment to make about that, and in fact, I will probably take this opportunity to do that. I would like to pose it in the form of a question to the people here both at DOE and the panel.

The question is obvious, and I have heard it outside. If we do this sort of evolution in licensing, will the NRC perceive it as being a 20 percent power license from my PWR as I bring it on line? Or, the analogy that I like to draw that may be a little bit more concerning is I am the director of the research reactor at Iowa State, and we have a little 10-kilowatt reactor that has aluminum clad fuel, and we dump the core tank at the end of each run because we don't have that much residual decay heat. So we are in an almost dry environment, sometimes moist.

Is that the same as scaling it up by a factor of 150,000 and going down the road to the Arnold Energy Center to a 550-megawatt BWR which operates with a different material in a different environment where it is hot and boiling? Is that more akin to what the NRC might perceive when we try to scale this up?

The converse to that is also true. If you take a look at the form that I have for my fuel, it is aluminum clad, and it is optimized for the cool, wet conditions that it is in. There is no way I could go over and get an assembly from the Duane Arnold plant and bring the BWR simply back and stick it in my reactor and dump the core tank at the end because it is too hot.

So these are the kinds of questions that you might want to debate when you take a look at this perspective.

I think that -- my own personal opinion now -- I think that the evolved license is probably a very good approach, and that the minimal disturbed moving to something higher is probably a very good approach. How difficult that is going to be is anybody's question. The answer to that is anybody's perspective.

With that, what I am going to do is ask for reinforcements from all of the people who were at the session and maybe take a few questions or ask people to respond to the questions that I posed.

DR. CORDING: Thanks very much.

Let's, perhaps, go to some of the people who were at that session and get some of their comments and see what thoughts they have, in particular, with regard to some of the items here that Dan is summarizing, and certainly in regard to what we think are critical issues that need to be considered in this licensing and site selection phase, and perhaps some of your preferences in terms of how this might be approached.

Let's have some brief comments, perhaps, and then come back to a more general discussion with the whole group.

Scott, would you care to start?

DR. SINNOCK: Thank you, Dr. Cording.

I have the opportunity of taking the advocacy position for the middle thermal option of, essentially, the 57 kilowatt per acre, plus or minus a little bit.

Just to summarize, I think the main reason I saw to stick with the middle or SCP option is we have done performance analyses, and I don't want to tread on Bob Andrews' talk tomorrow, but from what we have done in the performance assessment so far, we have seen very little difference in the performance among any of the thermal options.

Tom Buscheck yesterday raised the issue there were certain things that were not in the performance assessments that did that. Well, of course, there are abstractions involved in the performance assessment, but also, I don't think that we are so far off that there are major differences in the performance that we somehow overlooked or we probably would have acknowledged them.

So it looks like there is not a great deal of difference in the overall system performance for the system over 10,000 years or over a million years for the various thermal options.

We do have sort of the reference of the SCP. Secondly, the cost seems to be somewhat similar from what I can gather, too, among the options, the total system life cycle cost. So on a standard systems project, the baseline is what you go with unless there is some obvious reason to change it in some defensible reason.

So far, I don't think there is a consensus on which would be an improvement over the current reference.

So, until such an obvious argument is put forward, I guess in the advocacy position, I would advocate staying with a reference.

This is a quick summary.

DR. CORDING: Thank you.

Larry?

Larry Ramspott is with the M&O, also.

DR. RAMSPOTT: I wasn't advocating anything yesterday, and the summary of my talk has already been given.

[Laughter.]

DR. CORDING: Are you advocating anything today, Larry?

DR. RAMSPOTT: I guess I would only have one comment. I never got to make it yesterday. I have come from one meeting to another and I get to make it. I don't see the large difference between the two end member concepts, the minimal disturbance and the extended dry, but yet, a lot of people do.

To me, you have to do some of the same things in order to optimize those concepts, including possibly fuel aging or some form of fuel management, but the kind of colder you get the fuel before you get it in, in the one case, you pack it close together to get an extended dry, and in the other case, you spread it out.

I got the sense this morning in the talk here that there was a tremendous difference. There was an awful gap in that somehow we would not be able to make this jump from the minimal disturbance concept to the extended dry if we did that.

I really don't see that. I think that, basically, the conditions of the fuel that facilitate one, facilitate the other, and basically, we could start off with the fuel spread out and pack it closer together and go from one to the other.

So I think that is the only comment I would have right now.

DR. CORDING: Thank you.

Ed Taylor, you have some perspectives on this.

DR. TAYLOR: I talked after Scott, and he left me kind of speechless with the speech he just gave, which said there isn't any difference between any one of these things, they all work, and there is no point in talking unless you got a good reason to find what you are talking about better than all the others, and I thought about that. It is no accident that I agree with Mike and Steve and Jean because I was on the team that put this thing together.

There is a reason for preferring -- which is -- may be true -- if any, it is the one that can be proved or demonstrated the quickest.

Now, my talk was, essentially, a literary review or a review of the scientific literature, and the literature suggested the ambient repository is okay; that is, if waste didn't generate any heat, we would have no trouble. You would just stick it in there and that would be the end of it.

If you read a little further and look between the lines and do a few calculations, the first effect that gets you as you start adding heat is what Jean brought up or Mike brought up or both of them brought up, refluxing.

The refluxing, however, is very interesting. It comes in very slowly as a function of temperature increase, and until you get the walls up to 50 degrees, it doesn't grow very fast. So it looks like we are pretty close to ambient when we keep the walls at a low temperature. So that thing ought to work.

What is more and the thing that aggravates the whole thing is heterogeneity, but even that when you look into the thing there isn't going to be a problem with the refluxing remaining.

So I would argue this is the one that we can get to fastest, if true. Our job is to define what that temperature is at which trouble starts happening and define our repository for the technical suitability determination, to be something with temperatures of the wall below that, and that was it.

DR. CORDING: Let's take a couple of questions.

DR. BULLEN: This is Dan Bullin.

In all fairness, I have got to acknowledge David Stahl because David did take a hot advocacy role and took it from a perspective that is very near and dear to my heart. That is from the materials performance perspective.

I would have to, maybe, defer to David to make a few comments here just with regard to why we think hot dry is going to be better.

Dave did a very elegant job of coming up with some correlations of temperature and degradation rates, and in fact, if we can get to hot and stay hot and not rewet, which is one of the conditions that Tom may want to address a little bit later -- Tom did a very nice job of identifying that yesterday -- that may provide superior performance to being cold.

In fact, being cold and wet is a bad scenario for almost any material that we look at, and so maybe I could ask Dave to stand up and just summarize in about a minute or two.

DR. CORDING: There is a mike right there, Dave, or you can come up to the table, whichever.

DR. STAHL: I have a conclusion slide here, if I may.

DR. CORDING: Certainly.

[Slide.]

DR. STAHL: My emphasis was on looking at the subsystem requirements, particularly substantially complete containment and meeting control release.

As indicated here, we did a set of simple kinetic equations to determine the depth of penetration, and I started firstly with the corrosion-allowance barrier because I had some data there. I was able to extrapolate that to many thousands of years using existing short-term data and elevated temperature data.

The interesting thing is if you have an oxide film which is protective, as you have a time exponent of T to the one-half, parabolic-type relationship, any thermal load will work. So that, you get depth of penetrations which are very much less than the proposed thickness of the corrosion-allowance barrier which is 100 millimeters of carbon steel.

However, as I indicate here, if the time exponent is higher, if you have microbiological corrosion, pitting, or, perhaps, pH due either to microbes or radiolytic problems, then the depth of penetration will exceed the container thickness, certainly for the low and the intermediate thermal loads.

However, that is not the case for the high thermal load, where there is only the outer edge within an equally loaded repository, and certainly, that would disappear for a repository with a higher edge thermal load.

Now, what this indicates is that if, indeed, we have a non-ideal world where we have some other events and mechanisms operational, then we have to evaluate the use of a third barrier which we talked about early on, and we are just getting started with our corrosion test program at Lawrence Livermore Lab. We would expect to have the results in time for license application, but to have something in time for the 1998 site suitability would be in doubt at this time given the current budget constraints.

I think that is all I wanted to say. I would certainly be happy to answer any questions.

DR. CORDING: Yes. Don?

DR. LANGMUIR: Dave, don't take down your overhead.

DR. STAHL: Oh, okay. Sorry.

DR. LANGMUIR: Maybe I could clarify some of the points on there.

DR. STAHL: Sure.

DR. LANGMUIR: You are talking about corrosion either in steam or liquid water?

DR. STAHL: Yes.

DR. LANGMUIR: It wouldn't matter which?

DR. STAHL: Right.

DR. LANGMUIR: You are saying an oxide film will limit the corrosion substantially?

DR. STAHL: Yes.

DR. LANGMUIR: What is the likelihood of having an oxide film like that on the materials we are considering.

DR. STAHL: Well, for carbon steel, you do have oxide films built up in aqueous environments or atmospheric environments where you have water film on the surface. It is pretty well demonstrated in the field, in a laboratory testing, where you do get this kind of exponential or parabolic, I should say, behavior.

DR. LANGMUIR: What about the pitting, the possibility of pitting?

DR. STAHL: Pitting is very likely in some of these where you would only have what we call a pitting factor, and for carbon steel, pitting factors are generally in the range of three to four. That is not a major problem.

However, as I indicated, microbiological corrosion or radiolytic problems could give you factors of 10 to 100 in which case that system would not work and you would have to look at other materials.

DR. LANGMUIR: Will you be addressing those concerns in your tests?

DR. STAHL: Yes, we will.

DR. LANGMUIR: On what kind of a time scale?

DR. STAHL: Well, we have what we call our five-year omnibus test, and Dan McCrite can give you a lot more information about that. We are looking at a broad range of materials, environments, conditions, replications.

Basically, we had four major environments, J-13 water, a concentrated J-13, a concentrated J-13 with a very low pH in the range of 2, and a concentrated J-13 in the high pH in the range of 12 to 13, and those kind of mock-up end points that we would find in various aggressive environments, given either microbial attack in the case of the low pH or the impact of manmade material such as concretes in the case of the high photograph, and we will also be doing some potential dynamic polarization studies to interpolate between the end points and the normal J-13 environment.

So we will have a feel for the conditions that would lead to degradation over a broad range of conditions.

DR. CORDING: You are assuming on that, that after the high thermal loads are dissipating that there is a cool dry period? Is that part of the assumption as well?

DR. STAHL: The cool dry period depends on the thermal load.

If you have a high thermal load, then you do have a dry period when the temperature drops. So you don't have a lot of aqueous corrosion going on.

However, when you have an intermediate or a low thermal load, you have cool and damp conditions, and that is the perfect time for getting microbial corrosion.

DR. CORDING: Good. Any other questions with regard to this or comments in regard to this, this item right here?

Tod Rasmussen.

DR. RASMUSSEN: I just had a question about the oxygen levels. Is this atmospheric oxygen? If you had high thermal loading, you could drive off all the oxygen in the system.

DR. STAHL: Well, if you drive the oxygen that helps you, certainly, we have assumed for this that it is atmospheric, basically, anoxic.

DR. RASMUSSEN: Anoxic conditions would be more favorable?

DR. STAHL: Absolutely.

DR. CORDING: There is no way you are going to maintain anoxic conditions in Yucca Mountain for any length of time. The system breathes too well.

DR. STAHL: That is the assumption we made.

DR. CORDING: I think one of the questions that has been coming up here is in regard to the rewetting and the potential for local spots where we aren't maintaining the dry conditions, and I know there are several different views on that at this time and some concern that we still don't know all the answers to that, and perhaps we could have a little bit of comment in regard to that.

Tom has made some comment before. Maybe you could comment again.

Karsten Preuss, do you have some comments in that area that you think would be helpful here?

DR. PREUSS: I think that what we know about thermal effects is largely based on computer modeling, and it is largely based on volume averages.

I am not disputing the results from that type of modeling, but what I think we have to be humble about is that we don't know the relevance of these results for the behavior of the mountain.

I would suggest that high thermal load does not necessarily imply dry. There could be numerous, possibly episodic context of water with the waste packages at temperatures way about 100 degrees celsius.

As a matter of fact, with high thermal load, you mobilize an awful lot of condensate of the order of 1,000 cubic meters of water per waste package, which you don't have to imagine things too much to anticipate that some of those condensate could perch, and then as thermal stress is evolved -- every once in a while, one of these bath tubs, or however you want to picture this perch water, could discharge.

Furthermore, I really shouldn't say water. We could be talking highly concentrated brines. In an extended period of heat piping where water is always vaporizing as it approaches the heat source and the vapor is driven away, even if you start with just ppm salinity initially, it will not take very long and you will reach complete saturation with dissolved solids for whatever you have there.

So that could, I think, have possibly very severe impacts on corrosion, not just the moisture, but the fact that it comes with high salinity.

I think we have to expect that a lot of focussed, fast flow of water -- in my role, I would expect it is more the rule rather than the exception at Yucca Mountain, and I am basing that on the Ranier Mesa experience, which admittedly has higher permeability and is a more humid system, but has a lot of analogous properties, but this is something that we will know a lot more about as we get underground and construct the ESF.

I can see that there is some romance in this minimal disturbance scenario because you could hope that walking through the ESF and then with other means that you could pick up where those seeps and weeps are, and then you want to avoid those and put your waste packages away from them.

Now, with high thermal loads, you might dry some of these up, and that certainly would be beneficial. Some of them, you may dry up in part which actually could make things worse because the effect of flowing velocity would diminish and the the speed at which you transfer water particles would increase, and that certainly isn't desirable.

Plus, you could generate a whole lot of new fast paths because of the response of the fracture system to thermal stress as well as because of you putting a lot of additional condensate into the mountain.

My suggestion, generally, is that we should be very humble about not reading too much into what we know at this point, which is largely theoretically based and modeling based, and that we have to, I think, focus very much on developing experiments from lab scale to field and deciding then on the basis of the experimental evidence.

DR. CORDING: There are some interesting points here.

DR. STAHL: I just want to respond briefly to that, Karsten.

When I was at Batelle -- and previously, also, Dan McCrite did some experiments on dripping of various concentrated J-13-type solutions onto stainless steels --

and we saw no real problems in regard to enhanced corrosion at the surface.

I also want to point out that as part of our experimental program, we will be doing some additional drip-type tests to determine whether we are getting any boiling point elevation problems in the surface films that might build up on the surface of a container that would see dripping water.

So, hopefully, we will cover that issue. That was not covered in the studies that I did here.

Certainly, you could get boiling point elevations to as much as 130 degrees Centigrade.

DR. LANGMUIR: Could I carry that question a little further?

DR. CORDING: Please.

DR. LANGMUIR: I am concerned, Dave, that you could have certain saturated brines. Is that part of your concern and part of your experimental test effort?

DR. STAHL: Yes. Yes, that will be covered.

DR. CORDING: Karsten, in terms of the humble approach, if one is trying to come up with an approach for licensing and site suitability, do you see a minimal disturbance as the preferred way to go at this point?

DR. PREUSS: I would advocate to really focus on experiments, to learn quickly as much as we can, and not all experiments have to be large scale and take a long time.

Some basic issues can be addressed on the scale of a few feet in the laboratory, and I think that should be done with priority.

I am reluctant to advocate any particular thermal loading. I am more on the science basis side, the way I see myself, as opposed to the engineering side, and I see my role more in giving the engineers a hard time with reminding them of things that, perhaps, aren't as well understood yet rather than --

DR. CORDING: Say what the program should be. All right. I appreciate that.

Tom Buscheck?

DR. BUSCHECK: May I use the projector or not?

[Laughter.]

DR. CORDING: How big is your notebook, Tom?

[Laughter.]

DR. CORDING: Has Tom had a chance this meeting to put anything up? If not, then you certainly should get that chance.

DR. BUSCHECK: I will say one thing. It is tougher to listen to myself than to give it.

[Laughter.]

DR. CORDING: Please, everyone, when you do comment, make sure you are speaking about the way I am, this is about what you have to do to be heard with the mike.

DR. BUSCHECK: I would like to endorse what Karsten says about waiting for experiments.

DR. CORDING: After each slide, Tom, if we could have a discussion or comment.

[Slide.]

DR. BUSCHECK: Just going back philosophically, what we have found attractive about the extended dry is nominally what it can do to performance, to the waste package performance Dave Stahl has been analyzing, but what I like about it is that it is testable.

Dale showed an example for buoyant convection, whether it be in heater tests or be at the scale of the repository, the problems we manifested when you drive things above the boiling point, and the same thing that applies to buoyant convection applies also to focussed vapor condensate flow.

Also, there are a number of issues, I think, which are readily diagnostic in terms of the performance of a high AML.

I have been wanting to try to be positive and optimistic about conditions at Yucca Mountain. So I will talk in the positive.

What we can state, in general, is that the conditions that would benefit a minimally disturbed repository, and people wonder if I am saying "mentally disturbed," but I am not.

[Laughter.]

DR. BUSCHECK: Actually, I have a comment about that. I will tell you what MD really stands for.

I have been doing a lot of analysis, and I believe also others have. When you look at the drift scale, it is mighty difficult not to dry out the rock around the waste package with an MPC.

In fact, you can get upwards to 10,000 years of sub-ambient saturation even at 24 MTUs . I prefer to call this the mini-dry repository. I think it is going to be really difficult. We will have a hard time demonstrating that we can't dry out, maybe, a substantial amount of rock, even if we don't try.

Anyway, what conditions benefit these two concepts? Well, hopefully, the infiltration flux is sufficiently low. I think we are hard pressed to say whether a high flux negates the benefits of a high thermal load any more than it makes a low thermal load difficult to demonstrate.

I think the absence of substantial mountain-scale buoyant gas convection is important to demonstrate for both concepts.

I think the absence of substantial focussed condensate drainage and infiltration drainage is important to demonstrate, and we have been doing a lot of bounding calculations, and we have had to go to some really absurd, extreme scenarios to focus condensate where we got the ground surface temperature up to 83 degrees C. We literally developed a magmatic situation.

We have worked very hard at trying to develop conditions that defeat an above-boiling system and also apply that all the way across the board, and you can't focus vapor flow even if you are below the boiling point and cause condensate to be focussed.

However, there is at least one effect, but I think there is actually two effects that may be deleterious to a minimally disturbed repository while either benefitting or having no impact on an extended dry repository, one in which Karsten Preuss is probably most familiar with because they have been doing the most work on the possibility of enhanced vapor diffusion.

The NRC in talking to me last week said that there is almost a factor of 1,000 in terms of the effective water, vapor, air, gas phased diffusion coefficient currently being applied, and I think Karsten would agree that is a big unknown right now.

If we have substantially enhanced vapor diffusion, if, indeed, we want to have a minimally disturbed repository, you have the potential for developing reflux even though you are well below the boiling point, and I think that is something that we really need to address in heater tests.

I think the large block test is a very good opportunity to do that.

DR. TAYLOR: Isn't the vapor diffusion coefficient working right now in Yucca Mountain?

DR. BUSCHECK: It is. Karsten has pointed that out. The fact is that we have ongoing vapor diffusing down to thermal gradient with a geothermal flux from the water table to the ground surface.

DR. TAYLOR: What do you think when you look at the saturation profiles?

DR. BUSCHECK: Actually, if you are wetter than gravity capillary equilibrium, there is one explanation, and Karsten's -- I believe you have shown that in your report that if you have enhanced diffusion, you actually have to be draining water through the matrix and you elevate the saturation because of the low hydraulic conductivity in the matrix.

So the fact is that the current saturation distribution has to be understood in light of thermal effects.

I think to understand the initial saturation distribution and to adequately or appropriately understand what the effect of infiltration flux is, you have to consider the impact of heat under ambient conditions because heat is driving water vapor out of the mountain.

My feeling is that thermal issues apply whether you even put heat-producing waste in the mountain.

DR. PREUSS: Could I make a brief point?

DR. BUSCHECK: Sure. Go ahead.

DR. CORDING: Yes, Karsten.

DR. PREUSS: We are always concerned about the length of time that it takes to hit a significant volume of rock and examine some of these effects.

As to the possibility of buoyant gas-phase convection, there is a test that can be done that doesn't involve thermal effects. Rather than take the time to heat up a significant volume and then get the buoyancy from the warmer, less dense gas, you could simply place a plume of a different kind of gas that is less dense, like helium, say.

I haven't thought about it too much, but some gas like that, but the source of buoyancy wouldn't be thermal, but would be the intrinsic lower density of the gas.

You could simply inject that kind of a plume into a fracture system, and you don't have to wait years until your thermal front does this for you. You could inject it in a very short period of time and then watch it rise and get an assessment of effective vertical gas permeability that way.

DR. FARRELL: J.J. Farrell.

That is all well and good, but what happens when you put basically 200 to 1,000 psi confining stresses on that fracture network due to the heat you put in? How do you know that the temperature that you put into the mountain doesn't change your fracture pattern?

DR. BUSCHECK: There are a couple of issues that may impact whether or not these phenomena are important. I think that is a novel scheme. In fact, if we have a parallel of ways of assessing phenomena, I think it adds to the robustness of our understanding about the system.

[Slide.]

DR. BUSCHECK: One of my primary ways of looking at the heater test, in addition to understand coupled processes, I want to emphasize that these also apply to all thermal loads.

At the first level, these hypothesis could be used to understand how significant -- whether it is even impossible to avoid significant heat mobilization under whatever thermal scenario we are looking at, and if we are looking at MPCs, you have to consider real heating conditions. We have to actually heat an underground experiment with something that is comparable, perhaps, to an

MPC to look at the local effects.

Russ, were you going to say something?

[No response.]

DR. BUSCHECK: No.

Anyway, then these --

[Laughter.]

DR. BUSCHECK: You were jumping up.

This is the third one, isn't it?

[Laughter.]

DR. CORDING: Russ promised that anything you didn't cover now that he would be glad to sit with you and go through it.

[Laughter.]

DR. CORDING: And I am interested, too, but let's go ahead.

DR. BUSCHECK: The point I want to make is we put an L here, but, really, these are fundamental hypotheses. These hypotheses also apply for AMLs that do significantly mobilize fluid flow, and they address how that fluid flow mobilization takes place, and we think that is quite important.

A comment about the moisture build-up to the condensation, I was looking at Claudia and she got sick and tired of my 84 Darcy case. That showed pretty pronounced convection.

The fracture for 85 darcy -- the cubic may be wrong, but it is 10^{-3}. At 10^{-3} with the waste package spacing for a high ML, that would be 12 cubic meters of fracture in which to store the water. So Dale was asking the question where does the water go. There is not a lot of place -- if the water is in the matrix, I have a hard time visualizing how it is going to come pouring out unless the matrix properties have changed substantially. I don't want to rule that out, but there is not a lot -- it is not like we have a large tank up there with a lot of volume to it.

84 darcy could be quite high. We may have a lot less than 12 cubic meters per waste package of fracture up there.

Thank you.

DR. CORDING: Thanks very much, Tom, and you bring up some good points for discussion as well.

Some of this, I think, we will get into the discussion tomorrow, but how are we going to be able to resolve some of these issues or to be able to understand the -- I guess a better way of saying it -- how are we going to be able to understand this phenomena in the testing program?

Kartsen brought up one point. Are we going to be able to see these features with the in situ thermal testing that is being planned?

Dale, you have been commenting on some of that, but are we going to be able to really understand some of these effects in the testing program?

MR. WILDER: I think that the point has been well brought up that we cannot in any single scale of tests be able to address all the issues. I think that is no question about that.

Secondly, I think that any heater test that we do is, by definition, going to be some sort of an accelerated and, therefore, not representative of all the features.

I do think, however, that some of the things that we can address will get to issues like Kartsen has raised, whether or not we have the ability to build up condensate, whether if we do build up condensate, is it going to be in the fractures or is it going to be tied up in the matrix, as Tom mentioned.

Those sorts of things, I think we can address in heater tests, and so I guess the best we can do is try to address the issues and build our confidence with a number of different type tests aimed at the fundamental concerns.

DR. CORDING: I think a general question for any of us or all of us to consider is, is it necessary to do the thermal testing that is being proposed prior to licensing.

First of all, the proposal for the testing that is being laid out is, is that necessary that we really do some of these tests and start to see these phenomena in actual tests prior to licensing.

I guess the second question would be is it enough. Do we have to do it? Is what Dale has been outlining enough?

We may see, again, more of that tomorrow in more detail on what those are, but any comments on that?

DR. TAYLOR: As the advocate of the minimum disturbance, I learned something. That helium is a good idea.

The remark about closing fractures with high heat doesn't pertain to the minimum disturbance.

DR. BUSCHECK: It might.

DR. TAYLOR: Yes, it might. Anything might.

DR. BUSCHECK: I am answering for J.J.

J.J. Farrell, we have had many discussions about this, and in light of what they found at Climax, it appeared that most of the closing of fractures occurred in the first 33-degree rise in temperature.

DR. TAYLOR: We are not going to raise the temperature. We are just going to stick helium down there.

DR. BUSCHECK: No. I am talking about the minimally disturbed repository is going to raise temperatures.

J.J.'s observation was similar to what we saw at Climax. Most of the closing of fractures occur the first

30-degree rise in temperature, and after that, they didn't see more subsequent change.

I think it is not appropriate to assume that if we just have 30-, 40-, 50-degree rise in temperature that you won't be closing fractures.

DR. TAYLOR: That is good because it cuts down.

DR. BUSCHECK: It does for a time.

DR. TAYLOR: If we test and establish with no temperature rise, that buoyant flow is either significant or not significant, we are not too concerned.

DR. BUSCHECK: No, I think that my feeling is that both approaches would be useful.

You haven't seen it in the official presentations. I have been a very strong advocate of running parallel sub-boiling tests and then starting the large block under sub-welling conditions because I think understanding the phenomology both below and above boiling adds to our robustness for any concept.

DR. TAYLOR: What we are addressing here, is it reasonable to think that we can complete enough tests to do licensing in 2001 or to do a suitability determination in 1998.

DR. CORDING: Dale?

MR. WILDER: I wonder if I could just make a couple of comments.

One is there seems to be an implied feeling that if you go to an extended dry or -- I hate to say higher temperature because it depends on how heat is loaded, but at least if you go to an extended dry approach, the testing involved is going to be of longer duration.

I think there is processes that take place at the minimally disturbed zone which are somewhat like corrosion processes which develop so slowly that if you test them at all, you are probably going to have tests of equal length or maybe even longer than the higher thermal loading.

The second point I was going to make -- and I think that Dave Stahl made it quite well -- is that I heard the comment about all of the cases are essentially the same in terms of their total system performance and cost and, therefore, why not go with minimally disturbed.

I guess my question would be if they really are basically the same in performance and in cost, why not, then, go to the higher loading which seems to favor the MPC for the use of a large MPC and also the waste package performance in terms of the materials.

DR. CORDING: Steve Saterlie of the M&O.

DR. SATERLIE: Let me comment on that and also give a little synopsis of the understanding I had of yesterday's meeting.

First of all, the synopsis, I think we had a very heated, lively discussion, shall I say, of some of the pros and cons and how real were those pros and cons and how demonstratible were they.

I think we all came to the conclusion that at least some subset of testing was needed either for hot or low to be able to go forward with a license application.

The issues that Karsten and Dale have brought up are very good issues. They are issues that we continue to struggle with, but the thing that is difficult -- and that is why the program is moving the way it is, I believe, with a low thermal loading, I believe, at the start -- at least that is the intent of this point in time -- is that we don't believe that we can necessarily demonstrate to reasonable assurance, the perturbations on a large mountain scale that would very likely be needed for the high thermal loads.

You saw a lot of the charts that Dale showed, and you saw those perturbations out all the way down to the water table and further.

There certainly may be some perturbations with low thermal load, but they are significantly lower temperatures, and we have to understand the effects of those, yes.

Anyway, that is the difficulty, and that is why, I think, the program has chosen the path that it has at this point in time, but we need some test data.

DR. DOMENICO: Ed, can I say something on that?

DR. CORDING: Yes, please, Pat.

DR. DOMENICO: Pat Domenico.

I guess we are going to the minimal disturbances which is advised here because it is probably easier to license, but Kartsen made a good point and another point was made over here that suppose in Tom's calculations it indicates that you mobilize one-and-a-half times as much water as you have space for because you already are at a high saturation. Would that bother anybody.

Or, if you design for an extended dry and it is not a spaceship -- if you design for an extended dry and you don't get it, could anybody see any negatives effects to that? I mean, some very serious effects. If you design for it, don't get it because of heterogeneities.

I still haven't heard too many people who are involved in geothermal resources who have come forward and tell us what the overall geologic effects would be of boiling the mountain for 10,000 years. I mean, I would like to hear what it does to the geology, and I think we have spent a lot of money on this, so-called natural analogs, and this is a perfect natural analog, and someone should say something about what their feeling is on the overall transformations that take place over long periods of heating which you are never going to capture in a model and which you are never going to capture in a laboratory.

DR. LANGMUIR: Is Dave Bish here? I think he could answer that. Dave is the perfect person to answer that question, Pat, I think.

DR. DOMENICO: That is only part of a question.

DR. LANGMUIR: The part that has to do with 1,000 years of boiling would be something that I think -- Dave is supposed to be here tomorrow, I believe, at which time we may get an answer, if he is not here now.

DR. RAMSPOTT: I don't think you boil the mountain for 1,000 or 10,000 years. What you do is get the rock above the boiling temperature, but if you have the absence of water, it doesn't matter what the temperature is.

So I think in geothermal fields, you have a very large amount of water and a continuous heat source. In this we believe we can remove the water.

DR. DOMENICO: I said if you design for a dry and you don't get it, this is not a spaceship.

Karsten made that very clear. You have heterogeneities here. You may have heat pipes you may design for dry on paper, but you may not necessarily get it.

DR. RAMSPOTT: You may not. I agree, but I think that Tom has shown a lot of calculations and not pointed out the fact that the surface boundary conditions there were, essentially, 100 percent relative humidity, and that is not realistic in the desert where you have got about 30 percent relative humidity.

If you run those calculations, which he's begun to do, you pretty well dry out the mountain, and so you don't have this big, overhanging amount of water up there.

It is a pretty open mountain, and I think the water will probably go out the top.

DR. LANGMUIR: This difference from a natural analog, the usual natural analog in the sense that most of them are open systems and the fluids entering them are being continually refreshed and renewed. Whereas, you have got an chemical system here, by and large, and you are simply moving things around within it. There is quite a difference there in timbres of the effects.

DR. DOMENICO: Let me ask a question.

DR. LANGMUIR: Yes.

DR. DOMENICO: You certainly have made calculations on the volume of water. You have mobilized under the dry-dry. How does it compare with the volume of floor space you may have above the repository level. Is it larger, smaller?

DR. BUSCHECK: It can't be larger because, as I pointed out, and as Larry was mentioning a moment ago, we virtually prevent the water from leaving the top of the model. Our model has to obey a mass balance.

If it doesn't drain below the repository, it will be above. Of course, we don't make the water magically go away.

This doesn't address a minimally disturbed zone, however, if you are above about 50 MTU per acre, the maximum duration of refluxing conditions is about the same. The difference is, as Dale showed, where does that refluxing zone sit for the longest period of time.

If you were to choose something on the order of 15 MTU per acre, that refluxing zone would virtually be in the repository.

It would also be in your basal vitrophyre, your volcanic glass, which I think we have some concerns about today because, apparently, you perched water within the basal vitrophyre, and that raises the question.

I have heard many presentations by Shawn Levi, and she had told me that something in excess of 40 degrees C in the basal vitrophyre may start causing significant geochemical changes.

So a question about that unit is, is it more advisable just to raise its temperature to 60 or 70 or wherever degrees, C, or have warm condensate passing through it, or would a dry-out zone passing through that unit have worse or less consequences.

I think the altered zone problem is a big problem, but I think we have to differentiate between just the total amount of water mobilizing and also where those aqueous processes are likely to take place and how do they impact performance.

If they are talking place in your EBS, for example, you have sub-boiling reflexing or near-boiling reflexing in your EBS, and you are using a backfill as your primary barrier. You have to address the potential alteration of that as well.

So I think that all of these loadings still need to look at these coupled issues, and I don't think you can immediately make a decision that one necessarily can be inferred to be worse than the other.

DR. CORDING: We had a couple.

Scott Sinnock first.

DR. SINNOCK: If I could just take a minute, there are a lot of questions, and I think they are fascinating questions, but we need some measure. Which thermal load do we want? Is it important? I think we need some standard against which to measure these questions we are raising technically.

I just want to throw up a picture here you have all seen. Again, I hope I don't offend Bob Andrews or the Sandia folks. This is their performance assessment. Many of you have seen a complementary cumulative distribution. I am not sure that is the measure, but I think we need some sort of standard or reference by which to judge this.

This is a sensitivity study, and I don't want to get into all the details, but what it does is it compares various thermal options in terms of something we are interested in; in this case, total system releases.

Dave has, of course, mentioned -- Dave Stahl -- he is interested in the sub-system performance objective, the package.

But I think rather than debating to ad infinitum the technical details of what might happen technically, we always have to fall back against some standard of how influential is that in the real questions we are interested in.

This is a representation of protecting people from the waste. That is what we are all about is protecting people from the hazards posed by this waste, and all of these technical questions have to be somehow judged in the context of some measure such as this. I don't want to go into the details of this, but I just want to caution us all that I think we have to always bring these questions back to a measure of what it is we are trying to achieve with this system.

As far as I know it, there are the performance objectives that are laid on us by the NRC and that we have basically incorporated into 10 CFR 960 is DOE's rule.

Just as a caution, I think we need to discuss these issues of the importance. Do we need the data? We need the data if it significantly influences a measure of what we are interested in.

So I think we need that analysis of how it affects those measures we are really interested in.

DR. CORDING: Any comment on that?

I think one of the points that we need to, perhaps, pursue a little bit here is with regard to -- I mean, I think our collection of information and the valuation on this -- of conditions in the exploratory facilities.

The primary purpose of it is not to feed data into models. It is to better understand the mechanisms that we are dealing with. So I think that is one of the issues here on our overall performance assessment models. We are not including some of the effects that are being described in our discussion here.

Perhaps some comment on that and what do we need to do to be able to use this sort of performance assessment model.

Todd Rasmussen?

DR. RASMUSSEN: I would like to just comment first and lead into a general discussion.

I think that we had done some work at the University of Arizona that was field-related to examine what are the different pathways that could lead up to a diagram such as that, and we quickly came to the conclusion that in a heterogenous system, there are great uncertainties about each one of those predicted, cumulative exposures.

So, when you are trying to make a decision, that uncertainty is collapsed to just a very thin line, but yet, there is a confidence interval about each of those lines that you have to judge the uncertainties that surround that line.

In order to assess those uncertainties, there was an INTRAVAL program that I haven't heard discussed here. It is an international validation of geosphere transport codes. As part of that, we developed a series of field and laboratory experiments that looked at various components of uncertainty and how to quantify those uncertainties.

One of my concerns here is that everybody seems to dog the question of uncertainty. How much uncertainty is there in each of these processes, nobody will answer that.

What we would like to do is to develop a range of observations and models that help to quantify what those uncertainties are. Those are field datasets, laboratory datasets.

We also would like to identify failure scenarios.

We developed a series of field tests, and in each test, we found something new and interesting. The sensors would fail due to some unusual circumstance. It was something unexpected, unanticipated that caused the system to fail. A model cannot tell you what you don't expect.

What we would like to do is to develop the criteria, as Scott mentioned, for evaluating which of these alternatives is better.

I would argue that a discrete system as something that is homogeneous is better than a discrete system if you have strong heterogeneities, fractures. The heterogeneities are what make our system difficult to quantify and understand. A homogeneous system is easier to understand.

So one of your criteria for selection, I would argue is what are the degrees of heterogeneity in your system.

We know that thermal transport processes are much more homogeneous. You don't get a hundred orders of magnitude or a variation of a hundred in thermal properties.

Hydraulic properties are more heterogenous. Air transport properties are more homogeneous. If a thermal loading increases the gas-phased competition that leads to a greater heterogeneity in material properties, I would argue that that would be a much safer alternative.

So I think the quantification of uncertainties will lead you to the correct answer, and I don't see that formally represented here.

DR. CORDING: Thank you.

I also need to come back to Don Gibson here. Don, did you want to come in at this point?

DR. GIBSON: I will wait.

DR. CORDING: You will wait until we get through with this. Okay.

Tom?

DR. BUSCHECK: I agree with the problem with heterogeneity.

We, this past summer, took all the data available for the welded tuffs which was in the LBL report. I believe there are five units that were extracted from Flint City.

Do you remember those five sets? We compared that to our reference set, and we looked at the relative humidity throughout the repository for each of those sets and found that during the boiling period, there was very little variability in the humidity.

However, after the boiling period, the time that it took to rewet that to a given humidity, say 90 percent, varies immensely. The rewetting varied from something like 20,000 years to 300,000 years, which would infer that, yes, the -- especially after the boiling period or after the boiling period is going to take place if we have a range of units vertically that have that range of properties. We would observe a lot of variability.

Part of the problem, though -- not the problem, but a challenge is that heterogeneity, vertical or not, in the case of matrix properties. So it is not only whether you have heterogeneity present, but how that heterogeneity is structured, and I think we can learn a lot by deterministically looking at these variable systems. We won't get absolute answers, but I think we will get answers about sensitivities, and we have been partaking in that.

Getting back to my first point, during the boiling period, we found when there was thermal dynamic control, there was much less variability in terms of the humidity at the waste project, in spite of the fact that after the boiling, we saw great divergence in every wedding behavior.

DR. CORDING: Thank you.

Buz?

DR. GIBSON: Buz Gibson, M&O.

I wanted to go back a little bit to some of the things that Steve Saterlie was talking about. If you believe that we don't have the sufficient data at this point in time or a sufficient understanding at this point in time to point to the answer in terms of thermal load, the discussion around which is best is kind of a discussion that has no end point at this point in time.

What was articulated this morning was an overall thermal strategy that we are currently developing, and you will hear it articulated again tomorrow that takes into account some of the overall programmatics and mission needs of the program as we are there at the onset. We are diverging a little bit away from that, and I just wanted to bring that back into focus.

The mission of the program, the mission of the repository in general is to take all of that spent fuel, commercial spent fuel that is out there, and other waste, and put it in a geologic repository.

If you go to the projected end of life with all the reactors, that is about 87,000 metric tons. So that is one mission driver.

Another basic programmatic driver -- and that is to try and do that in a reasonable cost -- all of that has to be consistent with risk and the performance of the mountain, but if you take that and look logically at where this drives you -- if you could choose any thermal load you wanted.

It drives you to a couple of things. One, you want the highest probability that you are going to be able to operate a repository independent of the amount of spent fuel you can put in there, and second, ideally, you would like to get it all into a repository which drives you to the highest thermal load being more advantageous.

Cost also drives you to the highest thermal loading. It was mentioned earlier that cost is not much of a driver, but I guarantee you, as you go to lower thermal loads, the overall system cost will increase because of low local temperatures you need around the waste package driving you to smaller and smaller waste packages, which means more waste packages, and that is more cost, or just lower areal mass loading which drives you to longer and longer, more and more drifts, more and more area, which is a higher cost.

So what has been developed, has been a strategy for defining that thermal load that is consistent with almost all of those driving forces, and it takes into consideration the fact that we believe, for the most part, around 2001, we are unlikely to gain any more information that we have at that time with respect to a low thermal load performance, but we acknowledged that even though you might consider that a higher thermal load to be easier to conceptualize in the long run, we are still missing data at that point, so that the strategy that is in place says let's got to the highest probability of sticking to a reasonable schedule. That is the producer's driver at the other end, kind of the customer that is paying for this through the ratepayers. So, in order to stay on schedule, you drive to something that we think will work, which is a low thermal load at that point in time. It doesn't preclude the higher thermal load. That drives that out a little bit farther in time when we think we are going to get the additional data.

By going through those two steps, by trying to find a repository that you can license for a low thermal load and still looking to license a higher thermal load because of all the mission drivers, your overall probability of success goes up. It has got to be higher than picking a load and driving straight towards the one.

So you are going to hear that articulated, I think, very well, and Steve, his wrap-up slides articulates that again.

DR. CORDING: It could even be that regardless of which loading strategy you want selected that your initial loadings may not look that much different or do not need to look that much different in the first number of years before you start to get enough in a repository to actually develop the higher loadings.

Steve Brocoum?

DR. BROCOUM: I am going to slightly say what Buz said a little differently. I mean, the director has made it clear to us that -- he is under a lot of pressure from Congress and we are under a lot of pressure for him to demonstrate progress.

The strategy we have come up with is not trying to argue that low thermal loading on the low range is better technically. How can we get there the quickest with the kind of information we think we will be getting in the five to seven years.

So, when you hear people like Dale say, oh, I'd prefer the high, there is a lot of advantage to that, we are not arguing yes or no. What we are arguing is how can we clearly demonstrate progress the next three to five to seven years, including the TSS and the license application.

So the strategy, focussing on the amount of information we think we will get as opposed to what we think is the best thermal loading strategy, that we will determine later.

DR. CORDING: I think the one point that is being brought up, though, is also given a low loading or a high loading, what do you need to do for that to be able to get to that 2001 decision?

Certainly, I think some of the discussion here is saying you need to -- that there was some unknown with the low loading that you need to certainly be doing some testing for.

DR. BROCOUM: I haven't heard that yet, but if one can argue, we can get there faster by taking a high thermal -- for example, I will just use it as an example because we leave less information -- I would like to hear that. I haven't heard that.

It seems that given everything we have heard so far, you are likely to have information to support your case, and if you stick on the low thermal loading, always at the beginning.

If there is someone that really disagrees with that, we need to get that on the table.

DR. CORDING: Kartsen?

DR. PREUSS: I see a lot of attractive things about the present concept of starting with a low loading and learning. It keeps your options open, and it would allow us to learn as we go.

What I am really concerned about is whether -- and I don't know whether anyone would want to venture any guess there, but such a strategy, whether the NRC would go along with that, whether the regulators wouldn't say, well, guys, this is simply a copout, because you haven't done the homework to know how this thing would behave. So you are going to start one at a time and learn as you go, and that is one way to license a repository. I mean, that would be my concern, but if this can succeed, I think it is clearly the best way to go.

DR. BROCOUM: We can't talk for the NRC unless the NRC themselves who are here would like to make a comment, but they have told us at one of our management meetings, they understood our strategy, and they at least in a verbal sense -- that was a strategy that could be viable.

DR. CORDING: Thank you.

Ardyth?

DR. SIMMONS: Thank you.

With regard to that comment about the NRC, we did have a technical exchange with them just a week ago on coupled processes, and at that meeting, we discussed our approach. It was essentially the approach that I laid out in my overview this morning or early this afternoon, and because this is directly coming from the NRC guidance, I believe I can say that we did not have any objection from the NRC to the approach that we were taking, which is the approach where we were going to look at the significance of the coupled processes to performance, and that we would try to quantify the uncertainties with those processes that we cannot understand in the mechanistic detail that we would like to be able to understand.

There have bene a lot of comments today about how will we know that the experimental program will get us where we need to go. What can we learn from analogs studies? Modeling isn't enough. All of these points are very well taken, but what this program needs to do before we can come to a decision on thermal loading is to be able to put together the information that we do know from natural sites, such as Yellowstone, such as New Zealand, such as other hydrothermal sites that we might study in the future, such as what we know from Yucca Mountain is an analog to itself and the past.

Take a look at that, and then use the information to develop conceptual models which is well under way, design experiments at various scales that will help us to understand the processes that are working at those scales, and then see how that information will help us in meeting the performance objectives that we have, but it is not a matter of just testing the validity of the models. We have to make sure that the modeling and the experimental program are suitable to as much as possible, hoping to alleviate the uncertainties that we have in system performance.

So I think what many people have been addressing this afternoon is maybe little pieces of the elephant, "What happens to the permeability if a mineral dissolves? What happens if you get concentrates brines?" All of these are part of the overall system behavior that we have to fit into our understanding through both modeling and experimentation before we can come to what the best thermal loading decision would be.

DR. CORDING: Thank you.

I think one of the things that is encouraging to me is to see that there is a growing link between what is technically of concern and what the program is trying to accomplish in terms of the objectives of the testing.

I think it is coming closer together. That is my perception, but having it at the table within the presentations, people involved in the details, people involved in focussing on where we have to go, I think, has been very helpful to me. So I am very encouraged by that.

Other comments?

I don't know that we have had the chance to let everyone make a statement or a comment. Some of you haven't made presentations.

If there is anyone else here who would like to comment. Jean, I don't know. Jean Younker, I don't think you made a presentation today.

Anyone else?

Yes. Randy Bassett from the University of Arizona, you have not had a chance to say something here.

DR. BASSETT: I have some viewgraphs. They are right behind Tom. Tom, would you get those --

DR. CORDING: I think we will want to have that tomorrow.

DR. BASSETT: I would like to make a comment about the perch zone because we have a field site in Arizona, the Apache Leap research site, and there is a very interesting analog here, and it relates a lot to what Pat Domenico said, and that is what difference does it make.

Let me ask you a couple of questions. What do you mean by hydrologic heterogeneity? Could you predict where, in fact, a perch zone would form if you began to boil your water and let it condense?

Could you predict under that conditions that zone would form? My guess is that you could not, and it is based on field observations which is that we found three perched zones in a rather heterogeneous tuff in Arizona, and if you look at the core that passes through these zones, visually, you cannot see the difference. So that is the first observation.

I am not sure I know yet what the exact hydraulic properties are that determine where you form a perched zone. That needs to be investigated because Yucca Mountain will find perched zones, and we have found them. So we must move forward in that regard.

The second question is if you have a perched zone and there are fractures that transect this perched zone, can you predict our priority, which ones will drain, and I would say you cannot. The reason we say that is because there is a mining tunnel we have access to built by Magma Copper Company, a multimillion-dollar tunnel which goes under the perched zone.

If you walk the length of the tunnel, you will see dozens and dozens of fractures. Some of them drain. Some of them do not. Some of them are wet. Some of them are dry. Some of them seep. Some of them flow a liter a minute.

It is interesting that we have sampled the composition on many of these fractures, and it is basically the same water composition. So they are all coming from this same zone.

Right now -- and I don't think before '98 we will gain this capability. I don't think we can predict where the zones will form, nor can we predict which fractures will drain.

The third thing, one final thing and then I will be finished, can you predict how fast the water will move down a fracture?

Again, at our site, we have this opportunity because the creek, Queen Creek which you may have heard of, transects a tunnel, and there is a distance of 120 meters through this fractured, unsaturated tuft.

The water traverses this fracture network probably on the order of seven or eight days. Now, that is a remarkable speed.

Normally, it is an intermittent dry stream. You have a surge of water through the creek. We monitor the composition of the water in the creek and monitor the composition in the tunnel. It is the same water. We have done isotopic dating. We have done all kinds of tests on it.

The water is moving, long distance, rapidly flowing, and it is making it to the tunnel very quickly, but then I ask you Pat Domenico's question. What difference does it make?

You can have 12 feet of water or 3 meters, if I use the right units, of water in the creek, and the fracture than begins to flow, increased rate within, say, a week or so, but it is a manageable flow, and we drain it in and it flows out the tunnel.

So three questions, and I guess, maybe, by analog, you might be able to say it is worth looking at these, and they, perhaps, would have impact on the consequences of those particular processes at our site.

DR. CORDING: Any responses to those questions?

DR. TAYLOR: I would like to ask him a question.

DR. CORDING: Ed?

DR. TAYLOR: What do you think we are going to see when we dig into Yucca Mountain?

DR. BASSETT: Well, I am really interested in that because I think you are going to find -- well, you have already found at least two perched zones, I think. You may find more. I would not be the least bit surprised to see some fractures that are moist.

I am a little concerned. I don't want to cast negative comments on any of the plans, but I am not so sure that we will observe them weeping because of the method of drilling and sampling and the protocol that is established. We may just blow them dry and go right past them, but I would not be surprised to see some fractures that are weeping.

DR. CORDING: Thank you very much.

Some of those questions, I think, are good ones. They may be rhetorical at this point, and we will consider them further, even tomorrow.

We appreciate the input we have had from the speakers today and the people on the panel.

We have one or two items left, and I would like to just ask if there is anybody -- first of all, I want to have any comments from the audience just for a few minutes. If they would like to make comments before that, I would like to ask if the Board members have any remarks they would like to make at this point.

Don Langmuir?

DR. LANGMUIR: When Todd Rasmussen was talking, it reminded me of some comments I had heard from others about the DOE program and the question of whether the DOE models are being evaluated. It is my perception that they, perhaps, have not been internationally validated; that the TSPA models, for example, which deal with flow and transport, need to be aired and reviewed internationally the way most of the European ones and the Japanese ones are being.

I guess I would like some comments from the DOE on their perception. I mean, the uncertainties in the TSPA and the dose curves, for example, that Scott showed us. I am always worried about the uncertainty in those curves. They look wonderful. They miss the EPA standards very nicely, but how wide are the bounds that those really represent in terms of unknowns that went into those models at the subsystem level.

DR. CORDING: Scott?

DR. SINNOCK: Todd raised this question, too, what are the error bounds on that curve, and I would just like to say, that is a complimentary cumulative distribution function. It is a probability curve. That curve is a statement of uncertainty.

Now, how well that uncertainty actually captures our state of knowledge of the mountain, I think, is at issue, but theoretically, if it is done right, there is no error bounds on that curve.

So what we are talking about is -- and I think you mentioned it, too -- have we captured the processes through our abstraction process adequately, and that is a validation issue that I think we need to address.

I am always uncomfortable when I hear error bounds on that curve because there is no way I can do a calculation to draw an error on that curve.

In other words, each point on that curve is an alternative conceptual model. So that, the uncertainty and the behavior of the performance of the site ranges across six orders of magnitude. That is an uncertainty statement about performance, with each point being an alternative conceptual model, if you will.

DR. LANGMUIR: What if you change some fundamental assumptions upon which the curve is derived at the subsystem level, major assumptions about source term.

DR. SINNOCK: Theoretically, I can, perhaps, do that by parameterizing a very abstracted model, how the system behaviors, but do we capture that on the certainty in the model? I don't know. Theoretically, we should be able to. The NRC is saying somehow you can't put alternative conceptual models into that curve.

I can also argue that ever point on that curve is an alternative conceptual model. It represents 10,000 models of how the site will behave because each point is a selection of parameters, perhaps a disillusion rate, perhaps a container behavior, et cetera, representing one particular model.

I think we have to be very careful about error bounds on that curve. We have to realize that there is uncertainty in the curve, but if we can think of the uncertainty, we should get it in the curve.

DR. CORDING: A question on that from the audience for comment?

COMMENTS FROM THE AUDIENCE

MR. CODELL: Richard Codell from NRC.

I just wanted to bring to your attention that there is another way of portraying CCDS, and those who are familiar with WIIP know that they presented as a different way which you can look at a spread in the CCDF.

What Scott is referring to is what they would call the mean CCDF. If I had a picture of this, it might be interesting.

The way they portray it, you can present the percentiles of the CCDF with the mean in the middle.

DR. CORDING: Thank you.

Jean Younker?

DR. YOUNKER: Yes.

I was just going to comment on the question of how we will go through getting the confidence in our codes and models that underlie the codes. The standard answer, I think you already know, Don, is that I think through the National Academy review of the technical basis documentation for suitability, I think we hope that that is where the confidence in the conceptual models gets developed and alternative models brought in if we need them or if we haven't considered them.

Then, as far as the codes go, we will have to go through the rigorous process of qualifying those codes which will involve independent peer review and the steps that you go through when you take a code through and use it as a basis for licensing, and we will have to do that with the codes that are a fundamental basis for the license application prior to 2001.

So much of the effort that I think the M&O team has been working with me on in the last couple of years, is to figure out how DOE figured out which of the codes we are going to want to take during that process and get ready to use in the licensing. There is a big effort. I was just managing one big effort on the thermal hydrologic side of that.

From the process model up to the top of the pyramid, the total system modeling, that is kind of the approach we are going to have to take is to figure out which ones to concentrate on, which ones have the best -- not that you will only get down to one in any given area of process modeling, but get your best ones developed and then take them through that process of qualification.

DR. CORDING: Thank you.

Any other comments from the audience?

[No response.]

DR. CORDING: Karsten, you are not the audience, but I will let you --

DR. PREUSS: I have a very brief comment.

What Jean just said looks like a good strategy for deterministic process codes, but I don't know that one knows how to validate probabilistic models, and another brief comment there at Yucca Mountain is a deterministic system, and you can try to figure something out about the system using probablistic models, but it is going to be one representation, if you are lucky, off all of the probabilities that you consider and you don't know which one.

DR. CORDING: We have reached several end points here. We are running out of energy, and the time has escaped us. So I would like to thank you all for being with us today and look forward to our session tomorrow morning.

Don Langmuir will be chairing that. We will begin tomorrow morning at 8:30. So thank you very much.

[Whereupon, at 5:30 p.m., the meeting was recessed, to reconvene at 8:30 a.m., Friday, November 18, 1994.]