UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

THERMAL LOADING:

The Integration of Science and Engineering

July 13, 1993

Stouffer Concourse Hotel
3801 Quebec Street
Denver, Colorado 80207

NWTRB MEMBERS PRESENT

Dr. John E. Cantlon, Chairman
Dr. Donald Langmuir, Co-Chair
Dr. Ellis D. Verink, Member
Dr. Patrick A. Domenico, Member
Dr. Edward J. Cording, Member
Dr. Clarence R. Allen, Member
Dr. Garry D. Brewer, Member
Dr. John J. McKetta, Member
Dr. Dennis L. Price, Member

STAFF MEMBERS PRESENT

Dr. William D. Barnard, Executive Director
Mr. Dennis G. Condie, Deputy Executive Director
Dr. Robert Luce, Senior Professional Staff
Dr. Daniel Fehringer, Senior Professional Staff
Dr. Leon Reiter, Senior Professional Staff
Dr. Carl Di Bella, Senior Professional Staff
Mr. Russell K. McFarland, Senior Professional Staff
Dr. Sherwood Chu, Senior Professional Staff
Ms. Karyn Severson, Congressional Liaison
Ms. Nancy Derr, Director, Publications
Ms. Paula Alford, Director, External Affairs
Mr. Frank Randall, Assistant, External Affairs
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

I N D E X (Continued)

P R O C E E D I N G S

DR. CANTLON: Good morning. This is the summer meeting of the Nuclear Waste Technical Review Board. My name is John Cantlon. I'm Chairman of the Board, and former Vice-President of Research and Graduate Studies at Michigan State. My field is environmental biology.

Let me briefly introduce to you the other members of our Board, if they'll hold their hands up so you can get the back of the heads lined up with the names:

Dr. Allen, who is Professor Emeritus of Geology and Geophysics at Cal-Tech; Garry Brewer, who is Dean of the School of Natural Resources and Environment at the University of Michigan, and Professor of Resource Policy and Management there; Ed Cording, Professor of Civil Engineering at the University of Illinois; Patrick Domenico, who is David B. Harris Professor of Geology at Texas A&M; Donald Langmuir, Professor of Geochemistry at the Colorado School of Mines; John McKetta, Joe C. Walter Professor of Chemical Engineering Emeritus at the University of Texas; Dennis Price, Professor of Industrial and Systems Engineering and Director of the Safety Projects Office at Virginia Polytechnic Institute and State University; Ellis Verink, Distinguished Service Professor of Metallurgical Engineering Emeritus, University of Florida.

Also in attendance are our professional staff and technical group. They're seated over here on my right, your left.

Board member Warner North, Consulting Professor of Engineering and Economic Systems at Stanford, and principal in Decision Focus is recovering from a ruptured appendix, and will not be with us today.

As most of you know, the Nuclear Waste Technical Review Board was created by Congress in 1987 in the amendment to the Nuclear Waste Policy Act. The Board is charged with providing an unbiased source of expert assessment of the technical and scientific aspects of DOE's work in high-level nuclear waste management. We report formally at least twice a year to Congress and to the Secretary of Energy.

The major subject of this meeting is thermal loading, how the radioactive decay heat from spent fuel and high-level waste is managed in the repository, and how that affects the entire waste management system. We have allotted two very full days to this and related topics.

The last Board meeting about thermal loading was in October, 1991. Much of the information from that meeting served as the basis for our findings and recommendations issued in our fifth report a little over a year ago.

The Board is pleased to note that some of the things happening and actions taken in the thermal-loading area have been very well organized. For example, DOE has embarked on a very serious examination of the large robust drift-emplaced waste packages. Also, DOE is devoting some significant resources to examining universal waste package concepts, such as a multi-purpose canister, including the examination and the influence of the MPC design on repository design, and the design of the entire waste management system.

At the same time, as we all know, we're engaged in a first of a kind system, and for the safe and effective operation of these kinds of systems, it needs to be a functional whole, so it's important that less than optimum designs not be set in concrete, baseline designs that everyone realizes won't be used, but become very resistant to change.

As you can see from this agenda on thermal loading and the integration of science and engineering, we have invited the participation from organizations with wide-ranging responsibilities and perspectives. The subjects that we will be discussing relate to themes that have been or are being pursued by several of the Board's panels, or by the Board as a whole.

Therefore, as is becoming increasingly our custom, we will divide up the job of moderating the presentation and discussion sessions. Dr. Langmuir, who co-chairs the Board's panel on hydrogeology and geochemistry with Dr. Domenico, will be chairing today's sessions on DOE's plans and progress on geothermal analogues and on thermal modeling of Yucca Mountain. He will also moderate the round-table discussion at the end of the day.

Tomorrow morning's session on waste package issues associated with thermal loading will be chaired by Dr. Verink, who is chairman of the Board's panel on engineered barrier systems.

The session after that on repository conceptual design, as well as thermal testing, will be chaired by Dr. Cording, who chairs with Dr. Allen, the Board's panel on structural geology and geoengineering.

The afternoon session, which we are entitling, "The Big Picture," will be led by Dr. Brewer, who is chairman of the panel on environment and public health. We gave the afternoon session that title because of the content of its talks, and particularly because of our belief that performance assessment is or should be not only a unifying activity for all the program's scientific work, but also an important tool in guiding the research program.

Time has been provided for questions and comments at the end of both days. Furthermore, I'm sure that the session chairman will solicit questions during the session at their discretion. To facilitate discussion, we will ask the speakers to change places with the Board members during the discussion session.

Before Don Langmuir gets the morning session underway, I have the pleasure to introduce Carl Gertz, DOE's project manager for site characterization. He will both introduce his new boss, and give us an update on the project's activities.

Carl?

MR. GERTZ: Thank you very much, Dr. Cantlon. I hope you'll bear with my voice. I was cheering for my daughter in a softball game that lasted four hours and 35 minutes last night in Las Vegas--yeah, they won in 11 innings, and they're going to California as part of a national tournament, so I'm pleased for that aspect.

Before I start, I'd also like to welcome you all. From our point of view, we're glad to be here to talk about thermal loading. Lake Barrett was unable to attend. Jerry Saltzman, who is your new point of contact, is acting for Lake in Washington, D.C., and I'll just put up on the view graph machine for a second our current organization so you know who the players are.

As you can see, Lake is the Acting Director. Frank Peters has moved to some other activities right now, and Jerry Saltzman was the Acting Deputy Director, as well as filling these two boxes. Tom Isaacs has moved on an assignment at the labs, and Jerry will be your point of contact in the future in this role, but they express their disappointment they were unable to make it, but I sure express the pleasure of my team that we're here today to talk to you about several aspects.

And before I move on, you can see the Office of Geologic Disposal now has two elements to it, and I'd like to introduce the Acting Associate Director for Geologic Disposal, and my current boss, Linda Smith.

MS. SMITH: Thank you very much, Carl.

Chairman Cantlon and Board members, it's an honor and a pleasure to be here with you today and to be able to share with you some of the changes that are going on. I know that when someone new is inserted, as Dr. Brewer and I were talking, into these processes, there's obviously an active interest in what the dynamics are, and I'm not sure I can fully explain the dynamics here, but we'll certainly give it a try.

I have, of course, often heard of your activities in my role with the Nevada operations office, which is on the defense program side of the house, which is where I have spent most of my career, so I appreciate very much an opportunity to participate more directly in the Board's activities on this side of the house. We deal, of course, very directly with the Defense Nuclear Facility Safety Board, and while I'm sure there are analogues, they're very different processes.

I was asked to join the Yucca Mountain Project a couple of months ago by program officials on an interim basis, and for a somewhat undefined period, probably because there was a strong recognition that with the growing scientific and technical activities at the Yucca Mountain site and in the State of Nevada, that it became obvious it was important to have two senior management positions in the State of Nevada; thus, allowing Carl Gertz to focus very heavily on those aspects of the program.

I have been asked to assume the broad-based management role for the State of Nevada for Yucca Mountain, and to focus my efforts on predominantly the institutional aspects, but Carl and I are working very closely together to assure that is a team effort, because although they are equally important, of course, that cross-cut, as we see it, is very, very important, and we're--Carl and I have worked together a very long time from different programs and have a lot of respect for each other, and I think can work the integration issue very well.

Just briefly, I am a long-time Nevadan. I came there when I was very, very young, in 1949. I have spent my career with the federal government in the State of Nevada. I began with the Bureau of Reclamation, went very early on to that thing called the Atomic Energy Commission in 1965, was there in increasingly responsible management roles until I left to go with the Bureau of Reclamation in 1979; actually, with Western Area Power Administration first, and then the Bureau of Reclamation with the Central Arizona Project, which also gave me a taste of being a manager in a very highly technical, very large construction project with a lot of political ramifications.

My role in the Nevada Operations Office of the Department of Energy has been focused in the business management and the management arenas. I have served as the Acting Deputy Manager, in the absence of Bob Nelson, our Deputy Manager, who was sent on an acting assignment to Rocky Flats for six months, that lasted for three years, and we're hoping that isn't the analogue here, but we never know about these things.

I have a masters in business administration. I bring a business management focus to this very competent technical management staff that we have, and I'm enjoying it very much, and I look forward to working with all of you over the months.

Thank you very much.

MR. GERTZ: Before we start into the detailed recitations on thermal loading, I was asked to give an update on where we stand at Yucca Mountain, and there's a lot in the book and we'll skip a lot that's in the book, but I just want to hit some highlights about what we're doing out there right now.

I want to remind you, it is a program of underground and surface-based testing. We are currently, over the past six or eight months, done three different drillings and done several other activities. We have almost 20 activities in process on any given day out there, so we'll talk about each one for a minute, and you can see my time, so I won't go over that, but it's a balanced program. It's surface based and underground testing.

You might be interested in our current schedules, a change you've seen to this chart. We now have a "to be determined" for license application. While we know what we need to do during site characterization, we're now sure what our funding will be, so if you could tell me what our funding will be, we can tell you what our date will be. But, based on that and the ongoing Secretary's review, it's undecided whether we're going to submit a license application because it is out of our resource and control, in our view, at this time.

What's after license application, you're then, of course, approaching three to four years of licensing with the NRC, and construction and operation should Yucca Mountain be suitable for a repository. There's a possibility to still meet 2010. That depends upon funding, how long this takes, and what you do when we get started with it.

As I said, there's lots of activities going on. If you haven't been to the mountain for while, each time I go out there, it amazes me at how much is going on.

I thought I would update you on our current level. I know Pat, you had talked to this a couple times ago with us. At UZ-16, when we went to 1686 feet, we were averaging about nine feet a shift. The first 950 feet was almost nine feet a shift. At UZ-14, we're doing quite a bit better. We're averaging about 16-17 feet per shift, and this is core drilling all the way. So that's just a point of information for you, but it's out there, and we expect maybe this will improve as we get further into the formation.

I'd also remind you that prior to '87, a little-known fact is that we had lots of holes and trenches done. Since we overcame many obstacles, including state permits and endangered species, we've done several boreholes, trenches and soil pits, and from July, '93 to the end of the program, whether that's 2001 or whatever, we have several deep boreholes of over 50 feet planned; several less deep boreholes, trenches and soil pits. That's kind of our surface-based program in a snapshot.

Let me now talk to you a little bit about our ESF. John, I think it's rather noteworthy that you talked about evolving change in the process. That's tomorrow from Bob Sandifer. You're going to hear about some changes we're making in this preliminary design, and I'll highlight it a little bit, but essentially, we're reducing the grades and we're changing the orientation across here, the main drift.

But in the meantime, we are progressing forward very aggressively at Exile Hill. This map shows we're 172 feet in with the pilot drift and slashes. We did another blast yesterday, so we're over 180 feet. We expect to be 200 feet in by the end of the week. We then expect to come back with a one face excavation on the bench. We expect by mid-September to have that concluded, and have our 200 feet in, and hopefully by the end of September, we'll also have another alcove, a test alcove off to the side, small in diameter.

So that's how we're progressing. I'll show you some brief shots of that. Rock quality is becoming a little better as we move in, less lithophysae. We're doing standard wire mesh rock bolts and shotcrete for ground support as we get into the drift. That's fibrocrete, I guess, rather than shotcrete, is a better term.

The first 37 feet or so, we did use some lattice girders for additional ground support, and that's looking outside. As you can see, the original lattice girders are out now. We believe fibrocrete rock bolts will suffice.

Here's a shot of the face. Bob Sandifer will talk to you a little bit tomorrow about, originally, if you remember our plans, we were going to put a kind of covered tunnel in, coming out again. We're probably not going to do that, for lots of reasons. Right now, one of them is the savings of money. We think this face is stable enough that we'd be able to perhaps shotcrete it in place.

The other things I'd like to point out is this is the schedule I think we showed you last October. In effect, we're pretty much on that schedule. We will be completed with this upper bench maybe a little bit later in July than we originally anticipated, but we're pretty close on schedule. We'll then do the lower bench by mid-September, and our delivery of our total boring machine is now 4/94, and you see some of the milestones that have slipped.

Our longer range schedule remains. This is different on this view graph than in your book. The book has the latest dates, but I guess I'll just highlight what Bob's going to tell you about tomorrow, but our new alignment, in effect, was like this. You can see that parallel to the Ghost Dance Fault, and we'll talk more about that tomorrow, but at least I wanted to set that up for you. Once again, that's a proposal.

While we are tunneling by drill and blast, we anticipate we will be able to do it ten times faster with a tunnel boring machine. We have one on order. We are monitoring that procurement. Things seem to be going well.

The design's complete. Long lead items have been ordered, so I guess the bottom line, there's going to be 70 truck loads delivered to the ESF pad prior to April 5th. April 5th is the last truck load that we expect to receive. Then we expect to take approximately 60 days to assemble it, and to have a look; maybe even shorter than that.

While we are doing the drill and blast, I want to point out we're not just making tunnel to make tunnel. We're also obtaining lots of scientific information for both future design and also for the understanding of the mountain. We're doing the mapping, monitoring the high wall, doing monitoring of the tunnel with load cells and closure pins, doing blast monitoring, and doing rock support monitoring. So we're gaining important scientific information.

To give you a little bit of view of what's in the future, assuming our evolution of design takes place, we're going to have a design package review on 7-19; continuing drill and blast up to, but not through the Ghost Dance Fault, probably about halfway to it.

Then we'll go to our ventilation and structural steel package design review on 9-20, and then the rest of the ramp down to the Topopah Springs about the first of next year.

We are, in effect, designing just ahead of construction. The first package will be tiered; that's 2A.

2C will take all the rest, and 2B is the mechanical aspects of that.

In case you're wondering about project priorities next year, we're still sorting through them. In fact, my staff has a lot of homework to do this week and this weekend, because we're now sure that our split for 1994 is going to be this way. This is our current plan, which is, once again, a fairly balanced program of 54 million for site, and about 54 million in the ESF.

However, when I talk to my staff, they believe to do the work that I've asked them to do, we'll need about 300 million, so we're going to have to sort our way through that as we get ready for the '94 project. There's always a chance Congress would enhance our appropriations. If they do, we'll be very pleased.

Rather than tell you what we are going to do for 261 million, I have a view graph of what we're not going to do if we get 261 million, and you can just see that as a planning process. This is not at all the final answer. We have lots of things that will go on between now and implementing our '94 plan.

I do want to point out, though, while all the field work is going on, we do need to pay attention to several of the environmental issues we deal with. One of them is the desert tortoise. Fortunately, this wasn't our program, but to let you know that the Fish & Wildlife is serious about this, a contractor in Clark County was fined $100,000, in effect, for killing a desert tortoise at one of its construction sites, so--and they stopped work, which is even more devastating to a big project.

So while we're doing all this work, we're also doing many other activities that are necessary for work to go on, and, John, just to let you know, also, about the site's big changes, there's also small changes that go on, and since October, which is six-seven months, we've had over 205 field changes, both in the surface-based and the ESF programs, many cost/schedule changes. We have the GAO monitor us continually, so we want to assure our cost information is up to date, and then, of course, we're always changing procedures. Nothing is set in stone on this project.

Hopefully, we're improving as we're changing. In effect, that's our goal, and that's my very brief summary. We'll provide you some photographs of things right from the mountain, to some video, if you'd like to see video about some program changes.

John, I hope I didn't delay you too much. I'm glad to show you part of this.

DR. CANTLON: Thank you, Carl.

Any questions from the Board members?

(No audible response.)

DR. CANTLON: All right. Thank you.

Don, you're on.

DR. LANGMUIR: I'm going to introduce the overall agenda for our meeting. My purpose is to set the stage for the two days of open Board meeting on thermal loading, while emphasizing the first day's proceedings in particular.

This morning, we're going to first learn about the DOE's plans and progress towards evaluating a thermal-loading strategy. Next, we'll examine the use of unsaturated geothermal analogues to predict the performance of the proposed repository at Yucca Mountain under various thermal loads.

In the afternoon session, we will hear about the status of models that are being used to predict the long-term geochemical and fluid-flow behavior of a repository, and analogue geothermal systems.

Tomorrow's sessions, as you've already heard, will examine thermal-loading issues and goals related to waste package performance, repository conceptual design, waste retrievability, the desert ecosystem, and total system performance assessment.

Now for some more logistics. You've gotten some from John, but a little more from me. We obviously have two full days. Speakers are asked to stay within their allotted times. Assistant chairmen are going to be nasty and rigorously adhere to the schedule. In other words, I've got to have a talk that fits within my schedule or I'm in trouble.

Both days wrap up with a summary discussion period led off by invited panel members, and involving the day's speakers. The audience will be invited to participate at the session chairman's discretion, time permitting. All those participating should identify themselves and give their affiliations.

Could I have the first overhead, Carl?

Let's go to the next one. Just to show we've been thinking about this a long time, at the Board's October, '91 meeting on thermal loading, and in our June, '92 fifth report, in which the main emphasis was thermal loading, the Board has emphasized the critical importance of thermal loading as a cross-cutting issue affecting most aspects of the waste management program.

We also expressed our concern that the Department of Energy might select a thermal-loading strategy for Yucca Mountain based on inadequate scientific understanding of the possible and probable consequences of such a choice to total system and repository performance.

Listed on this view graph are some controls on thermal loading. The first three are largely givens, with the average fuel age now approaching 30 years. The last three can obviously be adjusted, with the constraint that the waste must fit within the proposed repository block or primary area at Yucca Mountain.

The next overhead shows a map, and it seems reasonable to ask whether, in fact, we need to restrict our discussion to the pork chop-shaped primary area given on the SCP, which is shown in the middle of the map. There are plenty of alternate areas if we choose a below-boiling repository, or if site characterization discovers areas we wish to avoid.

Let us assume, for simplicity, that the usable primary area is 1,000 acres rather than 1278 acres shown on the map. Limiting the repository to 70,000 metric tons gives an average mass loading of about 70 tons per acre. Actually, the requirement is somewhat less because of the roughly 7,000 tons of defense waste which generates comparatively little heat.

Given the large rock volumes contiguous to the primary area, it seems probable that an additional 1,000 acres could be identified that would be as suitable as the primary block for the siting of a repository. In other words, the choice of a below-boiling strategy is probably not precluded by the availability of suitable rock.

The next two view graphs show some aspects of the waste management system that are affected by the choice of a specific thermal-loading strategy. Waste package design and repository design, canister corrosion, and waste retrievability are topics for tomorrow's sessions. Today's talks will focus on the predicted effects of various thermal loads on fluid movement and mineral alteration around the Yucca Mountain repository. Such coupled effects will, of course, influence canister corrosion, and especially the potential release and transport of radionuclides to the accessible environment.

The predictability of long-term repository performance, and, thus, it's licensability are also dependent on the predicted effects of different thermal loadings on liquid and gas movement around a repository.

On these two view graphs, I have suggested some implications of the various thermal-loading choices. The meanings low, medium, and high have changed with time. As recently as last year, a loading of 120 kw/acre was termed too high by some DOE scientists. The review draft of the DOE's thermal-loading discussion task force report, dated September 18th, '92, has reduced the headings to simply low and high thermal loadings, without defining their meanings in terms of kilowatts per acre, although, apparently, last year's too high is now considered the upper end of the high range. We'd like some clear definitions, as much as possible, on these terms.

Based on past DOE statements, I have suggested what thermal loadings might correspond to the conditions experienced by waste packages in a repository. Ben Ross has suggested that because of the uncertainty in our knowing whether a given thermal-loading choice creates boiling or sub-boiling conditions, we should assign probabilities to such assumed conditions.

In the view graphs, I have summarized possible implications of the various thermal-loading choices. Most are related to the topics we will be addressing today and tomorrow. The weightings I have assigned are personal opinions, obviously subject to debate.

The critical issue is if and when one or more waste packages in a repository will experience wet conditions. All loadings, except extended dry, apparently lead to wet conditions at times less than 1,000 years. Further, it remains to be proven what thermal loadings, if any, will create above-boiling extended dry conditions for all waste packages in a repository.

Wet conditions lead to waste package corrosion and failure, and aqueous and gaseous radionuclide transport and release. Dry conditions may minimize such risk. Wet conditions require a robust corrosion-resistant, long-lived container. Dry conditions may or may not require such a container.

The incredible difficulty of understanding, predicting, and probably, also, of licensing the long-term performance of a wet repository convinced me that we need to learn more about the extended dry concept, which may prove simpler and easier to license.

Borrowing some terminology from Larry Ramspott, a large part of the licensing defense of a wet repository must, inevitably, deal with the mitigation, in his words, of radionuclide releases by the geological environment after waste package failure by corrosion.

One can hope that the licensing of an extended dry repository would instead focus on how extended dry conditions prevent waste package failure and radionuclide releases. This would seem the more defensible position for licensing.

Significant unknowns in the extended dry concept include modes of cladding and container failure in the absence of liquid water and resultant gaseous radionuclide releases, and the consequences of increased boiling and refluxion to radionuclide transport.

In a recent letter to Russ Dyer and Ardyth Simmons of the DOE, Chin Fu-Tsang, Karsten Pruess and others from LBL have expressed concerns regarding the extended dry repository concept. Their thoughts are summarized in these two view graphs, along with a couple of my own concerns that have been added.

Going through these briefly, they suggest that the rock may retain liquid water, even at temperatures well above boiling. That condensate water may flow in fractures, even if most of the rock near the repository dries out; that fracture flow may be enhanced by high thermal loadings; that differential drying and condensation may dry some waste packages and increase liquid flow near others; that fuel cladding--this is one of mine--that fuel cladding and high-level waste glass may be unstable in high loadings; that the migration and escape of gaseous radionuclides such as C-14 will be enhanced by high temperatures.

The stability of mined openings decreases at increased temperatures, another concern. A geochemical issue, that the sorptive ability of zeolites and clays for radionuclides probably decreases with elevated temperatures because of alteration of those minerals; and operational safety and waste retrievability are more difficult at elevated temperatures; and, finally, on the overhead, that you may generate hydrothermal-type conditions by the refluxion, which could lead to migration of solutes and precipitation of a variety of solid phases, which could clog pores and perhaps increase the pressure, and conceivably create explosions. Hopefully, we'll hear something about this sort of thing from the analogue discussions today.

Let me wrap up on my introduction with some thoughts on key data inputs needed for the modeling and prediction of repository performance under different thermal loads.

Information essential to the DOE's selection of a defensible loading strategy includes additional site characterization data. Data from surface-based testing and from the ESF is fundamental to a thermal-loading decision. Such data includes further information on the gas and liquid transport properties of the rock, and controls on fracture versus matrix flow.

Also needed are results of the heater tests recently proposed by Livermore scientists. Such tests will apparently require five to seven years to complete and interpret. The heater tests would be designed, in part, to improve our understanding of how various thermal loadings might impact fluid movement at Yucca Mountain, and the consequences of such movement to repository performance, including coupled process effects.

Even if heater tests are completed successfully, there is apparently real question of their value for predicting long-term repository performance. The fluid refluxion and thermal gradiants created by a repository will lead to the dissolution of minerals in the tuff, and the possible sealing of fractures and fracture walls by precipitates, including silica, iron oxides, clays, and zeolites. These precipitates will obviously change rock fluid transport properties. Understanding of such coupled processes is essential to the prediction of repository performance.

Another key input to this effort should include the study and interpretation of unsaturated zone geothermal analogs, carefully selected so that their behavior is relevant to the performance of the proposed repository under different thermal loads. It seems likely that geothermal analogs can provide us with essential spatial and temporal information on potential repository behavior that cannot be obtained solely from site characterization data, heater tests, coupled process experiments and calculations, and related computer modeling efforts.

Further, I would hope we could use insights from unsaturated zone geothermal analogues to help validate geochemical, hydrologic, and coupled process models for different thermal loads.

Finally, if an early decision on thermal loading is found necessary, then that decision will probably have to made without reliance on heater tests, because such tests may not be completed. Without the tests, the only data on thermal effects on which to base a decision may be that from natural analogues. This is another major reason for interest in geothermal analogues.

That concludes my comments, and I'd like now to turn it over to our first speaker, Bill Simecka, and the DOE's discussion of its approach towards deciding upon a thermal-loading strategy for the Yucca Mountain repository.

Let me introduce Bill first, before he comes up. Bill has Ph.D. in mechanical engineering from U.C. Berkeley, and has over 40 years of professional experience in nuclear and conventional weapons development and nuclear waste. He is currently the Director, Engineering and Development Division, in the U.S. Department of Energy's Yucca Mountain Site Characterization Project Office.

Dr. Simecka was recently the Engineering Department Manager with the SAIC Corporation, also working on the Yucca Mountain Project. Before 1990, he managed the Mechanical Engineering Department at Livermore, and he has a long, illustrious history before that.

Bill?

DR. SIMECKA: Thank you, Don.

Our lapel mike isn't working up there, so we're going to have to have the speakers stand up here.

Okay, if I could have the first slide?

(Pause.)

DR. SIMECKA: My discussion this morning is with regard to the decision strategy on thermal loading, and before I talk about the strategy, of course, I thought it was important to indicate what we believe our goal is in working towards a final thermal-loading decision.

And the goal, of course, is to develop a Civilian Radioactive Waste Management System--that's CRWMS--in which all system elements contribute to meeting applicable regulatory requirements, and for the MGDS, that's both pre-closure and post-closure; and for MRS and transportation, those applicable regulatory requirements.

Now, from my view, the strategy is that we must use the thermal-loading decision to enhance the performance of the CRWMS by appropriate use of the repository waste heat. In my view, if we could use the heat to control the near-field environment, such that we can maximize the certainty as a function of time, that the containment in the waste package will be there; in other words, we want to contain the radionuclides as long as possible.

Now, there is a regulatory basis for thermal loading. I've just excerpted some of them here, but each of these say, of course, that the underground facility shall be designed so that the performance objectives will be met, taking into account the predicted thermal and thermomechanical behavior or response, and that the engineered barriers, in the last quote there on 60.133(h), says:

"Engineered barriers shall be designed to assist," and I'm going to modify that. From an engineer's viewpoint, the engineered barriers shall be designed to "--use the geological setting advantageously in meeting the performance objectives for the period following permanent closure."

In other words, if we can take advantage of the heat, we should do that, and it really is a combined effect of the engineered barrier performance and the geological barrier performance, or the natural barrier. That's important. In any event, the thermal loading is a key variable in the EBS performance.

Now, we recognize the importance of thermal loading. Dr. Langmuir just explained a number of them, but from our viewpoint, of course, it's important to concern ourselves with thermal loading at the earliest because it does affect the magnitude and the content of the site characterization program. At the cold or below boiling, we obviously have to investigate a considerably larger area of the repository block and its extended areas in order to make sure that we have fully characterized the repository block.

And, of course, the content of the site characterization program changes when we go from the below boiling up to the above boiling, because we must investigate the impact of the heat on the minerals, and the zeolites, et cetera, et cetera.

Secondly, it, of course, affects the material selection and design of the waste package. We certainly would want the different waste package, then, at the below boiling than we would at the extended dry, for example.

And it affects the repository design; not only the area, but also the emplacement drift diameter may be different and is likely to be different for the below boiling and the above boiling. The operation, obviously, is important, although even at the below boiling, it is rather an inhospitable environment for humans to be working. It gets even worse at the higher loadings that we're talking about. But in any event, those do have to be considered in the design. All of these, of course, affects the overall performance and licensability of the repository.

To accomplish the thermal-loading decision, of course, it requires the integration of all of these factors; site characterization, design, performance assessment, as well as we've got to take into account the multi-purpose canister that's now being pursued in Washington, trying to meet the 1998 deadline with a canister that would assist the utilities in handling their waste that they have to dispose of.

We're trying to do this, of course, through many things. We have an ongoing thermal-loading study, which will be talked about. We're doing modeling and code development extensively right now with many people. We are defining and getting ready to do laboratory and field testing necessary to help support the evaluation of our models. We are accomplishing performance calculations, developing models for those, also, and, of course, participating in the MPC design studies that are going on in Washington.

In my view, the major decision that we're facing is: Are we going to be above boiling or below boiling? Now, such a decision is going to require a lot of technical analysis, a lot of system trait studies, et cetera, et cetera, and that will be implemented carefully through our technical baseline control process, which takes into account all of these things to make a decision whether we should narrow some options or select the final range of thermal loading.

We, of course, would like to have that decision made as early as possible, because it has tremendous ramifications on what we do in site characterization, and the cost of the entire system.

The follow-on decisions that we visualize, of course, are to narrow the specific thermal loading range once we make the decision, and that is more pronounced at the above boiling than below boiling, because if we decided in our design process that we wanted to go above boiling, then we'd say, "Okay, do we stay at the SCP level of 57 kw/acre, or do we go to the extended dry?" And any decision we make there will be integrated with the testing that will be going on to make sure the decision is refined to the point where we select it at the place that gives us maximum performance.

And, in any event, we also will do confirmation testing to make sure that our decision is correct, and that it truly supports our license application.

What is our decision process? Well, our decision process is imbedded in a system engineering process, and I've laid out here my view of what the functional analysis results of the system engineering process are.

The first major function that we must perform is to control the system configuration in such a way that that system configuration hopefully will meet the requirements of license application, and we will have to iterate the subfunctions there over and over again until we do achieve what we need to achieve.

The guide for the control system configuration function, of course, is our regulations, our standards, our laws that we must meet, but the control system function is decomposed into design engineered system, characterize the site that's been selected, evaluate the integrated system; that is, the engineered system and the site in which it's sitting; and, finally, perform confirmation/operational testing.

Now, these design requirements are used to develop a conceptual design. In my mind, we have to develop a conceptual design in order to, let's say, fashion the site characterization program, and we may change that conceptual design as time goes on, but, in any event, we have to have something to guide us to determine what we must characterize.

But as those design results, the trait studies develop the conceptual design, those will be then imposed upon the site characterization program, which also is using the regulations to determine what they must characterize, so the two of those fashions what the site characterization program should be. Those results are fed back, may modify the design, and so forth, and all the time we are evaluating on a continuing basis through the performance assessment program how the system is performing at the defined conceptual level, and what characterization results we have at the time.

These keep operating until we feel like we've got a system configuration that will meet license application. We will then go to license application and confirm, through testing, and so forth, that the operations and that all of our test results are verified by the resulting confirmation testing.

Now, we have an intensive effort right now that is imbedded in three of those subfunctions; that is, the design engineered system, characterize site, and evaluate the integrated system, and that is, we're doing conceptual modeling and model development, code development, and out of those, right now we're looking at all the models, the state of the art models, and so forth, having many modelers work together to try to come to what we think is a reasonable set of models; not one specific model, but maybe a number of them that we believe will help us determine our final design or conceptual design.

But in order to validate or, let's say, evaluate those models, we must have test results, so we are developing the tests that are necessary, laboratory and field, to make sure that we are sure that the models are as good as we can possibly develop, and we feed those test results back in a refinement way, and we modify the models until we're sure that we have confidence that the tests and the predictions of the models are close enough.

At the same time, on the right side, the performance assessment people are also developing models and codes, some of which are similar to what is being developed over on the left, and they are defining what test requirements must be achieved in order to make sure that their models are predicting the performance of the system accurately enough.

As you know, there was a Phase I Thermal Study which was done at the system level, just recently completed last year, which essentially looked at all of the different thermal-loading options that, you know, the below boiling, the SCP above boiling, and the result of that showed that for all the thermal models, that the system could handle those, and so we've embarked on a, at the project level in '93, a MGDS thermal-loading study which will be discussed later.

And we had hoped, we had set a goal for ourselves at the end of this fiscal year. We would look and see whether we could narrow the options in any fashion, and we will try to do that, but, obviously, we don't have any test data and will not have any test data by that, I think, to validate such a narrowing of the option.

Then we will follow on after this year with continuing system studies that will look at all of the trade-offs, and so forth, as we go along. To support that, we have a set of, as I mentioned earlier, model and code development and evaluation, and some testing that we are conducting in unison, and as we go to the right, we will take the results of the tests, refine the models, and so forth.

In the site characterization block, we are doing both laboratory and field tests. We're talking about examining cores and blocks up to about a foot cubed in the laboratory to look at fracture density, et cetera, et cetera, and we are going to initiate next year a large block test at Fran Ridge to try to get more information about the performance of the rock under heat and what happens to the water movement, et cetera, et cetera.

Now, the large block tests will go on for 12 to 18 months, depending on the results, and then we're looking at probably 12 to 18 months hiatus, so to speak, until we get into the underground ESF at the MTL and start two kinds of tests; that is, we have an abbreviated in situ heater test, and then a more long-term test, and those will be discussed later.

But in all of this testing, we are feeding back the results, as soon as we have results that are useful, to the modelers in both the PA and the design area, to refine those models, and keep improving those as we go along, and hopefully, somewhere out in the future, we can optimize alternatives for the final study, and somewhere to the right, depending on--we're not schedule-driven, so we are scientifically being driven to decide the models, the tests show enough confidence that we can initiate license application design. And, as you can see, because of the heater test, we're talking about the '97-'98-'99 time frame.

Now, of course, we are cognizant of system-wide studies that are going on, and one, of course, I'm sure you have heard of is the MPC, of course. Now, the MPC is going through a feasibility study, conceptual design, actual design, and so forth, process now. We all know that if the

MPC turns out to be a multiple assembly canister of the 12 to 21 quantity variety, that that's probably not consistent with the cold or below boiling repository.

Now, the MPC is being developed for other reasons than site characterization. They're developing that to try to assist the utilities in disposing of their assemblies when they need to, and we recognize that money is being spent there so that if, indeed, we later say below boiling is the way to go, that we may not be able to use those canisters in the repository. So we may have to re-load those, so to speak, and recycle them to go get some more, so we recognize that that is a cost risk associated with moving ahead with the MPC.

But in any event, we will base our repository design on our performance attributes, but if we can't use them, we will have to re-load those at the repository.

Just in summary, the questions that we're addressing in this thermal-loading study activity is that we first want to see if we can demonstrate that the thermal option that we select will achieve post-closure performance; that is, release and containment limits, adequate multiple barriers.

Also, of course, will the thermal options meet pre-closure requirements associated with safety, environmental, and retrieval? And we're doing the efforts that I mentioned;

that is, looking at the analytical models that we need to predict the post-closure performance, using validated techniques, and, of course, looking at all of the coupled effects that are necessary, and we're looking at what test data is required to support all of those efforts.

I feel strongly that we shouldn't underrate the laboratory tests that we can do, and so we're trying to increase those, as well as the in situ tests if, indeed, they're useful.

And finally, of course, do we have a sufficient suitable site to emplace the waste, depending on which thermal-loading option we select?

The status is that we are now looking at a wide range of thermal loadings. We have not foreclosed on any, below boiling or above boiling. We're looking at the state-of-the-art models and evaluating the performance of each of those options as we go. Using the models, we've identified key hypotheses that must be tested and, therefore, finally, setting up a test program that will help us validate those models.

I've said that the thermal-loading decision requires an integration of all of these four items, which we are doing, and the following activities that I said we were using will be discussed by follow-on speakers: The thermal-loading study by Steve Saterlie; modeling and code development and laboratory and field testing will be done by Dave Stahl; performance calculations, tomorrow Jerry Boak will talk about those; and the MPC design studies, Tom Doering.

Any questions?

DR. CANTLON: John Cantlon.

As you look at arriving at a thermal strategy and the concurrent work on repository design, what waste package placement preserves the greatest option for adjusting the thermal strategy for the repository as new information comes in?

DR. SIMECKA: Well, you're talking about the emplacement technique. The drift emplacement, of course, looks much more flexible. Obviously, the hot repository regions, the vertical boreholes or any of the boreholes don't look promising at all, but in my mind, we could, even with below boiling, we could still go drift emplacement. But we are carrying on those studies to make sure that--and, hopefully, by the end of this year we ought to make, at least, that decision.

DR. SATERLIE: I think I might add to that that we do have emplacement in both system studies, so we are looking at that both ways.

DR. DOMENICO: Domenico, Board.

Bill, just one question. We hear a lot of rumors about premature decisions on formal strategy, but it's your position now that a decision on the thermal-loading strategy will be delayed until the models are complete, and field and laboratory studies are also completed? That is the position of DOE at this stage?

DR. SIMECKA: Let me say it this way: Technically, I think that is necessary, the approach that I've just outlined is necessary. We could always make the decision, based on cost and programmatic reasons, because we do believe that the below boiling will cost significantly more just because of the number of waste packages, and the handling, and the area that we have to characterize, as well as the area we have to excavate, et cetera, et cetera.

I don't believe that that will happen, but it could, and I don't speak for total DOE, but my view is we must proceed at this systematic process if we want to convince people that, technically, we have selected the right thermal-loading option.

DR. DOMENICO: I gather that means your answer is yes?

DR. SIMECKA: I don't preclude anything.

DR. CANTLON: Thank you, Bill.

We're right on schedule at this point. I'd like to continue with Steve Saterlie's presentation.

DR. LANGMUIR: Our next presenter is Steve Saterlie. He has a doctorate in physics from the University of Wyoming.

Over the last 12 years, he's worked for TRW Corporation, and the last nine months, an M&O contractor with TRW Environmental Safety Systems, supporting the DOE's Yucca Mountain Site Characterization Project.

Currently, he's the Mined Geologic Disposal System Thermal-Loading Study Manager, coordinating activities with several organizations, including the national laboratories.

Steve?

DR. SATERLIE: Thank you. I appreciate the opportunity to talk to you about our system study that's going on. Clearly, coming to a decision on thermal loading is a very complicated issue. It involves a wide variety of disciplines, and we feel that, based on that, we have chosen a systems engineering approach to examine many of those options and to evaluate different concepts.

I'm going to try to give you just a brief overview to amplify on some of the things Bill Simecka said, outline the process of the thermal-loading systems study and how it fits into the MGDS activities. I am going to explain the objectives of the system study, and the thermal-loading activities to date; give you an overview of the study approach; and summarize.

I won't dwell on this too much. Bill showed you a similar chart. This is just basically how the systems studies fit into the process, and, clearly, we start with the regulations, the requirements. Based on that, we then go into our analytic modelings and systems studies. This is a iterative process where we try to determine whether or not we have adequate models to demonstrate the waste containment. Then the design activities and the site characterization activities occur, and many of these activities continue all at the same time, feeding back data.

We continue with this process until we feel comfortable that we have adequate models to demonstrate that we can contain the waste, and that we have adequate site data to do that. At that point in time, we would go into the license application process.

Now, at any point in time, the performance confirmation is continuing, and if there should be some data that would impact this, then that would feed back into that process.

The objective, as Bill Simecka said, the thermal loading involves a wide range of activities, and through the systems study, we're going to be integrating many of those activities pertaining to the thermal-loading decision.

We're going to try to, as John indicated earlier in his introductory remarks, we want to try to identify what we mean by some of these hot, warm regions, too hot regions, and, hopefully, we can focus this thermal loading issue a little bit at the end of the present study.

We're going to try to provide some recommendations as to the range or ranges that we feel, at this point in time, are licensable. This will be a continuing process which will mature as the models mature, as the data matures, so that we can continue to build on this and provide a final thermal-loading recommendation down the road here several years from now.

Finally, one of the most important things that I hope to accomplish with this first year's study is to try to identify some of the work needed to resolve the significant uncertainties that do exist in this, and that involves both the testing and analysis areas.

The status of the efforts. Well, we have a number of things that we can build on. As Bill Simecka indicated, we had a systems study that was done. We have some throughput studies that we're building on. Based on this, the MGDS thermal-loading study has been approved and started. It's a systems analysis approach, and it involves a full range of M&O capabilities. We have the design people involved, the waste package people, the subsurface folks, and the performance assessment, to name a few.

It also involves the participation of the national laboratories. They are doing calculations for us in many areas, and this will be integrated in the studies, and we'll talk a little bit more about that.

Finally, other supporting studies are underway. There's the system-wide studies, and Bill Simecka showed that there are feeds from these studies and into these studies, so we'll be looking at the implications of the systems-wide areas; the architecture study, the MPC study.

Finally, there's a total systems performance assessment going on, and the information derived from that will be fed into this.

The analytic code assessment, Dave is going to talk a little bit more about that, and you're going to hear some more about that along the way. That's important to this whole process.

As I said, the Phase I thermal study indicated that a wide range of thermal options could be accommodated. The testing programs, we're going to hear more about those from various speakers in the next two days. This is critical down the road to getting the sufficient information for the thermal loading.

Finally, a short-term activity was done. We've re-looked at the thermal goals that are in the SCP, and I will talk more about that tomorrow, so you'll hear a little bit more about that.

The study approach. One of the things we plan to examine in detail is the pre-closure performance, such as safety and operability issues, and cost. We're going to be looking at some of these thermal calculations and look in detail at what we believe the post-closure performance predictions say about that.

From this, we're going to try to identify and address important uncertainties associated with waste isolation. The performance standards right now are being-- some of the regulations are being re-promulgated, and so we're going to do a parametric evaluation of this so that if these regulations come out differently, that we can, in fact, have some of the data to evaluate those.

As I said, we're going to incorporate input from the national laboratories to try to narrow the range of thermal loading, and provide recommendations.

This is just a schematic of the program, starting with various inputs from other studies, going through our requirements phase. The thermal performance objectives were evaluated based on the thermal goals. The "Assess Thermal Effects" is the thermal modeling, and then we're going to look at all those different areas to evaluate performance and document.

The feeds from this will go into future MDGS system studies, into testing and analysis activities, and into the system-wide issues.

In the development requirements and inputs that we've completed, the first activities involved there were with the waste package people, where they provided us a range of options to examine, and we went all the way over various capacities, from 2 to 21 PWR containers. We're going to be looking at several different concepts here, all the way from a single-wall waste package, to an MPC-type concept, and various thicknesses.

Now, these are going to be covered in more detail in the waste package performance allocation study that's also going on, and we won't talk too much about that, but those lifetimes and performance will be evaluated there.

Once that was done, then radiation calculations were performed so that we could determine what shielding might be necessary on a transporter, or what advantages might be gained from putting in vertical borehole versus in drift. The waste stream work being done in Vienna is feeding into this activity.

Then, next, the subsurface people did their work and provided us with some various generic designs based on these different waste package concepts. I might add that we're looking at three different emplacement modes and variations on those. We're looking at the vertical borehole, the in drift and the horizontal, and these are being done in more detail in the emplacement mode study that's also going on.

Finally, based on that, and the thermal goals reevaluation at various site parameters that were obtained from USGS and the RIB, the inputs have been developed.

The thermal calculations are about to start, and I want to indicate here that we are performing these calculations over a range of thermal loads, all the way from about 24 metric tons of uranium per acre, up to about 114 metric tons. So we're running over the whole range of thermal loadings here for this study.

The first set of these calculations that we're going to be performing is looking at the near-field effects, trying to determine what the effects might be on emplacement, how the different emplacement modes would look in the near term. Based on this, this input will go into some rock mechanics calculations to look at stability. Those concerns would be addressed. If there's rock stability issues, then that might impact the cost of the program, or possibly even containment, if there is an uplift that might damage the natural barrier, so we're concerned about that.

Finally, this data will be given back to the subsurface people, and they'll be looking at it in terms of other considerations, such as ventilation, retrievability, whether or not wheeled vehicles, tracked vehicles, or trains might have to be used, and the costs associated with those various options. Based on that, we hope to provide some sort of a recommendation as to what a practical upper limit might be.

Then we're going to be going into--and, actually, this information has already started here--we're going to be looking at some far-field, long-term type of effects. This primarily is oriented towards the post-closure issues, and we're going to be comparing this against some of the thermal goals to grade the performance, if you will.

This data will then be given to one of the laboratories to look at the geochemical aspects. We're going to take some of the data that we have available now from various borehole evaluations, and other evaluations to try to assess the changes due to the temperature increases, and whether or not there's anything that is of concern there.

Finally, we're going to take all of this data together and try to evaluate the performance against the thermal goals, and we'll talk a little bit more about what these thermal goals are tomorrow.

One of the things that I hope to get from this study is what are the additional needs? We're going to try to perform sensitivity analysis to evaluate the options and identify risks. The sensitivities may be different at the different thermal-loading options, and we need to look at those and try to determine how sensitive some of these parameters are; the permutivities, permeabilities, various things. How accurate do we have to get data to be able to pin those numbers down so that we can do an adequate job of prediction?

We expect that that will translate, then, into some recommendations on what test data is required, how accurate this test data needs to be, what additional analysis needs to be done, or possibly even what additional models need to be developed. We plan to integrate this throughout.

Finally, we're going to be working with the people in Vienna on the system-wide issues to try to identify those things that are important to the whole system.

In summary, what do we expect to accomplish? Well, we're going to provide input to integrate the activities to support the thermal-loading decision process. We hope to establish some balance to the problem, and this will be a continually maturing process as it goes along, but we hope to recommend at each phase of this the range or ranges that we currently believe would be licensable, and our opinions on that will change, I'm sure, as data comes in and modeling capability matures.

We hope to identify the uncertainties, as I said. We'll provide a reassessment of the thermal goals, and identify system-wide issues.

All right. Where are we going to go from here? You're going to hear more about this in the next couple of days, but we hope to coordinate all of these activities in these design areas with the testing activities to ensure that the desired data is achieved. We hope to develop approaches that will reduce these uncertainties, and we're going to update this analysis in the system studies as more data becomes available.

Finally, this is consistent with the phased design approach that I think DOE is pursuing. The decision process is going to require several years to come to a thermal-loading decision before we're comfortable with the modeling and the test data that's coming in, that we can, in fact, come to a thermal-loading decision. We're going to use the system studies to provide a framework to reach that decision.

That's all I have. Any questions?

DR. VERINK: Verink from the Board.

Perhaps this is going to come tomorrow, but must a successful model be able to handle both hot and cold conditions in the long run?

DR. SATERLIE: I'm sorry; could you repeat that?

DR. VERINK: Must the successful model that you're going to come up with in the long run be able to accommodate both high temperatures and low temperatures in the long run?

DR. SATERLIE: Well, yes. What we need to do is convince ourselves with these models, and with the data that will become available, that we, in fact, understand the processes in the mountain, and that we can optimize the system for, essentially, the optimum waste containment.

And so, to do that, in my mind, we're going to have to evaluate. We're going to have to have models that will evaluate both ends of the spectrum.

DR. CORDING: Ed Cording; Board.

You indicated the assessment of this is going to take several years, so are you concluding that prior to two or three years from now, that you are going to be carrying more than one thermal option forward; is that correct?

DR. SATERLIE: Yeah.

DR. CORDING: In terms of the planning, say, both a hot and a cold option?

DR. SATERLIE: Well, I envision that we'll probably end up narrowing the ranges, and there may end up being a couple of ranges that we feel are licensable and optimum as we go along, and I anticipate that this will continue to narrow and that we'll probably end up with an option with a couple of alternatives, possibly. I'm not sure. As we mature, we will...

DR. CORDING: But the intent is to continue with more than one option for some period?

DR. SATERLIE: Yes.

DR. CORDING: And the possibility--when you say narrowing the ranges, particularly, that you have, as you say, separate ranges, like a cold or cooler range, and a hotter range; is that correct?

DR. SATERLIE: Yes, that's very possible.

DR. CANTLON: Cantlon; Board.

Let me follow up on that. Is it conceivable that you could get to approval to construct before you've actually settled on a final thermal design, so that you're essentially building a universal repository, one that could go in either direction because you've got a flexible management system for regulating thermal. Is it conceivable?

DR. SATERLIE: I suppose it is conceivable. Bill Simecka touched a little bit on that, that there may be some decisions along the way that, based on cost and other issues, that we may decide to concentrate on a particular design. However, you know, I want to make it clear--and I think Bill's chart indicated that as well--that if, during the performance confirmation process, that we come up with some data that says that we can't do that particular thing, then I think we're certainly willing to drop back, you know. There are certainly costs going to be incurred.

MR. GERTZ: John, this is Carl Gertz. Let me answer that from a project manager's perspective.

I think, as I understand the licensing process, if we were going to have an option for different thermal loadings, we'd have to assure that each of those thermal loadings met the full regulatory requirements, and then we could adjust. But we have to prove each one, or else we would not be allowed to construct.

On the other hand, if we choose one, and as our confirmatory testing goes on, and even though we're licensed, we determine there may be something that's a more systems-wide a better option, then we can go for a license amendment, but, of course, those are always difficult, as people who understand licensing deal with every day.

DR. SATERLIE: Yes. We have to definitely be in a position where we feel that we can demonstrate the performance with the data available before we would go for that.

DR. LANGMUIR: We still have some time, and if there are no further questions for Steve Saterlie, I was reminded that there were a number of remaining questions from Board and staff members for Bill Simecka. If Bill would be willing to respond at this point on his topic, Board and staff questions?

DR. DI BELLA: Carl Di Bella, Board staff. This is a question for Steve Saterlie.

Steve, I didn't hear you mention fuel age or closure time as variables in your studies. I suspect they are. Could you confirm that they are and what their ages are that you're using?

DR. SATERLIE: Yes, Carl. I think what we're using is, we've looked at the various fuel streams, and we've selected an option that we believe has some conservatism at this point. It's a youngest fuel first, with a minimum of ten years, and the average age of that is somewhere around 22-23-year-old, but it does have higher burnup. It's in a 38 to 42, depending on if you're talking various components.

DR. DI BELLA: My question was, are you using a range and, if so, what is that range?

DR. SATERLIE: Okay, I'm sorry. That was the average fuel, and we are looking at fuel variability, so we are looking at the variability on a year-to-year basis as well; in other words, trying to determine whether or not this variability is going to result in possibly cold spots, depending on how you emplace it.

DR. DI BELLA: Your answer so far is you're not using a range; is that correct?

DR. SATERLIE: I'm sorry, I guess I'm not understanding your question.

DR. DI BELLA: Like 30 years of old fuel on the average, or 60 years old fuel at emplacement.

DR. SATERLIE: Oh, I see what you're saying: Am I looking at different ages of fuel?

No. Basically, we're looking at one average and the variability about that average.

DR. DI BELLA: Okay, and about the closure time, are you looking at a range of closure times? I assume closure time is synonymous with backfilling time?

DR. SATERLIE: Okay. Yeah, depending on the concept, we may or may not have backfill. We are looking at somewhere in the 50 to 80-year range for closures. I think maybe we need to talk about that in a little bit more detail.

DR. BARNARD: Bill Barnard, Board staff.

I'm quite confused about what decisions are going to be made when. Bill Simecka, in your presentation, you indicated the major decision about above or below boiling was needed as early as possible, and in your thermal-loading interaction graph you indicated a narrowing of options in probably around January of '94.

In Steve Saterlie's presentation, you indicate that in FY93 you're going to be narrowing the range of thermal-loading options. Do these narrowing of options indicate a decision on above or below boiling?

DR. SIMECKA: Maybe not. I think in my talk I showed you that we had a milestone to narrow the options, but I indicated, also, that I wasn't confident we would be able to do that. But narrowing the options could be that we say, "Well, we want to look at, instead of looking at the baseline in the middle there, we may say we want to look at the extended dry and the below boiling as two options." That would be narrowing the options.

But, in any event, I don't feel comfortable in making that decision until the analytical work and the test work indicates that we are able to make the decision, and I can't predict exactly when that's going to happen, but I'd like to have it as soon as possible, because we can start to converge this site characterization program. The earlier we make that decision, we can converge this program faster.

DR. REITER: Leon Reiter from the staff.

Bill, in looking at some preliminary documents that DOE put out, conversations with people in the program, one certainly got the impression that a decision had been reached to downplay the below boiling option and concentrate efforts, at least for now, on the above boiling option. Among the reasons cited was that, "Well, in the SCP, we're looking at above boiling."

And I'm not quite sure, is the below boiling at this point a full and equal partner in your considerations?

DR. SIMECKA: I think so, because I think we'll be challenged by the number of people, including, obviously, people on this Board, that if we were to preclude, or just say, "Hey, below boiling is not something we are going to pursue any further," and the basis for that, in my view, would be that it would certainly be a smaller area if we go above boiling, a smaller area to characterize, and as far as the design is concerned, and the construction of the repository, it'd be less, et cetera, so there's some compelling cost reasons to go to the higher thermal-loading options.

But, in my mind, we have to have a scientific basis to justify that we don't believe the cold or below boiling is the preferred way, because I believe maybe both are acceptable, but to prove that is going to require considerable scientific evidence.

DR. REITER: So, in other words, if it's a full and equal partner, can we anticipate that in the performance assessment studies for thermal loading you will include below boiling as an option to be examined, along with other studies?

DR. SIMECKA: Absolutely.

DR. REITER: Along with other strategies?

DR. SIMECKA: Absolutely.

DR. REITER: I look forward to hearing that tomorrow.

DR. SIMECKA: Yeah. You can ask Jerry Boak that question, but from my understanding, performance assessment will be evaluating because, you know, we need the performance assessment to guide us as we go along. You know, the performance assessment results will be guiding us as to whether we now have enough confidence that one of the options can be put in a below preferred category.

DR. PRICE: Dennis Price, Board.

Dr. Simecka, I got the definition from you that an integrated system is the EBS plus the site, but I definitely got the feeling from Dr. Saterlie that the integrated system goes beyond the site in the mountain, and involves a lot more; including transportation and interim storage and other items that might be involved in deciding about thermal loading.

I'm a little confused about the decision process and how it fully involves that greater concept of an integrated system.

DR. SIMECKA: Well, from my viewpoint the repository must prove adequate performance in order to get licensed. The overall system, if you're trying to optimize the overall system, the CRWMS, obviously, you have to consider other factors because it could be that two approaches in the repository could be acceptable from a licensing standpoint. One of those may have a major cost impact, adverse cost impact or some other impact on the rest of the system.

So we can't do repository independent of, and ignore the total system factors. That's all I'm saying.

DR. SATERLIE: Maybe I could amplify on that, if you wouldn't mind.

An example of that might be if we had to go cold and we had to go to smaller waste packages, looking at the system implications, we may decide to still use the MPCs to transport from the utilities to the site, and then break it down into smaller amounts, but those aspects would have to be looked at. That might be how one of those decisions would be evolved.

DR. LANGMUIR: I think we need to go on here. We're a little behind schedule. We can return to questions later on, if time permits, near the break.

Our next speaker is David Stahl. He's currently employed by Babcock & Wilcox Fuel Company. They're responsible for waste package performance analysis. Before that, he was an employee of SAIC Corporation for four years, supported materials programs to the DOE's Yucca Mountain Site Characterization Project; and a long history before that.

Dave? His presentation is titled: "Thermal-Loading Testing Needs and Test Plans."

DR. STAHL: Thank you, Don. Good morning, ladies and gentlemen. I'm pleased to give you an overview of DOE's project in regard to thermal-loading testing needs and plans.

The outline shows the content of the presentation. I'm going to begin with a chart showing the technical elements of thermal loading. It's basically a different cross-cut than was showed by Dr. Simecka. I'm going to identify these activities relevant to thermal loading, talk about current evaluation of the analytical model that's ongoing, and then get into some of the laboratory, field, and in situ studies that are underway and planned; also going to include analogue studies, because it's an important elements; and, lastly, summarize.

I'll start off with this chart. It shows the technical elements of thermal loading, and basically, as I mentioned, it's a different cross-cut of the chart that was shown by Dr. Simecka. I'm not showing a function of time, but showing the principal technical elements, beginning clockwise from modeling, testing, design and operations, performance assessment, and natural analogues. As I tell my students in my radioactive waste management class, this will be on the final, so you better know this.

We will be hearing from various speakers in the course of the next two days dealing with these issues. Certainly, in the modeling area, the first bullet identified there is coupled processes. We're going to hear a lot about the hydrothermal processes by many speakers, and a little bit about some geochemical interactions.

As far as testing, in the next box, we're certainly going to hear about the laboratory tests. We're going to hear a presentation by Dan McCright on corrosion. We're going to hear about the large block tests and the in situ heater tests by Dale Wilder, and also, the thermomechanical tests from John Pott from Sandia Lab.

As far as design and operations are concerned, we're going to hear, certainly, about the waste package designs, particularly as it relates to the multi-purpose canister that you've heard about. Tom Doering will make that presentation. There will also be a presentation in regard to the repository design. I believe Kal Bhattacharyya will be giving that. And, of course, some engineering integration work by Bob Sandifer.

In the performance assessment area, we will be hearing many talks tomorrow, I believe, in regard to the total system performance assessment work. The SCP thermal goal assessment will be addressed by Steve Saterlie, and, of course, we have some subsystem analyses as well. I'll address some of the issues in regard to the performance assessment model evaluation that ties some of this together.

We'll also hear some talks in regard to natural analogues. For example, Dave Bish and others will talk about some of the work there.

One of the things I wanted to point out on the chart is the fact that these elements lead to the thermal-loading analysis, and they're integrated and coupled, using the system studies and the system engineering approach that was talked about by Steve Saterlie and by Dr. Simecka.

Now, we've had various task forces that were put together to address some of these issues. We had one task force identified activities in FY93 and beyond that could narrow the range of thermal loads, and these are some of the issues that that particular task force evaluated. You can see them; I won't read them, but we felt this was all-inclusive of the issues that we needed to consider.

At the same time, we had a heater duration task force that evaluated the test requirements that would satisfactorily evaluate those coupled processes in situ as well as in prototypic locations, and several were analyzed during that task force effort.

As you can see, the task force was composed of representatives from the national laboratories, and the management and operating contractor. The bottom line on those activities is that we identified the modeling and the testing that was needed to support a thermal-loading decision.

More recently, a task force was established to evaluate the applicability of the multi-phase hydrothermal codes; for example, the V-TOUGH code that is extensively being developed by Lawrence Livermore from the initial Berkeley model. And you can see the representatives. Basically, many of the same people were involved in that evaluation.

The objectives, as they identified them here, we wanted to look at those model and code conceptualizations and compare them with other models. We wanted to be able to review those results and, if possible, develop explanations for those differences, and, hopefully, reach consensus.

As you can see, this is the current status. The M&O has supplied some reference input information. Livermore provided the code assumptions, and the user information for V-TOUGH. USGS has provided some geologic data, which has been evaluated, and currently, the task force is reviewing those calculational results. They just had a meeting on the subject last week.

I'd like to move on now to the laboratory studies that support thermal loading. The first group is the small block tests at Lawrence Livermore, and this is a subset of the work that we will be doing at Fran Ridge. As part of generating the large block for the test, we will be selecting some small blocks to evaluate some of the rock properties that we've identified here, and also to be able to look at some sub-model validation as well.

At the same time, we'd like to do rock thermomechanical evaluations both at Lawrence Livermore and Sandia, and, basically, we're going to start from some of the existing block stability codes that are available, use them to quantify the testing, and then we'll be analyzing the properties of the rock, and later we'll be determining rock strength as a function of temperature.

There's a whole host of other laboratory tests that are going on that support thermal loading. We will be hearing, as I mentioned, about corrosion testing. There's some waste form work and Carbon-14 release evaluations that we won't be discussing. It was the subject of another Board meeting. We will hear a little bit about geochemical and mineralogical evaluations. There are some tests going on at Lawrence Livermore in a core flow-through experiment. That's an integrated test that is going on in the lab that's not covered in this particular meeting.

I'd like to move on to the large block tests at Fran Ridge. As I note here, these are the major objectives. We want to evaluate those coupled processes in a large block of tuff. As I mentioned, we'll be examining small blocks, then we'll have the large block tests, and then, of course, the in situ tests, and, hopefully, this scaling will give us greater confidence in our ability to model the coupled processes.

As I indicate here, we want to compare the pre-test and the post-test code calculations, and also provide an early evaluation of the equipment and instrumentation that we could use later in the in situ tests.

The status is as indicated here. The study plan revision is underway. A scientific investigation plan has been written and is being reviewed. We have gone out into the field and selected a rock outcropping at Fran Ridge, and you'll hear more about this later, and we've initiated the fracture mapping of that site.

The job package for the site preparation has been initiated. The test-frame design is complete, and we've initiated the bid process. As indicated here, our scheduled goal is to initiate thermal testing in mid-1994, and that was shown on Dr. Simecka's chart.

The in situ tests basically have the same objectives. There's a large block test, as I mentioned, but in greater scale. We want to look at the response of Yucca Mountain to that emplaced heat, do the same evaluation of the coupled processes and the calculations, and to confirm the analytical models, again, on a larger scale, larger block of rock.

The status, as indicated here, is a study plan has been written and it's under internal review. Scoping calculations have been performed, and you'll hear a little bit about those, and I wanted to emphasize that we do plan both short-term and long-term tests to confirm the model predictions, and Bill Simecka addressed that issue. I'll talk a little bit more about it on the next chart.

Our scheduled goal there is to begin the abbreviated heater test in June of 1996.

This is the schedule. As we talked about earlier, we do have the large block tests starting in mid-'94, and heating for about a year, and then cooling down and doing some concurrent analyses. Those will lead to the initiation of the ESF in situ heater tests, so there will be, as I said, lessons learned from the large block tests that we could take advantage of in the ESF heater tests.

As we mentioned, we'd like to start that in mid-'96. We will be starting the two tests, as shown here; the abbreviated LA test, license application test, and the cool-down test. As you can see, the heating period is roughly about the same, talking about 18 months to, perhaps, 24 months of heating. In the cool-down tests, we have a much slower cool-down. In the abbreviated tests, we have a more rapid cool-down so that we can get some early results that would feed license application.

One of the things that Carl Gertz had mentioned in his chart was the fact that with a reduced budget case, this will impact the schedule for the start of the main test level work and, hence, could impact the start of the in situ heater tests. So we're hoping that additional monies will be available to maintain this schedule.

As I mentioned also, that thermomechanical testing in the mountain will be going on concurrently with the in situ heater tests. The major objectives here, as I indicate, is to determine the rock mass response to the thermal load, and determine the stability of the rock openings.

The study plans have been written and approved, and we're currently doing some scoping calculations. As I've mentioned, you'll hear more of that from John Pott. Our schedule goal is to begin those tests in late 1996, so, as I said, it will just follow on behind the start of the in situ heater tests.

The last subject I want to cover is the natural analogues. We do have an interaction and agreement with the New Zealand folk to use a geothermal site there. As indicated here, one of those objectives is to be able to evaluate real sites with active hydrogeological processes going on. Thus, it would enable us to evaluate codes and models to natural occurrences. Another interesting part about this site is that they do have various man-made materials that they've used to reclaim or recover energy from the geothermal field, so we'll be able to look at some of those long-term effects on man-made materials.

The status, as I mention here, is an agreement is in place to study those fields, and the design and studies of those experiments are underway. The schedule goal, as I indicate, is to initiate phase one this year, and that phase deals with the observations of the mineral assemblages, and we'll hopefully be able to compare with some of the predicted analyses.

Now, there are other natural analogues out there. There are many other geothermal systems that could be used as natural analogues. You'll hear a lot more of those this morning and later on today.

It's also been suggested that Yucca Mountain could be used as a natural analogue, because a hydrothermal system existed there about 11 million years ago. Topopah Spring member may be an appropriate, and you'll hear more about that from Dave Bish. An outcrop evaluation is planned for that work.

Okay, in summary--and here's the test--I have indicated the various elements of thermal loading, and, hopefully, you'll be able to recall the various speakers that will come after me who will address each of these issues, and, hopefully, we'll be able to tie these together to the thermal-loading analyses that we show here within the framework of the systems studies, using the four major elements, as well as natural analogues to help us reach a decision.

Thank you.

DR. LANGMUIR: Questions for Dave?

DR. DOMENICO: Domenico.

David, are the plans in order and all ready to go for the large block tests at Fran Ridge? Are those plans in place? Do you know what you're going to measure, and how you're going to measure it?

DR. STAHL: Yes. We have a scientific investigation plan and we're preparing study plans right now to provide additional detail on those.

DR. DOMENICO: And my other question, I can see some of the parameters that you're going to go after for normal--the anticipation of heat loads, but the coupled processes sort of confuse me in the sense that that's very difficult to understand and to model and to observe and test; for example, the mobilization of silica, which a lot of people are worried about, which has some affect.

I'm trying to put this in a question, and I think the question is as follows: Can we fully understand some of those processes, because perhaps we're not as worried about what some of the parameters might be of the rock in response to temperature rise. Perhaps a lot of us are far more interested in the condition of that rock after it is heated, and largely because of hydrochemical effects.

Can you tell us something about that program?

DR. STAHL: Of course. In the experiments that we talked about, like the large block test, we'll be analyzing the block after the test. The objective is not only to evaluate the models, at least parts of the models with each kind of test, but to look at the results of the hydrothermal movement, so that's a very important element in those tests.

Did that answer your question?

DR. DOMENICO: Yes. Thank you.

DR. LANGMUIR: Questions from the Board staff?

DR. BARNARD: Bill Barnard, Board staff.

David, you described in great detail your in situ heater and thermomechanical tests. Do you plan on doing any in situ testing of waste package materials?

DR. STAHL: Yes. It's actually in both tests. In the large block test, we will have some materials and I don't know if Dan McCright is going to be covering that, but we do have either coupons or materials of construction in the large block tests that will utilize the materials that we're currently considering for the waste package materials themselves.

In the in situ heater tests, we plan to make the outer barrier of the heaters of the same materials as those being considered for the materials of the waste packages, so there will be consistency in the materials evaluation as well.

DR. BARNARD: Do you have any plans for using spent fuel?

DR. STAHL: Not in the current in situ test. That would come in later on, perhaps in confirmation testing, where we'll be evaluating real waste packages.

DR. LANGMUIR: Leon Reiter?

DR. REITER: Leon Reiter, staff.

Dave, still a follow-up to the question I was asking Bill; I guess the question about the below boiling scenario. What are the key questions associated, key scientific questions associated with below boiling, and what kind of tests are you planning to address those questions?

DR. STAHL: Well, there's some similar tests in regard to the chemistry of the water that contacts the package. This is important in trying to determine the corrosion processes and the rates, and in either the hot or the cold scenario, you will eventually have a potential for water contacting the packages, so we need to understand what water can come back, and the chemistry of that water. So there is consistency in both of those scenarios.

DR. REITER: So that's the key question associated with viability of a below boiling scenario, the chemistry of the water?

DR. STAHL: Yes, in my view; and the amount of water, certainly.

DR. LANGMUIR: Don Langmuir, Board. I'd like to follow that one up.

How do you physically sample the water from a block test? How are you going to get it out of there without changing its chemistry? You really want to know what its chemistry is at temperature. That's going to be a tough one.

DR. STAHL: That is a difficult one. I think I'll leave that one for Dale Wilder or some of the geochemists to respond to.

DR. WILDER: Dale Wilder.

I'll try to respond in two ways. Number one, we recognized it was going to be a very difficult task, and we're looking at options of using doped fiberoptics [selected chemicals on tip of fiber to react with anticipated chemistry], if they will survive the temperatures. We have a system in which we're going to try to take samples without pulling too much of the water out, so that we don't change the test. That's yet to be determined; and the other is, we're going to rely very heavily on looking at post-test evaluations of the mineralogy.

It's a major problem to us, and we are currently going through the studies to determine how we can do that geochemical sampling. For that reason, we also have designed the small block test, and we feel that they're very critical, because we will not be able to control everything we need to in the large block test.

DR. LANGMUIR: Dale, it would seem to me that even batch testing of those same rock materials at temperature will give you rather similar chemical information, and easier to sample the fluids.

DR. WILDER: Yes, and that is certainly part of the intention, also.

I might mention, as a follow-up to a question that Leon had--I believe it was Leon--had asked about some of the concerns over changes in the hydrologic properties and the geochemistry. We do have the large block test currently designed to where we will maintain a refluxing zone, so that we can look at things like fractures. I guess it was Pat Domenico.

Those are all issues that we're trying to get a balance between laboratory studies and the large block test, and the details are not totally worked, but certainly are issues that I think are of major concern to us.

DR. LANGMUIR: We're right on schedule now. If possible, I'd like to go to our break, and there will be plenty of time during the day, I hope, especially at the last part of the meeting in the panels to bring up further questions and issues.

Let's reassemble at ten-fifteen.

(Whereupon, a brief recess was taken.)

DR. LANGMUIR: The first speaker of the next session is Bo Bodvarsson.

Mr. Bodvarsson has a bachelor's degree in mathematics and physics; also, a masters degree in civil engineering, and a Ph.D. in hydrology. He has worked as a staff scientist at LBL for the last 12 years, and is currently the Technical Project Manager for Nuclear Waste Studies at LBL.

We're going to hear from speakers now, starting with Bo, who have studied the characteristics and behavior of past and present geothermal systems, including the historic system in Yucca Mountain itself, which can be considered analogues for the Yucca Mountain repository.

Bo's topic is: "Geothermal Systems as Analogues to Yucca Mountain, with Emphasis on Hydrologic Aspects."

DR. BODVARSSON: Thank you, Don, for your introduction. My name is Bo Bodvarsson from Lawrence Berkeley Lab. I'm going to be talking about geothermal systems as analogues to Yucca Mountain, with emphasis on some of the hydrological features, because the subsequent speakers will talk some about the geochemistry and the rock properties and other things.

The outline is shown here. I'm going to have a little slide show, showing you basically some of the geothermal systems around the world for about five minutes. Then I'm going to give you some classifications, some conceptual models of geothermal systems, talk about the hydrological and thermal aspects of them, and then mainly emphasize the vapor-dominated systems, because they are the most appropriate analogues to Yucca Mountain. I'm going to talk about heat transfer in lava flows, and then implications for Yucca Mountain; and, finally, talk about possible geothermal analogue studies.

So, if you can turn on the slides, I want to run through the slides really quickly. This slide shows some of the geothermal systems around the world. I cannot talk about that one no longer.

(Laughter.)

DR. BODVARSSON: Okay, I guess I can't talk about that one. I'm trying to go back, but it's not cooperating. Can you put the first slide on again, Mike, and can you hold it in place?

(Laughter.)

DR. BODVARSSON: I guess my time is up.

You see that most geothermal systems are located close to the plate boundaries. The main areas where geothermal activities are in California and Nevada in the United States. You have a lot of it in Italy, in Africa, here in Ethiopia, in Kenya, in Iceland, in New Zealand, of course, and in the Philippines and Japan. Those are the main areas.

I'm going to show you a few slides. This is Old Faithful. That shows one of the famous geysers, and as you probably know, geysers is an Icelandic word for a hot spring, and that has been adopted in the English language.

This is from New Zealand, another big hot spring.

This one happens to be in El Salvador, in Middle America. They have a lot of geothermal activity. It is down at El Salvador. This is of the geysers. This is the biggest geothermal field in the world. This is close to Santa Rosa, north of San Francisco, where they have about 1500 mega watts power conducted from geothermal.

All of these pictures I've shown you are now before development, so what I'm going to do, I'm going to go around the world a little bit and show you some of the fields after development has occurred. This happens to be in Adis Ababa in Ethiopa, and I apologize for the quality of these slides. I took most of these slides myself, so they're not very good. This is the Revolution Square in Adis.

This figure shows the rift valley in Africa, and they have a beautiful field of systems in Ethiopa called Aluto Langano. This is like a top of a volcano, like a beautiful golf course there.

This is Iceland, and there's a lot of geothermal activity there, with real volcanic soil going through the country. Eighty per cent of Iceland is heated by geothermal, and they also produce electricity from geothermal.

This shows one of the geothermal fields in Iceland, called Nesjavellir Field, and you see the massive fracturing on this basaltic, young basaltic rocks. This is another picture of it. This is a 200 mega watt space-heating plants.

You see the wells. And one more. This is in the wintertime in Iceland. You see the blowing wells; same view.

This one was taken out of a car in the Philippines. This guy is riding his buffalo to work. This is Palinpinon in the Philippines. You see the very steep terrain, so it's very difficult to drill wells in this kind of terrain, as you can understand.

This is in Kenya, and these are Mathias in Kenya. This is a beautiful field called Olkaria that I will talk a little bit about later, and it's a beautiful field because the animals are right beside the wells. The giraffes and gazelles enjoy going there a lot. This is a powerplant, a 45 mega watt powerplant in Olkaria, Kenya.

This is Wairakei, New Zealand. It's where there is 180 mega watt development and has been over 30 years; and finally, this is the geysers. This is in northern California, and, again, notice the very steep terrain and most of the wells are drilled directionally because it's very hard to build a platform there. I'll be talking mostly about the system like the geysers, which is a vapor-dominated system.

Back to view graphs. A brief description of a geothermal system, this comes from a classic paper by White, et al. In order to have a geothermal system, you need a heat source. You need the permeable rocks, you need heat transfer, you need a caprock, and you usually see some manifestations at the surface.

I'm going to talk now about the classification and conceptual model of some geothermal systems. We classify them according to different criteria. One is according to temperature. The higher the temperature, the better the use of that resource for electric or power production. We classify according to the phase composition. If you have low temperature, you generally have single-phase water. That means it's a liquid-dominated reservoir, and then you can have, if you have a high temperature reservoir, you're going to have a two-phase liquid-dominated, which means the pressure is hydrostatic, or you can have a two-phase vapor-dominated, which means that the pressure in the reservoir vapor static. There is almost no pressure change in the reservoir.

Flow classification. Most of the geothermal reservoirs are fractured rocks. There are a few of them in Imperial Valley that's a porous medium, and some in Nevada that are associated with single faults. So these are the basic classifications of geothermal reservoirs.

Now, the ones which are most analogous to Yucca Mountain are the ones I call vapor-dominated systems, and why is that? That's because the gas pressure is the dominating pressure of the phase in the reservoir, like in Yucca

Mountain. The pressure in the gas phase is one bar; right?

The analogues are this: We have like active geysers, we have a fractured porous medium with large faults, same as Yucca Mountain. We have small fracture spacing, and large fracture permeabilities, on the order of Darcies, like Yucca Mountain. We have small matrix permeability, micro Darcies; strong capillary pressures. Fracture pressures are gas static. Water is stored in the matrix blocks, like Yucca Mountain, and we have a heat source like we will have at Yucca Mountain when we put the repository in. It's very much an analogy to Yucca Mountain.

I want to talk briefly about what have we learned from geothermal systems? What is the heat transfer like, and what is the hydrology like in geothermal systems?

As we all know, heat transfer mechanisms are conduction, convection, and what we label heat pipes. Heat pipes is a phenomenon where there is counterflow of liquid and gas or vapor that allows you to transfer a tremendous amount of heat through the system, without a large temperature gradient. I will show you that heat pipes are the preferred heat transfer mechanism in geothermal systems, and this has very important implications for Yucca Mountain.

Of course, if you have single-phase systems, like liquid water systems, where the temperature is too low for boiling to occur, the main heat transfer mechanism is convection in the liquid phase. But when you have two-phase systems, the dominant heat transfer mechanism is the heat pipes, but in some cases, we have found in some deep vapor-dominated systems, there are conduction-dominated zones, or at least there seem to be.

For those that are not familiar with the concept of heat pipes, I want to describe it very briefly here. It's a very important concept, because it's very important for geothermal systems. It's also very important for Yucca Mountain, because the temperatures that are going to occur close to the canisters at Yucca Mountain depends strongly on if a heat pipe will develop or it won't develop; very important.

So what this just generally shows it that if you have a constant heat flow through some medium--and this happened to be porous medium--what happens is you have steam rising and condensing here, giving off the latent heat, and then water dripping down, and because the latent heat of water is very large, this is a very efficient heat transfer mechanism. You can carry a lot of heat in a heat pipe with a very small temperature difference.

I want to go through, very briefly, and show you a simple model, and show you how the heat transfer will occur in a simple model. This model is an idealized geothermal reservoir here, with a heat flux isolated over the 500 meter interval here like a pike or a magma body, in some sense, with a caprock and a constant temperature on top. This is a typical situation we see in geothermal, of a localized resource, and what we are interested in finding out is what kind of heat transfer will occur to transfer this energy through the system. In the caprock, we have conduction. Here we have some energy, and we want to find out what happens inside.

This is a slide where we have initial gas saturation of 25 per cent, and what this shows is that you develop a vapor-dominated zone on the top, with a vapor-dominated heat pipe, and below it you have a liquid-dominated zone. This is what the system prefers to behave. It wants to have a heat pipe, because that's the most effective heat transfer mechanism, but it cannot have a heat pipe over the entire region because the mass emplaces too much.

So if you look at the heat transfer in the system, you see that you have large-scale water convection in the liquid zone carrying the heat, and then you have a heat pipe in the vapor-dominated zone.

If you reduce the amount of water in place in a system, you get a similar thing, but not quite. Again, the vapor-dominated zone on top, heat pipe, with a temperature of 240☜; very low temperature gradient, because the heat pipe is such an efficient mechanism of transporting the heat.

Below it now, you have a smaller liquid body, and if you know your heat transfer, you know that when you have an aspect ratio such as this, the convection is not very efficient. You develop a convection cell which is too small, and it cannot carry the entire amount of heat, so you get much higher temperature here, and conduction carries the rest over here.

If you reduce the permeability in this type of system--and, again, you back to the case one where we have a larger amount of water in place--you will find that because of the low permeability--this is now 100 times lower permeability than we had before--the convection is not very efficient yet. The permeability is too low. So even if you have water convecting, the permeability and the water velocities are too low to allow you to get all the heat transfer through the liquid zone.

So what does it do? The system goes into a liquid-dominated heat pipe. So here we have a hydrostatic zone with a liquid-dominated heat pipe, overlaid by a vapor-dominated heat pipe.

This is also what we see in nature. Most of the low permeability hydrothermal systems develop two-phase zones; the liquid-dominated heat pipes and vapor-dominated heat pipes on top.

If you still reduce the amount of water in place, you get a vapor-dominated heat pipe everywhere, and the temperature is basically constant, at 240☜. All the energy is carried very efficiently in a vapor-dominated zone.

What does this tell us, then? Why does this have to do anything with Yucca Mountain? The reason is this: Let's take a look at a conceptual model on a vapor-dominated system. Now, this is based on field data. This is not simple model studies, but all the features you saw in the simple model studies, you see in this conceptual model.

This is The Geysers geothermal field, the largest one in the world. This scale that you don't see here is about five kilometers vertically, and about 20 kilometers horizontally. The wells--and there are 600 wells or more at The Geysers--all penetrate what we call the heat pipe vapor-dominated zone. This is where we get the steam out of the wells.

This zone has vapor-static pressure gradient. That means the pressure is uniform, of 35 bars; has a heat pipe carrying the heat through the system. All the water is in the matrix blocks and it boils off, and gets out of the matrix blocks into the fractures, and to the wells. They used to produce about 2,000 mega watts out of this system, which is enough for two million people. Now, the pressures are going down a little bit, so we only produce like 1500 mega watts.

What is interesting about this slide is that there is evidence for a hot dry zone underneath the heat pipe zone. Now, why is that interesting?

The reason that is interesting is that we are debating at Yucca Mountain if heat pipes will develop in the fractures so that temperatures will remain about 100☜, or if you can totally dry out the rocks around the canisters so that temperatures exceed 200☜, with large capillary pressure gradients towards the repository; very, very important question.

Here, we have exactly the analogue. We have the two situations, with the heat pipe, and a strong evidence for a zone where we actually managed to dry out the rock.

Now, you might ask, in this zone where the heat pipe occurs, is there a heat pipe in the fracture system itself, or does the vapor go up through the fractures, and the water goes through the matrix blocks? It's a very important question. Can the fractures provide you with a heat pipe without the matrix playing a major role?

If you look at data from The Geysers, you write down a simple Darcy's log times the latent heat--some of these slides are not in the order, and I apologize profusely for that. It's not because I was late with my presentation, I might add.

What this shows here, when you write down this equation and all we want to find out: Is Model A appropriate for The Geysers vapor-dominated systems, or is Model B appropriate?

Model A shows steam going up through the fractures and water going down through the matrix block, because water likes to be in matrix blocks where the capillary pressures are higher. Or is it a case where we have to have a heat pipe in the fractures?

When you go through the calculations, like I have gone through here, you find out that for Model A, you can never force all the water required to carry the energy through the system through this tight matrix block. It would require a tremendous pressure gradient in the matrix, which we don't see. What does that tell us? That tells us the heat pipes in vapor-dominated geothermal systems are in the fractures. They have to be in the fractures, and that's a very important conclusion, because that tells us perhaps at Yucca Mountain you would develop the same situation in the fractures.

How much heat can you carry in a heat pipe? That's a very good question, because you looked at the amount of heat flux through geothermal systems. You see they are very low. Our heat flow at Yucca Mountain is going to be much, much higher. When you go through the calculations--and this is the maximum heat flow in a heat pipe--you assume some relative permeability function, but it really doesn't depend so strongly on those. You'll find that you can carry much more heat through a heat pipe than the heat load we have assigned to Yucca Mountain, or are considering at Yucca Mountain, because heat pipes are so efficient at carrying energy.

Now, do these heat pipes then, if they occur in fractures, do they occur in all the small fractures and fissures so that we can take a block, we can heat it up, and we will see our heat pipes in that block? No.

The experience at least I have in vapor-dominated systems and liquid-dominated systems, in most all geothermal systems, the heat transfer is controlled by features hundreds of meters apart, on the order of 100 meters apart. Those are the major features in geothermal systems. Maybe the larger faults, we don't know exactly what these features are. Why do we think so?

We think so because when you drill through geysers, wells, and through the geysers, when you drill in almost any other geothermal systems, generally, the low circulation zone or the permeable zones are about 100 meters apart. At The Geysers, what we call steam entries are about 100 meters apart.

When we do modeling--and I've done modeling of the geysers and other geothermal systems over the last 10 to 15 years, along with my colleague, Karsten Pruess, you also find in order to match a history of pressures and flow rates, you have to have fracture spacings on the order of 100 meters apart, effective fracture spacings in the system.

Now, what have we concluded from this is that geothermal systems, high temperature like heat pipes, and the heat pipes seem to occur in the fractures. One example of this is kind of curious. How about lava flows, when you have heat on the surface? Can you put the lights on again, just for one minute?

I want to show you an example from Iceland, from an island called Westman Island in Iceland, where one early morning, about ten years ago, a volcano in the small island started erupting. All of a sudden, one morning, this volcano in the center of the island started to erupt. It is not good news if you live on the island, and this is a small island, and the Icelanders can take heat, but not so much heat as this, so what we had to do, we had to fly everybody out, on boats and whatever it took, one night for about ten hours. Five thousand people are all out by then.

You see the eruptions there, and then you don't see anything.

(Laughter.)

DR. BODVARSSON: See the town there, and the eruption over there? What is the worst part about this is that this is a fishing town, and the lava was going towards the inlet, the fjords where all the boats have to come in, and if that is cut off, that means you can't live there anymore, because they can only live on fishing.

Also, you see all this ash from the lava flow, which is going over all the houses, so let's take another, closer look. You see all the houses. They are buried under this ash, which is just not nice if you want to live there.

So what the Icelanders did, ingeniously, of course, like we always do, they saw the lava flow coming towards the fjords. They wanted to save the harbor, so what they started to do, they put water on the lava. They wanted to cool it down. They wanted to stop it from migrating, and that's why we have this data which I'm going to show you here.

They managed to stop it just before it hit the fjord, and actually, it provided more of a shield for the weather, so the harbor is much better now than it ever was before. Anyway, why am I talking about this, when we are so concerned with thermal loading? Because of this. It's a very interesting experience, experiment.

Here is our lava, and we put water on top of it. They also measured temperatures in this lava flow to see what the heat transfer looked like, and what did it look like? It looked like this. The water right away carried the heat from a thousands degrees up to the surface, a heat pipe right away formed, because the pressure is atmospheric, like it is at Yucca Mountain, temperature, 100☜; boiling temperature for one atmosphere of water.

The transition zone between the molten lava, 1000☜, and the heat pipe is a few centimeters of thickness, and it has to be so sharp, because conduction is such an inefficient heat transfer mechanism, so you have to have a huge temperature gradient to carry the heat across the boundary. Here you need none, because the heat pipe is so efficient. So, again, nature seems to be for heat pipes.

One more thing from our geothermal experience, that I think is also relevant, is that hydrothermal eruptions--and this is what Don alluded to in his presentation--many geothermal systems, you have hydrothermal eruptions, and when I talk about hydrothermal eruptions, it's not associated with magma. It's associated with two-phase effects; with gas and steam getting high pressures close to the surface because of high temperatures, to the extent that the pressure in that fluid phase is larger than the lithostatic load, so all of a sudden, boom, it blows up.

Why does it happen? It happens because you have like 35 to 40 bars, typically, in the vapor zone, in the gas zone in the geothermal systems. You might have an earthquake where fractures open up close to the surface. The gas, the vapor, moves up there, and now the pressure is really large, close to the surface; boom.

This is a paper by Bixley & Browne, 1988, summarizing some of the New Zealand experience in this area, and there are many other papers. Basically, what they say is that you have very large magnitude eruptions every few thousand years or so, typically, for all geothermal systems, where you have very large craters at the surface, you have smaller ones that may be going only a couple hundred meters below the surface, where the craters might only be 20 to 30 meters wide.

Now, if you look over the last five years, is this realistic, or does this happen? Ten years ago, TV, Philippines, eruptions killed three people, something like that. Two years ago, Guatemala, eruptions killed like two people. Three years ago, El Salvador, kills 18 people, hydrothermal eruption, the cause of this.

Implications for Yucca Mountain? There are basically two. This is an old slide. I don't need this slide, let's just talk about this slide.

With all this talk about the extended dry repository concept, possible failure modes, we talk about water flow in fractures and heat pipes may not allow you to get much above 100☜; again, a very important issue.

The second one, the issue about hydrothermal eruption. Can they occur at Yucca Mountain at all, or is it impossible?

Implications for thermal loading. My conclusion is, and I guess other people can conclude differently, is that heat pipes, geothermal experience suggests that heat pipes will develop in the fractures at Yucca Mountain. The temperatures may remain close to 100☜.

Geothermal experience also suggests that hydrothermal eruptions may conceivably occur at Yucca Mountain, and this is a schematic on that. I mean, this is not likely to occur at all, but this is something that we have to think about, too, because this happens in nature; is that if you have a hot repository where the heat, due to thermal expansion of the rock, closes off the fractures so that the permeability becomes very, very small, what happens is then if the permeability closes off and you have a finite amount of gas or air here, the air has to expand due to the temperature. So that raises gas pressures, and if you have a fault or something like that that goes close to the surface, there is a potential danger. Like I said in the last slide, maybe it's very small, but this is still a possibility.

Don also mentioned this--I'm about finished--that possible geothermal analogue studies. I think this is a very good idea, for the following reasons--these are, again, my opinions:

Maybe the only way to determine the likely heat transfer modes and thermal regime at Yucca Mountain, in my opinion, because perhaps the features that are going to control the temperature around the canister regions may be large-scale features, hundreds of feet or more apart. It certainly is going to help us understand two-phase volume fractures, and under what condition heat pipes develop, especially if we look at both the typical reservoir where the heat pipes are, and the hot dry zone deeper.

It also may help us understand the role of fracture fillings, fluid chemistry, matrix blocks, and I propose maybe we should think about a corehole at the most appropriate place, where we can drill a borehole or two or whatever where we look at what is going on in the typical reservoir and why we are getting a heat pipe there, and we also try to understand why we are not getting it here, in the deep, dry zone where the conduction seems to dominate.

Conclusions. Heat pipes are the preferred heat transfer mechanism in two-phase geothermal systems. Conduction-dominated zones may be present in deep vapor-dominated systems. Heat pipes seem to occur in preferential fracture/fault zones, about 100 meters apart. Heater tests will, therefore, not fully resolve this issue of likely thermal regimes. Of course, I'm all for heater tests, but it's not going to tell the whole story, in my view. Geothermal analogue studies may be essential.

That's all. Did I take too much time? Yeah. So if you have any questions, I'd be glad to answer them.

DR. LANGMUIR: Thank you, Bo, for a very stimulating talk. I'm going to take prerogative and ask the first question.

Of concern to me, you've shown the heat pipe effect as a critical one here. In your experience from looking at systems around the world, have you seen heat pipes effects close off their own fractures because of thermal expansion, and because of precipitation mineral phases? Have you seen this effect in such a way that you could predict it? This clearly is going to influence whether you have a heat pipe or not, isn't it? When you close off the fracture, no longer is the effect going to be there.

DR. BODVARSSON: You really want an answer. You're serious about this, huh?

(Laughter.

DR. BODVARSSON: I think that most all geothermal systems are self-sealed, so to speak. They seal themselves off. The caprock is there because of chemical sealing or some other factors.

Vapor-dominated systems are sealed in all directions, because you cannot have a system three kilometers thick, where the pressure is 35 bars uniform, if water outside it, and the pressure increases from 30 bars to hundreds of bars because of the hydrostatic head of water. If there was some permeability you would quench the geothermal systems. So vapor-dominated systems seal themselves in all directions.

So the facts that they like to do that, they like to seal themselves, they also like to break the seals, like with hydrothermal eruptions. They seal themselves and pressurize themselves, and once in awhile, they say, "Enough of that," you know, and there's an earthquake or something. Stuff gets close to the surface, you get a hydrothermal eruption.

So I think there is a lot of evidence that they can seal themselves up. Some geothermal systems are very hot, but they are practically impermeable. You drill into them, you don't get anything out.

Does that answer your question?

DR. LANGMUIR: What you're telling me is there is no answer; that anything can happen here. You could seal them off. They'll find a way to release. I guess what I was getting at was the likelihood that you'd be at 100☜ and no higher, because the fractures would maintain themselves in some way around Yucca Mountain, allowing the heat pipe effect to release the heat.

DR. BODVARSSON: See, like I said before, in two-phase geothermal systems where temperatures are high enough, you almost always have heat pipes. They almost always develop, so you'll find that it might be different in fractures, because some of them may seal off, but then you have new ones, so they almost always develop.

DR. ALLEN: Clarence Allen, Board member.

If, in geothermal areas around the world, the heat pipes are typically 100 meters apart, this would suggest to me that the preexisting fractures had very little control over them. There's something of a geomechanical, hydromechanical system that's driving this spacing and the preexisting fracture zone may not be an important element at all.

DR. BODVARSSON: You could be right. I mean, this 100 meters is kind of a ball park figure. Some cases, it might be 20 meters; other cases, it might be 200 meters, something like that, so they are not all uniform and 100 meters apart. What I'm trying to say is that it's not one meter, it's probably not 10 meters, and it's probably not a kilometer. So I'm not saying they're a uniform 100 meters apart. I'm saying that it's on the order of 10 to the second power.

DR. DOMENICO: Domenico, Board.

Bo, the large fractures in Yucca Mountain are not likely to be sealed by thermal expansion. They just don't get enough--the small ones may be. What is more likely is the walls of the fractures, all fractures, may be coated with moving silica and the permeability reduced to zero. Would that enhance the heat pipe effects of these throughgoing zones, if you basically cut off the permeability of the matrix itself?

DR. BODVARSSON: Like I was trying to say, is that I think our geothermal experience indicates that the heat pipes develop in the fractures, and probably the matrix are so impermeable that it cannot sustain those heat loads. With regard to Yucca Mountain, the matrix is also very tight at Yucca Mountain, like micro Darcies, and the heat loads are going to be larger than what we have in geothermal, so a heat pipe where the matrix is a very active part is probably not going to occur in Yucca Mountain. So if you decrease the permeability of the matrix, still, it probably won't matter because if a heat pipe develops, it's going to be within the fractures themselves.

Now, if the sealing precipitation closes up all the fractures, you're not going to get a heat pipe.

DR. DOMENICO: No.

DR. LANGMUIR: We need to cut if off, I'm afraid, and proceed. We'll have an opportunity later on today to question Bo further in the panel part of our meeting.

Thank you, Bo.

DR. BODVARSSON: Thanks.

DR. LANGMUIR: The next presentation is by Joseph Moore.

Dr. Moore is presently Section Manager for Geochemistry at the University of Utah Research Institute. He received his Ph.D. in geology at Penn State University when I was there, back in 1975. He joined the University of Utah Research Institute in '76, after working for several years as a uranium exploration geologist for Anaconda.

Since that time, his research has focused on the mineralogy, geochemistry, and fluid inclusion systematics of active geothermal systems. He has been doing geothermal system studies all over the world.

With that, Joe, it's up to you.

DR. MOORE: Good morning. I'm glad to be here, because I do think that geothermal systems can help us understand the chemical and physical changes that can occur in the unsaturated environment as the rocks are heated.

During the next few minutes, I'd like to present an overview of the kinds of changes that can occur, and I'll divide my presentation into two parts. In the first part, I'll re-look at liquid- and vapor-dominated systems in a slightly different way than Bo has. I'll concentrate more on the effects of hydrothermal alteration and chemical, chemistry of the fluids that can exist, and the second part will deal primarily with the fluid chemistry and their effects on the rocks.

Unsaturated environments can be found in geothermal systems in several different regions. In liquid-dominated systems, which these are systems that produce primarily liquid, as Bo indicated, they can be found above the water table, where temperature are likely to be no higher than about 100☜, so these may represent the far-field environment, or far-field analogue of the repository environment.

They can also be found in low permeability fractures within the liquid portion of the reservoir, and this occurs when recharge cannot keep pace normally with production. This is typically a production-induced characteristic, but it's interesting because we get some really nasty corrosive fluids that develop when the fluids dry out, so I want to talk about them a bit.

In addition, we can get unsaturated conditions in vapor-dominated systems. I'll talk mostly about The Geysers, because it is the best-studied vapor-dominated system in the world today, we know most about it, although there are half a dozen others elsewhere.

Conditions in The Geysers may be more analogous to the near-repository environment. Temperatures typically will range from 240☜C to probably 350☜C, so we'll look at these three environments in the next few minutes.

The Geysers is located in northern California. Here's a little index map. It's a rather large geothermal field. It's located on the southwest corner of the Clear Lake volcanic system, which has been active for the last two to three million years, so it's a fairly young volcanic field.

There's a variety of geochemical, geophysical, and mineralogic evidence that suggests that the vapor-dominated system we have at The Geysers, the system that Bo described, actually developed from a very large liquid-dominated system, and it turns out that this large liquid-dominated system has had a great effect on the present properties of the vapor-dominated regime.

So what I want to do in the next few minutes is to go through a series of cartoons and photomicrographs and show you how permeabilities have decreased, how permeabilities have locally increased in The Geysers, what permeabilities look like, and really allow you to maybe make a better decision as to whether it is an analogue or not.

And development of The Geysers' geothermal system began, oh, about 1.2, 1.4 million years ago with the intrusion of a large granitic stock. This is a composite stock. It's commonly known as a felsite, and it's shown in red. This stock was emplaced with a series of weakly metamorphosed graywackes, which form the present reservoir, and an overlying sequence of serpentinites, greenstones, cherts. These are fairly impermeable rocks, and they actually help to form an initial caprock over the system. So not all of the caprock in The Geysers is mineralogic, some of it's actually an original caprock.

And just for comparison with the next few slides, I've shown schematically position of the present caprock and the present reservoir, present-day predominated reservoir.

After emplacement of the felsite, temperatures near the margin were about 500☜C, maybe a little higher, and temperatures at the base of the caprock were about 350☜C. A second point is that the fluids that existed in the immediate vicinity of the felsite and the contact aureole around it were very, very high salinity. These fluids typically had salinities of 40 per cent, 40 weight percent NaCl; whereas, the fluids above this yellow line had much lower salinities. They were on the order of five to ten. We're going to see this as important in the development of some of the corrosive fluids, some of the acid chloride fluids that develop at The Geysers.

Much of the matrix permeability actually began to develop during the early emplacement of The Geysers. This is a section of core from one of the geothermal wells. It's a graywacke. You can see it's bedded. It's very weakly metamorphosed. What I want you to note here is there are a whole series of white veins, and these veins are composed predominantly of calcite and quartz. These veins were preexisting. They were present in the rock prior to initiation of The Geysers to a thermal system.

This is a photomicrograph. We're looking at about 5 mm x 3-4 mm, the large quartz vein going from upper left to right, and a second vein that cuts across. These dark patches are calcite. It's a common mineral in the geothermal system. It's one of the most common, but these calcite patches occur everywhere. You'll be seeing another one here; very irregular shape. Notice, too, that the quartz in here is very dark. It's actually full of fluid inclusions, but just note that it's particularly dark.

As the fluids moved up and away from the intrusion--and in the first slide that I showed you, the convection cell was up and away from the heat source--the fluids reacted with the calcite that was present in these early veins, and this is one of these early veins. You can see here the very dark quartz, typical of this early, early vein. Here we see some light quartz, and if you stretch your imagination--and I don't think you'll have to stretch it very far--you'll see that the shape of this lighter area is actually the shape of these earlier calcite blebs that were included within the quartz veins.

So, actually, a lot of the matrix permeability was developing very early. What happened here was that the calcite was dissolved. In its place some new silicate minerals--that happens to be an epidote--were formed, and a fair amount of pore space began to develop, and some of the quartz also re-precipitated, and it re-precipitated on the margins. So we had some quartz sealing here, but the net effect is an increase in porosity, matrix porosity over the original rock.

This is just a ultraviolet shot. The red material is epoxy, and you get a better handle of what the porosity actually looks like. It's really quite large, so in most of The Geysers, we're not really dealing with matrix porosity. That's very, very small. The bulk of the porosity is in these large cavities that are formed through the dissolution of calcite. This cavity is on the order of several millimeters across, so it can hold a fair amount of water and steam if it's available. It has also a very irregular surface, so adsorptive effects may come into play here.

This may be analogous to some of the vapor-phase cavities that are found in the tuffs at Yucca Mountain; fairly large-scale permeability.

If the temperatures went up, they also must come down, and as the system evolved, temperatures dropped. Temperatures at the top of the intrusion were pushing about 300☜C after some period of time, and we really don't know how long that period occurred. I've also shown here a second high-temperature reservoir. This is the secondary reservoir, the deeper reservoir Bo was discussing.

As this temperature declines, an important effect occurred. The circulation system now, instead of being out and away from the heat source, was down and in toward the heat source, so lower solidity fluids were being brought down into the deeper parts of the reservoir.

This had a very important effect on the geothermal system. Most minerals deposit as the fluids are cooled. Quartz, in particular, will deposit as the fluids cool. A few minerals, the sulfates and the carbonates, have retrograde solubilities; that is, these minerals deposit when the fluids are heated up, and as these fluids moved down and were heated, carbonate began to deposit, and this carbonate deposited across the top of the reservoir, and along the sides.

And it turns out that this has been an extremely effective seal, because, as Bo mentioned, pressures in the reservoir are well below hydrostatic, and yet, no fluid gets into the system, or at least no significant amounts of fluid get into the system, and vapor static conditions can remain within the reservoir. So this self-sealing was very important, and it was due mainly to carbonate, calcite.

The final stage in the development of our geyser system is the present one, the formation of the vapor-dominated system that we have today. Some fluid inclusion data tells us this actually happened when temperatures declined to about 260☜C, and I'm not sure that that temperature has any significance. Apparently, it was the temperature when boiling in the system, probably through widespread fracturing, allowed the outflow of the system to exceed the influx. So we have this zone around of carbonate that acted as a seal, steam went up, water did not get back in. Vapor-dominated conditions now began to develop.

I'd like you to note, though, that near the top of the caprock, condensate was forming. We have boiling at the top, working its way down, steam condensing near the top of the system, and then dripping back down. And the effect is that we're now dripping acidic fluids back into the reservoir, and these have the opposite effect of seal-sealing. These will tend to dissolve some of the minerals that are present there.

The properties of The Geysers reservoir, in general, we're looking at temperatures of 240☜C in the main part of the reservoir, in the normal reservoir; in the high temperature reservoir, temperatures up to 347☜ have been measured. Pressures are vapor static. They remain constant with depth. They're about 35 bars, and the porosities are about 1 to 5 per cent.

Not only do we have the matrix porosities, but we also have throughgoing fractures. A number of workers, notably White and others, concluded that the main throughgoing fractures, the pressure-controlling median in them is steam, but that water exists in the matrix of the rock, primarily based on this temperature up to 140☜.

Since about 1987, pressures have declined rapidly and markedly in The Geysers, and in many places now, the pressures are about half of these levels, but there have been no major temperature declines. There's a fair amount of debate about the cause of this. One idea proposed by Hank Ramey is that adsorptive water, the water is absorbed in these pore spaces, and that the water has remained, thus accounting for the constant temperature, but decrease in pressure off the boiling point.

Capillary may also be an important component here, and Karsten may address that later, but at any rate, it appears that we have water in the pore space and steam in the vapor fracture.

Let's turn our attention now to the various fluid types that can occur, and four fluid types are possible--and I'm not going to restrict myself to the 100☜ interval that Bo was talking about--being NaCl pore fluids, a typical variable salinity, and they're near neutral fluids, and in that, some temperatures are going to be 250☜ to 350☜.

These are the reservoir fluids in liquid-dominated reservoirs. These are the pore fluids at The Geysers.

In addition, we have some acid-sulfate waters, which have very low salinities and they're quite acidic; note, they're quite corrosive. We have some CO_2-rich waters; again, low salinities. These are not so corrosive, and, finally, we have some acid-chloride waters which are, again, low salinities and can be extremely acidic. The low salinities of these latter three waters and the fairly high acidities, or low pHs indicate that these waters all represent steam condensates. They're all derived from boiling, and we'll take a look at each one of these in turn.

The composition of the NaCl pore fluids depends on a number of factors; primarily temperature, but also on rock type, permeability, grain size, and time.

This slide shows some idea of the effect of rock type on alteration and, hence, on salinity of the fluids. The point I'd like to make here--and it's typical of these systems, (This is just a plot by ground a couple years ago); is that volcanic glass is the most susceptible to alteration during heating. This is followed by the oxide minerals, magnetite or titanium oxides, and, in general, it's followed by amphiboles, pyroxenes, and some of the sheet silicates.

Sometimes, the most stable minerals tend to be the plagioclases, although they eventually will go as well. Note that quartz is not affected, although quartz is highly soluble in these systems.

Although geothermal chloride fluids can have a wide range of salinities, the mineralogy of most systems is very, very simple, and I've listed this here for your reference. I'm not going to spend a lot of time on this particular slide. In general, the main minerals are clays, zeolites, quartz, and a few silicates; very, very simple mineral assemblages characterize most altered rocks.

These minerals, however, have very restricted thermal regimes with thermal stability fields, and this slide gives some idea. At temperatures less than about 200, the dominant minerals are the clays, montmorillonite, montmorillon or smectite zeolite. Kaolin is common, silica, either morphous silica or calcite generally below 200 or 180, and occasionally, we started to see some feldspars come in. Calcite is also an important mineral.

At temperatures between 200 and 300, the clay minerals are no longer terribly important. Illite and chlorite become more important; these are iron, iron potassium silicates. Feldspar has become more important. Some of the cal-silicates begin to show up; we have epidote, wairakite, and quartz is ubiquitous. Calcite also remains ubiquitous.

Finally, as temperatures exceed 300☜, a whole new series of minerals come in, and these are the chain silicates. These are the amphiboles and empiric scenes. Quartz still remains present. Calcite typically disappears as it reacts to form these new minerals. As these minerals form and react with preexisting minerals, the composition of the fluid changes. In general, with increasing temperature, silica will increase; and, in general, the sodium potassium ratio, the sulfate content, the calcium content, and the magnesium content will decrease with increasing temperature.

These are temperature-dependent reactions. We can use the compositions for the cation contents of the fluid to get back at the composition of the fluid.

Let's turn now to the acid-sulfate waters, to the first of our condensates. I picked this one to begin with because the thermal manifestations are really the most spectacular, and I'm sure most of you have seen these kinds of manifestations on various trips.

The key features here, the pH, we're looking between 2 and 3; very, very acid. Obviously, it's going to do a lot of damage to the rock. The salinities of No. 1, which is a pool from New Zealand, are quite low. They're a little higher in No. 2. The actual salinities of these fluids is really a meaningless number.

These compositions are not in equilibrium with the rock. Because of the very high acidities of these fluids, they quantitatively dissolve anything they come in contact with, and I'll show you an example in a minute, so we gain very little information from this, but they can significantly dissolve a great deal of quartz, a great deal of silica which can re-deposit, can re-precipitate, can cause sealing.

There are a number of common features we associate with acid-sulfate waters. These commonly include fumaroles, bubbling mud pots, acid leach rocks. This is a typical vent area for fumarole associated with these fluids. These fluids develop as H_2S-bearing steam oxidizes in a surface environment. The fluids must be in contact with atmospheric oxygen. If condensation occurs and we're reducing the environment, we will not generate the very low pHs typical of these fluids, so the key here is oxidation of H_2S to sulfate, which forms sulfuric acid, which does all this work.

Here we see some small sulfur crystals that typically form around the vent, and the white rock is primarily silica. This is an acid leach rock. Sulfuric acid has attacked it, and has removed everything but silica. So we have created a new rock; in fact, this rock has much higher permeabilities than the old one. We've taken everything out. It's fairly friable, permeable rock.

Most of you have seen these, I'm sure. This is a bubbling mud pot. This is another typical manifestation of these acid-sulfate fluids, they bubble and something is boiling up. CO_2 and H_2S are real common, they all stink. This is really a slurry. It consists of water, condensed steam, and the dark ring there is kaolin. This is entirely altered to clays. It's real typical in this environment; very intense alteration.

The third shot is an area in southern Utah. This is the Coso geothermal field. Again, we see the white, acid-altered rock, some hills in the background that have not been altered. This particular rock originated as alluvium derived from a moderately to densely-welded ash flow tuff, and as you can see, there is not much left in this particular alluvium. Everything has been totally destroyed, and this tuff is not much different than some of the repository tuffs.

This is a standing pool of water, and it seems to be meteoric in origin. It's not condensed steam. The geothermal system itself, the hot water system, is located at a depth of about 400 meters below this, so in this instance, we're sitting over a vapor cap to a hot water or a liquid-dominated system, and the distance is about 400 meters. So, alteration is caused essentially by 100☜C boiling fluid at the top of the water table.

Because these fluids are readily neutralized as they react with the rocks and percolate back downward, you may have gathered at this point that most of this alteration starts at the surface and works its way down. Because they're readily neutralized, this kind of alteration does not extend, generally, vertically or laterally to any great distances. There's little data on this. There are a few good, illustrious examples, but this is one from Kamchatka that I found, and I thought it might be useful to show it.

This shows the temperature patterns. Here's the scale, and you see we're not dealing with large distances at all. These are the fumarole vents, and you can see that there are really three mineralogic zones that can be recognized; the zone of very intense alteration, the silicification or residual silica, the native sulfur, alunite is a common mineral and this is shown in green, and this starts at the surface where the fumaroles are, and extends down several meters.

As we get into the red zone, we're seeing effects of a more neutral fluid, not nearly so acidic, and you can see by the time we're out 20 or 40 or 60 meters, alteration is back to background.

Now, in most cases, the acid-sulfate alteration is restricted to surface conditions, but to make a story interesting, this isn't always the case, and it turns out that in some areas, like the Philippines, this acid alteration can extend considerable distances.

This is a schematic of one of the Philippine geothermal systems. You can see a well here, so we have some control; fumarole up at the top. We're generating some of these acid-sulfate waters, and in this particular reservoir, we have a fairly open fault zone or channel which allows these acid-sulfate fluids to drip back down into the reservoir, and they can do this because the fracture zones themselves have been sealed with acid-resistant minerals, probably quartz and a lot of clays, so the fluids don't react with the adjacent rocks, and they stay quite acid.

And you can see the distance here is, what, more than 1,000 meters. That's mean sea level, by the way. The minerals that form, typically, typify the acidic conditions, alunite, pyrophyllite, diaspore, montmorillonite. Once we get down into the level of the reservoir, these fluids are rapidly neutralized by dilution, and so we no longer see these strong acidic effects at greater depths.

Temperatures in this region are also quite high. In this particular case, the temperatures are pushing 300☜C, so we're looking at very hot, very acidic fluids.

Okay. Let's turn now to the CO_2-rich condensates, or the CO_2-rich waters, and I've shown three examples here. The one in your book, the analysis in your handout, this analysis differs slightly. The analysis in your handout is actually in ppm. This analysis is in millimoles/kilogram, so you might use your book for comparison. Two of these are pools. The third one is a highly-diluted well. It's from about 650 meters.

Note here that the pHs are much closer to neutral. Again, the compositions are variable. I'm not going to argue about what these compositions mean, other than to say that, again, in this case, these are not equilibrium concentrations. It represents quantitative dissolution of the country rock, so these fluids are corrosive enough to affect the rocks.

The steam that forms these CO_2-rich fluids is exactly the same steam that formed our acid-sulfate waters before, except in this case, condensation occurs in a reducing environment, and because there is not sufficient oxygen to oxidize the H_2S to a sulfate, and because carbonic acid is a much weaker acid, the pHs are correspondingly higher. So the steam is the same, it just depends on where condensation has occurred.

Despite this slightly more neutral aspect of this fluid, they can do some tremendous rock alteration, and the question was brought up just a few moments ago about the effects of self-sealing. Well, it turns out in many geothermal systems, self-sealing is produced primarily by the interaction of these CO_2-rich condensates on the rock, rather than by silica. We can talk more about that in a minute.

This is a volcanic rock. It's a large plagioclase-phenocryst, and we see here that the plagioclase can alter the calcite on my right, and to illite on the left. This clay carbonate alteration can readily fill fractures and create very, very effective seals in geothermal systems, effective enough to keep recharge from percolating downward even in very high rainfall areas, and effective enough to keep the fluids from moving upward. Instead, it often causes the fluids to move out.

These caps generally form an umbrella-shaped parapet over the geothermal systems, so they're effective both on the margins and on the sides where boiling can occur and steam condensation occurs.

Of course, not all of our effects are good effects. These are slightly acidic, and so if there are minerals that are susceptible to acid attack, they will be attacked. We do not have definite or unique evidence from The Geysers demonstrating that these CO_2-rich fluids have existed at The Geysers. However, we have three lines of evidence that says it does, or that demonstrates it does.

First, we have fluid-inclusion data, which demonstrates that low solidity fluids at temperatures exceeding 200☜ existed in The Geysers. Such temperatures are much too high for low salinic fluids in that environment. They can only represent condensate, and our first indication is condensate does exist.

These are bladed calcites. This is calcium carbonate. This formed as the fluid boiled. As you can see, the calcites have been highly-corroded. They've been dissolved, suggesting a slightly acidic fluid, the dissolution, and a CO_2-rich fluid is the likely cause here. The dark areas are an amphibole, which really don't affect us at this point. So here, the permeabilities have been increased by dissolution.

Further evidence for the presence of these CO_2-rich condensates are clay minerals. Here, this is an SEM image. You can see the scale, microns, looking at very fine-scaled material, but here we see the same blades of calcite, a large smectite grain, a large clay mineral growing right on the calcite, again, indicative of acid conditions, so we can see the effects even at The Geysers where these have occurred.

The last fluid type I'd like to discuss with you are these acid-chloride waters, and these have been recognized only during the last few years. They seem to be a very unique occurrence. Again, this is an analysis. This is actually an analysis from the Coso geothermal system, which is a liquid-dominated system, and I chose this one because I had access to it.

The chloride content is not terribly high. It's 6.9, but at The Geysers, chloride contents can reach 50 to 100 ppm, so these can be highly acidic, and I'll show you the effects in a minute.

These fluids seem to form only where super-heated steam can exist, and the probable origin which was proposed by Bob Fornier several years ago is that as the pressures decrease within fracture zone, or occurs within The Geysers as well, boiling is complete, and this leads to the precipitation of chloride, sodium chloride within the fracture zones.

As super-heated steam moves across the sodium chloride, it reacts with it to form HCl and NaOH, and this, then, can condense at some point above its formation, producing the damage and the corrosion.

We don't have any actual examples of what these fluids do within the rocks. We have not seen that yet, although we can predict that sodium and somatism will be common. We do know what they do when we see some surface casing. This is an iron pipe from The Geysers, and this particular well was affected by these hydrochloric acids, and you can see it destroyed the pipe.

A number of wells have had to be shut in, which means they cannot be used because of the presence of HCl, so it's a significant problem where super-heated steam can exist. The amount of chloride that will be generated is a function of the original chloride content of the fluid.

Let me conclude by just noting the conditions that these corrosive fluids can develop in. It seems that four conditions are required; the original liquid, the original pore fluid or reservoir fluid must have enough gas, H_2S and CO_2 to generate CO_2 or H_2S steam. Boiling must occur, and the steam must be able to separate from its site, original site of formation.

We presume the steam is channeled upward, and it must find a place to condense. These can be either in sealed zones, they can be in pore spaces, they can be against impermeable rock surfaces. If the condensation occurs under oxidizing conditions, conditions where there's a constant supply of atmospheric oxygen, acid-sulphate waters will develop. If there were reducing conditions, we'll get these CO_2-rich waters or the CO_2-rich condensates, and if the steam is super-heated, we're going to generate these acid-chloride waters.

I'll take any questions, but that concludes this presentation.

DR. LANGMUIR: I wish we had time for them. I have a half a dozen, a whole page for you here, Joe. I believe we will have time for many of the questions at the panel section this afternoon, and I'm looking forward to getting some answers at that point.

I think we need to go on. Thank you, Joe.

Our next speaker is Larry Myer of Lawrence Berkeley Lab. He's a staff scientist and principal investigator of the Earth Sciences Division there. He has his Ph.D. in engineering from the University of California at Berkeley. He's a very active Earth Science Division Coordinator for the Office of Basic Energy Sciences and Geosciences.

His presentation is titled: "Thermal Effects on Fracture and Rock Matrix Properties."

DR. MYER: I was asked to talk about the thermal effects on mechanical properties and hydrologic properties of rock. Now, there is very little data, if any, in the geothermal realm on this topic, so I've broadened my talk to asking the questions of just what is the effect of increasing temperature on the mechanical and hydrologic properties of rock, and what indications might these have for Yucca Mountain.

We can think of a rock mass as composed of two parts. It has the rock matrix, which is essentially the mineral grains and the pores and the cracks and the macrofractures which separate the blocks of intact material, so I'm going to talk about each one of these in two sections; the first section talking about the rock matrix properties, and the second, the macrofractures.

Now, you can make some general statements about the effects of elevated temperature on the effects of mechanical and hydrologic properties in matrix material, and that is that, in general, at a constant mean stress, you're going to see decreased modulus, decreased strength, increased permeability, increased thermal expansion as you increase the temperature.

DR. LANGMUIR: I'm sorry to interrupt you, Larry. I forgot to mention that Larry's overheads are on the way. They're being copied, and they'll be available to you, hopefully, this early afternoon, so don't keep thumbing through looking for them. They're not there.

DR. MYER: Sorry about that. I should have mentioned that.

Macrofractures are primarily sensitive to the thermally-induced stresses produced by the heating within the repository, so we have two slightly different scenarios to talk about.

Beginning with the thermal effects on rock matrix now, many of the effects of increased temperature can be related to the effects of crack generation, the fact that as you increase temperature, you begin to develop additional cracks in the material, and this affects the properties.

Now, there's several mechanisms that I've separated out here. The first one is thermal shock. That's the same thing as throwing an ice cube into a glass of water. I'm not going to talk about that very much, because it's not very relevant.

The second that I'm going to talk about is the effect of the heterogeneity at the grain scale. Rock consists of a heterogeneous structure of grains. The properties of these grains are heterogeneous. They differ from one grain to another. Their thermal expansion, their elastic properties differ, and so when you apply a increase in temperature to such a heterogeneous group of grains, you begin to generate cracks.

Then the third type of crack growth mechanism I will talk about is actually called subcritical crack growth, which means that your cracks are growing, but they are not at the critical level, where you have propagation to failure.

One of the general attributes of all of these is that if you increase the mean stress or the confining pressure on the rock, you're going to tend to reduce the crack growth.

So let's begin. What are some of the properties of interest? What do we know about these?

This is the effect of Young's modulus, the effect of increasing temperature on Young's modulus for a piece of grain, and you can see two difference curves here; one at 25☜C and one at 175☜C, with a slightly lower modulus at 175☜C. Both of these show an increasing value with confining pressure, and this is totally consistent with the mechanism of cracks producing a decreased modulus.

The effect here is only about 10 or 15 per cent, and the effect on strength is about the same order of magnitude. From the data that I have seen for the Yucca Mountain tuff, you're still talking about the same sorts of relative magnitudes for these kinds of effects. So if you have increased cracking in the rock, what happens to the permeability?

There haven't been very many studies done on this. This is a result of measurements that we made on a very tight marlstone. This has got permeability on the order of less than a nano Darcy. In fact, it's a hundredth of a nano Darcy or less, but here we see a very marked increase in the permeability as a function of temperature, on the order, in fact, of an order of magnitude, when you're talking about temperatures increasing, 150☜C.

This is typical of a rock in which the permeability is found almost entirely within small cracks of the rock. These are very low permeability rocks. The permeability is dominated by cracks. If you're going to increase the crack population, you get a substantial increase in the permeability.

Now, for permeability, it depends very much on other factors, too, let alone chemistry. Let's take a shale. This is a Devonian shale in which you start now to have clays present, and you see a much smaller--in fact, there isn't much of a change in the permeability with temperature.

You see a dramatic change as you start increasing the magnitude of the hydrostatic stress on the rock, and this is typical of most rocks. If you, as I said before, increase the amount of confined pressure and hydrostatic components, you start to close the cracks and you start to decrease the permeability.

But in this case, I wanted to just point out the fact that when you start introducing different mineralogies, you can have different behavior in rocks, particularly if you start introducing clays into the matrix of a system. At very high permeabilities, for example, in a sandstone, where most of the fluid is carried through the large pore space and you have very little crack contribution to the permeability, you have almost no effect of temperature, either.

So, in summary, thinking about the permeability of these rocks, it depends very much on the particular characteristics, mineralogic characteristics of the rock. Those with which the fluid is carried primarily through the cracks, you're going to see a significant effect of increasing temperature.

Thermal expansion. If you have additional cracks being produced as you increase the temperature, you will also increase the tendency of the material to expand when you change the temperature, so this is some data I obtained from Connie Chocas from Sandia on measurements on thermal expansion of tuff. These were conducted under unconfined air, dry conditions.

The lower portion of the curve here, now, this is just the actual deformation measured as a function of temperature. The slope of this curve gives you the thermal expansion of the rock. There is a slight curvature of this line which indicates the contribution of the additional cracks as you increase the temperature. Then you have all of these more radical changes at higher temperatures. In this case, this is due to--for example, here we have the tridomite phase transformation. There's another point up here, I think, over here, which is the cristobalite phase transformation.

So, for thermal expansion, you have not only the effects of the cracks to worry about, but the effects of the mineralogy and the potential transformations in minerals. Clearly, this produces a very non-linear thermal expansion curve which must be incorporated in the models in order to properly model the thermal expansion of the rock.

All of the effects that I've talked about now are for slow rates of heating, but relatively short term. I want to turn attention now to longer term heating and what might happen, and I introduce the concept of the stress intensity factor, which is used in fracture mechanics to describe the stresses in the vicinity of a crack tip, and so if we have just a block of material with a single crack under tension, the stress intensity factor is simply equal to the stress times the square root of pi times half the length of the crack.

Now, this stress intensity factor takes on different values, depending only upon the geometry of the crack system, and the type of loading that's imposed on the system.

The type of effects I've just been talking about, where we have slow heating or changes in stress producing crack growth, really result when we have the stress intensity factor approach what's called the K_{1C}, or the fracture toughness of the material.

If it approaches the fracture toughness of the material at the grain-to-grain level, you get fracturing of the grains leading to the types of behavior that we just saw, but there is also a phenomenon, when you apply the load over long periods of time, where you get crack growth at values of the stress intensity factor which are less than the fracture toughness, or at lower levels, and the data to show this--this is just one set of data.

This is for granite, looking at the crack velocity as a function of temperature, as well as the vapor pressure within the crack. There are some problems here. There are some missing decimal points. That should be 1, 1.2. This should be .05 and .1.

What I wanted to illustrate with this is that the subcritical crack growth as a function both of the temperature, as well as other properties, such as the vapor pressure in the crack. For example, we can look at these two curves here.

One of these curves gives the crack velocity for different values of the stress intensity factor for 15 kPa water vapor pressure at 200°C; whereas, the other one is at 2.5. That should be not 25, but 2.5. So decreasing the vapor pressure actually decreases the amount of subcritical crack growth that may occur.

On the other hand, if you jump in temperature from 200°C and either one of these vapor pressures over to 300°C, you get a very large increase in the subcritical crack growth velocity.

Now, the zero order analysis was done, using these concepts, by John Kemmeny and Neville Cook to look at the possible implications of heating in tuff, so let's now look at a volume of rock in which you've got a borehole or an opening of any sort.

Now, you have different sorts of stresses imposed on this. You have the mechanical stresses imposed by the fact that you're underground and opening an opening, and then you also have imposed on this the thermal stresses, which you can see here are a function of the thermal expansion of the modulus, the temperature field. Here's the temperature at the inside of the opening, and then a far-field temperature.

And what they did was simply look at the possibility of subcritical crack growth in a region right around the interior of that borehole, where you have both a stress field which is a function of the position where you are due to the mechanical loading, as well as due to the thermal loading.

I've included here an empirical equation for this crack velocity, which shows that it's an exponential function of the temperature and the stress intensity factor raised to this power.

They did this calculation for a variety of range of mechanical stresses in the tangential direction, assuming either that it's at zero, or up to 30 mPa, and here we see the effects of temperature, which shows that the borehole stress piece, at about 20 years, in this case, where they had a peak temperature of about 200☜C at 20 years, and then it decays off thereafter.

Now, the effect of that subcritical crack growth is seen here. This was assuming an initial population of cracks. Then you impose both the mechanical and the thermal stresses on that, and change in thermal stresses over time, and you look at how those cracks grow according to the crack velocity equation, and you can see that for most of the assumed conditions of mechanical stress, the cracks begin to stop growing, stabilize out at about after 20 years.

On the other hand, in a condition where you had a somewhat higher mechanical stress imposed, you get this unstable crack growth and failure.

So what this means, in terms of behavior in the repository, is that you would begin to get spalling if you had conditions like this. This was done with properties that they estimated for tuff, and began to make us believe that there could be the potential for subcritical crack growth and spalling instabilities even within the tuff rocks.

Now, you would have to add onto this the additional effects I showed previously, of slow heating, because what that does, is that actually changes the distribution of cracks initially, and then you add on as the long term effects.

There's not a lot of data on possible long-term heating effects and subcritical crack growth. At the end of the Stripa test, there were drillback holes drilled through the location of the heater core of one of the heaters. Now, this is a test in which the granite had been heated to maximum near-borehole temperatures of about 375☜C. The total duration of this heating was--I don't remember exactly-

-over a year, I guess, so after the holes were drilled, we obtained samples at different distances from the heater, and this shows the maximum temperatures to which they had been subjected, though those were not, by any means, the average temperatures.

Then we did some seismic measurements on those cores, which I show here, both velocity and amplitude measurements of a compressional wave propagated through those cores, and what I want to illustrate is that the core nearest to the heater at both the lower velocities and the lower amplitudes than those farther away, and this can be directly attributed, both behaviors, both velocity and amplitude behavior, to the presence of additional cracking caused by the long-term heating.

Now, whether or not there is a substantial amount of subcritical crack growth or other effects of crack growth due to just heating is not known, but, for sure, long-term heating doesn't increase the amount of cracking around the boreholes. And I just might add, if you have, of course, increasing cracking, it means increasing permeabilities, so we're talking about changing, in effect, the properties and characteristics of the damming zone here.

Turning to macrofractures, just briefly, if you take a macrofracture now, a single fracture in a piece of core, under constant stress conditions there's really not much effect of temperature. The principal point that I want to make which concerns the macrofractures is that they're very sensitive to the thermally-induced stresses imposed by heating within the repository.

Very quickly, we have done some measurements in which we had a single fracture in a piece of core, and then loaded the fracture so that we can measure the deformation within that pore, and then we heated it up, just to emphasize this point, and we saw no effect. So, this is not published data, because it's not very exciting.

Regardless of whether we had it saturated, dry, or hot, there is essentially very little effect in the changes in the property. We made seismic measurements on the same fracture, and we saw the same effects.

So, what are the important things to think about with respect to single fractures or faults? It is their response to the induced thermal stresses, and everyone is aware of all of the work done on single-phased flow in fractures and the effects of stress on that.

I only want to conclude with a couple of comments that not only are the single-phased properties a function of the stress, but the two-phased properties are also a function of stress, and this is some results from a test in which we did mercury porosimetry. We injected mercury into a single fracture at different stress conditions, and so these curves represent the amount of mercury injected into a single pressure and different capillary pressures from .1, .8 mPa, while changing the normal stress across the fracture.

Basically, what I want to point out is that the capillary pressure characteristics of a single fracture are a function of the stress, as indicated by this data.

If we have a system in which the fractures are very sensitive to the thermal stresses, then we need a system of evaluation or modeling which must be able to evaluate those effects, so one last comment is only to the extent that if we have a blocky system, where we have many fractures going through it, it is not appropriate to try to use the average stresses and strains within this system to evaluate the properties of these fractures.

These fractures represent very local discontinuities, which are very compliant compared to the rock matrix associated with it, so we must be able to evaluate these explicitly, which, to me, means that we need to incorporate the discrete element-type approaches, in which we can look at the effects not only of the blocks and the deformations within them, but those deformations then associated with the fractures of trying to develop and look at the sorts of paths that may be created around an opening.

So, in conclusion, many of the effects are understood in principle, because, as I said, many of the effects are simply related to the development of cracks on a grain-to-grain scale within the rocks. However, there isn't very much site-specific data available yet, and where the study needs to be done is the thermomechanical and hydromechanical measurements under in situ conditions, and, of course, I didn't talk about chemistry, I was told not to, but we cannot do these without the chemistry being involved and, of course, modeling which explicitly incorporates the fractures.

Thank you.

DR. LANGMUIR: Thank you, Larry.

Questions from the Board?

(No audible response.)

DR. LANGMUIR: I'll ask you one. Your measurements were of rocks without consideration of moisture content. I wonder if variations in moisture content in the porous rock will have predictable effects that you can talk about?

DR. MYER: In which respect? In terms of--

DR. LANGMUIR: Fractures, formation, healing.

DR. MYER: They certainly will, because the, particularly with respect to the long-term behavior, the subcritical crack growth. The amount of moisture is certainly an important area. Such mechanisms as stress corrosion cracking very much affect the subcritical crack velocity.

DR. LANGMUIR: Any further questions; Board staff?

(No audible response.)

DR. LANGMUIR: Thank you, Larry.

Our next speaker is David Bish. Dave has his Ph.D. in mineralogy and petrology from Penn State University; again, a time when I was there. He's been with the Los Alamos National Laboratory for 11 years, where he's worked as a staff mineralogist in the geology and geochemistry group. He's been participating in the Yucca Mountain Project since 1980, so I suspect he knows as much as anybody does about the mineralogy and alteration of those minerals at Yucca Mountain.

The title of his talk is, "Alteration History of Yucca Mountain Due to Thermal Effects: Analogue for a Hot Repository."

DR. BISH: Thank you, Don.

What I'd like to do this afternoon--or, it is afternoon; not quite afternoon--is present to you a little bit of information on the types of features that we see at Yucca Mountain that we believe may be possible analogues for a repository-type environment.

Now, the first thing that I'd like to do before I really get into that is something that Don asked me to do. In a way, I will give you a fairly good context in which to understand what I'm going to tell you about, but also, I think, it will allow you to understand some of the things that will be said this afternoon relating to modeling of the repository environment.

I'm going to show you a cross-section. This is the familiar pork chop. I'm going to show you a cross-section from A to A', showing you what the geology looks like, and just to emphasize a couple features.

First of all, just to point out to orient you, the potential repository is here. The static water level is here, and a couple of important things that I want to leave you with with this figure--and I'll put it on this so we can see it throughout my presentation--is, first of all, we can see that underlying the potential repository horizon, the rocks vary significantly, depending where we are, going from west to east, and also, I might add, from north to south.

In the westernmost portion, up next to Solitario Canyon, the rocks of the Calico Hills formation are, as you can see from the caption here, largely vitric, non-welded materials that have not been zeolitized; whereas, as we go eastward and northward, the rocks become quite zeolitized.

Likewise, the non-welded unit overlying the Topopah Spring member undergoes a transition from a largely vitric material to a vitric plus smectite-type of material.

Another interesting aspect of this is that just about everywhere across Yucca Mountain in this east-west cross-section, we see that between the static water level and the potential repository horizon, there is a significant amount of zeolitized rock, but you can imagine very quickly that when you see some calculations this afternoon, modeling calculations done, it will depend critically on whether we're through a section here, for example, where the underlying material is largely vitric, or whether we're looking at a section here where the underlying material is largely zeolitic. I'll just leave that up here and use that to refer to.

Now, in any natural analogue study pertaining to the Yucca Mountain Project, I believe our goal, at least from my point of view, is to predict the effects of possible repository-induced temperature and water vapor pressure changes on the present-day mineral assemblages.

We have a couple of different types of reactions. We have both alteration reactions, and dissolution-precipitation reactions that may include the alteration of the glass that's quite abundant at Yucca Mountain, to a zeolite/smectite and/or silica phase assemblage. We may see, at higher temperatures, the alteration of clinoptilolite and/or mordenite to analcime, or even to an alkali feldspar, and one of the things we've heard a bit about this morning--and I will address just a bit--is the potential for dissolution and precipitation of silica phases.

Don also asked me to comment on the potential hydrologic effects of these reactions. That's something I'll do, but primarily with reference to other individuals' published work. One of the things that you'll see as I go through my talk is that there can be pronounced decreases in permeability as we go from vitric or vitrophyric horizons to zeolitic horizons.

There can be--I'll demonstrate this with a simple calculation--potential increases in permeability if we alter zeolitic horizons, and one of the things that we've been realizing recently is that there's a significant change in the nature of the water storage capacity whether we're talking about vitric tuff or zeolitic tuff, even though they may have comparable porosities.

I'll just put this up very quickly to perhaps anticipate some of the things you'll see this afternoon, but to emphasize to you why, again, we're interested in these thermal effects. This is, again, that cross-section, the A to A' cross-section I showed you. Superimposed on this is a schematic of the maximum thermal dryout that I've modified from some recent work of Buscheck and Nitao.

Essentially, this is the boiling isotherm here, done for 114 kW/Acre, 30-year-old spent fuel, and this is at a time of about 2,000 years. So note that the maximum dryout zone, or condensation umbrella reaches virtually all the way to the top of the Topopah Spring member into this vitric and smectite-rich zone. The lower condensation and downward drainage zone reaches well into and, in some cases, beyond the Calico Hills formation, so it encompasses a large amount of potentially reactive mineralogy.

I just thought I'd summarize the type of information in our group that we're interested in obtaining when we look at natural analogues. Primarily, we want to get information on the long-term behavior of rocks and minerals in a repository environment. The reason this is important is that much of this information is difficult to obtain in laboratory experiments because of the relatively low temperatures, at least geologically low temperatures we're dealing with, and at least on a human scale, the long reaction times.

Something I'm trying to emphasize is that there are potentially a number of difficulties with natural analogues. Even in the case of using Yucca Mountain as a natural analogue, we have the problem of defining the past conditions that existed; for example, the temperatures, the pressures, water compositions. We need to locate, then, conditions that we feel are representative of what we might expect in a repository environment.

If we use Yucca Mountain, we don't really have the problem of identifying representative mineral assemblages, but that has to be kept in mind whenever we do it.

Really, it boils down to the fact that Yucca Mountain is, at least in terms of using it as a natural analogue, is not presently an active hydrothermal or geothermal system, so that we really have to infer a lot of information on the amounts of water present during alteration, and the water concentrations.

So I'll focus my presentation relating to natural analogues into three different areas: First of all, I'll discuss the hydrothermal system in northern Yucca Mountain, and I'll use primarily illite/smectite and fluid-inclusion geothermometry data to get information on the apparent long-term mineral stabilities of some of the phases present in at least northern Yucca Mountain.

Second, I'll move to the alteration zone between the Topopah Spring vitrophyre and the lower devitrified unit, just right around the potential repository horizon. This was an area of dynamic alteration in which we see alteration concentrated around preexisting, for the most part, fractures. But, as I emphasized earlier, there's some uncertainties here and we really don't know about the state of saturation, and it was certainly spatially variable.

Third, I'll mention the vitric to zeolitic transition that we see going from west to east here, and from south to north at Yucca Mountain, and make some inferences about what might happen to this vitric material if we alter it.

So, moving to the first case, the geothermal hydrothermal system that existed in northern Yucca Mountain, I'm going to show you some mineralogic data, and the data are in your package. Many of you have probably seen this at previous presentations, but we've put together this picture using, primarily, data from Drillhole G-3, farthest to the south, G-1 and G-2 farthest to the north, and I emphasize here the proximity of Yucca Mountain to the Timber Mountain caldera complex.

I'll go through this quickly, since it is kind of a saturated natural analogue. I show here on this diagram showing mineralogy for Drillhole USW G-2--this figure is in your package, also--on the left, I show information on the illite/smectite mixed layer that is present ubiquitously in most rocks at Yucca Mountain, and I've plotted here the per cent of illite layers or collapsed layers in the illite/smectite. That's really not important for those of you who are not into clay mineralogy, like I am.

The interesting fact is, though, that there are a number of published correlations between this information and temperature that allow us to obtain data giving us approximate temperatures of alteration as a function of depth.

Along with this information on illite/smectite, I've shown schematically on the right side of the diagram information on the relative percentages of a number of different minerals or phases at Yucca Mountain. The scale is down here. I've left out a few dominant phases, such as quartz and alkali feldspar, so these won't total to 100 per cent, but you can see some interesting trends. For example, you see the disappearance of cristobalite as a major phase at approximately the point where we get a tremendous increase in illite, in the mixed layer of illite/smectite. We also see the stratigraphic control on clinoptilolite and mordenite, and the disappearance of those phases with depth.

Now, using the illite/smectite geothermometer in Drillholes G-1, 2, and 3, and some very limited fluid inclusion data, we put together schematic paleogeothermal gradients for these three drillholes, and you can see, as I just showed in the last figure, we've got an abrupt increase in temperature in G-2 at some time in the past, with temperatures approaching, or perhaps even exceeding 250☜C. G-1, a little bit farther to the south, is essentially this curve depressed in depth. In G-3, we see little or no evidence for any elevated temperature alteration. I've superimposed, just for comparison, the present-day geothermal gradients. They go in the same order, but they're considerably lower in temperature.

So what do we conclude, then, about this northern Yucca Mountain hydrothermal system? Well, first, we have also applied potassium argon dating methods to these illite/smectites to get an idea of when the alteration event occurred, and we dated most of the illite/smectites at around 10.7 million years, which coincides almost exactly with what we call the moat rhyolites in the Timber Mountain caldera complex.

Based on the spread in potassium argon dates, we estimate that the hydrothermal alteration was less than or equal to one million years in duration.

Secondly, the paleogeothermal profiles, which I just showed you, are consistent with a change from a meteorically-cooled or a rain curtain-type system zone at shallow depths, to a convective-type of thermal system at greater depth, at least in G-2.

The important information we get from this, though, is the apparent long-term saturated thermal stability of a variety of minerals. We see that clinoptilolite appeared to have been stable up to about 100☜C; mordenite, slightly higher; analcime considerably higher, 175-200; and interestingly, there are a couple of experimental papers in the literature on the hydrothermal stability of analcime, which agree quite well with this, which is nice.

Cristobalite appears to have ceased being a major phase at about 90 to 100☜ in G-2, but, importantly, it ceased to be a major phase at much lower temperatures in Drillhole G-3, which shows us that's something we constantly have to keep in mind, that the reactions in these rocks are not solely a function of temperature, but they're also a function of water chemistry.

Now, just to sidestep for a moment and look at the importance of some of these reactions. You might say, why do we care if clinoptilolite disappears in this zone, for example? And I use this to emphasize that there are a number of reasons why we care about that.

I've diagramed here the transformation of clinoptilolite to analcime, and I've used these schematic formulae here, and I've included those simply so you can, over lunch, check up on my arithmetic, but I've diagramed going from 2.67 units of clinoptilolite--and units are here--going to one unit of analcime, one unit of quartz, and 48 units of water, and I've shown here the respective volumes for each of these what I'm calling units. They're essentially one unit cell.

Here we can look at the volume of the reactants; namely, 2.67 clinoptilolite; the volume of the products, assuming quartz, and look at it changing volume. So, obviously, right away we see that one of the very important effects of this reaction is that there is a very large negative change in volume going from clinoptilolite to analcime, whether we assume that we produce quartz or cristobalite.

The second, obviously, is that we produce a large amount of a silica phase, which can potentially be mobilized and subsequently affect rock permeability. There's also generation of a large amount of water per each unit, altered, and, of course, there's the loss of the important sorptive phase, clinoptilolite. So this is just one example to show you why these mineralogic reactions are potentially quite important.

Now, moving on to the second natural analogue, that of the transition zone between the Topopah Spring devitrified tuff and the vitrophyre at the base of the Topopah Spring member, we think that's potentially the best analogue that we have for a repository-type situation. We see that the alteration is concentrated around fractures, and was apparently quite dynamic.

This is a picture of a piece of core. You have in your package something that Xeroxed a lot better, but it's essentially the same kind of thing, the diagram showing this. I'll describe that in just a moment.

We've done some different types of analyses of this sample and related samples in the lower Topopah Spring vitrophyre, devitrified tuff transition. We see that we have an alteration assemblage of clinoptilolite, or to clinoptilolite, smectite and silica phases, both quartz and cristobalite, suggesting, based on oxygen isotope geothermometry, that alteration occurred before 100°C. So this suggests that mineral sealing of fractures--and I emphasize mineral sealing--in the vitrophyre may occur.

Just to briefly show you the data, these are also in your package. These are oxygen isotope data for three separate samples; two drill core samples and an outcrop sample. These are the oxygen isotopic compositions, and depending on which fractionation factors we assume, and assuming a particular oxygen isotopic composition for the water that did their alteration, we see that we have relatively low temperatures at which the quartz was produced.

Now, this quartz is in this fracture, running down the center. It's in the diagram in your package. This zone surrounding the fracture is a zone of incipient devitrification. In other words, we've gone from what is out here, vitrophyric material that is all glass, to something here that resembles, essentially, the central portion of the Topopah Spring member, where we have alkaline feldspar silica members.

Interestingly, right along the boundary between the vitrophyre and the devitrified zone where the silica activity was probably quite elevated, we have a clinoptilolite-smectite assemblage, so this really looks like quite a nice analogue to what might occur, at least in the lower vitrophyre in a repository-type environment.

You can see the reason that's important is that the vitrophyre is right here, and it directly underlies the potential repository horizon.

This just emphasizes a couple more aspects of the alteration of this vitrophyric glass material. This is a calculation that Schon Levy did for us just recently, in which she made certain assumptions about the densities, volumes of both vitrophyric glass and these different phases, and looked at the relative volume change going from a vitrophyric glass from either a smectite/cristobalite assemblage, or a zeolite/cristobalite assemblage. The original glass volume is represented by this line right at one, and you can see in both cases we experience significant volume increases when we alter from the vitrophyre to either zeolite/cristobalite or smectite/cristobalite.

In this calculation, the framework constituents; namely, silica and aluminum, were used to constrain the mass balance, and we can see that aluminum controls, essentially, the amount of the product, except in the case of cristobalite, because most phases have less silica, contain less silica than the glass.

What this emphasizes, again, is that we have the potential for producing large amounts of a silica phase that may, indeed, seal fractures. Whether or not it will is a different question, but it has in the past.

Now, moving finally to the third possible natural analogue, to repository-induced alteration, we'll look at the transition between the vitric non-welded Calico Hills formation and the zeolite Calico Hills formation.

Just a couple of interesting points that I've pulled out of some of the literature, the hydrologic-related literature. We see that going from vitric, non-welded Calico Hills formation, to zeolitic, the saturated hydraulic conductivity decreases by two to four orders of magnitude, and that depends a little bit on who you read. It's a little bit difficult to get a firm number from our literature.

The average porosity decreases just slightly, from about 37 per cent to 29 per cent, and there's actually some overlap, and as I mentioned earlier, this is a significant difference in the nature of the water reservoirs or the nature of the porosity in the vitric and the zeolitic materials.

In the zeolitic tuff, which contains primary zeolite, clinoptilolite, we've got about 29 per cent porosity, so we have .29 grams per cubic centimeter of water in the pores. Knowing what we know about the structure of clinoptilolite and how much water it can take into its structure, at under essentially 100 per cent relative humidity conditions, we would have about .26 grams per cubic centimeter water in the clinoptilolite, which is held, variably held, but in any case, much more strongly held than the water in the pores, so it will react on heating much differently than the water in the pores, and this contrasted significantly with the water that's held in vitric, non-welded tuff, which is all in the pores.

So you can see that, in fact, we have the potential to hold more water, of a greatly different nature than the zeolitic tuff, so any time we change from the vitric to zeolitic Calico Hills formation, or any formation, for that matter, at Yucca Mountain, will significantly affect the nature of the water reservoir.

And, finally, we've seen a little bit of this in some of the earlier papers on hydrothermal or geothermal systems. We have, also, experimental data obtained primarily at Lawrence Livermore Laboratory that show us that vitric non-welded tuff, which is all of this material in pink here, when in contact with warm water, reacts relatively quickly to a zeolite and/or smectite assemblage. Of course, it depends on the degree of saturation, and if this unit is dried quickly and we don't approach saturation, then it's less likely that the reactive vitric tuff would alter to a smectite/zeolite assemblage.

So, in conclusion, I think the deep alteration system that we see in northern Yucca Mountain probably represents a saturated end member of repository-induced alteration. It provides information on the stability of clinoptilolite, mordenite, and analcime, and also of some of the silica phases, primarily cristobalite.

The vitric to zeolitic transition in non-welded tuffs going from here to here again, gives us some information on the types of physical property changes we might expect if, in fact, we alter the vitric tuff to a zeolitic tuff. We see that there are relatively small decreases in porosity, but significant changes in the nature of the water storage capacity. The saturated hydraulic conductivity, however, has a potential to decrease by two to four orders of magnitude.

Probably the most appropriate analogue that we have may not be the best analogue, but it's the best we have, to repository-induced alteration around this zone here, is the alteration zone between the Topopah Spring vitrophyre and the devitrified tuff. It may have occurred in a partially saturated environment. It was definitely of geologically short duration, because it occurred during the cooling of the Topopah Spring tuff, and we see that it was dominated by the preexisting fracture system. We see good evidence for glass dissolution and subsequent mineral sealing of the preexisting fractures.

Now, we don't really have a good analogue, as I said, for the rock mass right around the potential repository horizon, but we do know that there's very little potential for alteration of the fractured, but densely-welded Topopah Spring tuff. The phases in that rock mass are relatively stable. There is a potential for silica redistribution in the reflux zone, the reflux zone I think Tom has spoken mostly about.

There is also a potential for changes in permeability and porosity, and I say that based primarily on some experimental work done about ten years ago by Jim Blacic at Los Alamos, something he called the soak test, where we actually saw, after long-term relatively low-temperature alteration, significant changes in permeability and porosity.

Now, just to tell you where we're going at present, itemize a few things of future work, we're beginning to embark on a program in conjunction with some individuals in a couple of well-known eastern universities that you might remember, look at the kinetics of dissolution/precipitation of the silica polymorphs, including opal-CT, and this, I think, is important, because we really don't have a lot of information on that at present, and it's the dominant silica phase in the zeolitic tuff.

We're also looking at the kinetics of dissolution and precipitation of clinoptilolite, mordenite, and analcime. Both of these sets of experiments will be conducted from around 50☜C up to about 250☜C. Using the results of these experiments, we may embark, in the future, on some coupled transport and chemical reaction modeling exercises. I think that's really kind of leading edge work right at the moment, and it's something we'll have to develop.

And also, ongoing at both Livermore and Los Alamos are some experiments looking at the reaction of some of the existing phases in the zone around the potential repository horizon under partially-saturated or steam conditions, some of the sorts of things we heard about in the previous talks.

Thanks.

DR. LANGMUIR: Thanks, David.

One of the big concerns we have, of course, in evaluating the suitability of the site is the water budgeting that's going to go on if you heat the system, and you pointed out that in one section you have perhaps as much water in pores as you have in zeolites, but that's in clinoptilolite, which perhaps is not available until the temperature exceeds 100☜.

One thing we've not heard much discussion of, perhaps, by the modelers is the possibility that significant amounts of water would be added to the system and be involved in refluxion that is not part of the present pore budget; in fact, amounts that certainly would exceed any infiltration we might consider under fairly maximum rates.

What are your thoughts on what that might do? I'll expand this a little bit. If you're refluxing, might there be alteration that would take up this water, as well as release it in different parts of the system at temperatures below 100, or do you have to be above 100 for these sorts of mineral changes to take up water and release it?

DR. BISH: I don't think you have to be above 100☜ for this to take place, and something that's important to remember is that we may, in fact, not alter clinoptilolite at 100☜C. I think that's a long-term saturated thermal stability. How applicable it is in a repository lifetime remains to be seen, and it's something we can estimate from the results of the kinetics experiments.

We know in certain cases from other natural analogues; for example, Yellowstone, that you can go way above 100☜C and have clinoptilolite still be stable. I think the refluxing is quite an important phenomenon, and something that I'm interested in learning more about from Tom, and really getting some more quantitative information, because it'll affect the degree of saturation. Whether the rocks are partially saturated or completely saturated will have a tremendous effect on how the rocks alter. Partially-saturated or vapor-dominated systems will react very differently than saturated systems.

The interesting thing about the water in the clinoptilolite, also, is that because it is much more strongly-held, chemi-sorbed, you may call it, then the water in the pores, the water will remain in the clinoptilolite to considerably higher temperatures than the water in the pores that's physically there.

DR. LANGMUIR: More questions from the Board?

DR. DOMENICO: Domenico, the Board.

If that's true, how do you get some dehydration of those zeolites, if you say you're holding the water in the clinoptilolite, which I presume is a zeolite; is that correct?

DR. BISH: Yes.

DR. DOMENICO: And you're going to a different zeolite through changing the water content, but I would expect that at boiling you would start to produce some sort of dehydration of the zeolites. Am I incorrect here?

DR. BISH: No, you're not incorrect. That's a good point.

Some of the water will leave the clinoptilolite structure, but the important point is that water is held with a range of energies, because it's chemically interacting with the clinoptilolite structure, with the exchangeable cations, so that some water will leave, but essentially will remain at equilibrium with the water vapor pressure. The important thing is the water vapor pressure, so that we could easily go above 100☜C in a 100 per cent relative humidity, in a vapor-dominated system, and retain a large portion of the water in the clinoptilolite.

DR. DOMENICO: Is a back-of-the-envelope calculation possible on the potential for the volumes of water release from the zeolites as a function of temperature? Are such calculations possible?

DR. BISH: I think very shortly that we can do easily a back-of-the-envelope-type calculation. We're doing some experiments right now where we're trying to map out, essentially, the amount of water contained in these zeolites as a function of not only temperature--and we have pretty good data for that in the literature--but also water vapor pressure, and that's why the results of these modeling experiments are so important to us.

Just given temperature, we really can't provide an answer to that question, so we're going to try and go around the side of that question and essentially determine the amount of water that would be available no matter what the water vapor pressure. So we have to do a number of experiments at a range of water vapor pressures.

DR. DOMENICO: But you have no information on that at this stage in terms of--I've heard that there's possibly as much water in the zeolites as might be in the whole unsaturated zone. Does that make any--not quite, but...

DR. BISH: Well, you can see from those numbers that the amount, just in, say, if you had a saturated zeolitic tuff that contained clinoptilolite primarily, the amount of water in the zeolite is comparable to what would be in the pores if it was saturated, so there's a tremendous amount of water available there.

DR. DOMENICO: Thank you.

DR. LANGMUIR: Thank you.

I think everybody wants to go to lunch, and I will try not moving the microphone again.

I'd like to mention to everybody here, the restaurant upstairs has told us that they can handle everybody in the audience for lunch, so feel free to go up there, all of us.

We'll reconvene--we're going to try and catch up with our schedule--at 1:35, is our goal.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. LANGMUIR: Our first speaker is going to be Dr. William Murphy. This afternoon's session is titled Modeling of Yucca Mountain Under Thermal Loads. In this session, modelers will discuss their results and predictions for Yucca Mountain Repository and for comparable geothermal systems under different thermal regimes.

We will hear about predictive modeling of gas-water interaction geochemistry and of heat related fluid flow associated with Yucca Mountain Repository with analogue geothermal systems. My hope is that the modelers will identify essential data needs. They should also discuss the assumptions, uncertainties and limitations of their models.

I hope also that they will talk on the use and usefulness of information from unsaturated and geothermal analogues in model development invalidation.

Bill Murphy is our first speaker. Bill received his doctorate in geology from U.C. Berkeley. He currently is at the Southwest Research Institute in San Antonio, he's been there since 1988, where he does research and provides technical assistance in geochemistry and other earth materials sciences related to the proposed repository.

At the Center of Nuclear Waste Regulatory Analysis, he initiated and conducts major research projects in theoretical and experimental geochemistry and in natural analogues of the Yucca Mountain Waste Repository System. Other activities include performance assessment modeling and near field environment characterization and simulations.

Bill's talk is titled Gas-Water-Rock Geochemistry at Proposed Yucca Mountain Repository Under Various Thermal Loads, Relations to Fluid Flow. Bill?

DR. MURPHY: Thank you, Don.

As the first of the speakers on modeling, I want to say that--

DR. LANGMUIR: Bill, I'm sorry. Before you begin, I should have mentioned that your overheads will not be available to us until a little later on. So, again, be patient folks, they'll appear later this afternoon.

DR. MURPHY: As the first of the speakers on modeling, I'd like to preface my talk by saying that modelers typically should give two talks; one is all the nice products from their models that seem to give reasonable results and seem to give insight into the system, and the other products are all the things that are wrong with the model. And generally in talks like this, you talk about all the good things, and the bad talks you give at night between 3:00 and 5:00 in the morning. But there are a lot of caveats that need to be associated with the modeling that I'll present, and I'll try my best to illustrate the major caveats and the major uncertainties, and at the same time, try to emphasize what we may learn about the Yucca Mountain Repository System through these sorts of efforts.

I'd like to acknowledge Dick Codell and Chris Goulet particularly for giving me support in the work that I'll present here. And also I'd like to acknowledge the work of Los Alamos and Livermore and other groups working on the Yucca Mountain project. I'm a very strict and vigorous follower of the literature that comes out of those labs, and much of my thinking about Yucca Mountain is motivated and conditioned by the results. I was pleased to see Dave Bish's talk just prior to mine because many of the ideas that he brought up clearly relate to the issues that I'm going to be showing and talking about now.

Some of the key geochemical processes and parameters for the repository at Yucca Mountain are illustrated here. Don Langmuir, I think, and others mentioned the significance of geochemistry and its role in containment, corrosion processes, in source term issues, the degradation of the waste form, the solubilities of waste elements in transport, the hydrologic properties of the system and the speciation of radionuclides and the distribution of radioelements between phases that lead to processes of retardation. These are all critical issues that depend on the gas-water-rock geochemistry of the Yucca Mountain System.

Here are a lot of the parameters, and it's from the chemist's point of view, temperature, pressure and for materials, you can read chemical potentials of the components of the system. This is what defines the system in a geochemical sense, and the other issues, oxidation, flow, evaporation, vapor pressure lowering, these are manifestations of some of these changes in these conditions. And I'll try to address all of these issues to some degree in the talk that I'll give now.

My basis for being able to talk about gas-water-rock geochemistry is the modeling, the numerical modeling, computer modeling I've done that is very specific to the Yucca Mountain Repository System. And much of my talk will be focused on those model results.

But here's something of a summary of some of the effects that I think that will be important at Yucca Mountain. And at first, I had titled this the major geochemical effects, but then with last minute skepticism of a modeler, I said, well, these are the likely geochemical effects, and you can take them for what they are.

First of all, we've heard a lot about volatilization and redistribution of water, and that's a very important process, clearly, under the SCP design, thermal loading or under higher thermal loading especially. But in addition, CO2 is very strongly fractionated into the gas phase at elevated temperatures, and this will have a big effect on the water chemistry and Yucca Mountain, and I'll illustrate some of that.

One effect is that it will change the Ph. of the aqueous solutions, which will modify mineral stabilities and modify reaction rates in the system. There are metastable phases at Yucca Mountain. The primary minerals, feldspars and cristobalite, are metastable under load temperature conditions. They'll alter in aqueous solutions that they encounter. So precipitation and dissolution or calcite; I think this will probably be a major effect at Yucca Mountain because of its retrograde solubility, because of the volatilization of CO2 and the increase in Ph. and because the reaction rate of calcite with aqueous solution is relatively fast.

Sodium bicarbonate concentrations will be likely to increase due to mineral hydrolysis. Ion exchange and growth of clay and geolite minerals, which are secondary products at Yucca Mountain, may occur to a greater extent. There's the potential for quartz growth. They did show clearly what I call the mineralogic transition at Yucca Mountain, where below a certain level, you see quartz and cristobalite, there's a suggestion that the aqueous silica activity is lower below that level, and that has a major effect on the stability of high silica minerals such as clinoptilolite, an important mineral at Yucca Mountain.

Brine and salt formations, ultimately if waters boil away completely, the residual soluble components will be left as salts. Those may be phases such as sodium bicarbonate or sodium carbonate or sodium chloride ultimately, depending on the various conditions. And I'll show some results related to that.

Now, to give a background for the modeling that I'll talk about, first of all, I want to emphasize that I've done it in a staged manner. I've developed a model for the National Geochemical System that represents many of the observed features of Yucca Mountain at present, the water chemistry, the mineral chemistry. I've used that as a sort of initial condition for repository perturbations that are specific to repository thermal loading, that is, the increase in temperature, the redistribution of CO2 and the gas phase and so forth.

Finally, I've modeled a condition in which this water may boil to near completion, the water that's already evolved due to repository heating. The key system components are listed here, I won't read this list, but they're the minerals mainly that Dave talked about. The bulk chemistry is relatively simple. In fact, you can describe about 99 per cent of the bulk chemistry at Yucca Mountain by one feldspar solid solution and one silica phase. And so the bulk chemistry is relatively simple. Calcite gets involved or sodium chloride in the system that may come in from the surface in recharging waters, their common secondary phases, clinoptilolite and smectite.

This is a somewhat simplified model. I've cut out some of the minor components from the system. I've generalized some of the phases into general chemistries. The data in fact for the clinoptilolite and the smectites are highly uncertain at this point. We have an experimental program at the Center for Nuclear Waste Regulatory Analyses looking into these thermodynamic properties, but there's still much work to be done.

The natural model relies on a notion that the ground waters at Yucca Mountain and the mineral chemistry seem to compel a notion of recharging water that's initially charged with calcite. The surface of Yucca Mountain has abundant caliche. There's dry deposition of carbonates on Yucca Mountain. The altered minerals at Yucca Mountain tend to be enriched in calcium relative to the bulk of glass compositions. There's an indication of a metasomatism of calcium that I suspect may be due to recharging water. So the natural model I am making use of starts with something like a soil zone water that's charged in CO_2, saturated with respect to calcite, and then it reacts with the silicate phase assemblage to produce the secondary phase assemblage of zeolites and clays.

The non-isothermal model makes use of time-temperature relations derived from a convective peak flow model for Yucca Mountain. The initial conditions are based on the natural system model. Variations in CO2 are based on an independent gas transport and carbon system model that I did with Dick Codell, and I'll show one of the key results from that in a moment. And the reaction progress time relations are generated by identifying where there's likely to be great limiting steps in the overall evolution of that silicate aqueous solution system, that is, dissolution of primary minerals, notably cristobalite and feldspars in this model.

The evaporative model I've taken different tests. One possibility is if the evaporation is very vigorous, there may be a Rayleigh fractionation of CO2 into the gas phase. This can lead to very high levels of Ph. Alternatively, under a more gentle system, a continuous equilibrium may be established between the gas phase and the aqueous phase, and the aqueous phase may essentially be buffered by the CO2 concentration in the gas.

Vapor pressure lowering under very extreme levels of boiling stabilizes liquid water at higher temperatures than the nominal boiling point. And I won't get into that in much detail. There's some work underway by John Walton and others that was initially started in the Center for Nuclear Waste Regulatory Analyses, which is still in progress.

So chemical principles and the computational methodology I used to list it here. I'm not going to go through this in any detail. My main point is to acknowledge the key collaborators, Dick Codell, Chris Goulet. I'm making extensive use of the data base that's developed at Livermore and associated with the EQ3/6 software. Jim Johnson's noted, with many other people contributing to this, Hal Hulgason, Eric Volkers, many people at Livermore, Tom Wolery and others. This is for the carbon system model. The codes were some independently produced codes.

For the more elaborate partial equilibrium reaction path models that invoke the silicate system, I'm making use of the EQ3/6 software once again developed by Tom Wolery and colleagues at Livermore. I've modified the data base in this case in a couple key areas, but primarily relying once again on the DATAO.COM data base Version 16, which is, I believe, presently the latest released version.

So here's some result of the isothermal system model. The lines are the model results and the spots or the circles are ground water chemical compositions measured from the saturated zone in Yucca Mountain. One of them is everyone's favorite, J-13, is marked by a black spot, but that's only one in this spectrum of water compositions. All of these are plotted as a function of calcium molality; other major components, sodium, potassium, silica, bicarbonate concentration as a function of calcium molality.

As my initially calcium and carbonate charged water react with the silicates, calcium goes preferentially into the secondary phases and as a long reaction progress, calcium decreases in these diagrams. So read reaction progress from right to left in these diagrams.

One observation is that I think that the trends and the absolute values of these major chemical components at Yucca Mountain are relatively well represented by the geochemical modeling. This is not blind by any means. I tweaked some of the thermodynamic data. I tested many different steps of initial conditions and finally settled on ones that seemed to be realistic. But I think it's very important in developing models for the perturbed system at Yucca Mountain that it, in any case, your initial conditions and your initial model can give you some representation of what you see there now.

If you can't represent what you see there now within some bounds, even given major uncertainties, and I can talk about uncertainties for a long time, if you can't reproduce the initial conditions, then you'll have a hard time reproducing the perturbations for which you have much less control. So I'm taking this as my initial condition for repository perturbations, results of the models that led to the lines in this figure.

Now, I want to talk about one other condition on the more elaborate modeling, and this may be a little hard to read, but I think it's important. This is results from Codell and Murphy '92. And what's illustrated here is our results from a one dimensional carbon system, gas flow and reaction model. The reaction occurs locally. There's local equilibrium among the aqueous phase, the carbon and the gas phase, and calcite as the only mineral. There's no silicate system reactions considered in this model, however, there's some sodium in the system to make the water chemistries relatively realistic.

The flow part of it is one dimensional. It's an average uniform 1-D flow that is nevertheless transient with time. It's based on modeling that Dick Codell has done for gas based flow at Yucca Mountain. It's based on a nominal 57 kilowatt per acre thermal loading at Yucca Mountain, and basically it's an average upward flow over the center of the repository. You can imagine a one dimensional upward flow.

And what I've plotted here from the water table to the ground surface is the distribution of carbon in the Yucca Mountain system among the aqueous and the gas and the solid calcite phases as a function of time at 100 years, 500 years, 2,000 and 4,000 years. The dotted lines represent the CO2 in the aqueous phase, mostly as bicarbonate. Most of the carbons is in the aqueous phase. This dotted line here represents the CO2 and the gas phase. All gas transport is as gaseous CO2. There's equilibrium between the phases. And the solid line represents calcite.

The waters in this simulation were initially five times under saturated with respect to calcite, but due to heating, due to volatilization of CO2, a big plug of calcite precipitates right around the repository horizon and then gradually grows toward the water table and up toward the ground surface. In fact, the CO2 pressures I think in this model are somewhat too high. At lower CO2 pressures, you see a much more extensive development of calcite precipitation. This, I think, was likely to occur on a mountain-wide scale due to repository heating, but it's limited in extent.

The reason it's limited is that you can't precipitate more calcite than there is calcium available. There's very little calcium in the rocks to start with. There's a little bit in the water. And, in fact, it's limited; this really represents nearly 100 per cent depletion of the ground waters in calcium.

So now one of the questions to ask is to what extent do silicate system reactions, the alterations of feldspar or glass and the production of zeolites and clays, modify this general results about the redistribution of carbon in the system. Well, in order to address that, I've taken the results of the isothermal system model I showed previously--here it is--I've taken these results for a calcium contraction of three times 10 to the minus 4, used that as an initial condition. I'll combine that with the gas CO2 pressures calculated in the carbon system model for a point 75 meters above the repository horizon.

The emphasis is not to look at near field material, but how is the natural system going to respond to repository heating and redistribution of carbon in the gas phase. At that point, for this 57 kilowatt per acre nominal loading, the temperature follows a path like this as a function of time out to 4,000 years. So this is the temperature path followed as a function of time in my model.

The Co2 fugacity I've tested two different cases in order to give you some sense of the uncertainty in the analysis and some of the parameters to which the modeling is sensitive. A higher CO2 gas fugacity, which was the one that I showed in the CO2 model previously, and a lower CO2 gas fugacity, which I think is in fact more realistic, it corresponds more closely to the data from Thorstenson and others recently collected from Yucca Mountain. This is the CO2 fugacity in bars, temperature and degrees. Temperature goes up to about 80, and then over a very long time, slowly decreases.

So this is the temperature, time and CO2 relations imposed on my silicate system starting with the initial conditions from the isothermal model. And here are some of the results from that simulation. Once again, as a function of time, we see that feldspar dissolves. This is the amount of feldspar dissolved per kilogram of water. There's no liquid flow assumed here. This is a model for a static liquid system. The feldspar is dissolving in the water under either the higher CO2 pressure conditions or lower CO2 pressure condition. Smectite is one of the products that forms due to feldspar alteration. Clinoptilolite also forms. Calcite also forms, but calcite is limited to a relatively small amount, once again, because of the small amount of calcium in the Yucca Mountain system.

The Ph. goes through a gyration that's very interesting because in the near field, as the CO2 is volatilized out of the water, it exists as bicarbonate in the water, as that heats up, it's strongly volatilized into the gas phase, and that gas phase moves up into the mountain. There's a pulse of CO2 that rises in the mountain due to the initial boiling of CO2 out of the near field waters. And I predict that this will be the first surface manifestation of the Yucca mountain repository, is that there will be a small puff of CO2 enriched gases percolating out of the cracks near the surface of the mountain, or on the side, and that may occur within tens or a fairly short stint of years or a fairly short time after, perhaps hundreds of years after repository heating starts.

The Ph. as a consequence of this pulse passing up through the point 75 meters above the repository horizon goes through a transient dip, as the CO2 pressure goes up, the Ph. goes down in the solution, and then as that pulse passes by, the Ph. continues to go up as a consequence of the general distribution of Co2 into the gas phase and the hydrolysis of the feldspar minerals.

The time in this silicate system model is connected to the CO2 pressure, temperature, time relations in the previous slide through the kinetic relations that govern feldspar dissolution and cristobalite growth actually. Cristobalite can both grow and dissolve kinetically in this model, depending on the aqueous silica concentrations.

Here's one other result from this silicate, more general silicate system modeling, once again showing the variations in results that can occur in relatively modest variations in the sets of initial conditions. Each set of two lines represents the bicarbonate and the total sodium in the aqueous phase at this point 75 meters above the repository.

You see for higher dissolution rates, actually I've doubled the dissolution rate by doubling the surface area available to react to the feldspar and cristobalite. With higher rates and higher pCO2, we get these curves. With the lower rate, half the rate constant on surface area product and the higher pCO2, I get curves like this, and with the higher rate of reaction and the lower pCO2, we get bicarbonate and sodium evolution as a function of time out to 4,000 years that look like this. And you can see that the pCO2 really has a fairly strong effect on the variation in the water chemistry.

Now, to illustrate one aspect of the more evolved situation at Yucca Mountain, I imagine that somehow a packet of this water that's evolved for a thousand years at 75 meters above the repository horizon gets entrained in some water flowing down and it lands on the waste package, or on something that's hot down in the repository horizon at about 100 degrees C. Well, it's going to boil away.

What will happen as this water boils, how will its chemistry further evolve and what kind of conditions can that lead to? And here's one example of that, and there are a lot of assumptions here. One assumption I've made is that secondary phases are permitted to precipitate at equilibrium. So I'm not allowing large super-saturations with respect to secondary phases as this water starts to boil.

But one thing of importance is the waters are diluted at Yucca Mountain. They're 10 to the minus 3;

they're good drinking water dilute, tuffaceous aquifer drinking waters, and you can concentrate them by a factor of ten and they're still dilute waters, and you can concentrate them by a factor of ten more and they're still really pretty dilute water. You precipitate a little bit of silica and a little bit of calcium silicate perhaps and calcite certainly, but you can boil away a lot of the water before you see big effects on the chemistry.

So here we have the function of a fraction of this evolved water boiled, we've increased the concentration a small amount. The action really occurs in the very last stages of boiling of these waters. This is what the ionic spring does, you see once again out to .90 per cent boiled, the Ph. rises slowly. This is at 100 degrees C. But let's look at what happens as it gets more extreme. This is the part of the reaction between 99 and 99.6 per cent boiled. Then you start to see some action. Sodium goes up, the total carbon goes up, it's not all bicarbonate because the Ph. gets relatively high. The ionic spring finally starts to go up to high concentrations. It's only in the late stages that ultimately we'll see effect, major effects of strange chemistry due to boiling. And if the repository is dry, there certainly will be these effects in some areas.

An important result here also is that these results are sensitive to the CO2 pressures, and these calculations were done with relatively high pCO_2. Under relatively low pCO_2, one tends to evolve toward systems that are more sodium chloride dominated, and actually halite is a precipitated phase at the end of some of my simulations.

So now I'll try to address some of the questions that Don charged me to do, but I'm afraid I'm going to have to do it mainly as questions rather than answers to this problem. These are Yucca Mountain repository geochemistry relations to thermal loading, meaning increased thermal loading and fluid flow. While many of these issues were brought up already, very clearly earlier today and cogently, there will be a redistribution of water and CO_2 by the gas phase. This will affect the aqueous solution properties on a very large scale. On a mountain scale, the water chemistry at Yucca Mountain will be altered by this redistribution of CO_2 and H_2O.

Zeolitization of bedded vitric tufts could affect hydrolic properties. Based on Dave Bish's talk, I should say they will affect. How will they affect? Clearly, the bedded vitric tufts are the most vulnerable to alteration. How will their alteration happen and how extensive will it be and how will that affect hydrologic properties? Precipitation of calcite will occur all over the place, but not in huge quantities. And I restrict myself from talking about the near field. The grouts and other materials in the near field could make dramatic changes in this chemistry at Yucca Mountain. I'm talking about the natural system here.

Most chemical reactions, it seems to me, are likely to occur in the matrix because that's where the water is, and the water stimulates the chemical reactions. Whereas, most of the porosity and permeability that's important, particularly for gas flow, or even to liquid flow that's significant to repository performance, is in the fractures. Nevertheless, alteration, even minor alteration of fracture lining minerals may be significant for that fracture flow.

With respect to increased thermal loading, the time and space fields of all the things I described will increase. The boiling side or the drying side will go out farther. Calcite will precipitate farther away. Just the time and space scales will be increased. At very high temperature, decreased H2O vapor pressures, some of the hydrated minerals like the clays and zeolites might start to break down. And that issue was brought up earlier today.

And there could be important effects of high thermal loads on the near field materials, and I will not address near field materials now, but clearly they'll have a very significant impact on repository performance, once they start to get wet particularly.

So that concludes my presentation. Thank you.

DR. LANGMUIR: Thank you, Bill. Questions from the

Board?

I have one for you. You were talking about CO2 as a key here obviously of what's going to happen in terms of sealing of fractures and Ph. control. Presumably, there will be some exchange in CO2 from the atmosphere, and obviously it's a very open question now. Did you see CO2 in the mountain? Given the time scale of your processes, there will be a reasonable time scale for CO2 exchange, and how might that impact what you're suggesting with regard to calcite precipitation?

DR. MURPHY: Under ambient conditions at Yucca Mountain, there's clearly exchange of CO2 between the atmosphere and the ground gases and the ground waters. There's C-14 to depth at Yucca Mountain that's been measured, and I've done some modeling and Don Thorstenson's done similar modeling showing that it's consistent with diffusion of C-14 downward from the atmosphere. There is also the possibility that there may be mixing between upwelling gases. Gas tends to blow through the surface of Yucca Mountain, and also there's a potential for it to rise, particularly in the winter. There may be some competing effects of diffusion and upward advection, gas phase advection of CO2.

I think under repository conditions, and particularly under increased thermal loading, the advective transport of CO2 will be a really important component of the transport overall. The modeling, for instance, that Ben Ross has done and others, the flow velocities at Yucca Mountain, due to repository loading, are high, meters per year, or even tens of meters per year of gas flow velocities.

And so I think that what will happen is CO2 will get sucked into the mountain wherever that gas recharges, and expelled again wherever it discharges.

DR. CANTLON: John Cantlon, Board. What did you assume in terms of the length of time the repository would be open and there would be free atmospheric exchange through the ramps and tunnels and so on?

DR. MURPHY: I assumed that the initial conditions were similar to the initial conditions as they exist in the mountain right now. So I did not take into account the long period of emplacement and conformation and retrieval period and so forth. I think that may have an effect clearly on the near field in drying it and also altering the CO2 pressures there.

DR. CANTLON: Do you think that's going to be a significant element, particularly in that near field?

DR. MURPHY: In early times in the near field, I think that will be significant. I haven't done calculations to be able to speak very authoritatively about how long that initial perturbation will have an effect.

DR. LANGMUIR: One more. Langmuir, Board. We heard this morning from Joe Moore about the possibility in some systems at least of the fact of generating acid refluxion systems. What's the certainty that in fact Yucca Mountain system will be exclusively a CO2 dominated alkali system and could not become an acid system?

DR. MURPHY: The results of my model, as with everyone's model, are clearly a consequence of the things I put in. I did not put any acid refluxion in this model, other than the CO2 distribution. I think that the localization of HCL intuitively does not seem to be a very significant problem there. That's my intuitive reaction.

I think that with regard to other acids, such as sulfite, clearly bisulfite oxidizing to sulfuric acid, that's very unlikely because the system is completely oxidizing and there's very little sulfur there. I have not included that. I'll have to go back and scratch my head some more about possible HCL migration. There is chloride in the waters in low concentrations, and some sulphate too, but also quite low. I'll have to look at that more carefully.

One issue that I meant to address was that of the masses of material that can be precipitated. I don't want anyone to take these calculations to be the absolute definitive model, but taking them as an example, the total amount of feldspar dissolved in 4,000 years in my system amounted to less than 1 per cent of the total porosity. In fact, it was closer to a tenth of a per cent of the total porosity. And so the actual masses of mineral alterations that I've modeled here are relatively small compared to the porosity of the mountain.

Now, even small changes in porosity can have big effects on permeability if the precipitation is judiciously placed. So I can't go much further.

DR. LANGMUIR: Thank you, Bill.

We'll go on now. We're just a little behind schedule. Our next speaker is Dr. Karsten Pruess. Karsten has his Ph.D. in physics from the University of Frankfort, Germany. He's been at the Lawrence Berkeley Laboratory for 17 years now in the Earth Sciences Division. He's presently senior scientist and principal investigator on projects relating to Yucca Mountain to geothermal energy and to environmental remediation. And he, incidentally, was a major author, or perhaps the major author of the TOUGH program, which is the grist of all the modelers this afternoon dealing with transport in the fluid flow. So, Karsten?

DR. PRUESS: Thank you, Don.

I'd like to summarize for you some of our efforts to model and understand heat driven flow processes of the potential Yucca Mountain repository.

Emplacement of high level heat generating nuclear wastes at Yucca Mountain would generate a host of complex processes that would be played out in a very complex hydrogeologic setting. The complex processes include heat transfer by various different mechanisms. Liquid and gas phases would flow under different forcings. We would have, in addition, vapor-air diffusion with quite probably pore-level phase change effects and enhancements.

The fluid flow and the heat transfer would be strongly coupled. And if that isn't enough complexity, we also have to deal with highly nonlinear relative permeability and capillary pressure behavior. So just to do justice to all of these complex processes is a pretty tall order.

We have attempted to do a rather comprehensive modeling of these processes borrowing from geothermal and petroleum reservoir simulation methodology and developing and using the TOUGH and TOUGH2 codes.

The complex hydrogeologic setting, the watchword here is heterogeneity, which occurs on many different scales. The mountain basically is a layer of units with contrasting hydrologic properties. These units are tilted. We have large faults. We have fractures, fracture networks that are heterogeneous, down to heterogeneity on the scale of individual fractures, which represent heterogeneous porous media.

So, ideally, you would like to construct a model that includes all of the processes and all of the complex hydrogeologic settings fully and gives you all the answers, and I think that that is a pipe dream, not only with present capabilities, but I don't think we will ever see the day where that modeling of that all encompassing in nature would be feasible. And, instead, I believe we have to develop models that are very specifically targeting to capture specific aspects of system behavior and hope that by looking at a whole number of models which overlap between them, that viewing them all together, we can obtain a sufficient understanding to feel confident that we can predict repository performance.

Now, to set the stage, I'd like to briefly go over some modeling that we did quite a number of years ago on the waste package scale where the processes are most intense. In this particular cartoon, you see an infinite string of waste packages which we don't propose to emplace in that fashion, but this is simply for modeling convenience to get rid of end effects, so we modeled one infinite string of waste packages here, and we assumed that all these waste packages are intercepted by fractures at right angles, which are equally distant and just plain parallel.

Then we tried to, as a beginning into the complexity of the system, we tried to understand fluid and heat flow in this kind of a system. And the basic behavior that we see in our simulation is shown in this view graph here as the heat from the canister comes into the rock, we soon reach boiling temperature. The water will start to vaporize, the vapor will be driven away from the hot region mostly towards the fractures and to a small extent out in the matrix. But here is where most of the permeability is, so the vapor prefers to go down here, and then into these high permeability fractures, the vapor will be driven outward away from the heat source. And then at some distance, it will encounter the cooler walls of the rock and will condense.

The next important issue is what is going to be the fate of this condensate, and that depends critically on whether or not these fractures are able to conduct liquid water and ambient saturation conditions, ambient capillary conditions.

Parallel plate fractures certainly wouldn't be able to do that, but real fractures aren't parallel plate. They have wall roughness, they have numerous disparities. The issue of fracture relative permeability adds significant suction conditions and saturated conditions is a critical one to predict with thermal behavior and the moisture transfer behavior is an issue that has not been satisfactorily answered to this day.

We have assumed two alternatives here. One alternative, the fracture cannot conduct liquids, the other, it can. If it cannot conduct liquid, then this condensate that is formed here is being sucked into the rock matrix by capillary suction, mostly capillary. And inside the rock matrix, it's mostly sucked back towards the region of diminishing liquid saturation and increasing capillary suction here. But this back flow of liquid occurs to low permeability and is not able to match the outflow of vapor, so we have a net loss of water in the vicinity of the heat sources and a drying process takes place by which this whole pattern of vaporization and condensation and vapor liquid counter-fluid is migrating away from the heat source.

Now, if we assume as an alternative that the liquid is mobile in the fracture walls or in some fashion inside the fractures, then capillary suction will try and bring liquid back inside the fracture walls themselves towards the region of vaporization. And because now we have a much higher permeability here, this return flow of liquid is pretty soon able to match the outflow of vapor, and at that point, the drying process stops. It doesn't go any further because we have no further net loss of water in the vicinity of the waste packages.

The impact, as you might imagine, of whether this backflow of water in the fracture is possible or not is drastic. These are temperatures that we predict just outside the emplacement holes into the rock matrix, this is a logarhythmic time scale in years, we reach 100 degrees vaporization boiling point fairly quickly. And then if we assume that the liquid is immobile in the fracture, temperature continues to rise because the vicinity of the waste package dries and we get into a conductive regime with high temperature radiance.

In the alternative model, the liquid remains mobile, the heat pipe develops and extends all the way through the wall of the emplacement hole, and temperatures stabilize near 100 degrees C. There is some evidence from heater test conducted by Sandia and Lawrence Livermore that both types of behavior are possible.

This model of an infinite linear string of waste packages is of limited utility when you want to get a realistic engineering assessment of the waste package environment, but it is quite useful in a number of ways. For example, by making this idealization, this system has what's mathematically known as a similarity variable, radiance over square root of time. Even though this multi-phase fluid and heat flow behavior is governed by complicated partial differential equations, the relationship between space and time is such that everything that happens in this system only depends on the variable radiance over square root of time.

And by utilizing this remarkable feature, one is able to convert these partial differential equations into ordinary differential equations which can be integrated on any computer to any precision you want in just a few seconds these days. So this gives you something that is virtually as good as an analytical solution for a complex coupled multi-phase fluid and heat flow problem with no process nonlinearity is compromised in any way.

The only approximations are in systems geometry, and this is quite valuable for confirming the performance of numerical codes on which we would need to rely so much in performance assessment. And this shows, as an example, the behavior in an infinite stream of waste packages. Logarhythmic of the similarity variable, and you see temperature and pressure profiles, liquid saturation profile and air and the gas phase profile, and the similarity solution and the data points are the TOUGH2 simulation results, you see excellent agreement.

I'd like to shift gears now and go to the larger scale repository type processes. And before you can model a repository perturbation from the heat source, you have to obtain some kind of initial state, and that is easier said than done for reasons that have a lot to do with the numbers you see on this table here. And that's the extremely different time scales on which these interacting multi-phase processes occur.

For example, in the Topopah Spring unit, the fastest process for typical propagation distance of 1000 meters, just for comparison, the gas flow perturbation would be felt over 1000 meters in 207 days. And at the other extreme, liquid perturbation that you impose on the system would require over 200,000 years to be felt at 1 kilometer distance. These various interacting time scales make the system behavior very complicated, and they are a headache for numerical simulation, as you might imagine, because it's the fastest process that limits the time, and it's the slowest process that you need to obtain some kind of equilibrium for.

These numbers also suggest that the natural state at Yucca Mountain is, in all likelihood, not a steady state. These characteristic times are of the geologic changes and certainly climatic changes. So there is no reason to believe it is a steady state, however, we believe it should be a stable state in the sense that if you let it go, that it won't change rapidly relative to the time scale on which heat perturbations would cause it to change.

We have developed a number of complimentary two dimensional models that are patterned after stratigraphic sections that were developed by the Sandia group in the mid Eighties. This is an example of one of those sections from west to east. It shows the major hydrogeologic features, including the surface topography, major fault zones, tilting layers of alternating welded and non-welded tufts. This already is a schematic picture and highly simplified in a number of ways. For numerical simulation, we simplify it even more and look at something like this.

I guess the resemblance at least to the previous figure is obvious, but also that this is much simpler. We do have the alternating porous layers, welded and non-welded, and so on. We do have provisions for incorporating fault zones, in this case, the Ghost Dance fault can be represented as we choose to assign specific proper names appropriate for fault zones in this region. We have the tilting of the layers. So there is a reasonable amount of hydrogeologic site specific detail that this kind of a model has, and it actually could have a lot more, but we have intentionally designed these models to not exhaust our computing. We want to be able to add complexity to them as we understand more how they behave, and be able to learn more details about the mountain.

While this kind of an approach does a reasonable job to represent the site specific hydrogeologic features, a two dimensional section model is a rather poor model when it comes to modeling heat transfer because the heat is injected into a volume, and the question always arises, well, how much of total repository heat do you want to allocate to the section. So to not leave this up too much to just conjectures, we developed a complimentary model, which is also two dimensional, but only by virtue of symmetry of actually models of three dimensional volume of rock, and it imposes a radial symmetry around the C axis here.

The imposition of radial symmetry forces us to compromise some of the hydrogeologic features. You see we cannot allow the layers to tilt any more. We can also not represent fault zones. So it's a game of winning a few and losing a few and hoping that between several such models, you have enough realism, both for the natural system and for the man-made perturbations, to be able to confidently represent repository behavior.

Let me show you a few results from these models. Temperature just outside the emplacement hole is--this is for 57 kilowatt per acre thermal loading. You see we're peaking at 180 degrees C. at about ten years. The repository average temperature peaks a little over 90 degrees at a somewhat later time, forty years or so.

Just for comparison, we also ran a purely conductive calculation. That's represented by the solid circles and solid squares. And you see the temperatures agree within 5 to 8 degrees typically at all times, and that suggests that at least for these parameters that were used here, that if you're interested in the temperature field alone, the conductive calculation is quite adequate.

A thousand years after waste emplacement, and I should say in this fairly unsophisticated model, all of the waste was emplaced uniformly over the repository. After 1000 years, we have temperature patterns as shown here. The hottest known is just above 75 degrees C. enveloping the central portion of the repository. The repository actually extends to 1500 meters, and you see these end effects here. Away from the repository, the ambient geothermal gradient is non-perturbed at a thousand years.

This shows liquid saturations. You see a zone of partial dry-out around the repository, liquid saturation below 20 per cent. And the ambient liquid that originally was present here has been boiled away and been driven away by convective flow and to a large extent also by vapor diffusion and condensed, forming this condensation halo.

Now, in this kind of model, you have a very strong capillary gradient from the high liquid saturation toward the low liquid saturations. So if you would believe that this kind of a model is literally true, it would suggest that there is no way that water can ever flow through the repository. It's all flowing towards the repository. And if that were a fact, it would be wonderful because it would add to the waste oxidation capability, but I've seen such an inference it's not at all justified from this schematic model that it does not represent any smaller scale features such as water channels and so on. And I will dwell more on those things in a few minutes. I think this just gives sort of an average on repository behavior in a large basal average kind of a sense.

This shows gas velocity after 1000 years of 1 meter a year. And, interestingly, you might expect just a chimney effect to gas by just flowing upward, but you see that it's flowing upward above the repository horizon and below, it actually flows down. And this is an effect of the vapor air diffusion that leads to some subtle pressure increases at the repository horizon, which are just enough to overcome the normal thermal flow so that you have gas being pushed away from repository above as well as below.

This is a comparison of the simulated water saturation profiles. For the RZ model and the XZ model on the right, if you cut the figure up and try to superimpose, they actually superimpose quite nicely. So this kind of comparison gives us confidence that maybe these modeling results of either model have some meaning.

We have done a number of sensitivity studies, some of which are shown in this figure looking at different thermal loadings, 57 kilowatts per acre, then half that loading, twice that loading, and the different age of fuel here for 114 kilowatts. And you see that as you would expect, for higher thermal loading, temperatures rise to much higher levels and stay at higher levels longer. And it is this kind of behavior with temperatures well above the normal boiling point of water at 100 degrees C., this kind of behavior has led some people to suggest that it would actually be feasible to literally drive the vicinity of the repository out, make the water go away, and if the water goes away, then hydrologic problems go away and you don't even have to look at them any more.

Now, I don't think that that kind of a suggestion can be justified at all based on the sophistication of current modeling, and I would like to spend the rest of my time here showing you some analyses that suggest to me that even for very high thermal loading, you would not be able to ever gain an assurance that no liquid water can contact waste packages, even at early times.

And before giving you these various analyses, I'd like to state my conclusion up front, paraphrasing a famous quotation of a famous president, I think you can keep some of the waste packages dry all of the time and all of them some of the time, but you cannot keep all of them dry all of the time. So let me tell you why.

The obstacles against dry repository operation are of three kinds. We have thermodynamics obstacles that were already mentioned by Bill Murphy, and this morning by several speakers; vapor pressure lowering, salinity effects. We have infiltration, or we might have infiltration in the future that poses concern. And most important of all, I think, we have heterogeneity, which will lead to a tendency for water to flow in a channelized manner and to be ponded in numerous places, and water ponds could be released by various mechanisms and could contact waste packages. So let me amplify on these matters a little bit.

Vapor pressure lowering; when you have water inside a porous medium, it is subject to suction pressures which can be capillary in origin and stronger suction pressures would simply be absorptive in origin, absorption of a liquid phase on solid phases. And the stronger these suction pressures get, the higher temperature do you require to attain a certain vapor pressure. So the nominal boiling point of 100 degrees C. at which vapor pressure is one bar, if your suction pressure goes into the ten to the eighth pascals, or thousands of bars here, which I think is to be expected, then these temperatures go to 150, possibly to 200 degrees before you see the kinds of vapor pressures that would give you vigorous boiling. Salinity effects would amplify this, and they haven't been included in this particular figure.

Let's look at the interaction between waste heat and infiltration. This is a very simple back of the envelope type model for the purpose of which I assume a regular geometric arrangement of waste packages. So you have to view them as emplaced perpendicular to the picture plane here, so we're looking down in the repository, if you will. And so each waste packages in Area A associated with it, and then let us assume we have a certain net infiltration of so many millimeters a year, and then you can simply sum that up over Area A and calculate out how many kilograms per second of water then would come the way of one waste package from this particular infiltration.

And if you want to have an assurance that all of this water can be vaporized, that none of it can survive in liquid form, then you have to demand that the heat requirements of vaporization can be satisfied by the waste package heat output G. And so this is a very simple model. Some refinements can be made, but they don't change the main message which is contained in this figure here, which shows the output per waste package here on the logarhythmic scale starting at something like 3000 watts initially for ten years of fuel, and the time scale in years, and I've shown one curve here. At 2000 years, you get down, this can all be converted; the heat output into so many kilograms per second of water that you can vaporize or so many millimeters a year per unit of infiltration that you could vaporize.

And you'll see that after 2000 years, you're getting below 4 millimeters a year, and it continues to decline. So if in the 2000 year time frame you have a few millimeters per year infiltration, then even without invoking preferential water flows, just on an average argument, you can argue that the heat generation from the waste packages is not able any more to vaporize at all.

The strongest reason I believe, though, why one should expect that some of the waste packages will be contacted by water some of the time is the nature of water flow in unsaturated fractured media, which is governed by heterogeneity that occurs on various scales from layering to fracture networks to individual fractures which will lead to preferential paths. This is a very common experience from mining around the world.

One example from the Stripa mine, a 50 meter long drift was mined out with 3 meter diameter. 57 per cent of the inflow in this entire mine occurred over .2 per cent of the drift area. And this is not at all an unusual case. This is typically the way infiltration behaves. Now, this is in a saturated system. There were literally hundreds of fractures that intersected the drift and only a few of them carried water. I would surmise that in an unsaturated system, this kind of a flow focusing would be even stronger than under saturated conditions.

So the question then is, as I would surmise that water flow under natural conditions at Yucca Mountain would be channelized and there would be plenty of ponding going on in various places, can these water channels, all these water ponds, can they surmount the thermal effects from the repository as they're trying to flow past the repository, or would they all be vaporized, or would they be taken up by imbibition into the matrix.

In other words, even if you concede water flows in channels and it ponds and some ponds come down, can the thermal effects or the matrix imbibition mitigate this, or can they not. That becomes a crucial question.

Again, I looked at a simple model. The most likely region where water channels would have a chance of persisting is right in between waste packages, like here. And I modeled one symmetry element of this area of the extended repository with vertical waste package emplacement here just for simplicity of the analysis with a water channel in there. Now, the reality of course is the heterogeneity on all different scales that will generate these channels, but I simply defined a channel by giving it appropriate permeability and injecting water at some shallow horizon.

This shows the outcome of the simulation with full repository heating in place. Temperature near the channel walls goes, I have looked at two cases with impermeable channel walls and permeable channel walls; in either case, the temperatures near the channel remain near 100 degrees C. Liquid saturation goes down. You do have a partial dry-out here, but they don't go down very far, and it doesn't take too long, and liquid saturations go back up. So through this entire process, the channel just keeps merrily flowing along crossing the repository horizon.

Now, as long as it doesn't contact the waste packages, you might say, well, what's the problem. Here's another little back of the envelope analysis. How much water do you need to throw at a waste package to overstretch its capacity to vaporize this. Are we talking a bucket full or a bathtub full or a swimming pool full of water? And I looked at this for three different waste packages. This is the MPC waste package here, the big one. The heat capacities there were given to me by Gary Johnson from Livermore, and it is a simple matter to just convert this heat capacity into an amount of water that it can vaporize, assuming that you take this thing from 350 degrees C. to 100 degrees C. And this is the vaporization capability in so many kilograms of water that you can throw at it, or that translates into so many cubic meters.

Now, I think this is actually a worst case, because who says that you have to inundate the whole waste package uniformly. You could throw it at part of the waste package. But the upshot is that the amount of water needed is more in the range of a bathtub, a fraction of a cubic meter, which is not large, I think. I think there are plenty of water ponds naturally present that I believe will be encountered as it's being mined out.

But in addition to that, of course these generate huge amounts of liquid water through the vaporization condensation process, and that is shown here. If you assume that all of the heat liberated from the waste package goes into vaporization and, by implication, then into making condensate, this is the amount of condensate in thousands of cubic meters per waste package that you will generate over these times. And you see the condensate is counted in the thousands of cubic meters, whereas to inundate one waste package, you need a fraction of a cubic meter. So I don't think it takes too much stretch of the imagination to lead you to expect that this will happen at some waste packages sometimes.

Let me conclude. I think the current status of our modeling activities is sort of a good news, bad news type story. We're dealing with a difficult system of coupled multi-phase fluid and heat flows and a very complex and difficult heterogeneous hydrogeological setting with large range of space and time scales. Nonetheless, I feel that modeling capabilities are adequate for the processes that are being played out here, and that application of these capabilities has given us a basic understanding of the fluid and heat flow mechanisms.

However, I think the present models, and this should be said with equal emphasis if not more emphasis, the present models I feel, and not just Berkeley's models, but other groups as well, are quite schematic and approximate. They can only provide a rough outlook on repository behavior, and they shouldn't be over interpreted.

We lack quantitative information, especially on multi-phase behavior of fractures. We do need a more realistic representation of heterogeneity on a multitude of scales. And until we have achieved all of this, we have to interpret our model predictions with a great deal of caution.

DR. LANGMUIR: Karsten, you finished to the second. Do we have some questions from the board members?

DR. CANTLON: Given some caveats of caution here on using these, how much better do they have to be, in your judgment, to be the effective licensing models?

DR. PRUESS: Well, I think it depends on what you want to do with these models. If you want to have a model that you feel substantiates a waste package design that is predicated on the waste package never seeing any liquid water, I think then you have to work extremely hard because then you have to somehow deal with all possibilities of small scale preferential water flow and find ways to demonstrate either it won't exist, or engineering means to prevent this from happening.

If, on the other hand, if you're willing to design a waste package that can withstand dry as well as wet conditions and different Ph. and different oxidation states and so on, then I think you don't need to prove that waste packages remain dry, or you don't need to try to prove that. So then the demand on the performance assessment goes down.

DR. LANGMUIR: A related question. What about the size of waste packages? For example, if you just put seven packages per acre as opposed to twenty packages per acre, with the same overall thermal loading, aren't there significant consequences in terms of what the water is liable to get into the system and by it and in packages?

DR. PRUESS: Well, the more you localize the heat source, the harder you hit the mountain, you know, with the perturbation. And so you would increase the rate of vaporization, you would increase the rate of condensation and I think you would increase the likelihood of non-equilibrium type conditions. I don't think it is a make or break type issue. My personal sense is that once we get down and see the actual complexity of the natural environment there, that common sense will prevail and we wouldn't put waste package into weak zones and zones that are dripping, and I think that is the way these things will be dealt with rather than sort of putting them into premeditated positions and showing it's safe anyway.

DR. LANGMUIR: Thank you. We'll go on now. Thank you, Karsten.

You've probably already noticed that we have the same titles for all three speakers, including Karsten. The reason for that is each individual is very unique in his approach to it, as you'll find, so we're going to learn a lot from each person about his approach.

Tom Buscheck got his doctorate in geological engineering at U.C. Berkeley, and has been eight years with Lawrence Livermore Laboratory as a hydrologist in the transport group of the Earth Sciences Department. Currently, he's the task leader of hydrology in Livermore's Yucca Mountain site characterization project. And we've all heard from Tom. I gather, Tom, you've got a new talk now that you've written this on the overhead here. You've been so busy in the concepts that we're going to hear some more ideas today.

DR. BUSCHECK: I guess people are accustomed to the fact that we always like to use color in our talks, but today we're using temperature, and you notice how hot the room is.

Anyway, this has been stated several times, but I want to restate the obvious; critical hydrothermal or thermal hydrological issue for hydrologic performance is whether or not water can contact a waste package thereby accelerating its failure and possibly transport radionuclides to the water table.

I think it's also been accepted increasingly over the last several years that the only credible means of getting water to the waste packages and transporting nuclide is by non-equilibrium fracture flow. And that fracture flow could originate from two predominant sources in general. It could originate from natural sources of infiltration. It can also result from condensate drainage due to repository heat.

I want to kind of go over just for a moment a notion that if we have a dry site today and if the water is held by capillarity in the matrix, that if we just add a little bit of heat, it will be a little bit dryer. Well, the fact is that that's not true.

As I said, there are two ways of getting water to the packages. It either has to come from the outside through fractures; the other way is by heating the rock, and even subtle heating loads can vaporize significant quantities of water vapor where it could get out in the fractures and can condense. And so even low thermal loads could liberate a lot of water, and that's what I want to talk about today as well.

So, therefore, both boiling conditions can drive condensate flow, but we can also get buoyant vapor flow driven under sub-boiling conditions as well as boiling conditions. But under sub-boiling conditions, this can also cause significant quantities of condensate generation.

And another important point to note is that preferential pathways don't need to be connected to overlying meteoric sources.

While I was sitting there thinking, I thought I should redo this slide. There's really two questions we can ask, not three. First of all, and this is a subset for the second one, is it possible to manage the heat or to engineer it so that we can limit it and distribute it in such a way that heat doesn't matter, essentially that the impact of heat is negligible; is that achievable. That's certainly a worthy objective to pursue.

But on the other hand, if heat is important and if it's difficult to show that it's not important, is it possible to manage heat in such a way that its impact is less deleterious or perhaps beneficial for waste package performance. So that's sort of the second part of the second question. If heat matters, could we make it matter in a less deleterious way, or perhaps beneficial way?

I just want to kind of touch bases with what people have read and heard from in the past. Past calculations, you know, going back a couple years, have addressed, and we should underline, averaged thermo-hydrological performance, because we're using models that average the thermal load over the repository for the calculations. We do not look at detailed heterogeneity in the system. We haven't, in most of the calculations, looked at non-equilibrium fracture flow. We haven't emphasize also spatially variable heating conditions. And so these calculations give us some sort of an average perspective of what the performance can be.

However, more recent, and as Karsten pointed out, it's really difficult to get all the features you would desire in a single model. So we have to rely on complementary models and analyses. And more recently, we've been adding to our analyses to get out the impact of heterogeneity and non-equilibrium flow and spatially varying heating conditions. And so I'll be talking a little bit about that also.

Also more recently, we've launched on a very aggressive sensitivity analysis where we've looked at a broad range of thermal-loading parameters, thermo-hydrological properties and boundary conditions. And I would like to emphasize, based on a comment this morning, that we're spending over half of our time looking at cold repository concepts over the last half year. So scientific investigations are looking heavily at colder options. And we're doing this broad range look in order to identify distinct regimes of thermo-hydrological behalf or performance, and we have identified very distinct regimes and the threshold conditions when you move from one regime to another. And in doing this, we've also been able to identify critical dependencies.

We've also gone back, and realizing our models are always some sort of idealization, we're aggressively looking at analyzing the impact of the assumptions built into our models and how they impact our analysis and our interpretation of that analysis.

We've also been trying, through our understanding of these regimes, to develop some fundamental hypotheses which address the fundamental thermo-hydrological performance issues. And through this parameter of sensitivity look, we try to identify the conditions for which these hypotheses are invalidated. In other words, where does performance begin to break down relative to what would be a preferred behavior. And so what we're doing here is we're trying to understand what types of manifestations are indicative of less deleterious performance, and from that, we're developing a comprehensive strategy to test through a variety of tests, including in situ heater tests, and analyzing those tests.

I want to just maybe refer to my appendix. This is just simply the model assumptions. We're using Livermore's version of Karsten's TOUGH code. It's been developed over the last six or seven years. It's important to note that we're assuming equilibrium conditions between fracture and matrix, and that's what's meant by the equivalent continuum model. So you cannot get non-equilibrium flow in a lot of these calculations, and so that averages conditions.

We also have assumed that the units are horizontal and constant thickness. We've been looking at performance with an RZ model, which Karsten describes allows you to look at microscopic heat flow and the microscopic impact on the unsaturated and saturated zones. But we've also had a drift-scale model to look at the details in the near field performance. And initial conditions and thermal loading histories are as described there.

In addition to our large kind of averaged models, we've been recently developing ability to look at statistical variability in condensate drainage. And in a sense, what this is it's an analytical filter that we apply to our average calculations. We take the average condensate flux, put it through this filter, and recently we're using statistical data from the Stripa experiments on fracture statistics. And we assume that condensate return flow returns as a log-normal random field. It estimates the variability about the mean of this flux computed from these average calculations.

We have two basic approaches. One is to look at transient flow where you might have a sudden input of water from the ground surface, or upon release through something. And in that analysis, it should be emphasized if you have a climate change, it's not just the instantaneous heat output, but it's the stored heat in the rock that also affects the transient pulse of water.

And for focusing of condensate, we've also looked at steady state systems where the instantaneous heat output is more important and stored heat isn't, and what we've found is that, while this is very preliminary, but it takes something like a thousand fold focusing of condensate into a region in order to overwhelm the heat capacity in a local region. And that's not to say that it's not possible, but this model calculates the probability based on the fracture statistics that you get for the condensate hit and what the flux associated with that hit would be.

One of the important things that looking at a broad range of conditions is I think we've been able to map out the major means in which heat affects the hydrologic system at Yucca Mountain. These are the three major sources of liquid water in general. Of course, we have natural infiltration, and I'm showing this lifted off the page because a natural infiltration actually affects what happens within this larger box. But I'm also showing it separate because we don't even need any natural infiltration to generate water at the package environment. There's enough water in situ, and heat can drive enough changes where we don't need to have outside sources of water.

We've talked a lot in the past about boiling driving condensate drainage. But what has not been talked about until recent work that we've done is that buoyant, gas-phase convection can drive considerable condensate build-up and possibly drainage.

Now, for both of these effects, they occur at a small as well as a mountain scale. And what I view my small scale in the case of buoyant convection is local temperature conditions within the repository, when you have significant temperature differences which can drive small scale convection cells. It's very much dependent on what you assume for package spacing and package design.

When we get to a mountain scale problem, the mountain sees a large anomaly in the heat load, and the details of the waste package heating are less important. So, therefore, the areal mass loading, which is a global heat loading parameter, becomes important.

Likewise, for the boiling effect, small scale effects occur as long as we have sub-boiling conditions within the repository. And if you have a high enough areal mass loading, on average, you can completely elevate the repository to above boiling, and that's my mountain scale effects.

As far as what will be needed in site characterization, the small scale effects depend on near field properties; the mountain scale effects depend on properties over the entire scale of the mountain. And we also find that the time scales are critically different. The small scale effects, as I'll show, can persist on the order of a thousand years for the current SEP design, but for some designs being considered, it may last longer.

The mountain scale effect takes on the order of a thousand years to fully develop, and if the mountain scale buoyant effects are going to be important, it's going to be important on the scale of 100,000 years because it takes so long for this thermal anomaly to dissipate through the mountain.

For small scale boiling effects, you could engineer them to be minimized in duration by either putting in very low amounts of heat wherein boiling conditions dissipate relatively early on, or you could put in a lot of heat wherein you overwhelm the system and you have boiling conditions coalescing.

If you're in some sort of intermediate range, you may have small scale boiling and condensate effects for the entire time that you're above boiling. And I'm showing a thousand years for mountain scale effects. Generally, about 90 per cent of the total dry-out due to boiling occurs in the first thousand years. That's very important when comparing with some analogues and the time scales over which they may be pertaining to. And if these effects occur, the residual effects of dry-out can persist on the order of 100,000 years.

Just to show an example of small scale buoyant convection, and this is from an actual calculation done recently, the other one was my imagination before calculation, this is the real thing, and what we find is--and this is where you can go back and kind of work this out again for yourself--what we have is the cooler gas in the pillar between the waste packages. That cooler denser gas displaces the warmer less dense gas. As it warms up, its relative humidity is lowered, it therefore evaporates water from down here and is convected upward where it cools and it either goes in the rock and can possibly drain back to the waste packages. We've shown through the SEP spacing that this problem can persist on the order of a thousand years.

Now, given enough time and enough connective permeability in the mountain, it's possible to drive this same type of buoyant process through the scale of the mountain. And, again, the cooler denser gas comes in, displaces the warmer less dense gas and we get the same processes occurring. But in order for this process to be significant, we need to have large permeabilities over much larger length scale. And so I think it's much more likely that we're going to have significant permeability, at least on the small scale. It's yet to be determined whether we have enough large scale connectivity to drive this effect.

There's one additional physical phenomena that occurs, and that is as the lower unsaturated zone is dried out, imbibition from the saturated zone replenishes that water, and you can get a net build-up of liquid water saturation in the unsaturated zone over the time span that this occurs.

This is to show again the schematic of the small scale versus the mountain scale boiling effects. I won't go too much over this. But small scale effects persist as long as we have regions of sub-boiling temperature within the repository. The mountain scale effect occurs given a high thermal load where you generate just above boiling conditions throughout the repository. You could still have return flow of liquid water. That's not to say that that's not possible. But that's just to show the two scales over which these boiling effects can occur.

Now, I'll give some brief examples of mountain scale buoyant convection. Look at thermal regime. We found that there are different thresholds, and depending on the thermal loading condition, these thresholds have different values. For the SEP design, which is most frequently referred to about 50 MTU per acre, we find between one and five darcy, that the effect of mountain scale buoyant convection begins to dominate the performance of moisture movement in the mountain.

Now, I guess I should have mentioned this. These red areas show areas of net dry-out, not hot areas. The blue areas show areas of net condensate build-up. And you can see here from the gas phase velocity factors, that this cooler denser gas is coming in and displaying the warmer less dense gas and causing this convection cell.

At 5 darcy, buoyant vapor flow is dominating moisture movement, but the isotherms above ambient are still largely--which you would get from your conduction model. What we get on 40 darcy, we find that we're developing a chimney, and now that buoyant vapor flow is dominating the thermal performance as well as the hydrologic performance.

Just to give an example to show that this does not require boiling conditions, this is a 20 kilowatt per acre repository with a peak average temperature of 60 degrees C. We assume one subtle change in these two examples here. In this phase, we have assumed this value of permeability throughout the unsaturated zone, and in this phase, we've reduced the permeability to just 6 1/2 per cent of the mountain in the PTM where I've been told it's likely that it's much less fractured, if at all.

And so just by reducing the fracture permeability in this unit, we've reduced the magnitude of these heat driven effects by at least a factor of three. We find that the gas phase velocities at the repository horizon, which are these, again this is the gas phase flow field, are reduced by a factor of three just by virtue of this one unit up here having a reduced permeability.

We also find that whether or not that unit has tight permeability can either make for conduction dominated flow or a situation where a chimney exists. And so this type of effect of far field properties becomes important in the thousand year plus time scale.

At the early time, buoyant convection at the local scale would not care what the property of the Paint Brush tuft was. And I just want to show another effect, that we have an open convective system to the ground surface. When this becomes a vapor cap, you can see that we have a much tighter system, and as Bo showed this morning, when you have a more compressed convective field, heat transfer becomes much less efficient when you compress the interval over which that convective process occurs.

And one last point that I have is that if you have sufficiently high bulk permeability, you can substantially increase the saturation in the upper unsaturated zone. What I'm showing here in blue is the initial saturation distribution assumed from capillary equilibrium. And in the red is the perturbation, and this occurs at 10,000 years. And we find that around 1 darcy, we begin to pick up noticeable effects of the buoyant convection, and as we move up into the higher permeability, those effects can become very pronounced. And you can see that the dry-out attributed here does not match the amount of net build-up which is due to that imbibition coming up from the saturated zone.

This is not to say that this is likely, but what we're trying to do on our analyses is to look over the range of possible hydrothermal performance.

Now, Karsten also mentioned earlier how when you have the diffusion of air and water vapor, that you tend to oppose the buoyant system. And with dry boiling conditions, you do it even more so. This is the 114 kilowatt per acre case out 101,000 years. And what we find is is that this gas velocity field, whereas before with the sub-boiling case, was sweeping up through the repository horizon, the flow of gas is dominated by the temperature distribution within the boiling region, and it actually suppresses and opposes buoyant convection for some long period of time, as we're showing here.

This is just to show the effect of that vapor movement on the saturation changes. Again, this is average conditions over this model. We're not looking at the effect of local heterogeneity. And as I stated earlier, about 90 per cent of the total dry-out occurs in the first 1000 years, which when you're talking about the effects of heat pipes and the like, the vast majority of those effects will occur over that time frame. Whereas, with buoyant convective effects, they could last for 100,000 years.

This is just when you're going through your booklet just so you can correspond these conditions to the three primary zones that we find. We had a single phase above-boiling zone based on this average model, a two phase zone and a condensate zone.

We've also looked very extensively at the effect of buoyant convection, single phase, liquid phase buoyant convection in the saturated zone on thermal hydrologic performance. The interesting thing that we found is when you look at the actual magnitude of the effects with respect to the liquid phase velocities, whether or not you have a relatively hot situation, 114 kilowatts, or in this case, 20 kilowatts per acre, the velocities are largely the same. And the difference is is that even though you have locally much higher temperature build-up here, the fact is that the volume changes that are associated with that temperature build-up are integrated over a much larger area. So if you're placing the same number of MTUs of waste, the effect overall, the driving force in the saturated zone is largely the same.

We also found that over a relatively wide range of conditions, that heat flow, in spite of this quite interesting effect, still is conduction dominated. And the other effect is is that we're looking at something on the order of 1 1/2 kilometers per year of fracture flow. If you were to impose ten to the minus three regional hydrolic gradient to the same model, you would only get 60 meters per year.

So this model does not account for lateral flow. Had we, though, that lateral flow probably would have been of secondary importance relative to the thermally driven buoyant flow.

Just to touch on the sensitivity of performance over some range of conditions, we clearly know that repository temperatures or temperatures anywhere throughout the system depend very much on the thermal properties and boundary conditions of the system.

I'm showing three examples here; one in which we modeled the saturated zone as part of this system, one in which we modeled the saturated zone, the water table, that is, as being a fixed temperature. We find the duration of boiling is cut in half if you make that simple assumption of the water table, drastically affecting thermal performance.

If you were to assume that the entire unsaturated zone with that fixed water table assumption is all comprised of TSW2, you reduce the duration of boiling by another factor of two. So you get these two effects; you get a four fold change in the duration of boiling. So, obviously, what you assume about your thermal boundary conditions and thermal properties greatly affects your thermal performance.

Now, we've also looked at a range of bulk permeability, and my preference would be to go up to 100 darcy, but it's a very intensive calculation to run these high thermal loads, very very high permeability. But over in the six order plus order of magnitude range, we find the duration of boiling was found to be insensitive to this change of bulk permeability. But we did have effects in the near term, which I'll show in a moment what they result from.

I'm just going to show the upper curve and the lower curve, 2 micro darcy and 5 darcy at the time that the peak temperature perturbation occurs, which is at 600 years. And that's when temperatures in the repository also peak. The blue curve is the 1.9 micro darcy, indicates there are no fractures in the model. We have virtually no blatant heater dry-out effects, and 100 per cent heat flow by conduction.

In the 5 darcy case, buoyant convection is so active that 100 per cent of the steam is flowing upward, and you can see what effect that has on the temperature profile in that high permeability case. It literally translated the boiling point by over 100 meters here. We're also calculating a heat pipe zone of about 116 meters in vertical extent. So if I had covered this, you would see definitely that the convective processes are dominating the temperature profile in this example.

Now, if we go out to the end of the boiling period, we find that they both boil for the same period of time. And that gets to my next point, which is there are a couple things to consider when considering the repository temperature profile. Heat pipes definitely have an effect, but when you consider the conservation of heat within the above boiling region, in other words, what governs heat transfer from the above to the below boiling temperature region, that's either heat conduction or buoyant convection. You cannot have heat pipes where you don't have boiling conditions.

Inside here, you have these two effects, plus the heat pipe effect. So what we find is is that the above boiling region, the duration of time that it's above boiling depends on how important large scale buoyant convection is. The effect of the heat pipe affects the details of the temperature profile within the above boiling region. That's not to say that you can't get this thing into the waste package horizon. But as far as the duration of boiling is concerned on average, it's really concerned about heat transfer across this boundary where the heat pipe does not affect that particular process.

Karsten was noting the large time scales with respect to gas flow, liquid flow, et cetera. Now, again, using these models, some of these time scales come out in terms of the long-term thermal performance. The rate at which heat is conducted, the thermal diffusivity of the system is much higher than the wetting diffusivity or the hydrolic diffusivity of the rock matrix.

You can see these relative time scales on the duration of time that the conditions stay above boiling. First is the re-wetting on an average. What we're plotting here is the vertical extent of the boiling point isotherm in red, and this is, again, the water table and the ground surface. And I'm sorry if I'm leaving out these details. In blue, we're showing the nominal dry-out re-wetting front, and we're showing that at the center of the repository, you have some regions of above boiling conditions on the order of 11,000 years.

On this longer time scale, you can see that the re-wetting takes on the order of 100,000 plus years. The time scale, I think, for gas flow is on the order of, what, 200 days, and for liquid flow is over 200,000 years.

So depending on what processes are dominating the re-wetting, if it is liquid phase flow coming back through the matrix, you have, in effect, hysteresis. The dry-out is largely a gas flow process, and it's true that flow is returning through the fractures, but the re-wetting process, one of the reasons why we find that the heat pipe effect is dissipating after about 1000 to 2000 years is that the point at which you have boiling conditions is retreating more quickly than the average saturation conditions can follow back through the matrix. So the only water than can come back to this refluxing front is that water which is returning back through the fractures, just a slow fraction of what was originally moved out of the matrix in the first place.

In terms of model validation, and I'll just go through this very briefly, this basically talks about the process, the scientific method that most of us use. And in this case, I'm referring to it with respect to hypothesis testing.

We first use our models to obtain a better physical understanding of the system. And I think what is also important is that, you know, one of the bottom line issues that we need to predict about performance, that's not to say that the coupling issues aren't important, we have to also keep in mind what we ultimately are trying to predict. We utilize this understanding about what's important to formulate fundamental hypotheses which are the basis of our conceptual model, and what I term the performance attributes of the system.

And then we perform analyses and experiments to attempt to test or to invalidate our conceptual model or hypotheses. And then we modify those conceptual models and hypotheses on the basis of those results. I just say that to introduce some general hypotheses that could, you know, possibly be changed. But at this point in time, these hypotheses pertain to an above boiling and a below boiling. Actually, Items 1 and 5 pertain to below boiling.

The fact is is that we have a reference design right now, and that reference design could have a boiling period in excess of 3000 years. There are thermal loading conditions where you can get up to 77 MTU per acre based on the SCP design.

We recently analyzed 58 MTU per acre that boiled for 2600 years. So these fundamental hypotheses don't apply to extended drive. It applies to our base reference cases right now.

The first thing that we have to ask is whether heat conduction dominates overall heat flow. If heat conduction dominates heat flow, i.e. buoyant convection, we're going to have, I think, a much more challenging time showing that we could do our predictive modeling of hydrothermal performance. If conduction dominates heat flow, then it's dominating properties which would be more readily quantified and measured.

And then the second question we could ask is if this region of above boiling temperatures, whether it would correspond to the absence of mobile liquid water. We're not saying that there will be 100 per cent dry-out in the matrix. But I think a predominant concern is whether that water is mobile and can get on the waste packages.

Then we ask the question whether these processes are sufficient to promote dry-out, long-term dry-out. And basically the first two hypotheses pertain to whether or not you can use this region of above boiling temperatures to argue that there are fewer waste packages perhaps on average that are wet than would have occurred had you not used above boiling conditions.

Then taking the fact that we have hopefully reliably predicted some average region of dry-out, the fourth question is how much does this re-wetting ride behind the above boiling region. There's some indications that this could even last on the order of a half a million years. What we're finding is that this re-wetting process is actually, in our calculations, dominated by buoyant convection because we're bringing water up from the lower unsaturated zone to the upper zone, and actually accelerating the rate of re-wetting.

If large scale buoyant convection isn't that important at the mountain scale, this re-wetting could actually take longer in time. Now, we have to impose on this uncertainty about the infiltration and coming back into the system, and so that's not to say that it's simple, but basically we're trying to develop a strategy wherein we could tie a lot of complex issues into some higher level issues.

And the fifth point which basically applies to all of our concerns is whether mountain scale buoyant, gas phase convection may eventually dominate moisture movement in the unsaturated zone. We feel that in situ heater tests at multiple locations are required to test these hypotheses, and I would add to that, you know, the use of analogues.

Getting on to the conclusions, basically we have looked at a wide range of conditions. We found that mountain scale repository heat driven, buoyant vapor flow may possibly substantially alter the flux and saturation distribution in the unsaturated zone for tens of thousands of years. And this can affect both the saturation and flux conditions in the vicinity of waste packages. In effect, that change in saturation acts as a filter upon which natural infiltration will occur. So it will also affect natural infiltration.

Given sufficiently large permeability in the unsaturated zone, buoyant vapor flow could actually cause the saturation in the upper half of the unsaturated zone to approach 100 per cent. And this can occur whether or not you have boiling conditions.

We feel that large scale in situ heater tests, and what we mean by large scale is not single heaters, we mean on the order of 20 heaters placed in multiple drifts, need to be conducted both under sub-boiling and above boiling conditions in order to test any thermal loading strategy we can imaging, including any cool strategy, the reference SCP thermal load or higher thermal loads which have the potential of generating extended dry conditions.

We've also found that the size and duration of these heater tests are independent of what thermal loading strategy may be eventually established for the system. And I'm showing this earlier slide that I had, and in order to adequately diagnose the potential for mountain scale effects and to understand boiling conditions over the scale of the mountain, we definitely have to drive both boiling heater tests.

If we're to have just a cold repository, these above boiling heater tests are required to get a significant signature due to mountain scale convection. To look at the small scale effects, it's important that we run thermal loading conditions in the heater tests under conditions which are directly applicable to the repository.

So, in this case, we have marginal boiling conditions where we have both above and below boiling conditions within the heater test. For this test, we would run strictly under sub-boiling conditions so the effects of boiling wouldn't obscure our understanding of sub-boiling point convection at the small scale.

Then also we've found that hydrostratigraphic units such as the Paint Brush tuft, which are found to have a substantially smaller bulk permeability, can act as vapor caps. And this Calico Hills can do the same thing. We more recently looked at calculations where the Calico Hills had a restricted bulk permeability, and it too can greatly restrict the amount of buoyant vapor flow at the mountain scale. And just this one unit alone can limit the vertical extent and magnitude of repository heat driven saturation alteration.

And my feeling is that the role that the Paint Brush may play in limiting these types of hydrothermal repository heat driven effects may prove to be more significant than its impact on attenuating natural infiltration. So there's a lot more about the Paint Brush, which I think is exciting with respect to long-term performance.

We've also found that the development of a large persistent region of above boiling conditions can suppress these mountain scale buoyant vapor flow effects for thousands of years. And, also, this large dry-out zone substantially reduces the potential for buoyant vapor flow generating condensate flow at the repository horizon.

Thank you for letting me run over.

DR. LANGMUIR: Thank you, Tom.

Questions from the Board?

DR. CORDING: Tom, Ed Cording, Board. In regard to the re-wetting at very long times, are you looking at that at a mountain scale model, and is there a heterogeneity problem and a local fracture problem that could cause those assumptions to change? Just like we're talking about the short-term, is it possible that you'd be getting the water coming right down through, and then slowly coming back into that dry-out zone?

DR. BUSCHECK: Well, I think some of these small scale effects are not going to guarantee that all packages are going to be dry all the time certainly. But at the same time, if you have ponds shedding through the dry-out zone and around the dry-out zone, you'll actually have less of a condensate build-up above the repository horizon than would have been predicted with the model that averaged out thermal loading conditions. So long-term performance could arguably be--you could actually, on average, re-wet more slowly if you have persistent non-equilibrium flow.

I think I didn't show enough about this, but the large scale buoyant convective problem I think is very important. And if we could dry-out on average a large volume of rock, you have these driving forces independent of whether you dry out rock. I think it's advantageous to remove liquid water from where these buoyant convective cells are operating. So there's a residual effect that may be rather subtle that these effects which could occur after boiling, could be mitigated if you remove the water on average from this system.

DR. CORDING: Just going back to that last part, though, regardless of how much water you have there at the time the re-wetting starts and you're starting to collapse, could the collapse be relatively unstable locally and not the way you're describing it if you started looking at the details?

DR. BUSCHECK: Well, the fact is is that, and I don't have a slide to show this, is that most of the water when the boiling zone collapses, some of the water stays in the fracture, but in time, that water is incrementally imbibed into the matrix and you no longer have heat driving water out of that matrix by boiling. So once you have this dry-out cell and you no longer have boiling conditions, that water is held in the matrix, and so it's much less likely to be subjected to instabilities if it's held by capillarity in the matrix. You still have the concern about natural infiltration coming through the system, and there also needs to be a lot of work done regarding the thermal effects on the hydrologic properties of the matrix. Do we substantially increase the permeability? Because that will reduce the re-wetting time.

On the other hand, we're not including capillary hysteresis in these calculations and we found a 20 fold delay in re-wetting when we actually used hysteretic data. And so there are a variety of effects which aren't included which may either make it longer or shorter.

DR. LANGMUIR: We need to cut it off I think at this point, Tom. There will be an opportunity to revisit many of these topics in the panel, and I intend that we do so.

Our next presentation is by Eric Ryder. He received his degree as a mechanical engineer from the University of Florida. He's been an Sandia National Laboratories for three years in the Nuclear Waste Repository Technology Department. As a member of the technical staff in the Performance, Assessment and Applications Divisions, he conducts analyses in the area of thermal design. Studies include the evaluation of diverse thermal loadings within potential repositories, estimations of repository area requirements, and evaluations of waste emplacement ceilings.

Eric, with the same title, Numerical Modeling of Proposed Yucca Mountain Repository Under Various Thermal Loads.

MR. RYDER: A very common title, isn't it. I'm becoming quite fond of it, in fact.

By way of explanation, let me just start by saying that under this generic title, what I'll be talking about are how specific thermal modeling assumptions impact our predictions of temperature profiles, and why it's so important to keep the assumptions firmly in our minds when we make conclusions regarding this.

Specifically what I'll be talking about are assumptions regarding heat source representations and material property representations within models. Under the heat source representations, we'll be looking at some results for models that explicitly account for each individual waste package as a heat generating body, and compare those to when we actually smear all the heat generation from the repository into an areally extensive plate.

In terms of material properties, I'll show some, I think one or two examples of comparisons between when you model the mountain as a homogeneous material, as a single material, or when you take the approach that we just saw and model it as layers of homogeneous materials, and then briefly I'll touch upon some work that's being done and hopefully will be pursued in the future regarding spatial heterogeneity and how it might impact thermal profiles.

So starting with the heat source representations, the model that I'll be showing you in just a moment is called a discrete source model. And what it does, it's based on an analytical solution to heat conduction equation, heat generating right circular cylinders in a semi-infinite media. In this particular model, we had about 31,000 spent fuel packages explicitly modeled, and a little over 13,500 defense high level waste packages also modeled.

Just by way of explanation, the little brown region is our familiar pork chop. The light blue rectangles are place holders for where the spent fuel canisters are actually defined in the model. And the darker blue regions are, in this particular model, where we segregated the defense high level waste. The depth of burial was assumed to be 350 meters, with an areal power density or an equivalent areal power density of 80 kilowatts an acre, and the waste characteristics as shown there.

So what do we end up with? This sort of thing. Again, we've got not quite the same blue, but we have place holders for where the spent fuel is emplaced, and also darker blue regions for where the defense high level waste is placed. The red is, we're looking down on a three dimensional model, so the red part is actually the planned view of the isosurface with a 95 degree C. isotherm. And what you can see is that the major features of this repository layout, which is consistent with the one published at site characterization plan, persists in terms of the thermal profiles a distance, and also through time.

Specifically the main drift accesses that run down through the panels, you'll notice no coalescence of this particular isotherm across that, and you have very weak coalescence at 500 years and also later times between panels. And these non-heated regions correspond to access drifts, barrier pillars, thermal stand-offs, things like that.

Now, by comparison, and you'll see this in just a moment again, what if you take the heat generation of all the waste that's proposed for emplacement and assume it's one big plate or a disk or something like that. Again, just so we can have a direct comparison, the model is the same, same areal power density, the same waste characteristics and the same total energy going into the system. What do you get when you do that? You get that. At 500 years, you end up with a profile that looks like this.

Now, I've put the place holders there so I can actually do this in terms of overlaying, so we can talk about the features that we see. By virtue of the formulation of the heat source as a large plate, what you're doing is you're imposing through that assumption early and persistent coalescence across the major geometic features of a repository layout.

Is that a bad thing? Well, it depends on what you're looking at. If you're, for example, wanting to see the far field stress field, the large plate source would not represent what you're really looking for because it would assume that you have a very uniform heating up of the mountain. The stress is distributed in a very different manner than with these pockets of panels that are heating up. So that's a thermal mechanical example. Also in the thermal hydrologic regime, you would have different behavior than you would anticipate.

So the answer is to use discrete source model? No. Unfortunately, life is full of compromises. If you go with the discrete source model and represent each individual package, what you have to typically give up is the phenomenological couplings, the thermal hydrologic couplings. It just gets too big. It's impossible to solve, as Karsten alluded to. So what you have to do, I mean, the first bullet is certainly relatively obvious, the distribution of the heat source impacts our predictions of thermal profiles. And the fact that no single model can capture the complexities usually of two sets, you can either capture the complexities of the geometry or you can capture the phenomenological couplings. So what we have to do is actually take both of those, because both of those have merit, and meld them into one set of conclusions regarding the response of the repository.

And I also indicated that I would be talking about material property representations, another very large assumption that we go through when we do thermal modeling. These are the three typical approaches. The homogeneous one, nice big blue box, everything is one material, everything has one set of material properties, whether they be constant or functional properties. And the second approximation of that is where you actually take a slide, just like Dave Bish showed, and you assume that the layers are homogeneous, that they individually have property designations.

The next approximation is something that we're just working on now, taking site data and such. When you have the layering, but within the layering you have microstratigraphic, in that you have pockets of higher porosity or different property values, how that impacts the thermal profile or the hydrologic characteristics we predict is unknown at this point, but it is being looked at.

The first thing I'd like to do is just talk about a comparison between these two, the homogeneous and the homogeneous layered approaches. These results are based on a discourse model, non-linear conduction model that actually, in this case, modeled each layer according to the reference information base on the even contacts, and we looked at two loadings. The first is 114 kilowatts an acre and the second was 57, 30 year old fuel, and I believe it was 33,000 megawatt days as far as the burn-up went.

What you see here in these upper curves, which are the 114 kilowatt an acre case, is that for the homogeneous layered model, you end up with higher peaks in terms of the temperature and also longer durations of those profiles. This is very easily explained in that the repository is here and a TSW2 unit, you'll notice the column of conductivity, the values from the rib are actually up here on the view graph. Right below the repository, starts a section in terms of this definition where the conductivity goes down rather significantly. And what that causes is an increase in resistance very close to the repository that starts to show relatively early in time, here 400 years. So you get a build-up or a reflection of your heat source back, so there's a slight increase in the reflection. You'll notice the peak temperatures are slightly larger in terms of difference.

So, again, depending on what you're looking at, is this important? If you're looking at peak temperatures, the differences really aren't that much, so it may not be that important. If you're looking at durations of certain, say, boiling duration to protect packages, then we're talking, this is a log scale, we're talking 3000 years difference. That does come into play then.

In terms of this other, the spatial heterogeneity, what's been going on, some work by Chris Routman at Sandia, is they've been taking data from, in this case, neutron holes, and these are N54 and N55, which I believe are a little west of UZ16, there are relatively shallow holes, they're about 200 to 250 feet deep. Data is taken every foot or so, so there's about 200 sampling points in the vertical direction, and then in order to fill the geostatistics within between the two holes, outcropping information is incorporated. And what you see here, this is the Tiva Canyon member, and then the PTN unit is here. It seems we don't have a homogeneous unit. What we have is some spatial]variability as far as the porosity goes. And porosity happens to be one of those factors that most material properties are functions of.

Well, this would be wonderful to be able to model on this sort of detail, but I'm not going to try it, and what's also going on Chris and others are working on are adaptive gridding techniques to take that sort of information and put it into a format that we can actually use and try to evaluate the true impact, or at least a feeling for the impact of the spatial heterogeneity. And what you end up with from this particular simulation is something looking more like that. Again, we can see the variations of the porosity which were transformed into variations of property values.

So regarding material properties, obviously how this particular view of the mountain is represented influences our temperature predictions. Unfortunately, we haven't had an opportunity to take this the one step further and look at what its true impacts are. Does it make a difference at this scale? But I feel it's a very important aspect that must be assessed in the next step of the thermal modeling effort.

So just to tie things up, nothing very earth shattering there, predictions of hot rock thermal response are sensitive to assumptions regarding the heat source distribution, material property designations. This is nothing earth shattering. How important it is to your particular area that you're looking at, that's another issue. You'll have to evaluate that as you do your model.

And just to throw a little bit of a wrench in the monkey works, or money wrench in the works, whatever, the major uncertainty that we have right now, I mean, we can get our assumptions and we can caveat our conclusions appropriately, but as modelers, we have to be prepared for surprises because we have very little site-specific information at this point in time, so it's all best guesses and limited data. So I would say that the primary uncertainty in repository thermal modeling at this point in time is the lack of that data, and I'm looking forward to seeing it come out as site characterization proceeds.

DR. LANGMUIR: Thank you, Eric. Questions from the Board? From the staff? Leon Reiter.

DR. REITER: Eric, Leon Reiter, Staff. The last thing you said was that your primary concern now was the lack of data. But I guess I'm--some of the other people, that the modeling would be very difficult--

MR. RYDER: Agreed. I mean, you will have to do some averaging on some scale, like the adaptive gridding technique on the geostatistical simulation is an averaging technique.

I guess if you look at the rib and you go back into the information, where it came from, a lot of the information like the conductivities I've shown, a lot of it is based on theoretical functional relationship. So actually right now we are getting data in terms of from the NRG. A lot of it we can handle, a lot of it may not be able to be modeled, I don't know. You know, I think your point is well taken. We need better property values. We need better constitutive relationships between, say, from the mechanical side of it, a fracture behavior as it heats up. We need to know more about the silica phase transformations and how that impacts the property values as well.

DR. REITER: I guess what I'm getting at is that you could spend an awful lot of money getting data and the question is it's nice to have that data, but at what point do you reach diminishing returns.

MR. RYDER: That's a very good point. There is a minimal data set that's required. We must know the property values better than we do now. But there is a point, like you're bringing up, where data is not going to give you anything. So I agree with you.

DR. LANGMUIR: Eric, Dr. Langmuir, Board. You showed the repository in some detail. If one ventilates, if one's moving in or around the system and leaving it open, how does that impact your calculations?

MR. RYDER: In terms of the long term, the ventilation is a very short duration effect. I mean, if you ventilate for 50 years, you will remove some of the heat, but in the long term, I think Tom has shown that the areal mass loading will dominate and you will get profiles that are virtually equivalent. You know, the long-term profiles stay about the same. In terms of operation, retrieval, that sort of thing, I think it's important. You know, it's not just the heat; there's some water as well.

DR. LANGMUIR: You showed in one configuration where there was clearly accessibility to waste packages for some period of time. It was one of your earlier overheads. That was a fairly high load. What does it look like at 114 kilowatts if you go to extended dry configurations? You have a tough time arranging it to get in there and look at it.

MR. RYDER: Well, there's a couple things you have to keep in mind. First of all, this is the lay of the panel arrangement from the SCP, and the current arrangement would be with tunnel boring machines, it's more like a fish bone effect, if you will. So a lot of these features here don't exist. This still does, except for 114, you might be able to isolate all the waste on one side.

In terms of operations and retrieval, it becomes more difficult because the profile becomes more like a plate source, not like a disk. It's more of a rectangular source.

You still have edge effects, but you no longer have these weak coalescence features that you were talking about. So I'd say it becomes more difficult. It's more of a problem and ventilation becomes more critical.

DR. LANGMUIR: Is there conventional or traditional ventilation technology around that would allow you to go to the higher loading with the current configurations being discussed?

MR. RYDER: I don't know. That's not an area of mine. I'm sure there's someone here that can answer that.

DR. GERTZ: Don, we'll get you an answer to that tomorrow when we talk about integration of ESF and repository design.

DR. LANGMUIR: Thank you very much, Eric.

Our last presentation before the break and the panel is Bo Bodvarsson, wearing a little different hat. His title is Experience in Numerical Modeling of Geothermal Systems, a slightly different hat only. No pictures of geysers this time.

MR. BODVARSSON: Thank you, Don. I've been asked by Don and the Board to summarize some of the work that we have been doing over the last 20 years at Lawrence Berkeley Lab on modeling of geothermal systems. And as Karsten told you before, some of our numerical models were developed under the geothermal program, mostly in the Seventies and the Eighties, and we have borrowed very heavily from that development in our nuclear waste research.

I haven't been involved with the performance assessment, but I find it very pleasing to see, after these three presentations, that it seems like there is a general agreement about where the status is; first of all, that we need more data, secondly, that heterogeneity is very important and, thirdly, we're going to get some dry-out, perhaps not dry-out close to the repository. So it's interesting to see all these different approaches leading to a similar approach.

The experience in modeling of geothermal systems and how it relates to what we do at Yucca Mountain. What I hope to convey to you just in a summary is that when you have a complex numerical model and then you have variables like we have at Yucca Mountain right now, you can get a variety of different answers from your models, and you have to be very careful that you believe them because we have nothing to compare to. They are just numbered. We have just unproven hypothesis coming out of the computer. But I also hope to convey that from our geothermal experience, is that when we have substantial amount of data, the predictions you make are generally fairly reliable, as long as you can grid the model over substantial time periods. So that's what I'm hoping to convey very briefly here.

The way I'm going to do that, I'm going to talk about basically the objectives in the modeling of geothermal systems, the approach we use, the available data and the history matching we use, more importantly, the data we never get and we always have to assume, the uncertainties and limitations of numerical models, and I'll give you one example and then I'll briefly say the implications for the modeling of Yucca Mountain as my opinion.

To start then with the first, the objectives, what are the objectives? Most of the time, and I've been involved in modeling of some 20, 30 geothermal systems worldwide, what they always want to know is how big a power plant can I put on my resource. Power plants cost hundreds of millions of dollars. So if you tell them you can build a 200 megawatt power plant, then it gets you 50 megawatts, they're going to call you and it's not a nice call. So either you have to do a good job or you have to move very often.

You also want to guide in the development of the field because they also want to know how many wells to drill and how far apart these wells are. Very often the system, especially in the past, they used to drill the wells very close together so that they all stole the same fluids from each other, each one costing $2 million; with half the amount of wells, you would get the same amount. It was a very poor investment.

We also want to guide where to inject the waste water, because we don't want the pressure to go way down. We want to inject waters to maintain the pressure in the reservoir. And finally, of course, we want to predict how they're going to behave in the future so we can put them into our economic models so the company can count their dollars.

Now, final point here is that in nuclear waste, our ultimate objective is to predict the transport of the radionuclides from the canisters through the water table to the environment and to the air or wherever. That's a very very difficult task, much more difficult than some of these because some of these are much more--so it's much easier to do this kind of modeling.

Now, how do we do this? We have over the last ten to fifteen years, we have developed some kind of a methodology for looking at geothermal systems. And what we like to do first of all, and this is about the most important thing, is that you have to understand all the processes that occur in your geothermal systems because you have to be able to model these processes. If you neglect some of the processes, you may be way off. You want to develop a conceptual model of your resource that matches all the available data. The name of the game in geothermal as well as Yucca Mountain, use every single bit of data that you can because all of it is going to tell you something about the system.

So geothermal system is fairly complex. We have the boiling zone, the chemical precipitation, we have mixing, cold water recharge, precipitation around the injection wells, and you have force convection or you have natural convection, heat pipes, all kinds of things that you have to look into.

A very simple methodology that we put together is this. Again, consider all the field data that you can. Develop the best conceptual model of where the fluid flow is in that system, where the chemical precipitation occurs, how the heat transfer is, and all of that. Then you have to model the system in its natural state, that means before any wells were drilled. That means to say match the natural temperatures and pressures. Yucca Mountain means that capillary pressures and saturations as they are today. It's very important.

Then you can put in your production history, calibrated, well test data, and then you get a reservoir model after you do all calibrations. From that, you can predict what kind of power plant you can build, after maybe some conservative assumption and doing some sensitivity studies.

This looks very simple. Here's an example of an actual model that we developed for the Ahuachapan time field, a field in El Salvador. First of all, when you build a three dimensional grid, you have to be sure to take into account all the geochemical data, all the temperatures and pressure distributions. Then when you do the calibration, calibrating the flow rates, enthalpy, pressure changes, spring flows at the surface, all the data you possibly can. And when you have incorporated all of that into your model, then you can be confident enough to make some performance predictions.

This is the basic methodology. Now, available data, history matching, and remember the more parameters that we match, the more confidence we will have.

Most important available data; this is data that generally are available. Temperature and pressure distributions in 3D. Yucca Mountain, capillary pressure saturation distribution in 3D, and of course other things. Horizontal transmissivities, porosities, permeabilities of cores, flow rate, enthalpy and chemistry histories of all production wells, injection rates and temperatures, reservoir pressure decline, repeat gravity surveys. Repeat gravity surveys will tell you if you develop two--because the gravity is sensitive to the fluid mass in place. All of these are very, very important. You have to take them into account.

Problems; data deficiencies. This is common for almost all geothermal systems. Most important one is the first one. We don't know how thick our reservoir is; we almost never know how thick our reservoir is. We drill three kilometers down because that's about the economic drilling. We know it's that deep, but we don't know how much deeper it is. We don't know in place liquid saturation, for example, vapor dominated systems. Same at Yucca Mountain; we don't know what the saturation is.

Vertical permeabilities, relative permeabilities and capillary pressure curves; same difficulties as we have at Yucca Mountain.

History matching; this is where you really have to spend your time if you don't want to get a phone call. You have to be very careful to match all this data, including the natural state, the horizontal permeabilities for individual wells, for every single well you have to match the flow rate decline, enthalpies and chemical concentration, if you can. The chemical concentrations are extremely important, because they are the signature of where the fluid is coming from and how it moves from one well to another. Very important.

Then, of course, the pressure decline and repeat gravity surveys.

Now, let me give you an example. Again, the message is going to be this. If you have a lot of data from a geothermal field, you can do a good position. In the beginning, you're not going to be able to because you don't have enough data.

I showed the picture of the field this morning. This is a map of the Rift zone in Kenya. Here is Nairobi City. The Olkaria field is close to Lake Naivasha. This is the field. It's basically a caldera of 80 square kilometers in size, and here is the East Olkaria well field where we were supposed to evaluate if we could put a 45 megawatt power plant on this system. And I've told this story several times, but I always like it because it kind of tells people that modeling is not easy. You can get different results, depending on what you assume. I got this job from the Kenya Power Company because they wanted a loan from the World Bank, they wanted a $150 million loan from the World Bank, and they say you have to give us a report, you have to tell the World Bank this field can handle 45 megawatts.

Then I decided, okay, what I'm going to do is I'm going to do an optimistic phase of the entire caldera being the geothermal resource there, and I'm going to do a pessimistic case saying that the geothermal resource extends about 12 square kilometers around the well field, and hopefully this case, a small case, is going to allow me to have 45 megawatts for 30 years, and that's great, then they can show the bank that and the bank will give us money. And then probably I'll run this case here and I'll see that the field can handle 45 megawatts for 300 years, and the bank will be gloriously happy because they know the caldera is very big. That's how the plan was.

Now, I was very happy, like I said before, when I ran the first case, it lasted almost 30 years. That's great. So I went home happy that night, and ran the other case over night and came back the next morning. Well, too bad, the big caldera gives you smaller results than the small one. This is amazing, but this is the name of the game. Everything I'm inputting into the simulation was correct. But this is the complexity of multi-phase flow when you don't have sufficient information. It so happens that the characteristic curves I had of the total mobility, you actually got less from this lower surface. You have to be very careful. This is a good example to say that when you have very little information, don't believe blindly what comes out of the simulators.

If I plot up here later on the--this field, in the beginning there were two studies, one was the typical study that people do when they estimate the amount of mass in place, the hot water in place, then they use that and say I can produce 45 megawatts for 300 years, very optimistic because it neglects the permeabilities.

And the other case was a first numerical study of the field done about 1975, or something like that, it concluded it can only have 10 megawatts because it was very conservative. Now, at the time, we started modeling about up to four years here. With time, you get more and more exploitation history.

So, almost finished, uncertainties and limitations. Again, for geothermal, we have learned that the conceptual model is extremely important. I'm sure you'll find that about Yucca Mountain, if you haven't found out already. Missing data; we have to be very careful that we know the importance of the data that we don't know so well in the assumptions. That the history match data and the calibrations are done as carefully and as well as possible, and it also depends strongly on the modeler. I've seen many, many results I don't believe because the approach was done very improperly, in my view.

So the conclusion with regard to geothermal systems, I think when we have a reasonable amount of data, we can predict reasonably well how the system is going to behave over some decades. But when we try to model the chemical transport of geothermal system, it's very complex because some of the paths show when we inject tracer here, it comes first to the furthest well away. It's very difficult.

So implications for Yucca Mountain, same as I said before. I think it is very promising what we are seeing for the performance assessment modeling, that it seems from all these different approaches and the limited amount of data, they are coming to a similar conclusion regarding the predicted behavior of the repository. And I'm sure when we get more information, we'll be much more confident in what we predict for Yucca Mountain.

So conclusions; numerical modeling of multi-phase, multi-component systems is very complex, and unless we have some history matching, we can only look at them as unproven hypothesis, in my view.

Experience from geothermal modeling shows many examples of poor hypothesis, with power plants worldwide, some of them running at half capacity because people didn't wait for the calibrations, they didn't wait to be able to get that data to be able to calibrate their model because they wanted to make money very quickly.

Now, we believe, and I believe strongly that if the right amount of data is collected, we can very accurately predict how big a power plant we should put on the systems.

And current methodology I think is solid and can very well be applied to some of the Yucca Mountain modeling. And still we have some difficulties in the modeling of chemical and heat transport.

DR. LANGMUIR: Thank you, Bo. Questions from the Board?

DR. DOMENICO: Domenico. I've got a question, Bo. Of course we're not going to build a power plant at Yucca Mountain. The modeling of geothermal systems and this business has different objectives than, let's say, the modeling of Yucca Mountain, but we agree that the--you have said that you must incorporate all the processes, and we know that they're conduction free or convection, transport by heat types. I think people agree there will be some drying out, but not all drying out. But there's some other processes going on.

I think Tom has mentioned that there's going to be a transfer of water out of the saturated zone, whether you're above or below boiling. We've learned that zeolites contain as much water as some of the pores, and I suspect zeolites will break down again at low temperatures, depending on the activation. We're going to move one hell of a lot of water from below through the repository, from the repository sitting up above.

First of all, can you model such a thing? And the other thing is what questions should you ask? It seems to me that one important question is could you possibly inundate that damn thing? Would the fact that if the heat pipes are indeed effective in keeping the temperatures below the criteria, then could you possibly even inundate the repository when you're actually trying to keep it dry? I don't know if you can answer that, but what I think the point is is that there are an awful lot of other processes, like sources of water, that have not even been considered yet, and also everything that has been considered has been considered in a very simple geologic environment, the model environment.

So do you really feel that it's possible to model this thing in total?

MR. BODVARSSON: Let me answer that partially as well as I can, and somebody else can answer it. As Karsten mentioned before, some of these complex processes taking everything into account, would probably require that you use a somewhat more simplistic model. So you have to find some balance between the dimensionology of your model and all the processes that you consider.

So I think with the proper staging of your modeling going from maybe simple geometry and complex processes to complex geometries and only a few processes. And I think that's how the project is going. For example, I am doing the science scale model, as you know, with USGS, and what we have spent our time on is first of all, we have decoupled the TOUGH code to make sure we only have to consider water, and then we can of course go into gas and thermal later on. We are running with the best geologic information in 3D, 6000 elements and 20,000 connections and it's running. So it's very promising to me. I was very worried that we wouldn't be able to do it, but we are able to do it.

Now, building on that with how fast the computational improvements are coming, I mean, one year ago, I bought the IBM machine and it's a 12 megaflops machine. Now they are 75 megaflops. They are five times bigger in one year. I believe that we are going to be able to, with great confidence, model a lot of these processes.

DR. DOMENICO: Only one slide, Tom.

DR. BUSCHECK: I thought Eric had a new model for tying the heterogeneous thermal conductivity distribution. Anyway, what I'm showing here is the 48 MTU set of calculations, SCP design, and we've considered in this case 280 millidarcy up to 84 darcy, the temperature in the center of the repository and liquid saturation. From 380 millidarcy to 10 darcy, you can see the re-wetting of the center of the repository is very, very similar. You can also see the temperature behavior is very, very similar. But we cross the critical threshold between 10 and 40 darcy.

At 40 darcy, things were drier for some time because of the rigorous effects of all this buoyant--buoyancy can actually dry things out, but I think it's very dubious to rely on dry-out due to thermal buoyance. But what happened was that through thermal buoyance, we built up so much condensate above the dry-out zone that it came crashing down and you can see now that the subsequent re-wetting is going to 100 per cent. Now, I'm not saying that this is likely at 40 darcy throughout the unsaturated zone. I think a lot of people would yell at me for showing this slide. But what we're trying to do through our comprehensive study is to show that we can show conditions under which some of these things can occur. And the critical point is to run heater tests to see whether or not we can diagnose the probability that such conditions are relevant.

DR. DOMENICO: But the other point is you have not taken into account the other sources of water which may be just as great if not greater than that already in the unsaturated zone. You said you're going to mobilize water from the saturated zone.

DR. BUSCHECK: In this case, I would argue that over this time frame, that water has not yet gotten up to that. No, I'll take that back. Some of that water is contributing.

DR. DOMENICO: Well, that's what I mentioned when I said all the processes and all the sources of water.

DR. BUSCHECK: And I think taking a broad brush look at things, we can look at these scenarios. And what if in fact we find that we're in this millidarcy range, knowing that you're one or two orders of magnitude below where you have a problem I think could afford you some comfort. That's why we're going over a wide range of conditions. I think it gives you some comfort to show that you can drive a failure, that we're not avoiding looking for it. What if we had gone up to 10 or 20 darcys and we thought everything was just fine and hadn't gone beyond that point, we wouldn't understand what would happen in this additional regime.

DR. LANGMUIR: I think we're approaching the kinds of questions and answers that are appropriate for the panel to follow the break. So I'd like to ask Bo something, but I'll ask him hopefully as part of the panel process.

Let's take our break, and it's about 4:09, and come back in 15 minutes.

(Whereupon, a recess was taken.)

PANEL DISCUSSION

DR. LANGMUIR: Let's reassemble. This will be our roundtable discussion period on geothermal analogues and modeling issues. We need some panel members.

What we're going to do here is first I'll introduce very briefly members of the panel and I'll ask them to introduce themselves in a little more detail. Then I'll pose several questions which are derived from the day's presentations, and then I'll let it loose and you can take it the way you want to take it. And if the Board would like to chime in with additional questions, they can do so. Of course, speakers of the day need to be in attendance so that we can query you and bring your thoughts into the discussion.

What I'd like to do now is introduced simply by name and affiliation the panel members and let them tell you a little more about themselves.

John Bredehoeft, U. S. Geological Survey. Sabodh

Garg, Maxwell S-Cubed Division. Bill Glassley, Livermore. Bill Herkelrath, USGS. Carl Johnson, State of Nevada. William Melson, Smithsonian. Benjamin Ross, Disposal Safety, Incorporated. And Jean Younker, TRW.

Now, Jean has asked to start this off with just a short presentation here, maybe a couple minutes, on her thoughts. Not even that; 30 seconds?

DR. YOUNKER: This is Jean Younker. I think one of the things that we haven't had today as of yet in this meeting is any real discussion of the proposed assessment of this whole question of thermal loading and the various ways that we're going to attempt to make a decision about what kind of thermal load makes sense for the repository.

And so I think the one comment that I want to make, since Don did offer the opportunity, was that I think the thought that comes to mind for me, and I'll throw this out as something for us to maybe talk about during this panel period, is that when we say we have a performance assessment program, and we've talked about this a lot, if Dr. North was here he'd certainly want to join in this discussion, one of the things that we really have to think about is that we rest our more abstract total system type of analysis on the confidence and the amount of consensus we have in these lower level process models, the models that you've heard talked about today.

And so I think one of the reasons why as we go back through the years that we've done total system performance assessment type calculations, you know, we present them and then we kind of wonder now why haven't people taken these more seriously. Well, clearly part of the reason for that is that until we have some fair amount of consensus in kind of what we refer to as the base of our performance assessment pyramid of models and codes, such that there's a pretty good agreement as to, for example, what the best representation of the thermally perturbed environment is, then it's hard for us to abstract from that into a system, or total system model that really gives us some high confidence in major sensitivities on total system performance.

And, you know, when we say we have a performance assessment driven program, what we're saying is that we can run sensitivities of some sort that show us how these various options that are being looked at make a difference in total performance of the repository system.

Well, it seemed to me in listening today that we have to be cautious, and I'm not trying to throw a wet rag on this, but we just need to be cautious as we think about exactly how we can talk about performance driven program or performance assessment driven program, given or with the consideration of the kinds of discussions we've heard today and that we're probably going to have right now. So that's the only comment I really wanted to make, Don, and thank you for the chance.

DR. LANGMUIR: Thank you, Jean. Let's go around the table now and perhaps the panel members could comment just briefly on their experience as it relates to this panel and what they're doing right now. Ben Ross?

MR. ROSS: Do you want me to do that and then give a little--

DR. LANGMUIR: On your expertise that pertains to the panel.

MR. ROSS: Ben Ross. I'm the president of Disposal Safety, a very small consulting firm in Washington. We have been working for the last five or six years on Carbon 14 migration at Yucca Mountain, which is driven by the heat and, therefore, we've gotten very involved with the whole issue of temperature and have developed a coupled model of gas flow and temperature, heat transfer.

DR. MELSON: I'm a geologist; I've been at the Smithsonian for 30 years and am very interested in volcanic explosions and have seen a number of phreatic explosions. So when Gudmundur talked about some of these things, I knew what he was talking about. And so I'll want to ask him a bit more about the possibility of what we call phreatic explosions.

DR. BREDEHOEFT: I'm John Bredehoeft with the USGS in Menlo Park, California. I'm a ground water hydrologist, been involved in developing flow and transport models for a number of years and have looked at a lot of applications of those kinds of models to problems in hydrology.

DR. GARG: I'm Sabodh Garg; I'm with S-Cubed in LaJolla, California. For the past 20 years or so, most of my work has been concerned with analysis of data and modeling of geothermal systems. So in a sense, my experience is in problems that Bo Bodvarsson and Karsten Pruess. My only familiarity with a nuclear waste problem comes from having served on the National Academy of Sciences Panel at Yucca Mountain.

DR. GLASSLEY: I'm Bill Glassley. I've been at Lawrence Livermore since 1986, principally guiding the geochemistry and mineralology effort there. Most of that effort is focused on laboratory and field based studies to establish geochemical interactions, rock water interaction and trying to couple hydrology and geochemistry to come up with some kind of fully coupled code.

MR. HERKELRATH: I'm Bill Herkelrath and I've been with USGS, Water Resources Division, since 1975. I started out on the geothermal program and then until about '83, I was working funded under what we used to call Nuclear Hydrology, and mostly doing laboratory studies of flow, high temperature flow in rocks. And the last five years or so, I've been working on a multi-phase flow in another application with contamination caused by organic liquids.

MR. JOHNSON: My name is Carl Johnson. I'm the manager of technical programs with the Nevada Agency for Nuclear Projects. For those who don't know, the Agency for Nuclear Projects is the agency that is responsible for the state's oversight of the high level waste repository program. My background is in geology and hydrology. I've been involved in this particular program since the passage of the Nuclear Waste Policy Act in 1982.

DR. LANGMUIR: Thank you, Carl. On your agenda sheets, there are several bullets listed. Let me re-cast one or two of them and make those the starting point of our discussion.

What I'm going to do is organize the way we deal with this in terms of the way the day proceeded. So we'll start with a discussion of analogues and then move to models, and then perhaps talk about the interface between them and the information that modelers are needing to improve their models.

I made a statement this morning which perhaps could be the basis for some discussion, and the statement was this. It seems likely that geothermal analogues can provide us with essential spatial and temporal information on potential repository behavior that cannot be obtained solely from site characterization data, heater tests, coupled process experiments and calculations related to computer modeling efforts. So if that gets someone thinking--Bill is smiling over there.

DR. GLASSLEY: A number of things came to mind during the discussions this morning, and one of the things that I think is really crucial in what we do today is try to establish what it is we mean by natural analogue.

One of the things that bothered me very much this morning was that although the information that was presented was extremely interesting scientifically, the presentation of geothermal systems in general as natural analogues to the repository I think is flawed for several reasons. First, in a geothermal system, you're dealing with, in most cases, relatively large magma bodies. Those magma bodies have been present for a long period of time. They've been dumping tremendous quantities of heat into the system, much more than the repository is going to. They operate for many, many orders of magnitude longer time periods than the repository is going to.

But even more important, and I think this is probably the key thing that needs to be addressed, is the fact that they represent a tremendous, absolutely tremendous reservoir mass material added to the natural system into which the magma bodies have--sulfur, chlorine and a whole host of other constituents are added. They're the things that result in the acidic nature of the solutions that often operate in hydrothermal or geothermal systems. And that is, in many respects, fundamentally different from the way the repository is going to operate. The repository is not a source of mass and it is not going to affect in any geochemical way the things that are similar to what one normally sees in a geothermal system.

On the other hand, geothermal systems can provide us with a superb natural laboratory to understand the kinds of processes that are going to take place in the repository, rock water interaction, dissolution precipitation kinetics, hydrothermal flow and fracture dominated flow, distribution of flow between matrix and fractures. It gives us an opportunity to measure the rates of those processes if we can find the kinds of systems that are operating today where those processes of concern can actually be measured today.

The other thing that's important is that many of the things that have been talked about as far as geothermal systems are concerned are systems where we come in after the process has been going for a heck of a long time. The repository itself is going to be kicked hard and fast for a short period of time, and it's that initial perturbation of the system that we need to understand. Most geothermal systems don't give us that opportunity. So I think we need to be really careful about what we're talking about in terms of the natural analogue to the repository overall.

Geothermal systems I don't think represent that. They represent the superb place to understand the processes that will be important, however.

DR. LANGMUIR: Let me ask you further then, how about Yucca Mountain itself as an analogue? They're presumably doing more than isochemical experiments, simply heating the system as you would with a repository.

DR. GLASSLEY: Well, I think the work that Dave Bish presented is along the lines of what we need to be doing. If we're talking about establishing a natural analogue for repository behavior, we need to find environments like that where the processes he was talking about, the conditions, the time duration are appropriate. And I think what he described provides some indication of what we need to be looking for. His work addresses most of the issues we need to be addressing when we're talking about natural analogues.

DR. LANGMUIR: Any further comments? I've been reminded that each of us at the panel should identify ourselves for the recorder before we speak. Carl Johnson?

MR. JOHNSON: Yeah, Carl Johnson. I guess, Don, in responding to your question, I guess my concern would be where you would take natural analogues. My concern is if natural analogues are used to make decisions about the Yucca Mountain site and loading decisions in the absence of having complete test results or fully characterize the site itself.

I really have concern about using analogues as a substitute for characterizing the site.

And to follow up on what Bill just said, I guess my reaction to Dave Bish's presentation was that the work that he was doing and future work he was proposing to do, in my view, would be site characterization of the site. It would not be in the context of evaluating some possible natural analogue. Since it's at the site, it would be part of the natural characterization that should be done of the site to fully understand the site conditions and the processes that are going on there.

DR. LANGMUIR: Although he's reconstructing the thermal history of the site, which is a little different. Maybe Dave would like to comment about that, if he's still with us.

DR. BISH: Dave Bish from Los Alamos National Laboratory. I think in a sense what you say is true, Carl, in a way. It does involve, to a large extent, a lot of site characterization. In fact, I've commented in the past that one of the advantages of using Yucca Mountain as a natural analogue to repository induced alteration is the fact that we have a tremendous amount of information on the site. It's much more difficult to go to another potential natural analogue site for which we have little or no information, maybe one or two drill holes, and do a comparable amount of work, or a comparable amount of useful work.

So, in a sense, you're right. But I think the way I would state it is that the reason we can use Yucca Mountain possibly as a natural analogue to repository induced alteration, the way Bill Glassley just spoke about, is because it's coupled with site characterization, and we can use the information that we've obtained during site characterization and that we are still obtaining to help us.

DR. LANGMUIR: Bill Melson.

DR. MELSON: I'd like to throw this out to a number of the speakers, this issue of over pressures. In the geothermal studies and in volcanic studies, we're used to seeing over pressures, i.e. pressures in excess of 100 bars within 100 meters of the earth's surface, and the failure from explosions that go with that. And a lot of additional thinking, the existence of such over pressures are not factored into the models, so I'd like to throw out to the various speakers the suggestion that Gudmundar did, that shallow over pressures could be developed, displaced situations where potential high pressures in a sealed system could be moved upward to a depth where rocks would fail and you would have a phreatic explosion or a small explosion.

Now, this doesn't mean necessarily disruption in the repository, anything of that sort. But it's a fairly dramatic process and I'd like to hear people, especially numerical modeling people, talk about over pressures, how they might develop and what they might be.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. We've looked at, as I was showing you, a wide range of permeability from the case where we have no fractures, all the way up to where we have literally 3000 micron or 3 millimeter fractures. And if I could show this, it would be more apparent. If we have no fractures at all, we can build up pressures, giving the most recent cases we're analyzing for the project, maybe 18 bars. That's assuming no fractures at all. And that 18 bars is really limited over a very narrow range right at the repository horizon, and the pressure gradients are very steep. It drops off to below ten in a very short distance. Personally, I don't feel that it's possible that we could build up enough pressure.

DR. MELSON: Well, Gudmundur suggested this possibility, and maybe he could respond to that. I mean, 19 bars is not very large, or 18 bars.

DR. BUSCHECK: This is assuming a repository that gets up to 200 degrees C. for over 10,000 years. And as I said, we don't have any fractures at all. If we use a permeability which is comparable to the East Olkaria field, we get a maximum pressure on the order of one or two bars at 5 millidarcy. And I think that that's a very--I doubt that we're going to have a system that's 100 per cent steel. One of the things we have to consider is that we may get the ceiling above where we get refluxing, but what happens with the condensate that drains below? Does that plug up the fractures below the system? I think that that will be much less likely since there won't be refluxing by gravity below the dry-out zone.

DR. MELSON: Is everyone in agreement with this model calculation that he did? I mean, that seems pretty low, given the rate of heat generation of these canisters.

DR. LANGMUIR: Karsten Pruess?

DR. PRUESS: I'd like to make a comment on the potential over pressuring. The way I look at it is more how can we proceed to possibly exclude the possibility of, you know, very large over pressuring. And, clearly, the conditions, it is not hard to state the conditions under which large over pressuring is possible if we have no fracture permeability at all available, either because locally it doesn't exist or because it gets plugged up, then pressures can rise to the saturation pressure at whatever temperatures you drive the repository to. And so if you drive it to 250 degrees C., then pressures can rise to 50 bars, and then if we include some kind of fault zone connected to the repository, then potentially these kinds of pressures can be transmitted to shallower depths.

Now, this may be a very highly unlikely scenario, but my question would be exactly what kinds of tests should we do to put this scenario in its place of unlikeliness, as I think it belongs. I think the issue is one of how well is fracture permeability connected large scale, and I would hope that we can find that out through numeric testing in the exploratory shaft facility, and this should be one of the easiest scenarios to put to rest.

MR. HERKELRATH: Would you agree that there's still a lot of disagreement between geologists about when these phreatic explosions have occurred in the past and where they've occurred. I mean, we don't really understand everything about what's happened in the geothermal systems. We can't predict whether they're going to occur in a given area within a geothermal system at this time, I don't believe.

DR. MELSON: You can't predict them, but they're a possibility, which in this game, we have to address.

MR. HERKELRATH: All I'm saying is the mechanics of all that I don't think--people don't agree on the mechanics of how phreatic explosions occur.

DR. LANGMUIR: Bo Bodvarsson?

DR. BODVARSSON: I just want to make one comment. Like Karsten said, I don't believe that it is likely to occur at Yucca Mountain, but I think there is a potential for it to occur at Yucca Mountain. So the only thing we have to worry about is just--it's not enough to do phreatic gas testing because, for example, the expansion of the rock matrix flux might totally go into the fractures and make them impermeable at times. But if we just monitor pressures close to the repository, the gas pressures, we will soon find out it's a race with time, and if they start to exceed 10 or 20 bars within the repository, the thing to do would be to drill and relieve some of the pressures if we could.

DR. LANGMUIR: Bill Murphy?

DR. MURPHY: Bill Murphy from the Center for Nuclear Waste Regulatory Analyses. I'll make a couple brief comments. One is that in many geothermal systems, the over pressuring is due to mass transfer and mass transport of material and the formation of some kind of cap, a silica cap, or other material cap. If that is to occur at Yucca Mountain, I think it would probably require some relatively vigorous recycling or heat pipe effect, if it would occur at all.

I'll repeat; in my calculations, I showed a relatively small mass effect of the geochemical reactions I predicted. And having looking and stomped over Yucca Mountain, it's an extremely fractured environment in most of the units, and this allows me to make a point on analogues which occurred in the discussion of thermal analogues.

I think it's critical to look at the chemical situation of the analogue because that makes a tremendous effect on the hydrologic effects, as well as the chemical effects. And I'll draw to the Board's attention a natural analogue study that was sponsored by NRC in which a contact zone was studied where an obsidian flowed up against the non-saturated silicic tuft. This was conducted by Krumhansel and Stockton from Sandia, and they searched for evidence of mass transport in that contact zone where the temperatures got to several hundred degrees and were very hard pressed to detect any. They saw a little mass transport of some volatile species, fluoride and chloride, but there was not the kind of mass transfer that would be required, I think, to generate some kind of pressurized cap.

DR. LANGMUIR: Ben Ross?

MR. ROSS: Yeah, I'm a little concerned that all the pretty pictures of the explosions in Bo Bodvarsson's talk might have obscured another point he was making, which in my mind is much more important, extremely important, and that's what he said about the ubiquity of heat pipe effects in geothermal systems.

Now, let me get a little ahead of us and talk about modeling to say why that's so important. One of the big unknowns in all the modeling is the question that Karsten Pruess raised in his talk and, in fact, has been talking about for ten years. He had a student, Chris Dody, whose work emphasizes that a lot, is the question of whether water can flow in fractures if there's a substantial suction to drain the fractures. This is extremely important because you'll get much, much more effective heat transfer if the water can flow in the fractures when they're drained, or partly drained.

Now, this assumption gets hidden in your model because it's hidden in the shape of the relative permeability curve, and nobody can measure that curve directly on a large scale, as was also pointed out earlier. Most of the calculations that everyone's relying on are based on the assumption that with a very small amount of suction, you drain out the fractures, and then all the flow is through the matrix. And the matrix in the welded tuft is not very permeable, so you have to work real hard to get a heat pipe effect. You can get it, but you have to work real hard.

Now, if you have the same kind of curve in a geothermal system fracture, then you should not be getting all these--I don't think you should be getting all these heat pipes that were told you're getting. Now, there are differences, as Bill Glassley pointed out, between Yucca Mountain and the geothermal systems.

One he didn't mention that may be important is the presence of a lot of dry air constituents. But if you see this, you know, a lot of this is the mechanics of flow in these fractures, and I think it puts a burden on people who want to rely on conclusions from one of these models, what Karsten and Chris Dody called sequential saturation, where you just drain a little bit of water out of the fractures and then there's no more flow in the fractures.

Before you rely on conclusions from models that assume that, you've got to show why these observations in the geothermal systems are not relevant.

DR. LANGMUIR: Let's move on into the modeling, unless Duane Chestnut has something to say.

MR. CHESTNUT: A couple things that I'd like to address, if I could.

DR. LANGMUIR: Duane Chestnut, USGS.

MR. CHESTNUT: This last question I guess is one that kind of follows me from a fuel standpoint. What do we have that tells us we have heat pipe? We have more or less a constant temperature result. And Bo has pointed out that one thing we don't know is the liquid saturation in that system. Until we can come up with some way of getting some in situ measurements of liquid saturations, I think the whole concept of heat pipe almost is a model artifact, or at least it has to be considered as a possibility.

And the other thing I don't think has been emphasized enough is this problem of an explosion. As Karsten pointed out, the limit is going to be the saturation pressure of the steam at whatever maximum temperature we could reach in the system. And in the repository, under any thermal loading scenario, we are not talking about temperatures much in excess of 250 degrees C. That corresponds to about 500 pounds per square inch steam pressure, which is about what is available at the geysers. And I don't know of any surface evidence that we've seen ruptures of hydrothermal venting any place in the whole geysers area. We've got a huge steam chest under there that is driven by many orders of magnitude more heat flux, or more total stored heat energy than we're ever going to have in the repository.

So I think we have some natural analogues that may help us get rid of this problem of a possible explosion, at least it seems to be consistent with what we see in terms of, like you say, the geysers.

DR. LANGMUIR: Bo Bodvarsson?

DR. BODVARSSON: Just to answer that question a little bit, I disagree with you a little bit on this. I think the pretty pictures I show about the eruptions and explosion, I agree with that, Ross, totally that this is just something that we should have on the back of our minds and is not worth discussing. I think it's much more important the point that Ben brought up about the heat pipes. I'm very concerned about them.

And to respond to your comment, if I understand it correctly, we know heat pipes clear from the systems for the one reason is that we know heat is coming out on top. We know it's isothermal in the middle, so convection doesn't take place, so the only possible way of carrying the heat is by heat pipe. So it's a known fact they occur, and I think they are extremely important analogues for us to look at because we can never look at them with heater tests on that scale.

With respect to Bill Glassley's comment about geothermal not being a good analogue, in looking at heat pipes, they're the perfect analogues. They're the only possibility we have to look at large scale heat pipes totally. But I'm not addressing the issues of geochemistry because you know more than I do, so I don't dare go into that theory.

DR. GLASSLEY: My point wasn't that they weren't useful for studying specific processes such as heat pipes. My point was in fact that that's what geothermal systems are well suited for. What they are not well suited for is repository scale natural analogues. And that we have to be very careful about. We can't treat geothermal systems as analogues in any way, shape or form for the way the repository will behave. But for understanding particular processes, great.

DR. GARG: Sabodh Garg. Two systems may be meaningful to Yucca Mountain in another sense, that we know from fractured geothermal system that the permeability is very heterogeneously distributed. Heterogeneity is not something that happened; it happens all the time in volcanic geothermal systems. We know that the major fractures would conduct fluid through one fold, or a difference of one fold, a space in the several hundred meters.

From the Yucca Mountain study that I did for the saturated zone, I see that the fluid where the bore holes were connected to the water table reservoir were also discrete in terms that they were not really homogeneously distributed.

So having said this, I go back to Karsten Pruess's presentation where he pointed out to us because of heterogeneity there is real question if this started, would it work. I think perhaps we should go to geothermal systems, look at questions like heat pipe, heterogeneity on the scale of one-tenths of meters to hundreds of meters, what's going on there and what its implications are for Yucca Mountain.

DR. LANGMUIR: Let me shift us a little bit here, but go more towards the models, we're already there anyway.

Karsten Pruess showed a table which was presented in his talk in Las Vegas which intrigued me as a neophyte in this business of geothermal analogues anyway. The table defined characteristic times, and this was mentioned in his talk today, 200 hours was suggested for--I'm sorry, hundreds of days for gas flow was the predicted kind of characteristic time for gases in Yucca Mountain, 200,000 years for liquid flow was suggested, and so on, the point being that you have very different characteristic flow rates for the different processes and energies in the system.

Now, this suggests to me that we have real problems with heater tests in terms of using information from them on a small scale to predict the mountain's performance. Now, there's been discussion of using several heaters in different places under different conditions, and perhaps this helps us out in that regard. I guess my question is do we have scale problems with either test that are going to leave some gaps in our knowledge, and how do we fill those gaps up when we wish to validate the models for application to the Yucca Mountain performance?

DR. BREDEHOEFT: John Bredehoeft from the USGS. It seems to me that one of the things that perhaps was missed in Bo's presentation was that our experience in the petroleum industry and ground water, in the geothermal business, is that when you do some kind of a history match, a calibration of the model, you then make some projection into time as to how the reservoir is going to behave. But usually that projection is of the same order as your history match, so if you match for five to ten years, you might be willing to project that reservoir for 20 to 30 years. And we're in an entirely different game. We're trying to do some kind of a history match which says, hey, indeed we've got the right conceptual model, and then use that model to project system behavior out to a thousand years.

You've got very difficult problems, and it seems to me that the time scaling, you've got both the spatial scaling problem in running a heater test, and a time scaling problem, and particularly with respect to some of the geochemical things. Are these things going to be long enough to see the kind of nonlinearities that you expect from the geochemistry. I think we've got very serious problems.

DR. LANGMUIR: Any comments? Tom Buscheck.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. In scoping out our heater test, we're not saying that all issues can be solved for all time. The one issue about the 200,000 years I think pertains to the lag in re-wetting time back to ambient conditions. The question could be asked can we get adequate information about, say, the first five or ten thousand years when that re-wetting is far from having progressed to ambient, but we can still proceed with the license.

There are some very critical issues that we need to get early diagnoses. One of the things I think is a misconception about the heater test is that the modelers are going to be handed on a silver platter some data, we're going to run our codes, we're going to run the heater test, and if they don't match up, you know, we're out to lunch. And what I think the heater tests are going to be more useful for is to actually diagnose which of the major thermal, hydrothermal regimes, we're going to find ourselves. I look at them as diagnoses, means to diagnose how the system is going to perform.

We're trying to map out what the possibilities are, and then we're also trying to show what types of signatures will be indicative of those various regimes.

One of the advantages to having something smaller than the repository is that if you're trying to show that the effect of mountain scale buoyant convection isn't important, well, if you were going to run a test at the scale of the repository, you'd have to be around, and Ben would concur, around a thousand years to confirm that. If you run a heater test at about one acre, the effects are manifested almost instantaneously. You will see those effects at that scale. So there are some advantages actually to running tests at smaller scale.

I agree that there would be some disadvantages if in fact one were to argue that only these predominate heat pipe zones are going to dominate the entire hydrothermal performance and whether you happen to be located there or not is an issue. But I think that we're going to learn enough about heat pipes at other scales that will give us better information about whether these heat pipes at the larger scale may be prevalent.

Now, I want to make one statement about the heat pipes. What I would like to see is a validated, you know, a history match geothermal model. I would be happy to give the thermal history of a repository. Let's put that thermal history in a history match geothermal system and see what heat flow regimes are prevalent. The heat flow, the thermal history of a repository is vastly different than a geothermal system. You have a very rapid spike and a very rapid decline and the bulk of the moisture movement driven by boiling occurs in a thousand year time scale. So I think there's some questions about how directly applicable the heat loading conditions of a geothermal system are to a repository, and I'd like to see some, you know, efforts to apply the repository thermal load to geothermal models to see if there are any quantitative or qualitative differences.

DR. LANGMUIR: Dale Wilder.

MR. WILDER: Dale Wilder, Lawrence Livermore. I think the point that you raised is a very good point, and a couple things we need to clarify if it hasn't been made clear in the past, and that is we don't expect heater tests to give us all the answers and there will be holes. As you pointed out, there are probably some holes that we won't know the answer after heater tests, and we probably won't know all of the answers even after the long-term performance confirmation.

What we have tried to develop is a strategy in which we can build our confidence, but we'll never have a guarantee that we understand some of those processes. I look at it very similar to what Bill Glassley said about the use of the natural analogues. We can look at specific questions, and some of those questions I think Tom has shown before in terms of can we recognize if we're under conduction dominated versus convection dominated. Some of those processes are rapid enough that we can't see them, and it's because of the vapor transport.

Some of the other processes, like this large hysteresis between the re-wetting and the temperature collapse we are not going to see in a heater test. One of the concerns that we have and the reason that we've put estimates for as long as we have on the heater test is that we recognize that some processes are very slow, and we don't have much time to look at the cool down in most of these abbreviated tests.

If we do have a period of time before closure, and I'll talk about it tomorrow, somewhere between 50 to 200 years, we can start to look at some of these large scale heterogeneity questions, and we can start to get a handle on do we have things like heat pipes. But I think we need to all recognize that the task that we're given is a very difficult task, that is, trying to predict performance to satisfy regulations up to 10,000 years or perhaps even longer, and at best, we're going to be able to monitor for a couple hundred years. So I think that we would be fooling ourselves if we said that those tests are the skill necessary to answer all the questions. They just won't be.

DR. LANGMUIR: John Cantlon.

DR. CANTLON: John Cantlon. Now, let me ask Dale before you get back, let me ask if one looks for the geochemical signature of cool down, the issue you just raised, you have a small cone out here very close by and very similar belief, shouldn't there be a set of concentric geochemical markers? You have fairly accurately dated heat source out there and the cool down, wouldn't that also give you a little bit better understanding of some of the chemistry that one might anticipate around the heater, around the repository?

MR. WILDER: I think that that's one of the best uses of an analogue, to allow us to look at some of these long-term effects. Now, I'll have to ask Bill Glassley for a little insight in terms of what we see geochemistry-wise. But certainly we ought to be able to look at the analogues for some of those longer term phenomena that we can't see in the heater test. There's no way we can do it just with heater tests.

DR. GLASSLEY: Bill's right, the heater test will be inadequate for looking at that kind of thing, and the natural systems, I don't want to use the word analogue, the natural systems are probably the best way to get a handle on that kind of thing. But it seems to me what the real use of a natural system is going to be is a means of testing our ability to simulate process. The conditions could be radically different from what we expect in the repository. But what we need to be able to establish is that even under those broad set of conditions, our modeling capabilities are adequate. That's really the key.

DR. LANGMUIR: On Bill's comment, it seems to me we already have a comfortable--I have a comfortable feeling based, for example, on what we heard with regard to Bill Murphy's modeling effort and what we've seen from Dave Bish, that it's consistent conclusions on the nature of the phases that are created from the heating and the transport process within the rock. And we know pretty much what's going to happen in terms of mineral precipitates at different times and temperatures. We don't have much handle on the kinetics of those effects, perhaps, but thermodynamically, we have a sense that--I have a comfortable feeling that the models are saying the things we see.

MR. HERKELRATH: This is Bill Herkelrath. I just want to say that I agree with John Bredehoeft that really we tend to use the models more in a survey to organize the data that we've already got and make some fairly short-term predictions. But I think that in this case, as soon as you put 1000 years in for T, well, I don't believe you can do it, but nevertheless, we have to do the best we can and as a minimum, you have to run the heater test in order to verify the model that you've got, which has got a T of 20 years or some human time scale, you've got to do that.

DR. LANGMUIR: Bill Melson.

DR. MELSON: I'd like to make a comment on what you're speaking of as intrusive analogues, and Greg Valentine, as far as the volcanology program, is looking into the effects of disruption of the repository by dikes. I think he hopefully will dovetail that with what's going on here in terms of seeing what he can learn of emplacement of magma at shallow depths.

I should say, too, I think the volcanology program will be drilling some very intrusive--is that true Jean? And those may in fact be intrusive sheets. We don't know whether they'll be volcanos or intrusive sheets, and those may provide analogue information also of a different type, but certainly somewhat relevant.

MR. JOHNSON: Carl Johnson. I was a little bit concerned about what I just, or at least I thought I just heard from Dale Wilder, and it goes back to, I think, your lead-off question for this session, and it gets back to the opening presentations of this morning's session which had to do with the decision process in thermal loading, and I think there was quite a bit of questions and discussion about the need that we do site characterization and that we do these tests and have that information available prior to making a thermal loading decision.

Now what I'm hearing is we're not going to have all the information. There's going to be gaps in what we know and what we don't know coming out of the tests. And I think it gets to the comment that John Bredehoeft just made, and that's dealing with both scaling and timing, and I don't see anything in the program that's going to fill in these gaps, yet we're going to make a thermal loading decision without this information.

DR. LANGMUIR: Duane?

MR. CHESTNUT: I'd like to kind of address a little bit of what problems he's talking about. I agree very much with John that this is a major problem and how do we take the models and extrapolate them with any confidence.

Now, I would like to point out I think in the petroleum industry, there is a comparable time of extrapolation that people have to make, and that is in something like off-shore oil and gas exploration in some area like the North Sea, it may cost you, say, $100,000.00 a day to do a well test, and yet people make multi-tens of millions of dollars decisions based on a 15 day test. They're having to forecast the production of that reservoir over a period of about 40 years in order to make that decision.

The time scale between 15 days and 40 years is about the same as the pre-closure period and the 10,000 years that we're talking about repository operations. So even though it's difficult and the predictions are not precise, it's something that we have to do. We have no other way of trying to forecast the future.

Where we I think really need some measurable performance that we can extrapolate, that's what we are trying to grapple with, and I think the thermal behavior of the system may offer that possibility, something on a relatively small scale, a relatively short time, we'll be able to have some increased confidence in long-term repository behavior, especially if we tie it in to some of the natural system studies.

DR. LANGMUIR: John Bredehoeft.

DR. BREDEHOEFT: Let me make a few general comments about models in general. We've gone back in the ground water business and tried to look at cases where we modeled the system and came back years later as a post-audit and said how did the model do, and our success is not very good. It's rather bad, as a matter of fact.

Now, when you come back and you think about the model, I think you've got to be very careful about how you think about that model and where the errors come. And where do the errors come? Well, first of all, they come in conceptualizing the system because you need some conceptual model for which you're going to write the mathematics and solve those equations.

Secondly, it comes in how do I solve the equations? Am I solving the equations adequately? And, finally, once you have a conceptual model and you're solving some set of equations, there's some set of parameters that you put into those equations, and there's always uncertainty about the parameters, permeability, porosity, we don't have an adequate sample, there's heterogeneity, all these things enter in.

Now, when we've gone back and looked at our post-audits, the problem comes usually in the conceptual model. What is the appropriate conceptual model? And that is the most difficult part of the problem because you cannot validate the conceptual model. You can only invalidate it. You've got a conceptual model, you test it, you either accept it, you say it meets this particular experiment or it doesn't, and you throw it away, but then you're left with sort of a priori deciding what the next conceptual model is.

Now, it seems to me that it's all important if you're going to try to predict repository behavior out for very long periods of times, that you're reasonably comfortable that you've got the appropriate conceptual model. And I think that is the most difficult modeling task.

DR. LANGMUIR: John, we've seen today with Karsten's approach and Tom's as well that it was too complex a system to use the codes as written to describe all the complexities. So you take different approaches; you simplify it to deal with certain aspects of it, you make assumptions of various kinds so you can deal with it on the computer. And maybe I'll ask Karsten this. That obviously biases what you're going to learn, and does that prevent you perhaps, can you, even if you have a conceptual model, if you can't paramaterize it and model it, what have you gotten yourself?

DR. PRUESS: I completely agree with John Bredehoeft that the conceptual model is the crux where you could make the biggest mistakes without having any clear-cut way of learning about it, and in this case, over 10,000 year type performance. I also agree with him that in applications like ground water, petroleum, geothermal and so on, we usually hesitate to forecast for much more than the calibration period that we already had to calibrate the model to.

And so looking just at that, one might think, well, this problem is just daunting, it's overwhelming, we can never hope to calibrate a model to a significant fraction over a 10,000 year performance period and then hope it will do fine for the rest. But I think a sort of simplifying aspect that comes in here is that in the modeling of Yucca Mountain repository performance, in some sense, it's a great deal more that's asked of us than, for example, in geothermal, but in some sense, it's also a lot less because we are really not asked to predict the exact rate at which all these different radionuclides will be delivered to various parts of the biosphere in the future. Often times, it will be good if we can have bounding calculations, you know, that often times will be good enough.

And having said that, I also want to say that I'm, you know, I don't think we should throw out our calculations. I think there is nothing as convincing as a sound understanding of system behavior based on an analysis demonstrated, an analysis of mechanisms. So I don't want to throw that away either. But I would say that for many aspects of the repository behavior, it will be, you know, if we can have calculations that show that even if we have these task paths and even if we have ponded water rushing in and even if we have this and that and the other, we have other barriers, a multi-barrier system that will not be affected by that and we can bound releases, we're good enough, quote, unquote, I think then we still can have it made, even if we fall very much short of a realistic prediction into the future, you know, the way you would predict sort of an eclipse in, I don't know, 20,000 years.

DR. LANGMUIR: Jean Younker?

DR. YOUNKER: Thank you, Karsten. He really made the point that I was going to make, but I'll just recap a little bit on what he just said. And I think that was my concern in listening to the discussion the last few minutes, was that it sounded like we felt it was the role of this program to solve all of the earth science process questions related to hydrothermal systems that exist today, and I don't think that's really where we're heading.

I think Karsten's exactly right. What we need to know is enough to get some confidence in bounding calculations, because clearly it's not the role of this program to fund all that basic science and let us reach resolutions, as much as earth scientists like myself might like to do that. It's exactly the point that Karsten makes, and I think it's getting to the point where we have enough confidence that the abstracted modeling, the abstracted codes that we'll use to predict repository performance in terms of actual safety of the system to build our confidence in those, and that's that balancing point that I think we all realize we're following or trying to get at, which is how much is it we need to know about the process models before we have enough confidence in our abstracted models to believe the results that we're getting that show how the site really appears to perform when you look at the combination of the engineered barriers and the natural system.

DR. LANGMUIR: Dale Wilder?

MR. WILDER: I could say amen to what has just been said about the use of some of our models. I'd like to try to clarify perhaps what I had said earlier, because I noticed Carl was a little concerned about our trying to make decisions based on what I said would be incomplete understanding.

As I look at many of the tests, and I think G-tunnel was a good example of this, it's our opportunity to try to check that our conceptualization is at least somewhat representative. And that was, I think, the biggest value of G-tunnel. It allowed us to look at some of the things like condensate drainage. Admittedly, it was not necessarily representative of the repository, but at least it pointed out some of those phenomenology issues that we had not incorporated as significantly as we should have in our conceptualization. And I think to a large extent, that's what our heater tests are going to do for us, to make sure that we've got that conceptualization right.

When I said that the information would be incomplete, I mean we need to be up front, we are not going to have the opportunity to monitor performance for 10,000 years. So the best we can do is try to gain confidence that we understand the system well enough that we can proceed at some risk. That doesn't mean that we aren't going to understand some of the phenomenology well enough to make thermal decisions and so forth. I think that we will have that kind of information coming in, not 100 per cent, but certainly a lot of it coming in.

Heterogeneity now is another issue, large scale. Now, we won't have that at the end of, for instance, the large block test. And so the only point I was trying to make is that I hoped that Livermore was not giving the impression that we think that the heater tests were going to answer every single question, and that that's why we're doing them. We recognize that there will still be some uncertainty. We don't want to oversell them.

DR. LANGMUIR: Ben Ross.

MR. ROSS: I just want to add one point to that, which is that the thing that looks like it's hardest to get in the heater test, which is the mountain scale buoyant flow, is probably the thing that's easiest to believe the models. Now, there's complications, you know, you can argue about fracture flow, fracture plugging and so on, but if the fractures stay open, you know, everyone knows hot air rises, it's something that we can do other tests, you know, pneumatic tests on the mountain under present conditions without even heaters. So it's something that you can get at by another method, I think.

DR. LANGMUIR: Tom Buscheck.

DR. BUSCHECK: Tom Buscheck, Lawrence Livermore. I think that the heater test in terms of resolving a mountain scale convective problem can be done sort of by some sort of a logical test. If you run a number of heater tests and you don't observe the effects on the small scale, I think it's arguable that there would be much less likely at a large scale. You cannot find rock which is locally connected that could support significant buoyant convection. By significant, I mean moving significant quantities of water vapor. And I think it's arguable that it won't happen on a large scale.

I have a lot of heartburn thinking about being handed data from pneumatic tests and being told then to run reference calculations for large scale buoyant convection. We talk about how heterogeneity is the most difficult aspect of this whole problem, especially for transport and for liquid phase flow. But as far as buoyant convection is concerned, I think if we have anything like what is seen in Stripa and other places where the bulk of the permeability is in a few pathways, you could literally--well, the way I put it is you waste your permeability on a few pathways. But in order to develop large scale buoyant convection, you have to develop coherent large scale cells.

So I think that if we have measurements of bulk permeability which are dominated by a few features and apply them to models which homogenize that effect throughout the entire unsaturated zone, we could calculate an effect which in reality is just not there. So I think much of the rock, the permeability could be very low with respect to buoyant convection, and I think the only way to test that is to do relatively large scale heater tests. Pneumatic tests will give you isolated connections, not integrated.

DR. LANGMUIR: Ben Ross.

MR. ROSS: I disagree with that, I think. You know, the nice thing about air in Yucca Mountain as opposed to water is that you can see the air move, and it's infinitely easier to measure its movement. And under present conditions, you can look at barometric pressure down hole and there are probably other--I'm sure there are other things you can do, they're being talked about, and you can get large scale measurements on gas flow and it doesn't take you anything like five years, either.

DR. BUSCHECK: Could I correct what I said? I was referring to packer tests. I was not referring to the types of tests that Ed Weeks has been analyzing. So I agree with you on that.

MR. ROSS: The problem is going to be, as you said, the smaller scale buoyant flow. There, you may get good information out of the heater test. But I guess my overall point is that it's easier to measure air movement in the mountain than it is water movement.

DR. BUSCHECK: I agree with that.

DR. LANGMUIR: Sabodh Garg.

DR. GARG: Geothermal systems suggest that heat pipes, typical dimension are hundreds of meters. Can you really afford to do heater tests that will be that scale? Can you move that out on the basis of bigger tests?

DR. LANGMUIR: Any responses?

DR. GARG: Typically, a heat pipe in a geothermal system is the order of a hundred meters--

DR. PRUESS: Karsten Pruess. I thought there was a rhetorical question that you posed. Obviously you cannot--I mean, heat conduction gets you 30 meters in 30 years, and so you cannot get at the larger structures with heater tests. You have to rely on natural systems.

DR. GARG: Well, if that's the case, then I think, you know, we need to go back to systems like the systems which have--where we can observe that. And in that context, I think the proposal that Bo made, possibly the geyser area, which is perhaps the closest analogue to Yucca Mountain that I know of, makes a lot of sense. Now, you know, there was talk earlier this morning, at least one view graph was seen that people wanted to use New Zealand for natural analogues. I don't really see the relevance of New Zealand fields to Yucca Mountain. Those fields are either liquid dominated or two-phase. They are not a predominated system, which is sort of the condition that we have at Yucca Mountain.

DR. LANGMUIR: Abe Van Luik?

MR. VAN LUIK: Abe Van Luik, M&O. Two very small points; one is that the reason I think we had the first couple of talks this morning and the last couple of talks tomorrow afternoon is to put this discussion into a context. The decision on thermal loading is going to involve the work that we have reported on this afternoon as partial input. Some of the other things that were on Bill Simecka's view graph I think were the considerations of the preclosure, safety of the workers, retrievability, et cetera, and some of these things may actually dominate the final decision that's made.

Another point John Bredehoeft just pointed out, you know, the uncertainties that multiply with time. This reminds me of an international meeting that I took part in where this same discussion was held in the presence of total system performance analyzers who had included the biosphere and who scoffed and said that our biosphere uncertainties, future populations, climates, et cetera, swamp your geologic uncertainties by two orders of magnitude, so why spend any more money on the geosphere.

So I think there are contexts that we have to keep in mind, and we shouldn't despair that one particular aspect of things cannot be nailed down to the eighth decimal place, because in the larger context of things, it is having confidence in reasonable people's minds that we have made a best estimate and that we've been conservative.

Thank you.

DR. LANGMUIR: John Czarnecki?

MR. CZARNECKI: John, I'd like to return to your comment that we know the kinetics or we know the systematic response of chemistry to heat. I did some simulations using a code called PHREEQE, it's a USGS code, and took J-13 water and subjected it to increases in temperature based on what some of Tom Buscheck's simulations showed, and with these elevated temperatures in the saturated zone, one sees precipitation of calcite at the elevated temperatures.

Now, a question I would ask is could the permeability in the saturated zone be reduced such that it impedes flow up gradient from the reduced permeability zone? If that is the case, one could conceive that the reduced permeability could cause a rise in the water table, and I wonder if that question should not be addressed a little bit more carefully.

The reason for bringing this up, I had experience in Minnesota on an aquifer thermal energy storage project where we were taking heat, applying it to ground water, reinjecting it into the ground water after passing the water through a heat exchange. I did a similar type of analysis taking that water, running it through PHREEQE and looking at the effect on the chemistry. It showed calcite precipitate. Some people said so what. Well, we found out so what when we did the experiment. We pushed water through the heat exchanger and it clogged the entire system, taking weeks to decommission and clean out.

Now, this may be a so what type of scenario, but I think it merits attention, and I haven't heard any discussion about effects in the saturated zone.

DR. LANGMUIR: Let me ask you something while you're still here, John. My sense would be that the concentrations of calcium, which would limit the amount of calcite you could create, would be such that even if you filled the-- precipitated all of it out, you'd only remove a few per cent of the porosity of the rock.

DR. BREDEHOEFT: Can I comment on it?

DR. LANGMUIR: Sure.

DR. BREDEHOEFT: Based on the different ranges and estimates for porosity that we have, and I used something like 10 to the minus 5 to 10 to the minus 2 for fracture porosity, we come up with ranges in clogging times from 50 to

5,000 years, something like that. And it's mainly a function not so much of the, I used bicarbonate concentration, but it's more a function of what the known porosity or effective porosity would be for the system.

DR. LANGMUIR: We're at 5:35, which was the originally decided closure time for the panel. Admittedly, we started late. If there are still burning questions and issues, I'm willing to stay a little longer. If not, I see a lot of tired faces.

DR. PRICE: Could I make just one question to the panel and not much of a question, but a couple of weeks ago, I was at Avignon at the Safe Waste '93 program there that was being held. They had a panel addressing the question can we design a repository for 10,000 years and design it with confidence. And one person on the panel rose to say that without natural analogues, we're dead, and let it drop at that. I wonder if that's in agreement here today.

DR. LANGMUIR: I like that. I think we'll stop there.

Thank you. I'd like to thank the panel members and the speakers of the day. We reconvene tomorrow morning here at 8 o'clock.

(Whereupon, at 5:35 p.m., the meeting was adjourned.)